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# An estimation strategy to protect against over-estimating precision in a LiDAR-based prediction of a stand mean

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**Abstract**

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A prediction of a forest stand mean may be biased and its estimated variance seriously underestimated when a model fitted for an ensemble of stands (stratum) does not hold for a specific stand. When the sampling design cannot support a stand-level lack-of-fit analysis, an analyst may opt to seek a protection against a possibly serious over-estimation of precision in a predicted stand mean. This study propose an estimation strategy to counter this risk by an inflation of the standard model-based estimator of variance when model predictions suggest non-trivial random stand effects, a spatial distance-dependent autocorrelation in model predictions, or both. In a simulation study, the strategy performed well when it was most needed, but equally over-inflated variance in settings where less protection was appropriate.

**Keywords**: prediction; precision; stand-effects; spatial autocorrelation; simulation

In forest inventories supported by an airborne laser scanning, it has become a widespread practice to develop stratum-specific models for the unit-level prediction of a forest attribute ($Y$) of interest using a few selected LiDAR metrics ($\boldsymbol{X}$) correlated with $Y$ as predictors (Næsset 2004; Kangas et al. 2016; Breidenbach et al. 2018). Here, a unit is synonymous with an area commensurate to that of a field inventory plot delivering data on $Y$ and a shape that facilitates a tessellation of a stand polygon. Stratum-specific models are typically applied to stands with a similar species composition, canopy structure, and other attributes used to define a stratum. Stands in a stratum are expected to have different conditional means of $Y$ given their $\boldsymbol{X}$ values viz. $Y|\boldsymbol{X}$ and, by extension, a non-zero among-stand variance in model residuals (Köhl, Magnussen 2014). A prediction of a stand mean and estimating the uncertainty inherent in a prediction is important for an efficient management of the forest (Melville et al. 2015; Saarela et al. 2015a; Kangas et al. 2018). While a model may provide a good first-order approximation to a stand mean, it is also possible that a model prediction is biased when the employed model does not hold true for the stand in question (Claeskens, Hjort 2008; Chambers 2011). In theory, models with a random stand-effect in one or more of the parameters of a model would minimize this risk (Nanos et al. 2004; Breidenbach, Astrup 2012; Maltamo et al. 2012; Fortin et al. 2016; Mauro et al. 2016; Magnussen, Breidenbach 2017; Magnussen et al. 2017). However, in practice within stratum sampling designs rarely – if ever – affords an estimation of stand effects since most stands are either without a representation in the sample or only represented once (Junttila et al. 2013; Saarela et al. 2015b). Stands represented by two or more sample units are rare.

A direct consequence of a bias in a predicted mean is that a model-based estimate of the uncertainty (here variance) in a predicted mean will be liberal (optimistic). A within-stand positive distance dependent autocorrelation in actual (true but unknown) unit-level model residuals will also contribute to an under-estimation of uncertainty unless dealt with (Breidenbach et al. 2016). Simulation studies have demonstrated the importance of obtaining reasonable approximations to an otherwise non-estimable among-stand variance in $Y|\boldsymbol{X}$ and to any spatial autocorrelation in model residuals (Magnussen 2016; Magnussen et al. 2016a; Magnussen, Breidenbach 2017; Magnussen et al. 2017). The same studies (Ibid) and Rao and Hidiroglou (2003) proposed estimators that could, to various degrees, "capture" simulated stand effects and thereby mitigate the risk of overestimating precision. Yet the simulations also indicated that a default – across the board – use of variance estimators designed to capture possible stand effects of not be the most efficient estimation strategy. A mixed strategy based on information indicative of the presence (absence) of important among-stand variance in $Y|\boldsymbol{X}$ or a spatial autocorrelation in model residuals might be better.

In this study, we propose an estimation strategy for the assessment of the uncertainty in a model-based prediction of a stand mean in settings where the sampling design does not support the estimation of an among-stand variance in $Y|\boldsymbol{X}$. To wit: the decision to inflate a standard model-based variance estimator depends on the outcome of a test on the significance of: (*i*) the among-stand variance in $Y|\boldsymbol{X}$; and (*ii*) the within-stand spatial-autocorrelation in model residual errors. The proposed estimation strategy is successful when it lowers the risk of over-estimating the precision of a predicted stand mean. Two indicators obtained from simulated sampling in artificial populations are used to assess success: (*i*) the ratio of expected to empirical variance of a prediction of a stand mean, and (*ii*) the achieved coverage rate of nominal 95% confidence intervals.

Results with simulated sampling from 27 artificial populations support the proposed strategy.

