Time as a Dimension of the Sample Design in National-Scale Forest Inventories

Francis A. Roesch and Paul C. Van Deusen

Abstract: Historically, the goal of forest inventories has been to determine the extent of the timber resource. Predictions of how the resource was changing were made by comparing differences between successive inventories. The general view of the associated sample design was with selection probabilities based on land area observed at a discrete point in time. Time was not considered to be part of the sample design because it was not considered to be an element of the sampled population. Over the last few decades, the general goal of many national-scale forest inventories has been changing to monitoring the dynamic forest ecosystem. Here, we explore the inferential advantages of replacing the two-dimensional areal probability paradigm with a three-dimensional spatiotemporal probability paradigm. Our general discussion is augmented with a simulated example for estimating annual growth by diameter classes. Two assumptions of temporal indifference and remeasurement interval length indifference, which arose because of the two-dimensional view, are investigated through a simulation. The simulation compares and contrasts five estimators that differ in their reliance on those assumptions to make annual estimates. The results of the simulations often show those assumptions to be bias inducing. FOR. SCI. ■■(■):000–000.

Keywords: sampling forest change, spatiotemporal sample design, size class estimation

NATIONAL-SCALE FOREST MONITORING EFFORTS are concerned with evaluating the dynamic state of a nation's forest populations. The sampling schemes for most inventories of this scale have been described, historically, as areal-based. There are many descriptions of areal sampling schemes, some of which can be found in Avery and Burkhart (2002), Shiver and Borders (1996), Husch et al. (2003), Kangas and Maltamo (2006), Köhl et al. (2006), Mandallaz (2007), and Gregoire and Valentine (2008). The view is often of an area sample selected at fixed points in time. Many countries adopted sampling systems using permanent plots, and change is determined by estimating differences between successive areal samples. The general target temporal interval between successive areal samples is often funding-dependent. In addition, the length of time that it takes to conduct a single areal sample can be funding-dependent, and the length of time between observations on individual plots can be logistics-dependent or random. Two facilitating, usually tacit and unacknowledged, assumptions arose from the two-dimensional view of National Forest Inventory (NFI) designs. The first is that variation in the time of observation for an individual areal sample is ignorable. This assumption had been implicit in almost every forest inventory ever conducted and was named the temporal indifference assumption in Bechtold and Patterson (2005). The second related assumption is that variation in the remeasurement period lengths between individual plots in successive areal samples is ignorable. For example, an average annual value would be calculated for each remeasured plot, and the values for all plots would be combined, regardless of the distribution of individual temporal interval lengths. We call this the remeasurement period (REMPER) assumption. Although these assumptions underlie almost all NFIs, their impact increases as the length of time for a single areal sample increases, the time between areal samples decreases, and the diversity of measurement interval lengths increases. In this article, we explore the effects of these assumptions and discuss how inference can be improved by negating their necessity through adoption of the three-dimensional sampling paradigm of Roesch (2008).

Roesch (2008) noted that in the more recent overlapping, panelized sample designs, the determination of the set of observation times can be treated as random, resulting in a three-dimensional sampled population and sampling frame, the two dimensions of land area and the third dimension of time. The sample unit is a three-dimensional puzzle piece or volume. The volume of a sample unit, in (area × time) units, is divided by the volume of the population to determine the probability of selection for the unit. The population is divided into mutually exclusive, exhaustive sample units (the three-dimensional puzzle pieces), which in toto comprise the sample frame. Under this model, each unit has a definite probability of selection, and the total of these probabilities is equal to 1.

The importance of the distinction between the two-dimensional and three-dimensional paradigms is becoming increasingly clear as the goals of NFIs have broadened in scope over the last few decades. Increased environmental awareness has precipitated this broadening of goals, morphing many national-scale forest inventories into

Manuscript received June 27, 2012; accepted November 21, 2012; published online January 24, 2013; http://dx.doi.org/10.5849/forsci.12-075.

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Acknowledgments: The authors were funded by their respective institutions, the USDA Forest Service Forest Inventory and Analysis Program, and the National Council for Air and Stream Improvement. The authors thank the associate editor and three anonymous reviewers for their helpful comments.

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Table 1. Diameter classes corresponding to standard merchantability limits in the United States.

