

Short-term predictive skill does not imply identification of long-term tree-growth trajectories in an Amazonian high-várzea forest

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1 **Short-term predictive skill does not imply identification of long-term tree-growth trajectories in an**
2 **Amazonian high-várzea forest**

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15

16 Abstract

17 Short remeasurement records may support accurate near-term forecasts without identifying the long-
18 term trajectories required for forest-management projections. We evaluated age-independent Richards and
19 Lundqvist–Korf equations using 6,487 survivor intervals from 1,602 trees and 192 species across six
20 permanent 1-ha plots in an Amazonian high-várzea forest. Spatial generalization was assessed by leave-one-
21 plot-out cross-validation, with leave-two-plots-out and progressive future-interval validation as stress tests;
22 persistence was the no-growth benchmark and Huber estimation tested sensitivity to heavy-tailed errors. For
23 the community, leave-one-plot-out RMSE was 1.570 cm for Richards and 1.569 cm for Lundqvist–Korf,
24 versus 2.066 cm for persistence. Corresponding values for commercially listed species were 1.643, 1.641, and
25 2.265 cm. Baseline basal area did not improve generalization, and spatial, temporal, and robust-loss analyses
26 supported the same short-term conclusion. However, support declined with initial diameter, the asymptote was
27 not point identified, and projected trajectories became increasingly sensitive to growth family, asymptote, and
28 estimator beyond the longest observed interval of 3.67 years. These models support short-interval survivor-
29 diameter prediction within the monitored domain, but not lifetime trajectories or cutting-cycle inference.
30 Model qualification must therefore match the intended projection horizon.

31 Keywords: age-independent growth model; blocked cross-validation; model identifiability; permanent plot;
32 tropical floodplain forest

33

34 1. Introduction

35 Forest growth models translate repeated inventories into projections of tree and stand development, but
36 their adequacy depends on the decision and projection horizon for which they are evaluated. Model purpose,
37 performance criteria, and operating context must therefore be specified before qualification for an application
38 [1], and error measures should be selected in relation to the question being asked [2]. A model may predict the
39 next inventory interval accurately while remaining weakly informative about decades of subsequent
40 development, particularly in long-lived, structurally complex forests. This distinction is consequential in
41 Amazonian floodplain forests, where high tree diversity and environmental control by the annual flood pulse
42 generate strong spatial heterogeneity. White-water floodplains, or várzeas, receive nutrient-rich sediments and
43 generally sustain higher forest productivity than nutrient-poor igapó systems, while flooding duration,
44 hydrogeomorphic disturbance, topography, and soil fertility shape forest composition and tree life-history

45 strategies [3]. Growth determines how rapidly surviving trees move through the diameter distribution and
46 contributes to longer-term structural recovery after disturbance. Yet management regulations have commonly
47 relied on broadly standardized minimum cutting diameters and cutting-cycle lengths despite heterogeneity
48 among Amazonian sites, species, and forest types [4]. Site- and ecosystem-specific growth information can
49 improve the biological basis of management only when its interpretation remains within the temporal and
50 ecological domain supported by the data.

51 Predicting diameter growth in species-rich, uneven-aged tropical forests is particularly challenging
52 because chronological tree age is generally unavailable and individual growth varies strongly with tree size,
53 resource availability, competition, species strategy, and environmental conditions [5–7]. Age-independent
54 difference equations provide one solution by predicting future diameter from current diameter and elapsed
55 time rather than requiring chronological age. Richards and Lundqvist–Korf functions can be reformulated in
56 this way and have been used to represent tree or stand transitions in structurally complex forests [8, 9]. Such
57 models are especially attractive for permanent-plot inventories because they can operate directly on successive
58 DBH measurements. Their predictive usefulness, however, is not equivalent to identification of the complete
59 biological growth trajectory.

60 Permanent plots and tree-ring data contain fundamentally different information about time. Repeated
61 permanent-plot censuses can provide large numbers of individual-tree transitions and direct observations of
62 contemporary forest conditions, but monitoring commonly covers only a small fraction of a tropical tree's life
63 history. Tree-ring records, when annual ring formation can be demonstrated, may instead reconstruct much
64 longer portions of individual growth histories and have consequently been used to estimate ontogenetic and
65 silvicultural quantities in tropical forests [7]. In Amazonian floodplain forests, dendrochronological growth
66 models have even been used to derive species-specific minimum logging diameters and felling-cycle estimates
67 [10]. Those applications rely on information spanning a substantially larger fraction of tree development than
68 a short remeasurement panel. A model fitted to short transitions may therefore predict the next observed
69 diameter accurately while leaving parameters controlling long-term curve shape, particularly the asymptote,
70 weakly identified. Short-term predictive skill alone does not establish that lifetime growth or management-
71 cycle trajectories have been identified.

72 Permanent-plot data also have a hierarchical dependence structure. Successive observations are repeated
73 within trees, trees occur within plots, and measurements from the same period share environmental history.

74 Growth itself may also be serially correlated, as demonstrated for tropical trees by Martínez et al. [7].
75 Consequently, predictive evaluation based on randomly partitioning individual records can mix closely related
76 observations between training and test sets and answer a weaker question than operational transfer to another
77 plot or a future census. Validation should instead reflect the intended prediction task by withholding
78 meaningful spatial or temporal units [11, 12]. For forest models, this distinction is consequential because
79 environmental and structural differences among plots can generate prediction errors that are not apparent from
80 in-sample fit. Empirical coverage also matters: model performance in well-represented diameter classes cannot
81 automatically be extended to large trees for which observations are sparse.

82 Projection horizon creates a second inferential challenge. Alternative growth equations can approximate
83 the same local transition over a few years even when they imply different upper trajectories. Uncertainty that
84 is small over one prediction step may then accumulate or become structurally important when the model is
85 iterated for decades. Forest-modeling theory has long emphasized that model purpose and projection length
86 should be considered jointly [5], and recent simulation studies show that uncertainty associated with
87 incompletely characterized tree and plot effects can increase markedly over long projection periods [13]. For
88 management applications, the relevant question is therefore not only whether a growth equation predicts
89 observed short intervals, but also whether the information supporting that performance is sufficient for the
90 much longer horizon at which management decisions are often discussed. Where it is not, long-horizon
91 trajectories are better treated as conditional structural sensitivity analyses than as validated forecasts.

92 Here, we evaluated age-independent DBH transition models for surviving trees in a species-rich
93 Amazonian high-*várzea* forest using repeated measurements from permanent plots. Analyses were conducted
94 for both the tree community and a commercially listed species group, the latter defined by registry membership
95 rather than harvest eligibility. We addressed three questions. First, how well do Richards and Lundqvist–Korf
96 transition equations predict final DBH when entire plots or strictly future monitoring intervals are withheld,
97 relative to a no-growth persistence benchmark? Second, does adding baseline stand basal area improve spatial
98 generalization sufficiently to justify the additional structure, and do the two growth families differ
99 meaningfully in short-interval predictive performance? Third, how do empirical DBH support, robust-loss
100 sensitivity, growth-family structure, and weak asymptote identification affect conditional trajectories as the
101 projection horizon extends beyond the monitored intervals? We treated prediction over the observed
102 remeasurement intervals as the empirical target of the study; trajectories beyond that range were evaluated

only to diagnose horizon-dependent structural and estimator sensitivity. This forest-growth model evaluation combines blocked prediction and identifiability diagnostics to distinguish short-interval generalization from long-term growth identification, while treating extrapolated trajectories as conditional sensitivities rather than management forecasts.

107

2. Materials and methods

2.1. Study area and permanent-plot monitoring

The study was conducted in the Mamirauá Sustainable Development Reserve (approximately 03°09' S, 64°45' W), Amazonas, Brazil. The reserve covers approximately 1.124 million ha of Amazonian white-water floodplain associated with the Solimões, Japurá, and Auati-Paraná river systems [14–16]. The regional climate is humid tropical (Af), with mean annual precipitation of approximately 2529 mm and a mean annual temperature of 27.8 °C [17] and unpublished institutional monitoring data from the Instituto de Desenvolvimento Sustentável Mamirauá (2018). The study plots are located in high-várzea forest on hydromorphic alluvial soils, where seasonal flooding and sediment deposition are major controls of forest structure and dynamics.

Access to permanent plots and monitoring records was authorized by IDSM under cooperation arrangements with the Mamirauá Sustainable Development Reserve and State of Amazonas; field monitoring complied with applicable regulations. Dr. Darlene Gris (IDSM) undertook formal taxonomic identification. Reference vouchers are deposited in the Acervo Botânico do IDSM, including accession IDSM00634; the complete voucher list accompanies this revision. The study analyzed existing inventory records and did not experimentally manipulate plants.

Six permanent plots (plots 6–11), each measuring 50 × 200 m (1 ha), were established in 2013. Trees with diameter at breast height (DBH) ≥10 cm were measured at 1.3 m above ground. The field censuses contributing to the present growth analysis were conducted in 2013, 2016, 2017, 2018, 2021, and 2022, generating five successive growth intervals: 2013–2016, 2016–2017, 2017–2018, 2018–2021, and 2021–2022. Interval lengths ranged from 0.92 to 3.67 years. The latter value therefore defines the maximum empirically evaluated prediction interval in this study. The locations of the six plots are shown in Fig. 1.

130

2.2. Analytical population and analysis scopes

132 The primary estimand was the out-of-sample prediction of final DBH for trees surviving a monitored
 133 interval. Because observed and predicted increments share the same measured initial DBH, the final-DBH
 134 residual, $D_2 - \hat{D}_2$, is algebraically identical to the residual for total diameter increment over the interval. We
 135 therefore modeled final DBH directly while interpreting predictive performance equivalently in terms of
 136 diameter increment.

137 The analysis was conducted independently for two prespecified scopes. The first comprised the
 138 monitored tree community and contained 6487 survivor intervals from 1602 trees representing 192 species
 139 across the six plots and five modeled periods. The second was the commercially listed species group, defined
 140 by exact reconciliation of corrected species names against the commercial registry included with the analytical
 141 dataset. This scope contained 1263 intervals from 319 trees representing 21 species. The term “commercially
 142 listed” refers to registry membership and not to current harvest eligibility: 17 of the 21 matched species were
 143 classified as permitted and four as protected. When registry records assigned conflicting regulatory classes to
 144 the same taxon, protected status took precedence. No species-specific effects were introduced into the principal
 145 growth models. The two analytical domains are summarized in Table 1. Species-level composition and registry
 146 classifications are provided in Supplementary Table S1.

147

148 Table 1. Analytical domains and empirical sample support.

Scope	Intervals	Trees	Species	Plots	Periods	Observed DBH (cm)	Max. empirical interval (yr)
Tree community	6,487	1,602	192	6	5	10–222	3.67
Commercially listed species group	1,263	319	21	6	5	10–222	3.67

149 *Note: The commercially listed species group contains 21 species: 17 classified as permitted and four as protected. Protected status*
 150 *takes precedence when registry records conflict. Registry membership does not imply harvest eligibility.*

151

152 **2.3. Data curation and provenance**

153 The consolidated analytical workbook contained 9031 source interval records. Five periods were
 154 prespecified for modeling, comprising 7201 source records before restriction to surviving trees. Survival was
 155 represented by status code 0.