## MATERIAL AND METHODS

**A proposed estimation strategy**. The proposed estimation strategy is to use a standard (benchmark) estimator of variance for a stand mean only when unit-level predictions suggest an absence of stand-effects in $Y|\boldsymbol{X}$ and an absence of a variance inflating autocorrelation in model residual errors. In all other cases, an inflated estimate of the standard variance is used as a protection against over-estimating precision. The strategy is evaluated with simulated sampling from artificial populations, and a linear model with two predictors for predicting a stand-level mean of $Y$. For the analyst who subscribes to the strategy, the variance estimator to use for a predicted stand mean is in Eq. 1.

**Artificial populations and sampling design**. Each population is composed of $N$ units arranged in a square array and subdivided to $M$ = 400 square stands each with $L$ units ($L$ = 9, 49, or 121). Therefore $N$ is 3,600 ($L$ = 9), 19,600 ($L$ = 49), or 48,400 ($L$ = 121). Random sampling without replacement of one unit from each of $n$ randomly selected stands was executed with sample sizes $n$ = 50, 100, and 200.

All populations are trivariate ($x_1$, $x_2$, $y$). The three variables are standard Gaussian (mean of zero and a variance of one) with a fixed correlation structure: $\rho(x_1, y) = 0.85$, $\rho(x_2, y) = 0.40$, and $\rho(x_1, x_2) = 0.50$. A spatial distance dependent autocorrelation in $\boldsymbol{x}$ and $y$ was introduced via first-order autoregressive (AR1) processes (Harvey 1981) with a one-unit-lag correlation coefficient $\phi_1$ equal to 0.0, 0.2 or 0.5. These levels are relevant to practice (Czaplewski et al. 1994; Breidenbach et al. 2008; Viana et al. 2012; Magnussen et al. 2016b; Mauro et al. 2017). The two predictors have identical AR1 lag-one correlation coefficients.

The proposed strategy is evaluated in a factorial design with 27 settings (3 stand sizes, $L$) × 3 levels of $\phi_{1,y}$ × 3 levels of $\phi_{1,\boldsymbol{x}}$) times the above three sample sizes. The sampling design does not ensure a spatially balanced sampling (Grafström, Ringvall 2013) and it has no incidence of multiple observations from a single stand. Sampling with each of the 81 combinations of $L$, $\phi_{1,y}$, $\phi_{1,\boldsymbol{x}}$, and $n$ was replicated 600 times. Six hundred replications yields an expected standard error of 1% on an achieved coverage of a nominal 95% confidence interval for the actual stand mean (coverage is the proportion of estimated confidence intervals that includes the true stand mean) (Rao, Hidiroglou 2003).

The simulation of the triplets $(y, x_1, x_2)_{i,j}$ $i,j = 1, \ldots, \sqrt{N}$ in a population with the above correlations and pre-specified AR1 processes is described in (Magnussen et al. 2016a). In a first-step, generate three $\sqrt{N} \times \sqrt{N}$ matrices with standard Gaussian variables, and the appropriate AR1 process along rows and columns (Gupta, Nagar 1999), then pre-multiply these values – after reformatting to a $N \times 3$ matrix – with the square root of a 3 × 3 matrix (Meini 2004) with the above variable correlation coefficients (Chilès, Delfiner 1999). In a last step, reformat the generated $N \times 3$ matrix back to a $\sqrt{N} \times \sqrt{N}$ matrix in order to re-establish the original (spatial) ordering of the population units. A standardization to a mean of zero and a variance of one in each variable was done prior to sampling. As stated above, there are no explicitly induced stand effects in the simulations. Locally, however, the global AR1 processes may, by chance, generate apparent stand effects in $\boldsymbol{X}$, $Y$, or both when the average covariance among stand units is non-zero (Nanos et al. 2004; Finley et al. 2008, 2009), or by spatial confounding (Hodges, Reich 2010; Paciorek 2010; Hughes, Haran 2013; Thaden, Kneib 2017).

**Prediction of a stand mean**. Upon completion of a sample, the sample data was fitted – by ordinary least squares techniques (Draper, Smith 1998) – to a unit-level linear model with $y_i$ as the dependent variable and $x_{1i}$ and $x_{2i}$ as predictors ($i = 1, \ldots, n$) (Claeskens, Hjort 2008). Accordingly, a prediction of $Y$ for the $i^{\text{th}}$ unit ($\hat{y}_i$) is obtained from Eq. 1:

$$\hat{y}_i = \hat{\beta}_0 + \hat{\beta}_1 x_{1i} + \hat{\beta}_2 x_{2i} + e_i, i = 1, \ldots, N \tag{1}$$

where:

$e_i$ – unknown residual error with an assumed mean of zero and an assumed normal distribution with variance $\sigma_e^2$. In all predictions, $e_i$ is set to its expected value of zero,

$\hat{\beta}_k$ ($k = 0, 1, 2$) – least-squares regression coefficient.