Diameter class	Lower limit (diameter ≥)	Upper limit (diameter <)
	. . . .(cm)	
D1	0	12.7
D2	12.7	17.8
D3	17.8	22.9
D4	22.9	27.9
D5	27.9	∞

full-fledged efforts to monitor many aspects of dynamic forest ecosystems.

Traditionally, many measures associated with forest trees have been reported within tree size classes, such as tree diameter classes. For instance, basal area or volume growth within 5-cm diameter classes for each year within a specific period may be of interest. In temperate regions, the contribution of measurement error to total variance is usually large enough to preclude the measurement of the same trees more frequently than approximately every 5 years. Although some minimum period of time is necessary between observations to reduce the effect of measurement error on change estimates, often this period of time, or remeasurement interval, will be long enough for a large number of trees in the population to grow through multiple life stages or size classes, which creates a potential problem from the viewpoint of successively applied two-dimensional samples. We show below that estimation of intermediate unobserved transitions through classes under the three-dimensional paradigm is both obvious and manageable.

To exploit the three-dimensional view, we must look at the data differently from the traditional view. With respect to annual growth, we note that each sample plot is not only

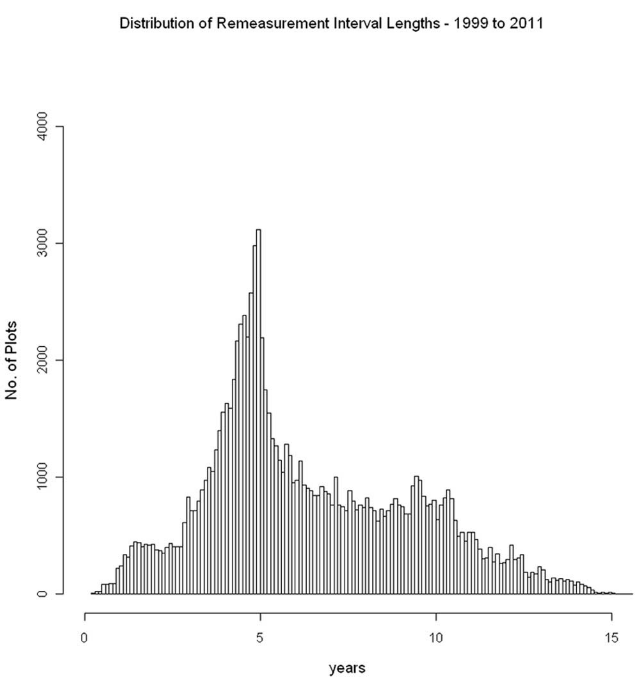

Figure 1. The distribution of remeasurement interval lengths for all plots remeasured from 1999 to 2011.

located in a particular place but is also observed at particular times, and that the times of observation are possibly more important than the place of observation. For simplicity, our notation will be limited to two observations for each plot. We will use the general three-dimensional selection model given in Roesch (2008) with the exception that time will be rescaled relative to the proportion of the growing season elapsed within each year. Assign to each observation of variable x labels for plot i and a growing season-adjusted beginning date t_i^b and ending date t_i^e, separated by the (adjusted) time span of s_i years. Represent each of these observations as x_i^b and t_i^e, respectively. The dates and times are adjusted to approximate the time of observation relative to the proportion of growing season elapsed within 1 year. Although beyond the scope of this investigation, this could be done using data contributing to the US Department of Agriculture (USDA) plant hardiness zone maps (US Department of Agriculture 2012). For simplicity, we make two assumptions, both of which can be refined by an appropriate model to suit a particular investigator or alternative application, as needed. The first is that we assume that the growing season spans from March 1 to November 30 everywhere within the area of interest. The second is that growth for each plot is uniform throughout the growing season. We can then represent each observation date as the year of observation plus the proportion of the growing season that has elapsed (i.e., in the format year · p), and s_i is simply the difference between the two. Because we have no observations between x_i^b and t_i^e, we make the further assumption that basal area growth for each living tree is uniform between the two observations (e.g., across s_i). This assumption could also be refined by the application of the appropriate model, such as conditioning on x_i^b or on annual precipitation. We then allocate the proportion of basal area growth observed over s_i to the proportion of each year spanned by s_i (thereby accounting for the marginal probability of the time dimension). This assumption of linear (basal area) growth is an approximation that should only be used for relatively short-time intervals. Well-developed growth models would provide better estimates for individual trees but can be unavailable for many of the species and condition classes encountered in a wide-area forest monitoring effort. The assumption that basal area growth is uniform between observations allows us to estimate when the threshold for each diameter class limit was crossed and to allocate growth within diameter classes to the years the growth occurred in those diameter classes. This development leads immediately to two simple time-adjusted estimators for annual basal area growth (within diameter class); the first is a mean of ratios (MOR) estimator (or a probability proportional to size estimator in the three-dimensional paradigm),