156 All negative diameter increments had been classified as measurement errors and removed during
 157 upstream data curation after verification against field and spreadsheet records. Because the excluded records

158 were not retained in the consolidated analytical workbook, their number and individual values could not be
159 reconstructed. The present analytical pipeline verified that no negative survivor increment remained in the five
160 predefined modeling periods and performed no additional removal of negative observations during model
161 fitting.

162 Zero growth was not treated as an error or exclusion criterion. A total of 569 survivor intervals with
163 exactly zero diameter increment were retained in the analytical population. Likewise, observations with
164 unusually high growth were not removed to improve model fit. The 46 survivor intervals with annualized
165 diameter increments $> 5 \text{ cm yr}^{-1}$ were individually reconciled against their original field measurement sheets
166 and confirmed as recorded field measurements; all were retained without correction or exclusion.

167 The source workbook also contained a nominal 2022–2023 period. Project records confirmed that no
168 field inventory was conducted in 2023 and that the 2022 DBH measurements had been copied into the 2023
169 fields for a separate analytical purpose. The period therefore did not constitute a remeasurement. The pipeline
170 independently reconciled 1830 source rows for this nominal period, including 1770 survivor records, all of
171 which had identical initial and final DBH. The entire 2022–2023 placeholder period was consequently
172 excluded before growth modeling.

173 Data-curation provenance was audited separately from model fitting. Documentary reconciliation
174 covered the field- and spreadsheet-verified negative-increment rule, all 46 retained high-growth records, and
175 the excluded placeholder period. The complete frozen provenance summary is provided in Supplementary
176 Table S10.

177

178 **2.4. Baseline stand basal area**

179 Baseline plot basal area was evaluated as a candidate structural predictor rather than assumed to be
180 required by the growth equations. Basal area was calculated from the complete 2013 inventories of the six 1-
181 ha plots, comprising 2577 trees, as:

182

$$183 \quad G = \sum_i \frac{\pi D_i^2}{40000} \quad (1)$$

184

185 Where D_i is DBH in cm and G is expressed in $\text{m}^2 \text{ha}^{-1}$. Baseline plot basal area ranged from
186 approximately 26.87 to 30.93 $\text{m}^2 \text{ha}^{-1}$.

187 For candidates containing basal area, the six plot-level 2013 values were standardized using the mean
188 and standard deviation of the unique training plots within each fit. The resulting standardized covariate was
189 therefore recalculated independently inside each validation fold, preventing information from held-out plots
190 from entering model estimation. Baseline basal area was interpreted only as a plot-level structural predictor. It
191 was not treated as a time-varying competition measure or as evidence of a causal stand-density effect.

192

193 2.5. Age-independent DBH transition equations

194 We evaluated age-independent forms of the Richards and Lundqvist–Korf growth functions [5, 8, 18,
195 19]. Let D_1 and D_2 denote DBH at the beginning and end of an interval, respectively, and let Δt denote interval
196 length in years.

197

198 The Richards transition equation was:

199

$$200 \quad D_2 = A \left[1 - \exp(-r\Delta t) \left\{ 1 - \left(\frac{D_1}{A} \right)^{1-m} \right\} \right]^{\frac{1}{1-m}} \quad (2)$$

201

202 Where A is the asymptotic parameter, m is the shape parameter, and $r > 0$ is the rate term.

203 The Lundqvist–Korf transition equation was parameterized as:

204

$$205 \quad D_2 = A \exp \left[- \left\{ \left(\frac{-1}{\ln(D_1/A)} \right)^{1/m} + r\Delta t \right\}^{-m} \right] \quad (3)$$

206

207 A log-scale parameterization ensured a positive rate. For models without basal area,

208

$$209 \quad r = \exp(\eta) \quad (4)$$

210

211 Whereas the basal-area candidates used:

$$r = \exp(\eta + \beta z_G) \quad (5)$$

Where z_G is standardized 2013 plot basal area and β is its log-rate effect.

Four structural candidates were fitted to exactly the same observations within each analytical scope: Richards_noBA, Richards_BA2013, Lundqvist_noBA, and Lundqvist_BA2013. Ordinary least squares (OLS) was the canonical estimator and the only estimator used for structural selection.

Parameter bounds were prespecified. The upper bound for A was 2000 cm. Its lower bound in a training fit was the greater of 223 cm and the maximum training DBH plus 0.001 cm. The 223-cm floor was fixed before validation and exceeded all prespecified scenario starting diameters; it was not calculated from held-out observations. The log-rate parameter was restricted to $[-14, 2]$, the basal-area log-rate effect to $[-4, 4]$, the Richards shape parameter to $[0.001, 0.999]$, and the Lundqvist–Korf shape parameter to $[0.05, 10]$.

2.6. Parameter estimation

OLS parameters were estimated by minimizing the unweighted sum of squared final-DBH residuals. Optimization combined global differential evolution with bounded local refinement [20]. The complete optimization sequence was rerun using training data only for every validation fold, so neither parameter estimates nor basal-area standardization from the full dataset were propagated into held-out predictions. Full numerical settings, deterministic seeds, and implementation details are encoded in the versioned R script (Supplementary Code).

Boundary optima were not automatically classified as failed fits because weakly identified asymptotic parameters can legitimately place the numerical optimum at the imposed boundary. Formal optimizer convergence and numerical solution acceptance were therefore recorded separately. When formal local convergence was unavailable, a solution was accepted only when independent bounded local methods produced usable solutions with objective values agreeing with each other and with the differential-evolution optimum within the prespecified numerical tolerance.

239 Boundary proximity and scaled Jacobian condition numbers were recorded as diagnostics of parameter
240 identifiability. These diagnostics did not alter the prediction target, structural-selection rules, or retained model
241 structures; their exact numerical thresholds are encoded in Supplementary Code.

242

243 **2.7. Structural selection and family equivalence**

244 Structural selection was based exclusively on OLS performance under leave-one-plot-out validation.
245 Within each growth family, inclusion of baseline 2013 basal area was retained only if two prespecified criteria
246 were simultaneously satisfied: (i) pooled LOPO RMSE improved by at least 0.05 cm relative to the
247 corresponding model without basal area, and (ii) the basal-area candidate produced the lower RMSE in at least
248 four of the six held-out plot folds. The rule was defined before inspection of the final results and was not
249 modified retrospectively.

250 After selecting the basal-area structure separately for Richards and Lundqvist–Korf, the two retained
251 growth families were compared using their pooled OLS LOPO RMSE. An absolute RMSE difference <0.05
252 cm was defined as practical equivalence. Under this condition, both families were retained to represent
253 structural uncertainty rather than declaring a unique winning growth function.

254 Gaussian AIC and BIC were calculated for descriptive full-data fits but did not participate in structural
255 selection because the principal inferential target was out-of-sample prediction and the repeated-measurement
256 residual structure did not justify using independent Gaussian likelihood criteria as the primary selection device.
257

258 **2.8. Spatial and temporal validation**

259 Three blocked validation designs were used, each representing a different inferential role. Leave-one-
260 plot-out validation (LOPO) was the primary internal spatially blocked validation. Each of the six plots was
261 withheld once, the model was refitted to the other five plots, and predictions were generated exclusively for
262 trees in the withheld plot. Every analytical interval therefore received exactly one LOPO out-of-sample
263 prediction.

264 Leave-two-plots-out validation (L2PO) was a spatial stress test. All $C(6,2) = 15$ pairs of plots were
265 withheld in turn and models were fitted using the remaining four plots. Because each observation occurs in
266 five overlapping L2PO test sets, these 15 splits were not interpreted as independent replicates and L2PO was
267 not used for structural selection.

268 Progressive temporal validation provided complementary future-interval validation. An expanding
269 training window was used so that a test interval was always strictly later than every interval used for model
270 fitting. Four forecasts were generated: training on 2013–2016 to predict 2016–2017; training through 2016–
271 2017 to predict 2017–2018; training through 2017–2018 to predict 2018–2021; and training through 2018–
272 2021 to predict 2021–2022. Fold coverage and eligible test-row counts for the three validation designs are
273 reported in Supplementary Table S2.

274 A persistence model, $\hat{D}_2 = D_1$, served as the common no-growth benchmark. This benchmark was
275 evaluated on exactly the same held-out observations as each fitted model.

276 Validation metrics were computed from pooled out-of-sample predictions within each validation design
277 rather than from the simple mean of fold-level errors. The primary error measures were root mean squared
278 error (RMSE), mean absolute error (MAE), and mean error (ME), where residuals were defined as observed
279 minus predicted final DBH. Positive ME therefore denotes mean underprediction. Predictive R^2 , modeling
280 efficiency for diameter increment, and annualized RMSE and MAE were retained as secondary diagnostics.
281 Improvement relative to persistence was evaluated on the common held-out prediction sets.

282

283 **2.9. Robust-loss sensitivity and residual dependence**

284 OLS remained the canonical estimator throughout the analysis. To evaluate whether short-interval
285 conclusions depended strongly on the squared-error loss in the presence of heavy residual tails, direct Huber
286 M-estimation [21] was implemented as a prespecified sensitivity analysis.

287 For each Huber fit, the residual scale was fixed from the corresponding accepted OLS training residuals
288 as 1.4826 times the median absolute deviation about the residual median, with root mean squared residual used
289 only as a numerical fallback when necessary. The Huber tuning constant was 1.345. The bounded Huber
290 objective was then optimized directly from the accepted OLS solution using the same parameter domain. Huber
291 estimates were not used to select basal-area structure, to select a growth family, or to replace OLS as the
292 canonical estimator.

293 Robust-loss sensitivity was evaluated for the selected structures under LOPO and progressive temporal
294 validation. Differences between OLS and Huber predictions and changes in RMSE, MAE, and ME were
295 summarized directly. Residual thresholds of 2, 5, and 10 cm were used only to characterize the contribution of
296 large out-of-sample residuals; no observation was deleted as a consequence of these diagnostics.

297 Repeated measurements also allowed a descriptive assessment of residual dependence. Lag-1 residual
298 correlation was calculated only for consecutive within-tree interval pairs. This statistic was treated as a
299 dependence diagnostic rather than as a basis for parameter-level Wald inference. Plot-specific RMSE, MAE,
300 and ME were additionally examined to detect spatial heterogeneity that could be obscured by pooled metrics.

301

302 **2.10. Empirical support across initial DBH**

303 Because large trees were progressively less represented in the monitoring data, prediction support was
304 characterized explicitly across eight initial-DBH classes:

305

306 $DBH < 20$; $20 \leq DBH < 40$; $40 \leq DBH < 60$; $60 \leq DBH < 80$; $80 \leq DBH < 100$; $100 \leq DBH < 150$; $150 \leq$
307 $DBH < 200$; $DBH \geq 200$ cm.

308

309 For each class, we recorded the number of intervals, unique trees, species, plots, and monitoring periods.
310 Support categories followed prespecified descriptive rules. A class was labeled broad when it contained at
311 least 30 trees, five species, and five plots; limited when it contained at least 10 trees, three species, and three
312 plots; and sparse otherwise. These categories describe empirical representation and do not constitute validation
313 of extrapolation.

314 LOPO and temporal RMSE, MAE, ME, annualized errors, and improvement over persistence were also
315 calculated within each initial-DBH class for OLS, Huber, and persistence. Class-specific comparisons were
316 restricted to descriptive reporting when the class was sparse, contained fewer than three plots, or contained
317 fewer than 30 validation intervals.