For the $m^{\text{th}}$ stand, the predicted mean ($\tilde{y}_m$) is calculated by Eq. 2:

$$\tilde{y}_m = \hat{\beta}_0 + \hat{\beta}_1 \bar{x}_{1m} + \hat{\beta}_2 \bar{x}_{2m}, m = 1, \ldots, M = 400 \tag{2}$$

where:

$\bar{x}_{1m}$, $\bar{x}_{2m}$ – means of $x_1$ and $x_2$ in the mth stand.

**Proposed estimator of variance**. For an analyst subscribing to the proposed strategy, the estimator of the variance in a $\tilde{y}_m(\tilde{y}_m)$ is given in Eq. 3:

$$\hat{\mathrm{Var}}(\tilde{y}_m) = (1, \bar{x}_{1m}, \bar{x}_{2m})^t \times \hat{\Sigma}(\beta) \times (1, \bar{x}_{1m}, \bar{x}_{2m}) + \delta_A \sigma^2_{\hat{y}|\boldsymbol{x},\text{ stand}} + \\ + (\hat{\sigma}_e^2 - \delta_A \ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\text{ stand}}) \gamma_{Wm} L^{-1} \tag{3}$$

where:

$\hat{\Sigma}(\beta)$ – standard sample-based estimator of the variance-covariance matrix of the OLS regression coefficients $\boldsymbol{\beta}$,

$\delta_A$ – binary indictor (1/0) of the assumed statistical significance of $\ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\text{ stand}}$,

$\hat{\sigma}_e^2$ – sample-based estimate of the residual variance computed from the empirical residuals $\hat{e}_i = \hat{y}_i - y_i$, $i = 1, \ldots, n$,

$\gamma_{Wm}$ – stand-specific variance inflation factor (≥ 1) due to an assumed spatial autocorrelation process in the (unknown) model residuals in stand $m$,

$t$ – transpose of a vector or matrix,

$\ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\text{ stand}}$ – proxy for the among-stand variance in $Y|\boldsymbol{X}$ (defined below) which cannot be estimated directly from the sample data.

The proxy $\ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\text{ stand}}$ is here taken as equal to the ANOVA-based ratio $\hat{\rho}_{\hat{y}}$ of the among-stand variance to the total variance in $\hat{y}$ times $\hat{\sigma}_e^2$ (Donner 1986; Searle et al. 1992). Note, the proxy differs numerically (slightly) from previously proposed proxies (Magnussen 2016; Magnussen, Breidenbach 2017). The current proxy is deemed more intuitive. When $\hat{\rho}_{\hat{y}}$ exceeds 1.96 times its standard error (Donner 1986), the stand-effects in $Y|\boldsymbol{X}$ are deemed statistically significant and $\delta_A$ is set to one, otherwise $\delta_A = 0$.

An autocorrelation in model residuals can arise from autocorrelations in $\boldsymbol{X}$, $Y$, or both. For the designs used here and in a typical forest inventory, it will not be possible to obtain a direct estimate of this autocorrelation. Again, we resort to $\hat{y}$ to provide an approximation (Magnussen et al. 2016a). Specifically, a maximum likelihood estimate (MLE) of the AR1 coefficient $\phi_{1,m}$ in $\hat{y}$ (Harvey 1981) was obtained for each stand $m = 1, \ldots, M = 400$. The inflation of the within-stand residual variance by a non-null $\hat{\phi}_{1,m}$ depends on the among-unit Manhattan distances (Black 2006) $d$ and their frequencies – $f_d$ (Magnussen 2001). The choice of distance metric is not important for the estimation of $\ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\text{ stand}}$, and as demonstrated in the results, also unimportant for the inflation of the residual variance due to autocorrelation. The inflation factor $\gamma_{Wm}$ was set to $L^{-2}\Sigma_d f_d (0.4\hat{\phi}^d_{1m}(\hat{y}))^d$ when $\hat{\phi}_{1,m}$ was greater than 1.96 times its estimated standard error, and otherwise to 1. Apart from the factor 0.4, the calculation of the inflation factor $\gamma_{Wm}$ follows standards procedures for an AR1 process within a square cluster of size $L$ (Magnussen 2001). The factor 0.4 was argued in Magnussen (2016).