$$BAG_y^{DCMOR} = \frac{1}{n_y} \sum_{i=1}^{n_y} \frac{bag_{i,y}}{P_{i,y}}, \tag{1}$$

where n_y is the number of plots observing growth in year y, $p_{i,\,y}$ is the product of portion of year y growing season observed by plot i and the portion of plot i area within the area of interest, and $bag_{i,\,y}$ is the basal area growth observed on plot i and assignable to year y. The second estimator is a ratio of means (ROM) estimator:

$$BAG_y^{DCROM} = \frac{\sum_{i=1}^{n_y} bag_{i,y}}{\sum_{i=1}^{n_y} P_{i,y}}, \qquad (2)$$

The general statistical properties of ratio estimators can be found in Raj (1968), Cassel et al. (1977), or Cochran (1977).

To investigate the effects of the temporal indifference assumption and the REMPER assumption, we compare these two estimators that do not rely on the temporal indifference assumption with three estimators that do rely on the temporal indifference assumption, after we compare all five estimators with themselves under varying levels of diversity in remeasurement interval length. The first of the three estimators is currently used by the USDA Forest Service's Forest Inventory and Analysis (FIA) Program. To facilitate an understanding of the estimator, a brief description of the sample design is as follows.

FIA conducts a continuous forest inventory using a rotating panel sample design, which has now been described in many publications, such as Bechtold and Patterson (2005) and Roesch (2007a). The design consists of g mutually exclusive, spatially disjoint temporal panels. One panel per year is measured, in turn, for g consecutive years, after which the panel measurement sequence reinitiates. Each complete set of measurements, on all panels, is referred to as a cycle, which constitutes a single, complete areal sample. Assume that the continuous inventory consists of n_c cycles and, therefore, $P = n_c g$ years. That is, if panel 1 is measured in year y, it is also scheduled to be measured in years $y + g$, $y + 2g$, and so on, through to year $y + (n_c - 1)g$. Panel 2 would then be measured in years $y + 1$, $y + 1 + g$, $y + 1 + 2g$, etc. Estimates of change in the earlier years of our data range come from observations in which plots from earlier designs were colocated with plots from this new panel design, using a variant of method 3a described in Roesch and Reams (1999).

Because FIA adheres to a two-dimensional view of this design, the program groups these data into evaluation