318

319 **2.11. Asymptote identification and structural uncertainty**

320 The asymptotic parameter was evaluated separately from short-interval predictive performance. For
321 each structural candidate, A was fixed successively at 223, 250, 300, 400, 600, 1000, 2000, and 5000 cm, while
322 all remaining parameters were re-estimated. Each accepted OLS profile point was compared with the minimum
323 profile SSE using:

324

$$\Delta_{\text{profile}} = n \ln \left(\frac{\text{SSE}_A}{\text{SSE}_{\min}} \right) \quad (6)$$

The asymptote was classified as not point identified when either the unconstrained full fit reached its asymptote boundary or at least two profile values with $A \geq 1000$ cm remained within $\Delta_{\text{profile}} \leq 2$ of the best fixed-asymptote solution.

The fitted asymptote was consequently treated as an optimization descriptor, not as an estimate of maximum biological tree diameter. Huber profiles were fitted over the same fixed-asymptote grid, using the corresponding accepted OLS profile solution as the starting point and fixing the robust scale from that OLS fit.

Sensitivity ranges generated across alternative asymptotes, growth families, and OLS versus Huber estimation were interpreted as structural and estimator sensitivity. Their union was not interpreted as a confidence interval, prediction interval, posterior interval, or comprehensive measure of forecast uncertainty.

2.12. Conditional horizon analyses and limits of inference

Conditional survivor-DBH trajectories were generated only after the short-interval validation analysis had been completed. Scenario starting diameters were 10, 20, 30, 40, 50, 60, 80, 100, 150, and 200 cm. Trajectories were propagated under time-invariant fitted parameters and the fixed-asymptote profiles to 90 years, with reporting horizons at 5, 10, 25, 30, 35, 50, 70, and 90 years. Numerical agreement between direct-horizon evaluation and repeated one-year transitions was audited to verify equation consistency under stationary parameters; this numerical property was not interpreted as ecological evidence for stationarity.

The horizons were assigned different inferential meanings before manuscript interpretation. Predictions over the monitored 0.92–3.67-year intervals were the only quantities subjected to empirical out-of-sample validation. Five- and 10-year trajectories were classified as short conditional sensitivity analyses. The 25-, 30-, and 35-year horizons were classified as management-cycle-scale conditional sensitivities, because they overlap time scales relevant to Amazonian forest-management discussions, but they do not constitute validated predictions or recommendations of cutting-cycle length. The 50-, 70-, and 90-year horizons were treated as long-term structural stress tests and as hypothesis-generating extrapolations only. This inferential hierarchy is summarized in Table 2.

353 Table 2. Prespecified inferential scope across empirical and conditional horizons.

Inference level	Quantity	Evidence or assumption	Permitted interpretation
empirically evaluated	survivor final DBH over monitored short intervals	internal plot-blocked and progressive temporal validation; OLS canonical with direct Huber robust-loss sensitivity; interval range 0.92-3.67 years	short-interval predictive performance within the monitored forest
short conditional sensitivity	stationary survivor DBH trajectories at 5 and 10 years	semigroup projection under time-invariant parameters; OLS/Huber sensitivity envelope across Richards/Lundqvist and fixed asymptotes 223, 250, 300, 400, 600, 1000, 2000, 5000 cm	near-term extrapolation stress test
management-cycle-scale conditional sensitivity	stationary survivor DBH trajectories at 25, 30 and 35 years	same stationary semigroup projection; horizons coincide with current management-cycle scales but are not validation	management-horizon structural sensitivity, not a cycle recommendation
long-term structural stress test	stationary survivor DBH trajectories at 50, 70 and 90 years and first passage to large DBH	lifetime-scale extrapolation motivated by tropical tree-ring literature; OLS/Huber union-envelope width, initial-DBH support counts and domain flags quantify structural divergence and empirical sparsity, not total uncertainty	hypothesis-generating long-term stress test only
not supported	cutting-cycle recommendation or stand/volume recovery	mortality, recruitment, harvest response, species-specific abundance and merchantable volume are not modeled	none from this diameter-survivor model alone

354 *Note: Only the 0.92–3.67-year monitored intervals were empirically evaluated. All longer horizons are conditional sensitivity analyses*
355 *under stationary model parameters.*

356
357 All conditional trajectories describe the DBH of surviving trees under stationary model parameters.
358 They do not model mortality, recruitment, harvest, post-harvest growth responses, future abundance,
359 merchantable volume, stand-level volume recovery, or replacement of harvested trees. Consequently, neither
360 cutting-cycle recommendations nor sustainable-yield conclusions can be obtained from these survivor-DBH
361 models alone.

363 **2.13. Computational implementation**

364 The analysis was executed in R 4.6.1. The versioned R script is provided as Supplementary Code and
365 contains the package-version records, optimizer settings, diagnostics, and code used to generate the reported
366 analytical outputs. The workflow retained fold-specific refitting and data-curation provenance throughout the
367 analysis.

369 **2.14. Use of generative AI and AI-assisted technologies**

370 During preparation of the manuscript, the authors used OpenAI ChatGPT for language editing and
371 journal-specific editorial review. The authors reviewed and edited all resulting text and take full responsibility
372 for the content of the article.

373 3. Results

374 3.1. Analytical domain and empirical DBH support

375 The community analysis comprised 6487 survivor remeasurement intervals from 1602 trees representing
376 192 species across six plots and five monitoring periods. The commercially listed species group comprised
377 1263 intervals from 319 trees representing 21 species. In both scopes, observed DBH ranged from 10 to 222
378 cm and the longest empirically evaluated interval was 3.67 years (Table 1).

379 Empirical representation declined markedly toward the upper end of the diameter distribution. In the
380 community, the initial-DBH classes $DBH < 20$ cm, $20 \leq DBH < 40$ cm, $40 \leq DBH < 60$ cm, and $60 \leq DBH <$
381 80 cm met the prespecified criterion for broad support, with 1102, 577, 170, and 38 unique trees, respectively.
382 All classes with $DBH \geq 80$ cm were classified as sparse; the $150 \leq DBH < 200$ cm class contained only three
383 trees and the $DBH \geq 200$ cm class only one. In the commercially listed species group, support was broad for
384 $DBH < 40$ cm, limited for $40 \leq DBH < 60$ cm, and sparse for $DBH \geq 60$ cm. The $40 \leq DBH < 60$ cm class
385 contained 26 trees from 12 species across all six plots, whereas only nine trees occurred at $60 \leq DBH < 80$ cm.
386 Detailed empirical support and diameter distributions are provided in Supplementary Table S7 and
387 Supplementary Fig. S1.

388

389 3.2. Structural selection and short-interval predictive generalization

390 Baseline 2013 basal area did not satisfy the prespecified criterion for structural retention in either growth
391 family or analytical scope. In the community, adding basal area increased rather than decreased pooled LOPO
392 RMSE, from 1.5696 to 1.5847 cm for Richards and from 1.5685 to 1.5833 cm for Lundqvist–Korf. The
393 corresponding changes were -0.0152 and -0.0148 cm when expressed as RMSE gain, and the basal-area
394 structure produced lower error in only two of the six held-out plots for each family. The same result occurred
395 in the commercially listed species group: basal-area inclusion increased LOPO RMSE by 0.0176 cm for
396 Richards and 0.0166 cm for Lundqvist–Korf and again produced lower error in only two of six folds.
397 Consequently, Richards_noBA and Lundqvist_noBA were retained in both scopes (Table 3A). Detailed

398 structural comparisons and practical-equivalence results are provided in Supplementary Table S3 and
399 Supplementary Fig. S2.

400 The two retained families had almost identical short-interval predictive performance. In the community,
401 pooled OLS LOPO RMSE was 1.5685 cm for Lundqvist–Korf and 1.5696 cm for Richards, an absolute
402 difference of 0.0010 cm. In the commercially listed species group, the corresponding values were 1.6407 and
403 1.6432 cm, differing by 0.0025 cm. Both differences were far below the prespecified 0.05-cm threshold for
404 practical distinction. The two families were therefore retained jointly as alternative model structures rather
405 than ranked as a unique best model.

406 Both models improved prediction relative to persistence. Community LOPO RMSE for persistence was
407 2.0659 cm, corresponding to reductions of 24.08% for Lundqvist–Korf and 24.02% for Richards. LOPO MAE
408 was 0.8639 and 0.8646 cm, respectively, compared with 1.2045 cm for persistence. Mean errors were small
409 and positive (+0.1105 cm for Lundqvist–Korf and +0.1101 cm for Richards), indicating slight mean
410 underprediction.

411 The gain over persistence was larger in the commercially listed species group. Persistence RMSE was
412 2.2648 cm, compared with 1.6407 cm for Lundqvist–Korf and 1.6432 cm for Richards, representing reductions
413 of 27.56% and 27.45%, respectively. MAE was 0.8852–0.8872 cm for the two models versus 1.3343 cm for
414 persistence, and mean errors remained small (+0.0823 and +0.0791 cm).

415 Spatial variation among held-out plots was appreciably larger than the difference between model
416 families. Community LOPO RMSE ranged from approximately 1.378 cm in plot 8 to 1.783 cm in plot 6 for
417 both equations. In the commercially listed species group, the range was approximately 1.106–2.151 cm, with
418 plot 8 again producing the lowest error and plot 7 the highest. The sign of mean error also varied among plots,
419 despite the small positive pooled bias. Pooled metrics and LOPO prediction diagnostics are reported in
420 Supplementary Table S4 and Supplementary Figs. S4–S5.

421

422 3.3. Spatial stress testing and future-interval validation

423 All 15 L2PO fits were completed for both retained structures in both analytical scopes; no L2PO fold
424 produced a failed or invalid prediction. Community pooled L2PO RMSE was 1.5697 cm for Lundqvist–Korf
425 and 1.5706 cm for Richards, essentially unchanged from LOPO performance and approximately 24% lower
426 than the persistence RMSE of 2.0659 cm. In the commercially listed species group, L2PO RMSE was 1.6470

and 1.6481 cm, respectively, compared with 2.2648 cm for persistence, corresponding to RMSE reductions of approximately 27.3%.

Progressive temporal validation was likewise complete for all four future intervals. In the community, pooled temporal RMSE was 1.5321 cm for Lundqvist–Korf and 1.5317 cm for Richards, compared with 1.9867 cm for persistence. Both models therefore reduced temporal RMSE by approximately 22.9%. Performance varied among forecast intervals: RMSE was lowest for the 2016–2017 forecast (0.775 and 0.774 cm for Lundqvist–Korf and Richards) and highest for 2018–2021 (2.336 cm for both models). Nevertheless, both models outperformed persistence in every temporal fold. The smallest relative improvement occurred for 2021–2022, when RMSE was approximately 12.6% below persistence.

The same temporal pattern was evident in the commercially listed species group, although the pooled gain over persistence was larger. Temporal RMSE was 1.5728 cm for Lundqvist–Korf and 1.5782 cm for Richards, compared with 2.1467 cm for persistence, reductions of 26.74% and 26.48%. The 2018–2021 interval was again the most difficult, with RMSE of 2.838–2.848 cm, but both models remained approximately 25.5–25.7% better than persistence in that fold. Across all spatial and temporal validation designs, differences between Richards and Lundqvist–Korf remained much smaller than differences among plots or monitoring intervals (Table 3B; Fig. 2). Fold-specific temporal results and temporal prediction diagnostics are provided in Supplementary Table S5 and Supplementary Figs. S6–S7.

Table 3. Structural selection and out-of-sample predictive performance.