Hence, if $\delta_A = 0$ and, by definition, $\gamma_{Wm} = 1$, the estimator in Eq. 3 converts to the standard model-based estimator of variance in a model-based prediction of a stand mean (Chambers, Clark 2012). For notational convenience, the standard estimator of variance will be referred to as $\hat{\mathrm{Var}}_0(\tilde{y}_m)$. With $\delta_A = 1$ and $\gamma_{Wm} \geq 1$, the estimator in Eq. 3 accounts for model error variance, among stand variance in $Y|\boldsymbol{X}$, model residual error variance, and autocorrelation among within-stand residual errors. With $\delta_A = 0$ and $\gamma_{Wm} \geq 1$, the estimator in Eq. 3 accounts for model error variance, model residual error variance, and autocorrelation among within-stand model residual errors. Either way, the estimator will be referred to as $\hat{\mathrm{Var}}_1(\tilde{y}_m)$.

Evaluation of the estimation strategy. The proposed estimation strategy is successful when it lowers the risk of over-estimating the precision of a predicted stand mean. Two indicators are used to assess the success.

First, the estimate of variance in a prediction of a stand mean with or without the estimation strategy is compared to the empirical mean squared error (MSE). The MSEs were computed from the 600 replicate ($r$) estimates of a stand mean (Eq. 4):

$$\hat{\text{MSE}}\left(\tilde{y}_m\right) = 600^{-1}\sum_{r=1}^{600}\left(\tilde{y}_{m,r} - \overline{y}_m\right)^2 \qquad (4)$$

where:

$\tilde{y}_{m,r}$ – predicted mean in the $r^{\text{th}}$ replicate,

$\overline{y}_m$ – actual stand mean of $Y$.

Under the assumption of a correct model specification and independent and identically distributed model residuals, the ratio $\hat{R}_0 = E_{\text{rep},\,m}[\hat{\text{Var}}_0(\tilde{y}_m)]/E_{\text{rep},\,m}[\text{MSE}(\tilde{y}_m)]$ with expectations taken over replications (*reps*) and stands ($m$ = 1, …, $M$ = 400) is one. The same holds for the ratio $\hat{R}_1$ based on $\hat{\text{Var}}_1(\tilde{y}_m)$. When $\hat{R}_0 < 1$ and $\hat{R}_1$ is closer to 1 than $\hat{R}_0$, the proposed estimation strategy should be preferred. When $\hat{R}_0 \geq 1$, the variance with a ratio closest to 1.0 should be favored. A clear preference may not emerge in other situations.

Second, we also use the achieved coverage of nominal 95% confidence interval as an indicator of success. A confidence interval for the true stand mean of $Y$ is computed on a routine basis in forest inventories supported by airborne laser scanner data (ALS) viz. LiDAR data. It is therefore important to assess how often a computed nominal confidence interval includes the actual stand mean. The relative frequency (in 600 replications) of inclusion is denoted coverage – $CO_{95}$ (Rao, Hidiroglou 2003). A coverage below the nominal (expected) coverage suggest an over-estimation of precision. The proposed estimation strategy is successful when achieved coverage is closer to the nominal expectation (here 0.95) than with the standard (benchmark) estimator of variance.

## RESULTS AND DISCUSSION

### Apparent stand effects

Introducing a spatial autocorrelation in a standard Gaussian variable $y$, $\boldsymbol{x}$ or both – in a population tessellated by stands with units arranged in a regular array – generated an apparent among-stand variance in $y|\boldsymbol{x}$ (viz. the intercept in the linear model). Across all replications, the mean of ANOVA-based estimates of the among-stand variance in $y|\boldsymbol{x}$ was 0.04 (median 0.0) when the nominal AR1 coefficient $\phi_{1,y}$ was 0, 0.12 (median 0.14) with $\phi_{1,y}$ = 0.2, and 0.29 (median 0.35) with $\phi_{1,y}$ = 0.5. A graphical representation of this trend is in Fig. 1. Parallel trends exist for $x_1$ and $x_2$. As expected, from the central limit theorem (Dai 2004), the apparent among-stand variance in $y|\boldsymbol{x}$ decreased with stand-size. The decline was approximately linear within the limits of the test settings with a slope of –0.6% per increase of one units in stand size. An autocorrelation in $\boldsymbol{x}$ also modified (confounded) the apparent among-stand effects in $y|\boldsymbol{x}$. In a non-linear exponential (log-linear) regression model with an intercept, stand size, $\phi_{1,\boldsymbol{x}}$, and $\phi_{1,y}$ as explanatory variables, the regression coefficient of 0.99 to $\phi_{1,\boldsymbol{x}}$ was highly significant $\hat{t}$ = 10.28 indicating an increase in $\hat{\sigma}^2_{y|\boldsymbol{x},\,\text{stand}}$ of approximately 13% for every 0.1 increase in $\phi_{1,\boldsymbol{x}}$. The effect of an increase of 0.1 in $\phi_{1,y}$ was approximately four times stronger.