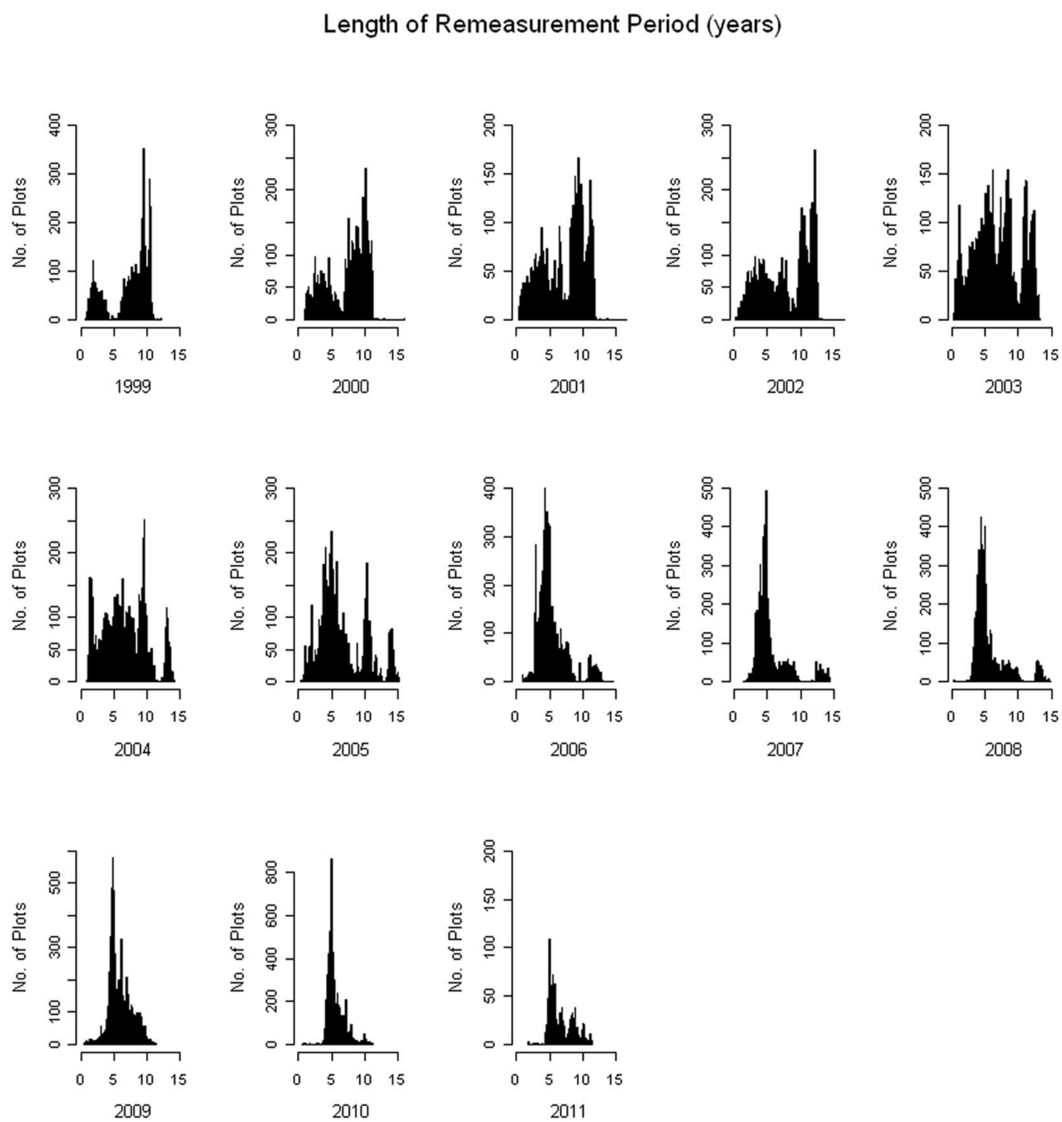

Figure 2. The distribution of remeasurement interval lengths for all plots remeasured from 1999 to 2011, by remeasurement year.