A. Structural selection by LOPO.

Scope	Family	no-BA RMSE	BA RMSE	BA gain	BA wins	Retained structure
Community	Richards	1.5696	1.5847	-0.0152	2/6	Richards_noBA
Community	Lundqvist	1.5685	1.5833	-0.0148	2/6	Lundqvist_noBA
Commercially listed species group	Richards	1.6432	1.6608	-0.0176	2/6	Richards_noBA
Commercially listed species group	Lundqvist	1.6407	1.6573	-0.0166	2/6	Lundqvist_noBA

447 *BA gain = LOPO RMSE(no BA) – LOPO RMSE(BA); positive values favor BA. The prespecified retention rule required a gain ≥ 0.05 cm and*
 448 *BA wins in ≥ 4 of 6 plot folds. Both families retained the no-BA structure in both analytical scopes.*

449

450 B. Out-of-sample performance of retained structures and persistence

Scope	Model	LOPO RMSE	LOPO MAE	LOPO ME	Gain vs. persistence	L2PO RMSE	Temporal RMSE
Community	Lundqvist–Korf	1.5685	0.8639	+0.1105	24.08%	1.5697	1.5321
Community	Richards	1.5696	0.8646	+0.1101	24.02%	1.5706	1.5317
Community	Persistence	2.0659	1.2045	+1.2045	—	2.0659	1.9867
Commercially							
listed species	Lundqvist–Korf	1.6407	0.8852	+0.0823	27.56%	1.6470	1.5728
group							
Commercially							
listed species	Richards	1.6432	0.8872	+0.0791	27.45%	1.6481	1.5782
group							
Commercially							
listed species	Persistence	2.2648	1.3343	+1.3343	—	2.2648	2.1467
group							

451 *ME = observed – predicted final DBH; positive values indicate mean underprediction. LOPO is the primary internal spatially blocked*
 452 *validation; L2PO is a spatial stress test; Temporal is progressive future-interval validation. All validation folds were complete.*

453

454 **3.4. Robust-loss sensitivity and diameter-dependent performance**

455 Huber estimation did not materially alter the short-interval predictive conclusions. Under community
 456 LOPO, Huber increased RMSE by only 0.0166 cm for either model while reducing MAE by approximately
 457 0.025 cm. The median absolute change between paired OLS and Huber predictions was 0.123 cm for
 458 Lundqvist–Korf and 0.114 cm for Richards. In the commercially listed species group, the corresponding LOPO
 459 RMSE change was <0.003 cm, MAE decreased by approximately 0.028–0.029 cm, and the median absolute
 460 prediction shift was approximately 0.12 cm.

461 The robust loss did, however, alter mean bias. Community LOPO mean error increased from
 462 approximately +0.110 cm under OLS to +0.305–0.306 cm under Huber, and mean error in the commercially
 463 listed species group increased from +0.079–0.082 cm to +0.270–0.272 cm. Temporal Huber RMSE differed

464 from OLS by less than 0.004 cm in the community and was 0.030–0.035 cm lower in the commercially listed
465 species group. All Huber LOPO and temporal folds were completed successfully. Complete OLS–Huber
466 comparisons are reported in Supplementary Table S6 and Supplementary Fig. S3.

467 A small number of large LOPO residuals were present, but Huber sensitivity indicated that they did not
468 determine the short-interval predictive conclusion. In the community, 24 of 6487 predictions (0.37%) had
469 absolute residuals >10 cm and the maximum absolute residual was 29.41 cm. In the commercially listed species
470 group, six of 1263 predictions (0.48%) exceeded 10 cm, with a maximum of approximately 21.8 cm. These
471 observations were retained in the OLS analysis and evaluated through the separate robust-loss sensitivity rather
472 than removed.

473 Residual dependence also remained detectable after out-of-sample prediction. Lag-1 correlations among
474 consecutive within-tree LOPO residuals were approximately 0.25 for both growth families in the community
475 and 0.25 in the commercially listed species group. These correlations were treated descriptively and did not
476 affect the structural-selection criterion.

477 Prediction error varied with initial DBH, but interpretation at large diameters was constrained by rapidly
478 declining sample support. Within the four broadly supported community classes (<80 cm), Lundqvist–Korf
479 LOPO RMSE increased from 1.340 cm at <20 cm to 1.721 cm at 20–<40 cm and approximately 1.923 cm at
480 40–<60 cm and 1.929 cm at 60–<80 cm; Richards produced nearly identical values. All four classes remained
481 better predicted than under persistence. Above 80 cm, class-specific results were designated descriptive only
482 because sample support was sparse. For example, the 150–<200 cm class had RMSE of approximately 6.9 cm
483 but represented only 10 intervals from three trees.

484 The commercially listed species group showed an even sharper contraction of empirical support. LOPO
485 RMSE for Lundqvist–Korf was 1.036 cm below 20 cm, 1.989 cm at 20–<40 cm, and 2.341 cm at 40–<60 cm;
486 Richards again produced nearly identical values. These three classes were broad, broad, and limited,
487 respectively. All classes ≥ 60 cm were sparse and were therefore retained only as descriptive diagnostics. In
488 particular, the 150–<200 cm class contained 10 intervals from three trees and yielded RMSE of 6.75–6.79 cm
489 (Fig. 3). Exact class-specific support and LOPO metrics are provided in Supplementary Tables S7–S8.

490

491 3.5. Short-term predictive equivalence despite weak long-term identification

492 The close agreement between the two growth families over monitored intervals did not correspond to
493 identification of their upper growth trajectories. In all four selected full-data fits—two families and two
494 analytical scopes—the estimated asymptote reached the imposed 2000-cm upper optimization bound.

495 Fixed-asymptote profiling over 223–5000 cm confirmed that this boundary behavior did not identify a
496 finite biological asymptote. For community Richards, fixed asymptotes from 1000 through 5000 cm remained
497 within the prespecified profile-support criterion, with three high-asymptote points satisfying $\Delta \leq 2$. Community
498 Lundqvist–Korf retained support from 2000 to 5000 cm, with two high-asymptote points satisfying the same
499 criterion. The commercially listed species group produced the same qualitative result: Richards retained a high-
500 asymptote plateau from 1000 to 5000 cm and Lundqvist–Korf from 2000 to 5000 cm. All selected structures
501 were therefore classified as asymptote_not_point_identified (Fig. 4). Fixed-asymptote profile summaries are
502 reported in Supplementary Table S9.

503 The 2000-cm boundary estimate was consequently not used as an estimate of maximum biological DBH.
504 Instead, conditional trajectories were evaluated across the full fixed-asymptote profile to characterize how
505 weak long-term identification propagated with increasing projection horizon.

506

507 **3.6. Horizon-dependent structural and estimator sensitivity**

508 Structural differences that were negligible over empirically monitored intervals remained small in the
509 first years of conditional propagation but increased progressively with projection horizon. For illustration, an
510 initial DBH of 30 cm belonged to a broadly supported empirical class in both analytical scopes.

511 Under canonical OLS, the community structural sensitivity range across Richards, Lundqvist–Korf, and
512 the fixed-asymptote profiles was 33.75–33.90 cm after 5 years and 37.72–38.03 cm after 10 years, widths of
513 only 0.15 and 0.30 cm, respectively. At the management-cycle-scale sensitivity horizons, the corresponding
514 ranges widened to 50.91–51.68 cm at 25 years and 60.43–61.66 cm at 35 years. At 90 years, the OLS structural
515 sensitivity range expanded to 111.98–128.89 cm, a width of 16.91 cm.

516 The commercially listed species group showed the same pattern but stronger long-horizon divergence.
517 For a 30-cm initial DBH, the OLS structural range was 34.37–34.61 cm at 5 years, 39.04–39.59 cm at 10 years,
518 54.92–56.53 cm at 25 years, and 66.06–69.49 cm at 35 years. By 90 years it had widened to 124.70–157.28
519 cm, a range of 32.58 cm.

Estimator sensitivity expanded these ranges further. For the 30-cm community example, the Huber median trajectory was 0.68 cm below the OLS median at 5 years, 3.95 cm lower at 25 years, 5.86 cm lower at 35 years, and 17.15 cm lower at 90 years. The union of both estimators, both growth families, and all fixed-asymptote scenarios consequently spanned 33.10–33.90 cm at 5 years, 47.07–51.68 cm at 25 years, 54.57–61.66 cm at 35 years, and 97.57–128.89 cm at 90 years.

For the commercially listed species group, the corresponding combined sensitivity ranges were 33.64–34.61 cm at 5 years, 50.22–56.53 cm at 25 years, 59.68–69.49 cm at 35 years, and 110.38–157.28 cm at 90 years. At 90 years, the Huber median was approximately 29.60 cm below the OLS median for the 30-cm starting diameter.

Thus, estimator and structural differences that had little effect on empirically evaluated short-interval RMSE became increasingly consequential as trajectories were propagated away from the observed time domain (Fig. 5). Only predictions over the observed 0.92–3.67-year intervals were empirically validated. The 5- and 10-year results are short conditional sensitivities, the 25–35-year results are management-cycle-scale conditional sensitivities, and the 50–90-year trajectories are long-term structural stress tests. The sensitivity ranges reported above are deterministic ranges across model structures, asymptote assumptions, and estimators; they are not confidence or prediction intervals. Conditional OLS trajectories and OLS–Huber estimator sensitivity are shown in Supplementary Figs. S8–S9.

4. Discussion

4.1. Short-interval predictive generalization

Relatively simple age-independent DBH transition equations provided useful out-of-sample predictions for surviving trees over the monitored 0.92–3.67-year intervals. Richards and Lundqvist–Korf reduced LOPO RMSE by approximately 24% relative to persistence in the full community and by approximately 27% in the commercially listed species group. The persistence comparison is important because final DBH is necessarily dominated by initial DBH over short intervals; high predictive R^2 alone would not demonstrate that a model captured diameter change.

The conclusion was stable across validation designs. LOPO evaluated transfer to a withheld plot within the monitored system, L2PO imposed a stronger spatial perturbation, and progressive validation restricted each test set to intervals later than those used for fitting. Comparable performance across these designs supports

549 short-interval internal generalization, while the marked variation among plots and periods shows that where
550 and when prediction occurred mattered more than the negligible difference between the two growth families.

551 Age-independent transition models are useful when chronological age is unavailable and current tree
552 size is the operational state variable [8, 9]. Their success here does not imply that ecological heterogeneity is
553 unimportant. Seasonal inundation, sediment inputs, competition, species composition, and tree strategies can
554 all affect growth in várzea forests [3, 6]. Rather, a pooled transition structure was sufficient for near-term
555 prediction within the observed domain. The commercially listed group remains an analytical grouping: its 21
556 species include four protected taxa, and grouped accuracy does not imply equivalent performance or
557 management suitability for every species.

558 **4.2. Structural parsimony and residual features**

559 Baseline 2013 plot basal area did not improve blocked prediction sufficiently to satisfy the prespecified
560 retention rule. This result has a narrow interpretation. The tested variable was a single plot-level baseline
561 measure, not a contemporaneous tree-level competition index, neighborhood measure, hydrological covariate,
562 or dynamic stand state. Competition and stand structure remain important in individual-tree models [6, 23];
563 the evidence shows only that this particular basal-area representation added insufficient predictive information
564 for the present target.