The success of the proposed estimation strategy for the variance of a predicted stand mean depends on how well $\ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\,\text{stand}}$ approximates $\hat{\sigma}^2_{y|\boldsymbol{x},\,\text{stand}}$. Fig. 2 indicates the achievements. Although the correlation was 0.7, it becomes clear that there are several cases with a non-trivial over-estimation; foremost

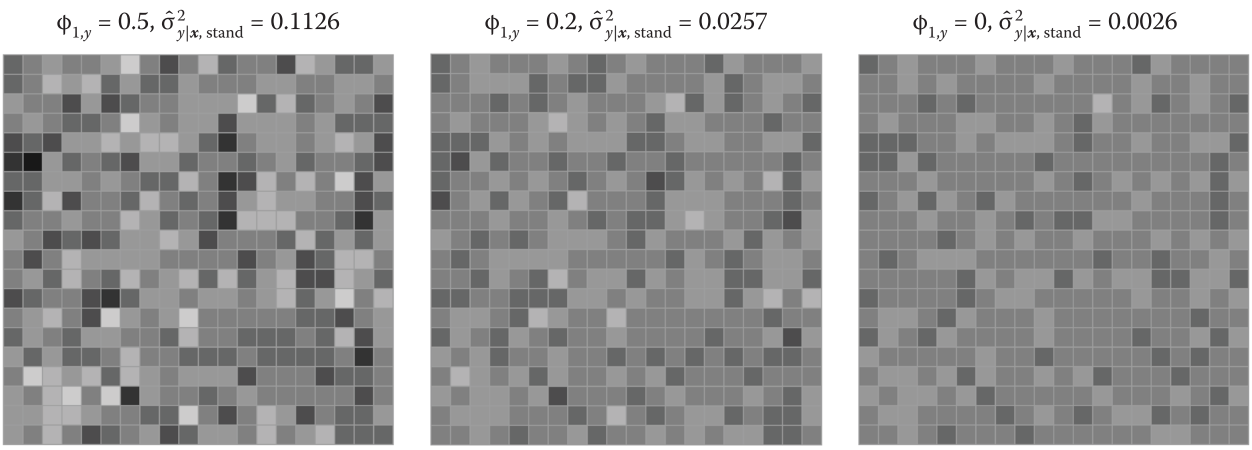


Fig. 1. Examples of apparent among-stand variance in $y|\boldsymbol{x}$ introduced by a spatial autocorrelation in $y$

Means of $y$ in 400 stands in a 20 × 20 array are indicated with gray tones (minimum is black, maximum is white). The examples are with a stand-size of 49 units in a square array. The strength of the AR1 coefficient ($\phi_1$) in $y$ and the ANOVA estimate of the apparent among-stand variance in $y|\boldsymbol{x}$ (i.e. $\hat{\sigma}^2_{y|\boldsymbol{x},\,\text{stand}}$) is indicated

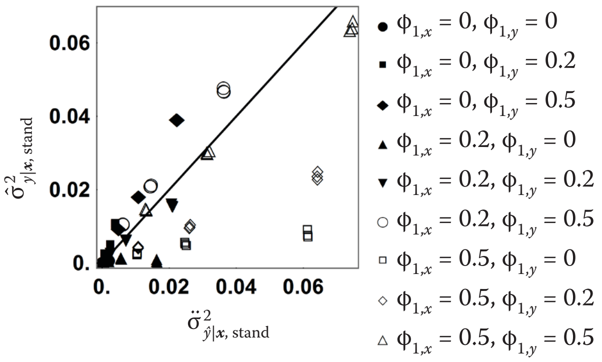


Fig. 2. The ANOVA-based estimates of the apparent among-stand variance in $y|\boldsymbol{x}$ ($\hat{\sigma}^2_{y|\boldsymbol{x},\text{ stand}}$) plotted against the proposed proxy ($\ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\text{ stand}}$).