565 Huber estimation changed short-interval RMSE little and slightly reduced MAE, indicating that the
566 principal predictive conclusion was not determined by a small number of large residuals. Its larger positive
567 mean error also shows that robustness involved a bias trade-off, so Huber was retained as a sensitivity analysis
568 rather than substituted for OLS. The 46 annualized increments above 5 cm yr⁻¹ were retained after verification
569 against field records, preserving the intended population rather than retrospectively truncating the response
570 distribution.

571 Positive lag-1 correlations and heteroscedastic residuals indicate that OLS is not a complete stochastic
572 model of repeated tree growth. Comparable dependence has been reported in tropical growth studies [7, 22].
573 Blocked validation evaluates grouped predictive transfer, whereas Huber estimation evaluates sensitivity to
574 the residual loss; neither establishes residual independence. Models intended for parameter inference or
575 individual heterogeneity would benefit from explicit correlated or hierarchical error structures.

576 **4.3. Short-term prediction does not identify long-term trajectories**

577 Near-equivalent prediction over monitored intervals coexisted with weak identification of the upper
578 trajectory. The fitted asymptote reached the 2000-cm optimization boundary in every selected full-data model,
579 and fixed-asymptote profiles remained nearly equivalent across multiple values up to 5000 cm. These values
580 are numerical diagnostics, not plausible biological maximum diameters. They show that the observed
581 transitions contain insufficient information about the decelerating portion of the growth curve to locate a finite
582 asymptote.

583 Different combinations of rate, shape, and asymptote can reproduce the same local transition while
584 implying divergent long-term behavior. The data therefore identify a local predictive relationship more
585 strongly than a lifetime growth curve. This is a qualification result rather than a contradiction: useful one-step
586 prediction does not require every parameter governing the global trajectory to be identified.

587 The horizon analysis makes this limitation empirical. For a tree starting at 30 cm DBH, the combined
588 sensitivity range was below 1 cm at five years but exceeded 30 cm at 90 years in the community and
589 approached 47 cm in the commercially listed group. OLS and Huber trajectories also diverged despite similar
590 short-interval errors. These envelopes are deterministic ranges across the selected families, asymptotes, and
591 estimators; they are not confidence or prediction intervals and do not include future climate, hydrology,
592 competition, mortality, recruitment, or parameter nonstationarity.

593 Model adequacy is conditional on purpose, criterion, and context [1, 2]. Permanent plots provide many
594 contemporary transitions, whereas tree-ring records can represent substantially longer portions of individual
595 development when annual rings are demonstrable [7, 10]. The two designs are complementary but not
596 inferentially interchangeable. Short permanent-plot intervals can qualify next-inventory predictions without
597 supplying the information required for asymptotic or lifetime inference.

598 **4.4. Management relevance without management prescription**

599 Forest-management decisions often operate over decades, far beyond the validated interval. Amazonian
600 regulations commonly apply standardized cutting diameters and cycles despite variation among species and
601 forest types [4], while dendrochronological and long-term studies have used broader developmental
602 information to derive species-specific quantities [10, 24, 25]. The 25–35-year trajectories examined here are
603 not candidate cutting cycles; they are stress tests of models validated only over intervals of at most 3.67 years.

604 Long-term survivor-diameter prediction would in any case be insufficient to determine sustainable
605 harvest cycles at stand level. Recovery also depends on mortality, recruitment, harvest removals, residual-tree

606 damage, regeneration, population dynamics, and volume accumulation, none of which is represented by the
607 present equations. The management result is therefore a boundary on use: next-inventory accuracy cannot be
608 converted directly into a multidecadal prescription.

609 Within the monitored domain, the models can update survivor DBH over short intervals, quantify
610 improvement over a no-growth assumption, and identify diameter classes where evidence becomes sparse.
611 These are useful operational functions. Species-specific recommendations, however, require species-level
612 demographic support, particularly because registry membership does not imply harvest eligibility and the
613 grouped analysis combines taxa with different ecological responses [4, 6].

614 **4.5. Scope, limitations, and transferability**

615 The spatial validation is internal to six plots in one high-várzea system. Withholding one or two plots
616 tests transfer within that system but not geographic portability to other sectors of Mamirauá or to low-várzea,
617 igapó, terra-firme, or other Amazonian forests. Independent external data would be required for those claims.

618 The response is conditional on survival: mortality and recruitment are outside the estimand rather than
619 prediction failures. Empirical support is also uneven across initial diameter. Community support was broad
620 through 60–<80 cm, whereas support in the commercially listed group was broad only below 40 cm and limited
621 at 40–<60 cm. Results for larger trees are descriptive because they depend on progressively fewer trees,
622 species, and plots.

623 The analysis was designed for prediction rather than causal explanation. Hydrological exposure, soils,
624 local competition, species traits, prior growth, and climate are plausible sources of variation but were not
625 estimated as causal effects. In addition, the same LOPO criterion was used to choose between basal-area
626 structures and summarize the performance of the retained structure. Because model selection was not repeated
627 within a separate outer validation layer, the reported post-selection error may contain some optimism. The
628 small candidate set and consistent rejection of basal area reduce, but do not eliminate, this concern; a fully
629 nested blocked design would provide a stricter post-selection estimate.

630 Data provenance imposes a further constraint. All negative increments had previously been classified as
631 measurement errors after field and spreadsheet verification, but the excluded records were not retained and
632 their number cannot be reconstructed. By contrast, the 46 high-growth records were available, field-verified,
633 and retained. The nominal 2022–2023 interval contained copied 2022 values rather than a new inventory and
634 was therefore excluded as a placeholder with no independent growth information.

635 Within these limits, both equations generalized to withheld plots and future monitored intervals and
636 improved on persistence, but neither the growth family nor the asymptotic trajectory was uniquely identified.
637 Predictive adequacy is horizon-specific: support for the next inventory interval should not be treated as
638 evidence for decades of subsequent tree development.

639 **5. Conclusions**

640 Age-independent Richards and Lundqvist–Korf equations provided useful out-of-sample predictions of
641 survivor DBH over the empirically monitored 0.92–3.67-year intervals in this Amazonian high-várzea forest.
642 Both models consistently improved on the no-growth persistence benchmark under plot-blocked and
643 progressive temporal validation, reducing LOPO RMSE by approximately 24% for the tree community and
644 27% for the commercially listed species group. Baseline plot basal area did not provide sufficient additional
645 predictive information to justify its inclusion, and the two retained growth families were practically equivalent
646 under the prespecified threshold at the validated time scale.

647 This short-interval predictive equivalence did not imply equivalent or well-identified long-term growth
648 trajectories. The asymptote was not point identified, and differences among growth families, asymptote
649 assumptions, and OLS versus Huber estimation increased as the projection horizon extended beyond the
650 monitored intervals. Empirical support also declined strongly with increasing initial DBH, particularly within
651 the commercially listed species group. Accordingly, 5–10-year trajectories are short conditional sensitivities,
652 25–35-year trajectories are management-cycle-scale conditional sensitivities, and 50–90-year trajectories are
653 long-term structural stress tests; none is a validated forecast.

654 For the intended next-inventory task, the results qualify both equations within the observed spatial,
655 temporal, and diameter domains; they do not qualify either equation for lifetime or management-cycle
656 trajectories. The principal management implication is therefore not a new cutting-cycle estimate. Sustainable-
657 yield or cutting-cycle inference additionally requires information on mortality, recruitment, harvest effects,
658 post-harvest growth, population dynamics, and stand-level volume recovery. More generally, predictive
659 adequacy in forest growth models is horizon-specific: models can generalize well to the next inventory interval
660 while remaining weakly informative about decades of subsequent tree development.

661

662 **Data Availability**

663 The individual-level forest inventory data analyzed in this study are controlled by the Instituto de
664 Desenvolvimento Sustentável Mamirauá (IDSM-OS/MCTI) under its cooperation arrangements with the
665 Mamirauá Sustainable Development Reserve and the State of Amazonas. The data cannot be publicly
666 deposited or distributed directly by the authors. Requests for access should be submitted by email to Darlene
667 Gris (darlene.gris@mamiraua.org.br) and are subject to formal institutional review and, when approved,
668 execution of an appropriate cooperation or data-use agreement. Because access depends on these institutional
669 procedures, a fixed response time cannot be guaranteed. Data can be provided confidentially to editors and
670 peer reviewers upon request within the applicable institutional terms. The complete R script used for data
671 processing, model fitting, validation, sensitivity analyses, and generation of the reported outputs is provided
672 as Supplementary Code.

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742 **Author contributions**

743 I.B.F.S.: writing – original draft, writing – review and editing, resources, methodology, investigation,
744 formal analysis, and conceptualization. L.P.R.: writing – review and editing, formal analysis, validation,

745 supervision, resources, and conceptualization. H.G.L.: writing – review and editing, methodology, and
746 validation. D.G.: writing – review and editing, resources, and validation. A.C.S., Y.C.M., S.J.S.S.R., and
747 A.L.S.: writing – review and editing. C.M.M.E.T.: writing – review and editing, methodology, formal analysis,
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757 **Additional information**

758 **Competing interests**

759 The authors declare no competing interests.

760 **Figure legends**

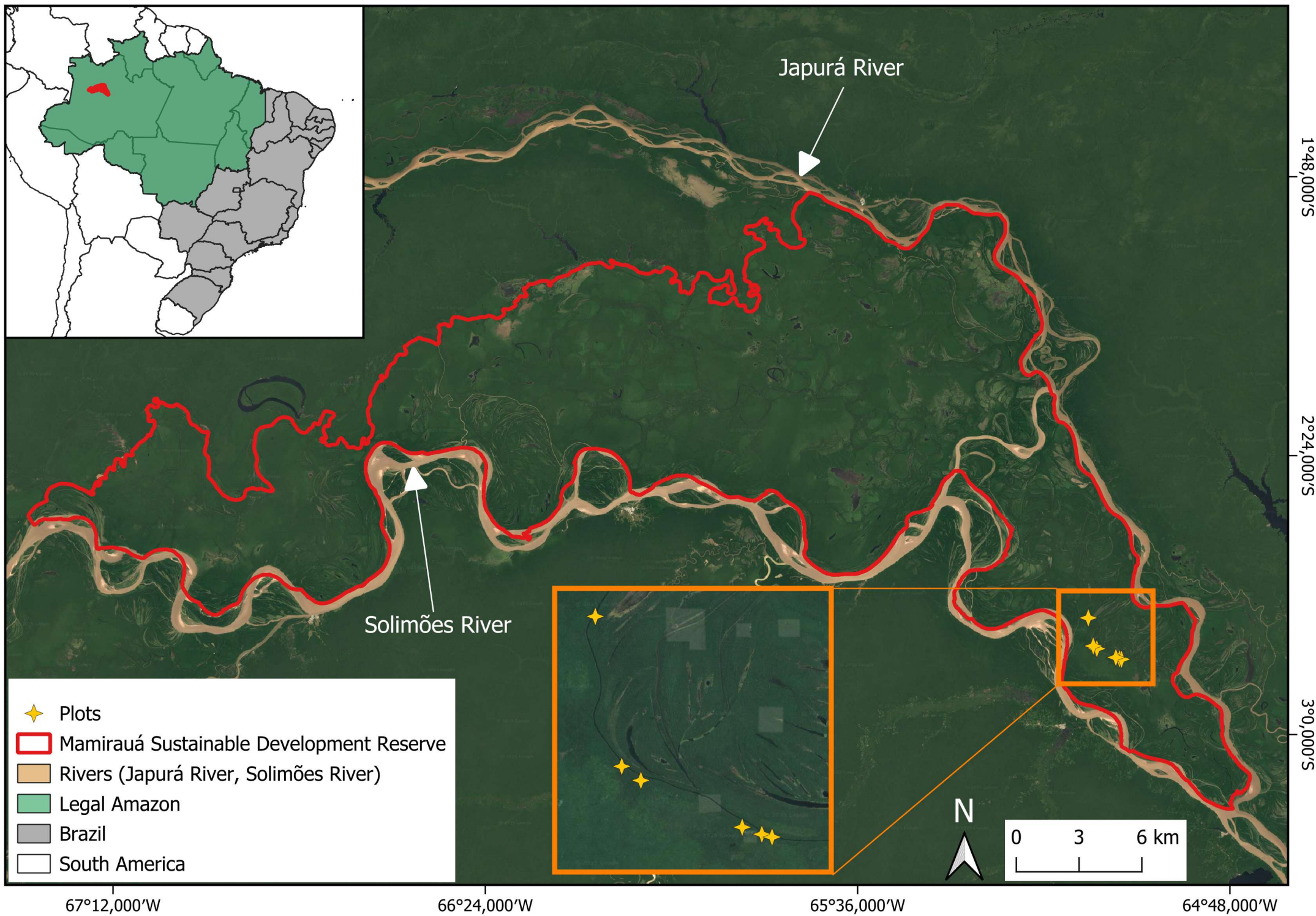
761 Fig. 1. Location of the six 1-ha high-várzea permanent plots (plots 6–11) in the Mamirauá Sustainable Development Reserve,
762 Amazonas, Brazil. Map lines delineate study areas and do not necessarily depict accepted national boundaries.