A one-to-one line is provided for orientation

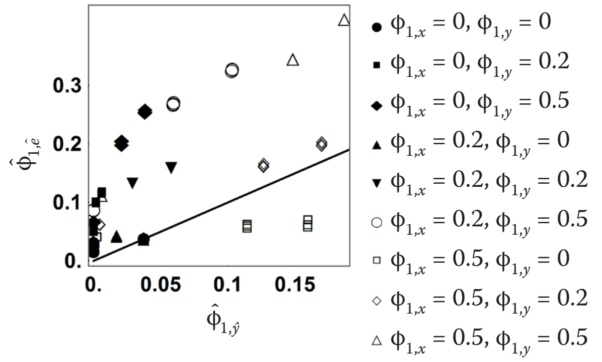


Fig. 3. ML estimates of the first-order autoregressive coefficient in empirical model residuals ($\hat{\phi}_{1,\hat{e}}$) plotted against proposed proxy ($\hat{\phi}_{1,\hat{y}}$)

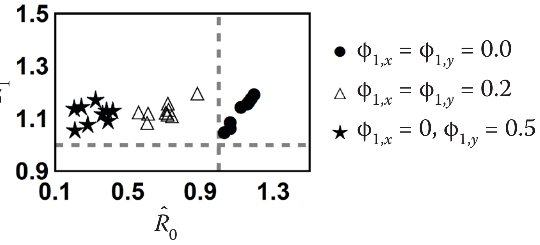


Fig. 4. Scatterplot of ratios $\hat{R}_1$ and $\hat{R}_0$ (cf. text for details) in settings with equal spatial autocorrelation in $\boldsymbol{x}$ and $y$.

A gray dashed line indicates a target ratio of 1.0

in settings with $\phi_{1,\boldsymbol{x}} > \backslash\phi_{1,y}$ where the overestimation appears to increase with the absolute difference between $\phi_{1,x}$ and $\phi_{1,y}$. On average, $\ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\text{ stand}}$ over-estimated $\hat{\sigma}^2_{y|\boldsymbol{x},\text{ stand}}$ by 38%. Stand size and sample size had no statistically significant effect on the inflation. Results would not have improved with previously proposed proxies (Magnussen 2016; Magnussen, Breidenbach 2017). The overestimation, with $\phi_{1,\boldsymbol{x}} > 0$ and $\phi_{1,y} = 0$, was expected since a positive $\phi_{1,\boldsymbol{x}}$ will, as detailed above, generate stronger apparent among-stand variances in predictions of $\hat{y}$ than in $y$. The restriction $\ddot{\sigma}^2_{\hat{y}|\boldsymbol{x},\text{ stand}} \equiv 0$, when $\hat{\rho}_{\hat{y}}$ was less than or equal to 1.96 times its standard error, did not provide an effective protection against an overestimation because $\hat{\rho}_{\hat{y}}$ was deemed significant in all cases with either $\phi_{1,y}$ or $\phi_{1,\boldsymbol{x}}$ greater than or equal to 0.2. A correction would require a de-correlation of within-stand predictions (Kessy et al. 2018). It was considered, but deemed impractical. In settings with the smallest stand size (9 units), and no autocorrelation in $y$ or $\boldsymbol{x}$, the $\hat{\rho}_{\hat{y}}$ was accepted as greater than zero in approximately one in every six tests of significance. In stands with 49 units, the frequency dropped to one in ten, and to zero in the stands with 121 units.

The proposed proxy for – an otherwise non-estimable within-stand AR1 coefficient – in model residual errors ($\hat{\phi}_{1m,\hat{y}}$) was only (on average) moderately correlated (0.67) with the empirical MLE estimates of the autocorrelation parameter ($\hat{\phi}_{1m,\hat{e}}$). On average (over stands and replications), the proxy was 2.6 times too small. Fig. 3 illustrates the association between the proxy and its target. The underestimation suggests, at first glance, an increase to the multiplier 0.4 (Magnussen et al. 2016a) applied to the autocorrelation in predictions of $Y$. However, when the simulations were repeated with a multiplier of 1.0 only minor changes in the averages taken over stands and replications were noted. For example, achieved coverages, with a mean change of 0.3% changed by less than 1% in two out of three cases. Where changes were larger they were in the direction of over-coverage. None of the differences were statistically significant different from 0 at the 5% level of uncertainty (Holm 1979). Hence, a user can freely choose a multiplier between 0.4 and 1.0. The under-estimation of the autocorrelation in model residuals leads to a reduction of the inflation factor $\gamma_{Wm}$ applied to the residual variance (cf. Eq. 3). Across the 81 settings, the inflation based on actual empirical model residuals would have been 1.7 whereas it was 1.2 when derived from the proxy.

The ratio $\hat{R}_1$ of the expected variance $\hat{\text{Var}}_1(\tilde{y}_m)$ – obtained under the proposed estimation strategy – to the actual mean squared error, was, as intended, always greater than or equal to the ratio $\hat{R}_0$ generated from the benchmark variance estimator $\hat{\text{Var}}_0(\tilde{y}_m)$. They were almost equal (1.135 and 1.127) and nearly perfectly correlated in settings with no autocorrelation in $\boldsymbol{x}$ and $y$ (Fig. 4). The positive deviation from 1.0 follows from the expectation of a quadratic form (Searle 1982). For the same reason, both ratios also increased with a decreasing stand size and a decreasing sample size. In settings with a nominal AR1 process in both $\boldsymbol{x}$ and $y$ of 0.2, the $\hat{R}_0$ dropped to 0.70 whereas $\hat{R}_1 = 1.14$ remained close to the value

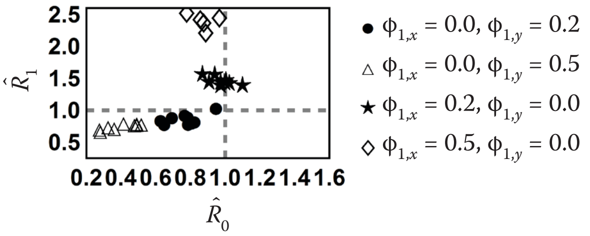


Fig. 5. Scatterplot of ratios $\hat{R}_1$ and $\hat{R}_0$ (cf. text for details) in settings with a positive spatial autocorrelation in either $\boldsymbol{x}$ or $y$ but not both

A gray dashed line indicates a target ratio of 1.0

in settings without a spatial autocorrelation. Here, stand size and sample size effects became less pronounced. Increasing the lag-one autocorrelation in $\boldsymbol{x}$ and $y$ to 0.5 caused a drop in $\hat{R}_0$ to 0.31. Again, $\hat{R}_1$ with a value of 1.12 remained close to the value in settings without a spatial autocorrelation. Thus, in settings where the strength of a spatial autocorrelation in the dependent and the explanatory variables are comparable, the proposed estimation strategy appears to work as intended by providing protection against an over-estimation of precision.

With different strength of the spatial autocorrelation in $\boldsymbol{x}$ and $y$ the performance of $\hat{R}_1$ became uneven. In settings with a nominal positive autocorrelation in $y$ but none in $\boldsymbol{x}$, both ratios were below 1 (Fig. 5) yet with $\hat{R}_1$ closer to 1.0 than $\hat{R}_0$, and $\hat{R}_0$ decreasing much faster with an increase in $\phi_{1,y}$ than $\hat{R}_1$. The estimation strategy seems preferable also in these settings. A different picture emerges when a positive autocorrelation is restricted to $\boldsymbol{x}$. Then $\hat{R}_1$ is 1.45 ($\phi_{1,x}$ = 0.2) and 2.64 ($\phi_{1,x}$ = 1.64) with $\hat{R}_0$ at 0.98 and 0.80. In these two settings, the estimation strategy is quite conservative with more protection – against over-estimating precision – than needed. The performance of $\hat{R}_1$ in settings with $\phi_{1,x} > \phi_{1,y} > 0$ and $\phi_{1,y} > \phi_{1,x} > 0$ is displayed in Fig. 6. The ratios for the standard estimator of variance came to 0.58 and 0.37, respectively. Corresponding values for $\hat{R}$1 came to 1.95 and 0.85. Thus, in one case ($\phi_{1,y} > \phi_{1,x} > 0$) the estimation strategy seems to

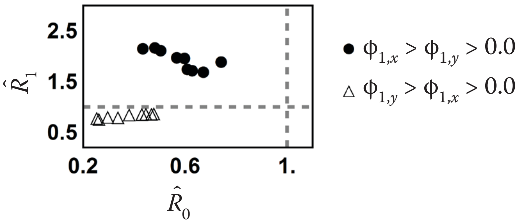


Fig. 6. Scatterplot of ratios $\hat{R}_1$ and $\hat{R}_0$ (cf. text for details) in settings with unequal but positive spatial autocorrelation in $\boldsymbol{x}$ and $y$.

A gray dashed line indicates a target ratio of 1.0

Table 1. Marginal Tobit regression estimates and standard errors of coverage in nominal 95% confidence interval obtained with a standard estimator of variance $\hat{\mathrm{Var}}_0(\tilde{y}_m)$ and the variance obtained with the proposed estimation strategy $\hat{\mathrm{Var}}_1(\tilde{y}_m)$. Estimates of standard errors are in parentheses

| $\phi_{1,x}$ | $\phi_{1,y}$ | $\hat{CO}_{95}$ via $\hat{\mathrm{Var}}_0(\tilde{y}_m)$ | $\hat{CO}_{95}$ via $\hat{\mathrm{Var}}_1(\tilde{y}_m)$ |
|---|---|---|---|
| 0.0 | 0.0 | 0.93 (0.01) | 0.95 (0.02) |
| 0.0 | 0.2 | 0.91 (0.01) | 0.93 (0.03) |
| 0.0 | 0.5 | 0.78 (0.01) | 0.91 (0.03) |
| 0.2 | 0.0 | 0.95 (0.01) | 0.98 (0.02) |
| 0.2 | 0.2 | 0.89 (0.01) | 0.96 (0.02) |
| 0.2 | 0.5 | 0.76 (0.01) | 0.93 (0.03) |
| 0.5 | 0.0 | 0.92 (0.01) | 1.00 (0.00) |
| 0.5 | 0.2 | 0.85 (0.01) | 0.99 (0.01) |
| 0.5 | 0.5 | 0.71 (0.01) | 0.96 (0.02) |

work, in the other ($\phi_{1,x} > \phi_{1,y} > 0$), the strategy provides a conservative estimate of variance.