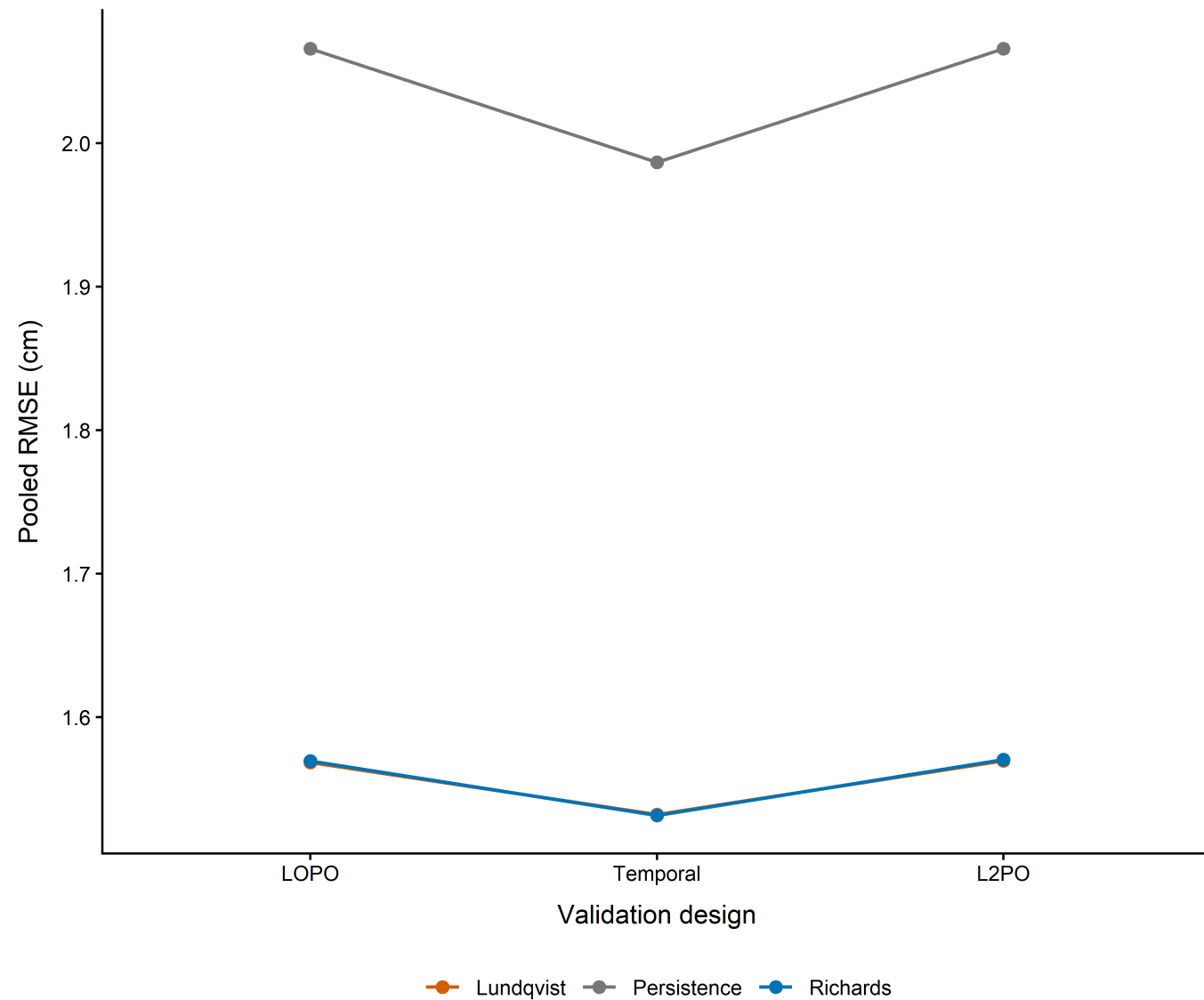
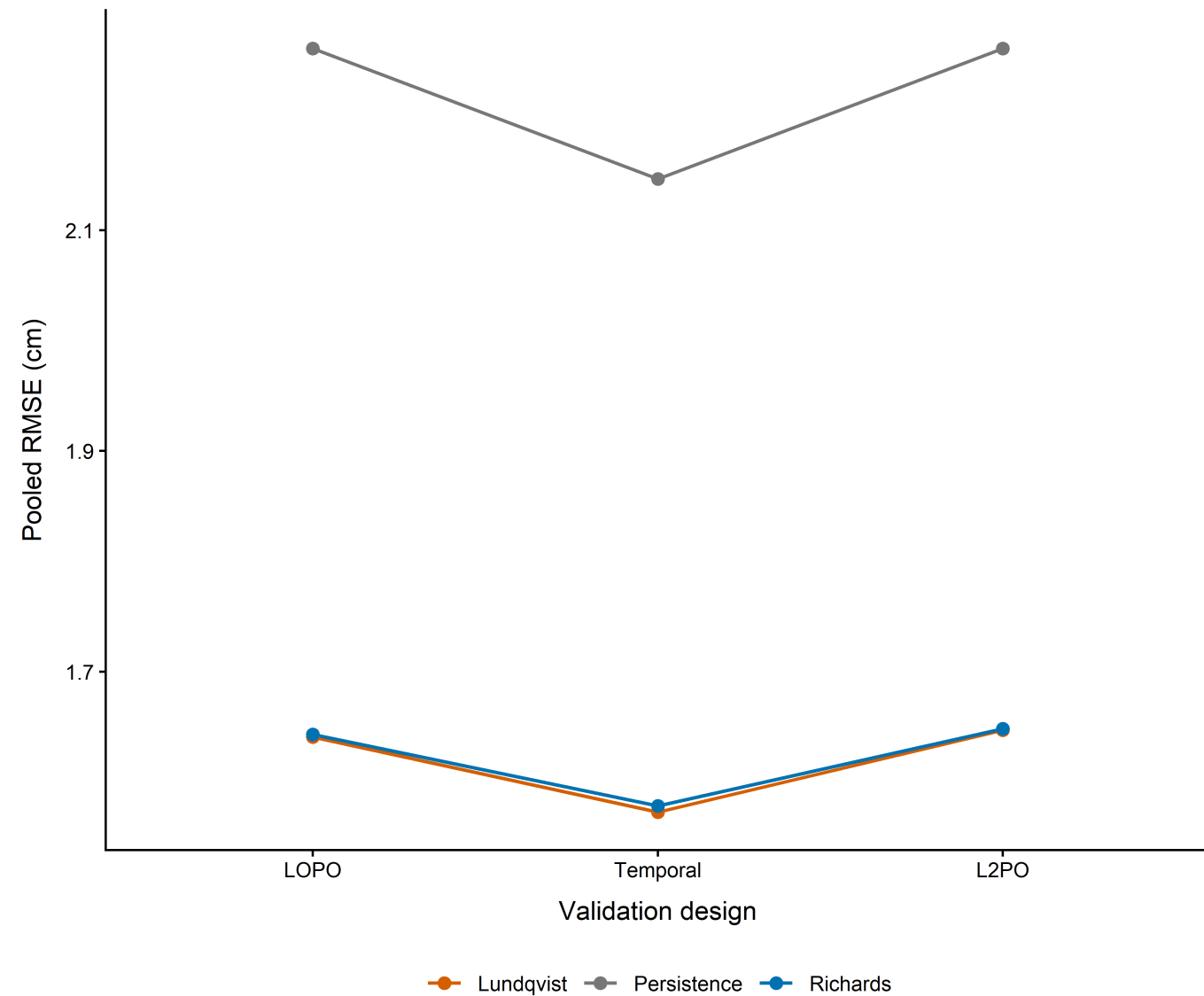
763 Fig. 2. Predictive performance across validation designs for (a) the tree community and (b) the commercially listed species
764 group. Pooled RMSE is shown for the retained Lundqvist–Korf and Richards structures and the persistence benchmark under
765 leave-one-plot-out (LOPO), progressive temporal validation, and leave-two-plots-out (L2PO). LOPO is the primary internal
766 spatially blocked validation; L2PO is a spatial stress test; temporal validation predicts strictly future intervals.

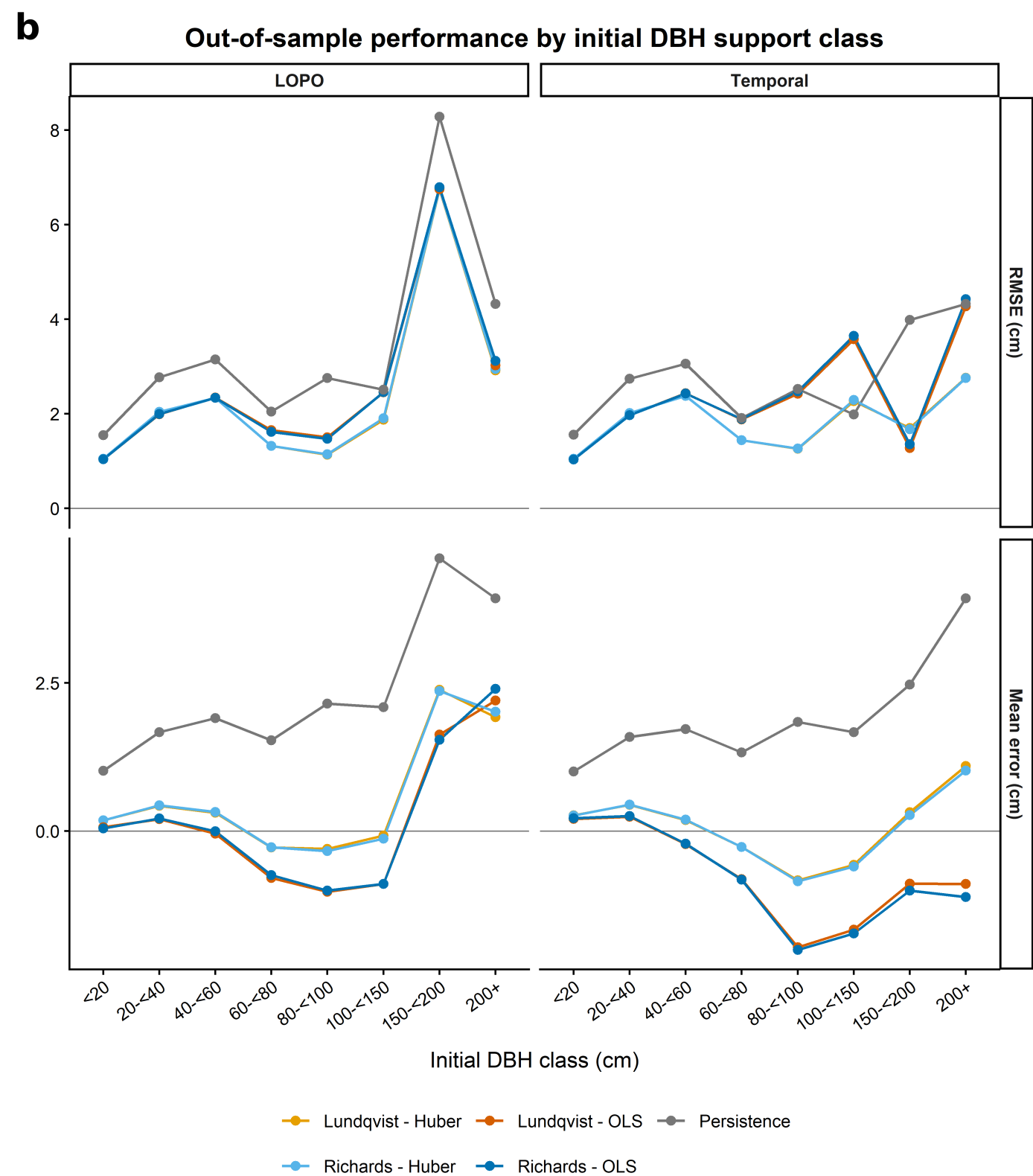
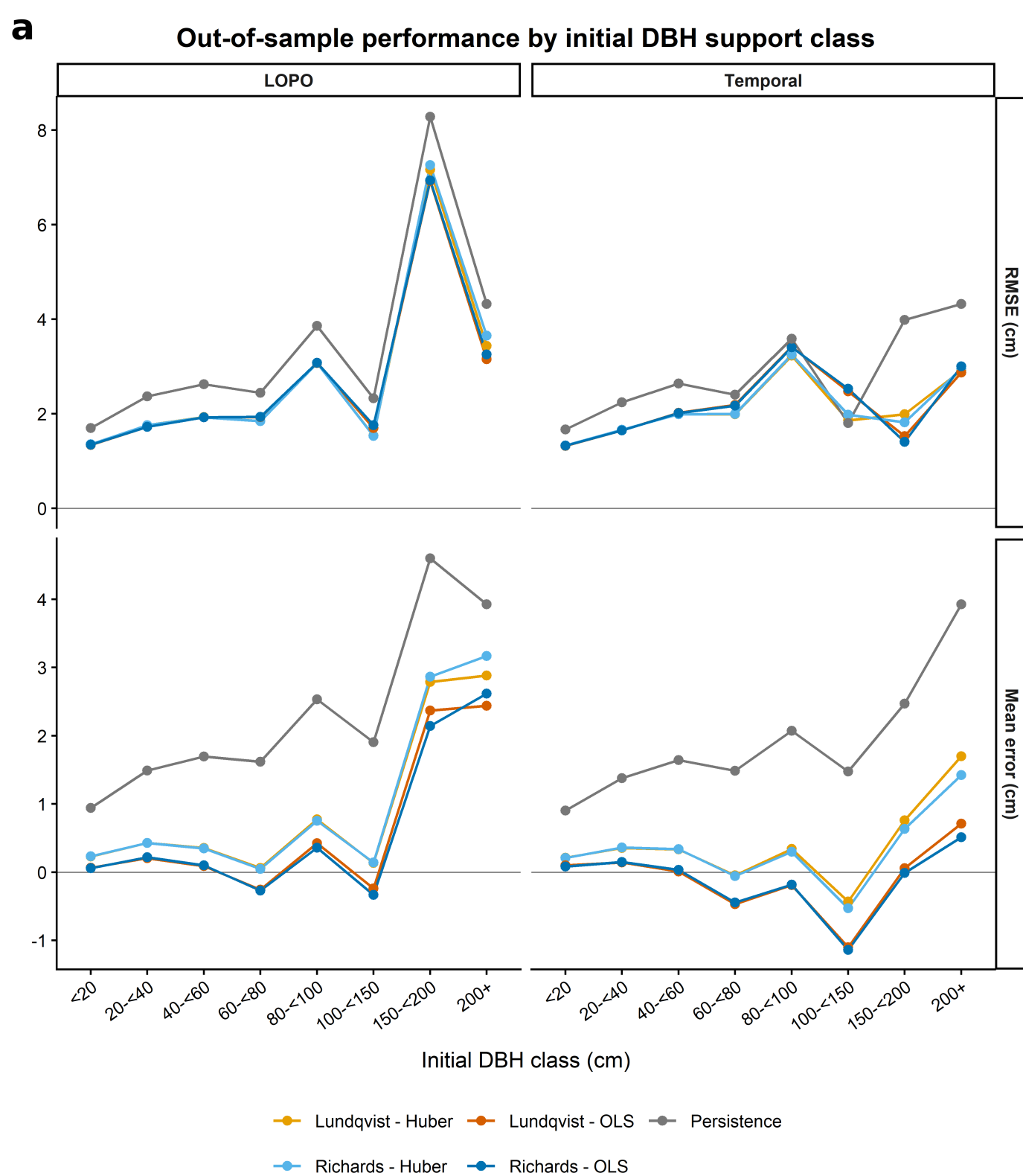
767 Fig. 3. Out-of-sample performance by initial DBH support class for (a) the tree community and (b) the commercially listed
768 species group. LOPO and progressive temporal predictions are shown for the selected structures, Huber sensitivity, and
769 persistence. Sparse classes remain descriptive only; exact sample sizes and support flags are summarized in the Results.

770 Fig. 4. Fixed-asymptote profiles for (a) the tree community and (b) the commercially listed species group. Profiles span Richards
771 and Lundqvist–Korf candidates with and without baseline 2013 basal area across fixed asymptotes of 223–5000 cm; the BA
772 candidates are shown as structural diagnostics although the no-BA structures were retained for prediction. The dotted horizontal
773 reference marks Δ profile = 2. High-asymptote plateaus demonstrate that the asymptote is not point identified; fitted asymptotes
774 are optimization descriptors, not biological maximum diameters.

775 Fig. 5. Estimator and structural divergence of conditional projections for (a) the tree community and (b) the commercially listed
776 species group. Estimator-specific ranges span families and fixed asymptotes; the combined curve is their union. The shaded 50–
777 90-year region marks long-term structural stress-test horizons, not total uncertainty. All horizons beyond 3.67 years are
778 conditional extrapolation sensitivities rather than empirically validated forecasts.

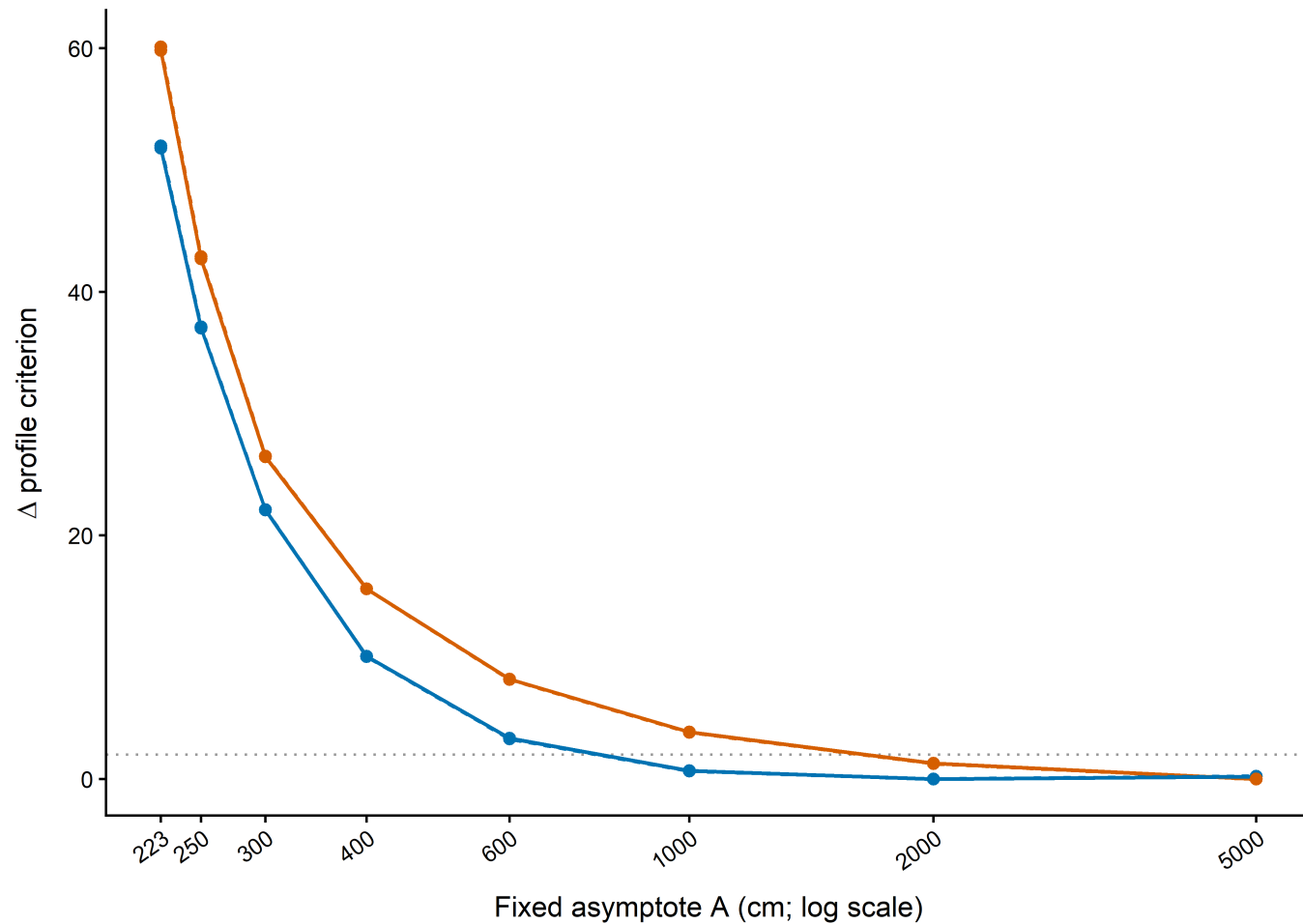


a**Predictive performance across validation designs****b****Predictive performance across validation designs**