Without access to estimates of $\phi_{1,y}$ and the among-stand variance in $y|\boldsymbol{x}$, an analyst must resort to $\hat{\phi}_{1,y}$ and $\hat{\rho}_{\hat{y}}$ or other means to get an idea about the need for a protection against a possible serious over-estimation of precision of a predicted stand mean. The proposed estimation strategy appears to provide a critical protection when the standard variance estimator fails. The possible prospect of an over-protection ought to be a lesser detractor than a gross over-estimating of precision.

Results on coverage of nominal 95% confidence intervals ($CO_{95}$) are in Table 1 as marginal Tobit regression estimates of $CO_{95}$ in nine combinations of $\phi_{1,x}$, $\phi_{1,y}$ (Johnston, DiNardo 1997). They are marginal with respect to stand- and sample-size, which had similar effect on the two sets of coverage values – test: Hausman's specification test (Hausman 1978). Moreover, their effect-sizes were an order of magnitude weaker than those associated with $\phi_{1,x}$ or $\phi_{1,y}$. As expected, the results reflect, to a large degree, those for the ratios $\hat{R}_0$ and $\hat{R}_1$. With the estimation strategy, coverage stays above 0.90 and it provides the desired protection when $\phi_{1,x} = \phi_{1,y} > 0$ but also provides too much protection in settings with $\phi_{1,x} > \phi_{1,y} \geq 0$. For confidence intervals computed with the standard estimator of variance, the coverage is poor in settings with a positive autocorrelation in $y$.

In simulations with random stand-effects in the intercept of a linear model linking $Y$ to $\boldsymbol{X}$ and a possible autocorrelation in $Y$, $\boldsymbol{X}$, or both, the performance of the standard (benchmark) estimator of variance was worse than depicted here (Magnussen 2016;

Magnussen et al. 2016a; Magnussen, Breidenbach 2017). In practical applications with actual stand effects, the proposed strategy can be expected to be least as attractive as in the settings tested here because the correlation between $\boldsymbol{X}$ and $Y$ will then generate apparent stand effects in predictions of $y$.

## CONCLUSIONS

In forest enterprise inventories – supported with census data of LiDAR metrics from an airborne laser scanning and correlated with the attribute of interest – it is now a common practice to predict the value of key forest inventory attributes for every unit (viz. pixel or ALS plot) in a stand from a subset of LiDAR metrics (Næsset 2004; Maltamo et al. 2010; Fernández-Landa et al. 2018). Due to cost restrictions on sample sizes in forest inventories (Junttila et al. 2013), a fitted model is typically stratum-specific and assumed to provide unbiased stand-level predictions. For stratum-level summaries, statistics, and inference the success of LiDAR is recognized (Wulder et al. 2013; Melville et al. 2015; Gregoire et al. 2016). At the scale of forest stands, however, predictions from a higher-level model may be biased, and textbook estimators of uncertainty may grossly over-estimate precision due to unaccounted stand effects, spatial autocorrelations, or both. The proposed estimation strategy did not address the bias problem. The proposed strategy is also relevant for strata with seemingly homogenous stands even if the within-stand spatial autocorrelation may be weak or zero because a within-stand homogeneity promotes the among-stand variance which can constitute an even greater problem than autocorrelation per se (Magnussen et al. 2016a; Magnussen, Breidenbach 2017).

The proposed estimation strategy is tied to a linear model linking $Y$ to $\boldsymbol{X}$. Alternative area-based and single-tree models are also sensitive to the impact stand effects and spatial autocorrelation (Mauro et al. 2017). Strategies to counter over-estimating precision are warranted also in these cases. Finite mixture modelling (Zhang et al. 2004), robust variance estimation (Binder 1983), or ANOVA-based estimation of an among-stand variance derived from clusters of stands with two to four field samples, may provide avenues towards refined estimation strategies.

The prospect of grossly over-estimating precision of a predicted stand mean, ought to act as a sufficient motivator for continued efforts to address the small area estimation problem in forest inventories.

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