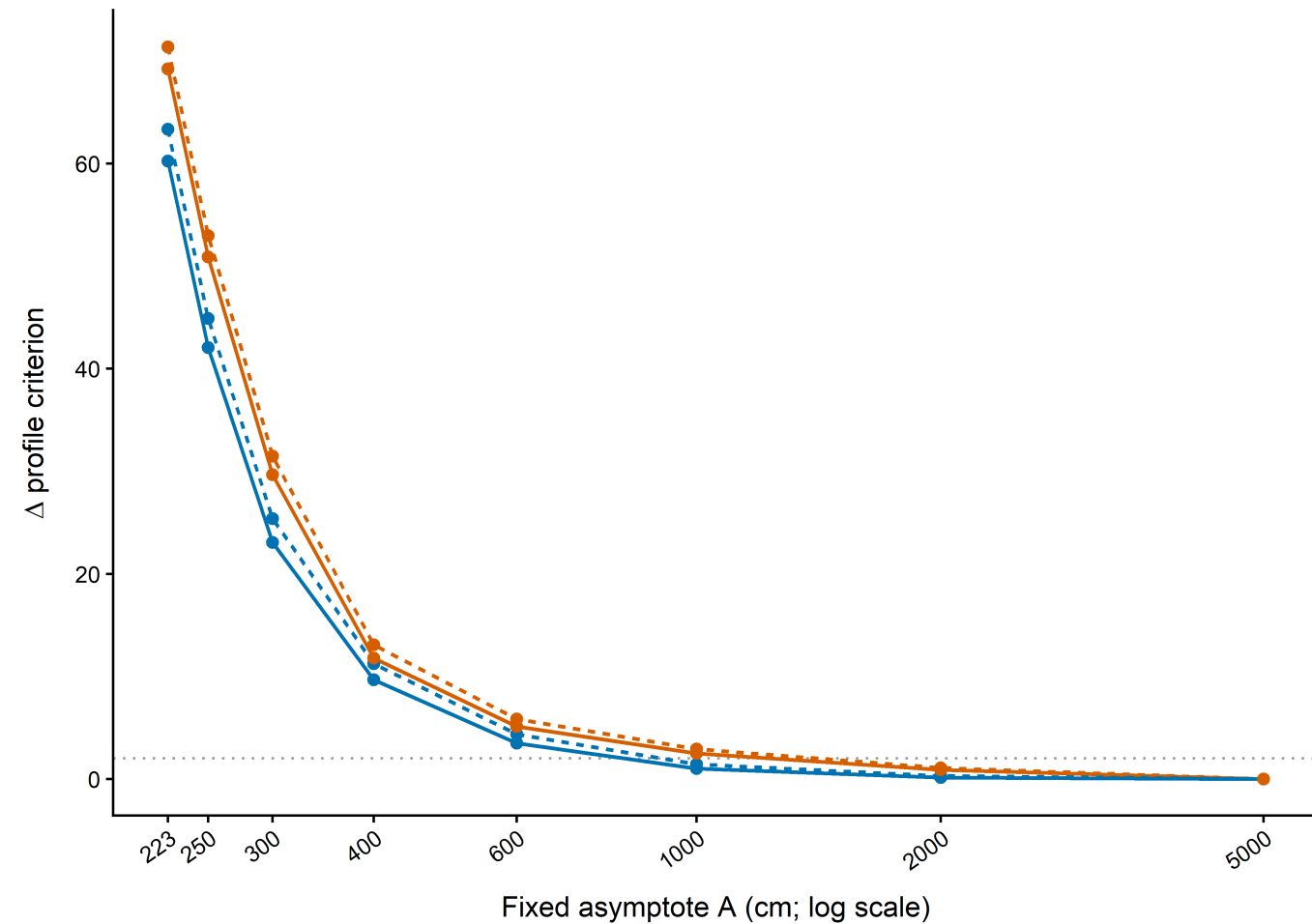


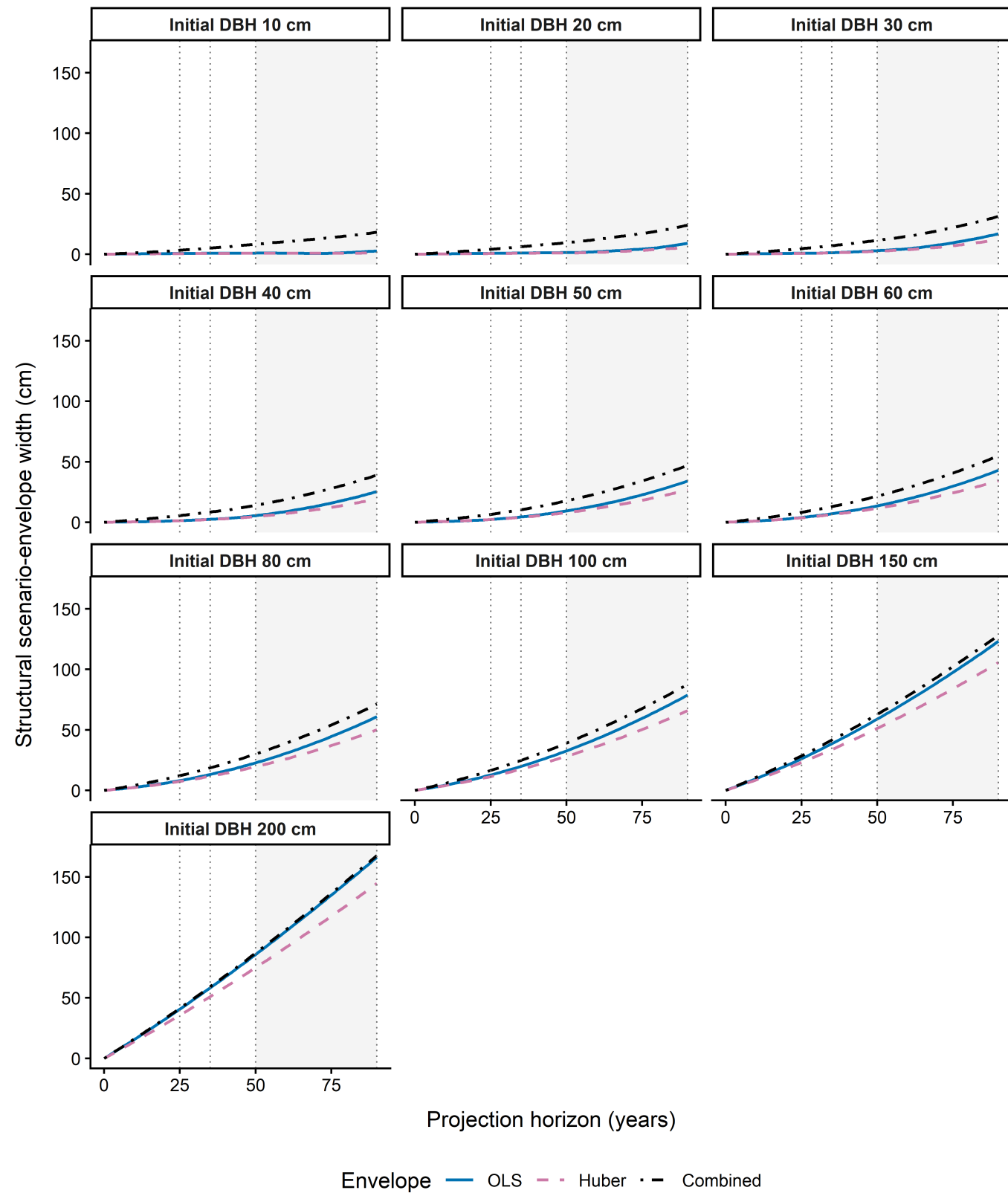
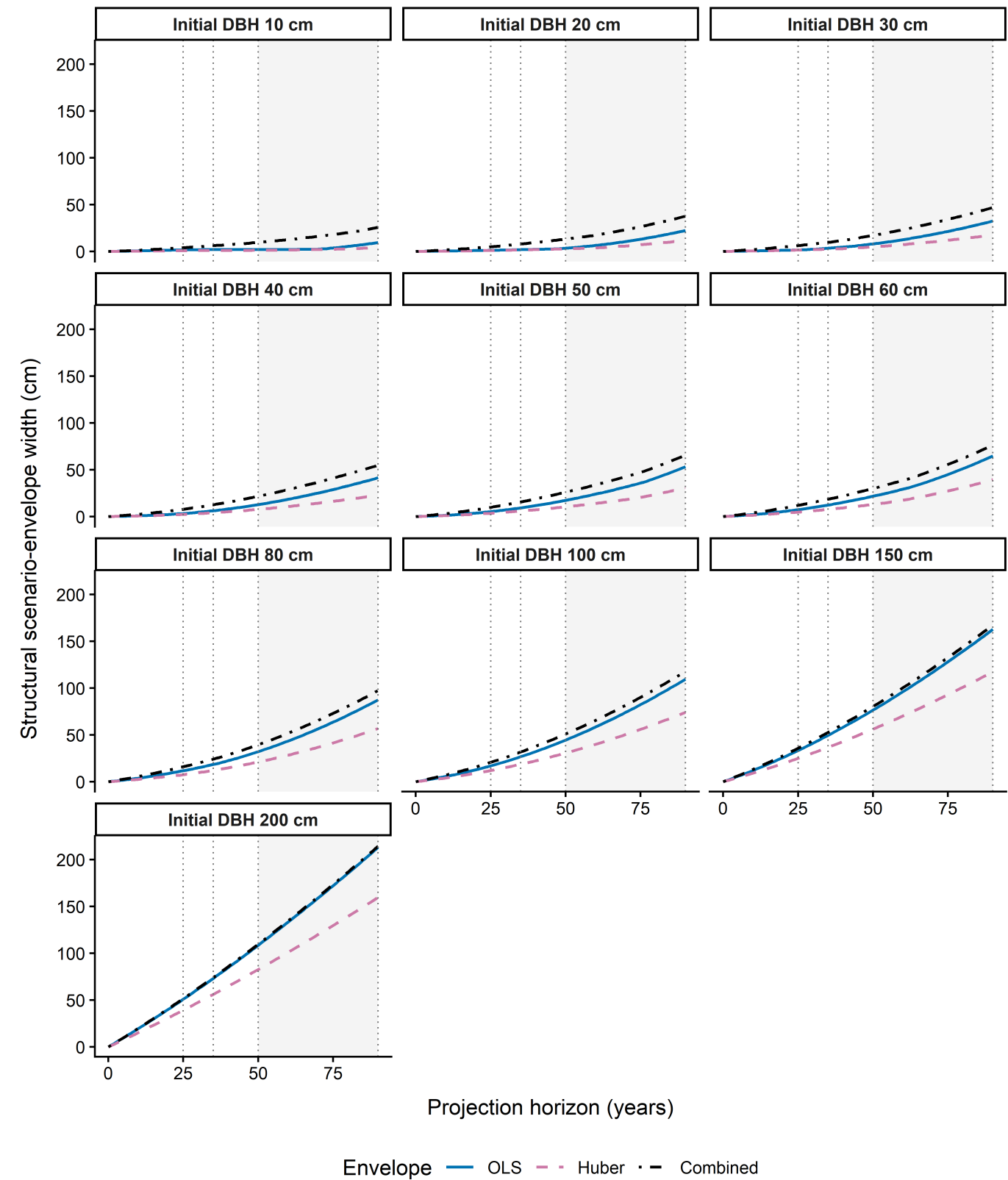
a

Global multi-start asymptote profiles

**b**

Global multi-start asymptote profiles



a**Estimator and structural divergence of conditional projections****b****Estimator and structural divergence of conditional projections**

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