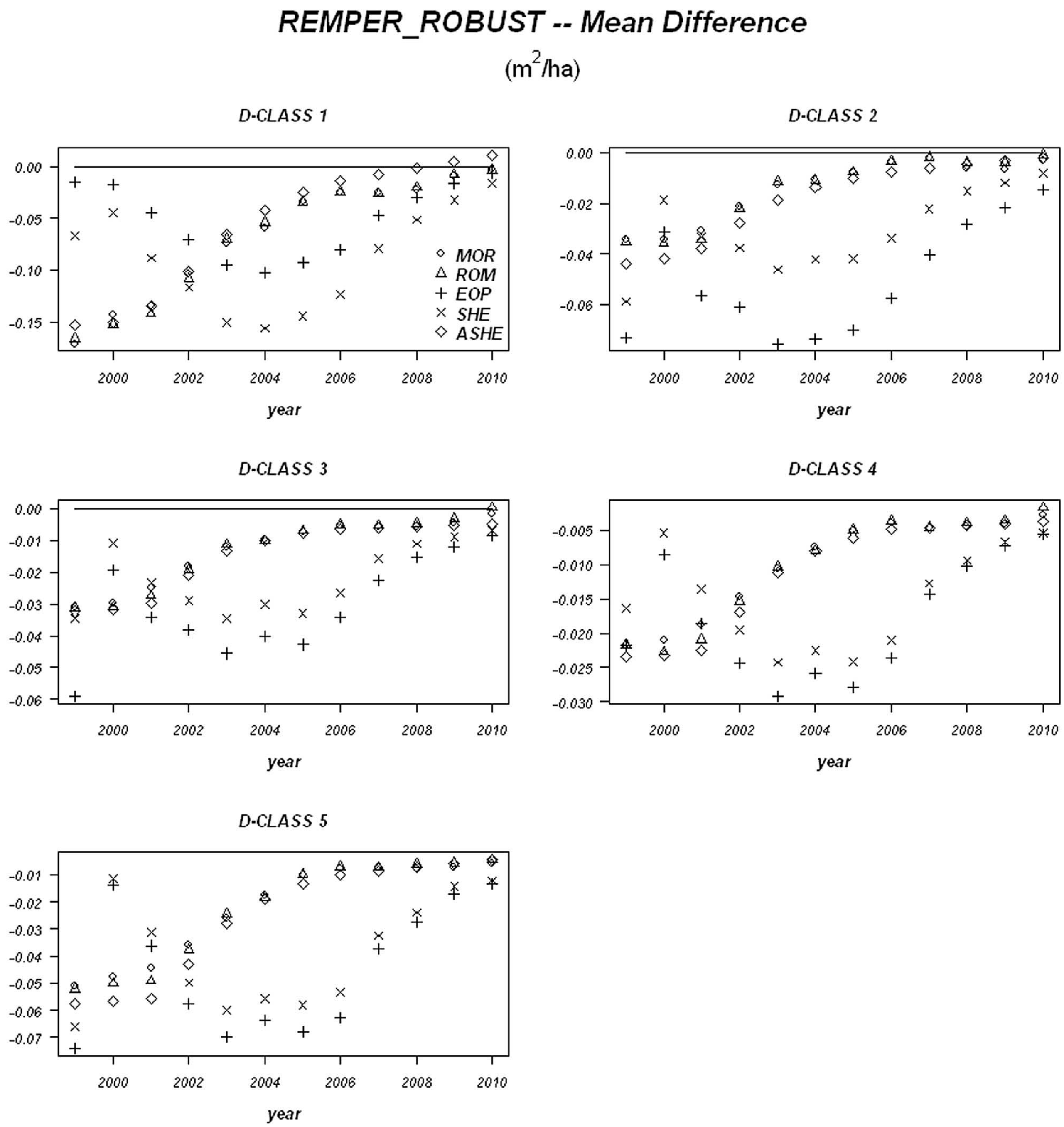

Figure 3. The mean difference (MD), over the 1,000 iterations of 1,000 samples each, between each estimator and the corresponding large sample result from the target population for the same estimator, by diameter class and estimation year.

groups of g years and then ignores any temporal differences in observations within the evaluation group. The interested reader is referred to the temporally indifferent method in Bechtold and Patterson (2005), which leads to the end-of-period (EOP) estimator:

$$BAG_y^{DCEOP} = \frac{\sum_{i=1}^{n_{EG}} abag_{i,r_i}}{n_{EG}}, \tag{3}$$

where Y is the scheduled final year of measurement for the final panel in the evaluation group, n_{EG} is the number of plots in an evaluation group, and $abag_{i,\,r_i}$ is the average annual basal area growth for plot i over the plot remeasurement period, r_i. Without loss of generality, here, we assume that an evaluation group consists of five successive annual panels. Note that in Equation 3, all of the growth for each tree is applied to a single diameter class, regardless of how many diameter classes the tree was in during the measurement interval. Keeping with the philosophy of an end-of-period (EOP) estimator, we use each tree's final diameter measurement to determine diameter class. Arguably, each tree's initial diameter could be used instead to determine diameter class; however, neither solution leads to an unbiased estimate of growth within diameter classes. This observation led to a suggestion by Sheffield and Turner

(2010) to partition each tree's growth into the diameter classes within which that growth occurred. For basal area, this is a simple assignment:

$$BAG_y^{DCSHE} = \frac{\sum_{i=1}^{n_{EG}} abag^{dc}{}_{i,r_i}}{n_{EG}}, \qquad (4)$$

where dc indicates diameter class. Note that in Equation 4, all growth is assigned to the diameter class within which it occurred, but the estimator is like Equation 3 in that it is an EOP estimator. In the presence of trend, both EOP estimators, Equations 3 and 4, will be affected by lag bias. In a five-panel system, the five panels are measured for growth over a 10-year period; that is, panel 1 is measured in years 1 and 6, panel 2 is measured in years 2 and 7, and so on. Roesch (2007b) argued that the average annual growth within each panel is best applied to the center of the measurement interval. The successive centers for these five panels are at years 3.5, 4.5, 5.5, 6.5, and 7.5, with a mean of 5.5, which suggests that the estimator in Equation 4 should be "time corrected" by −4.5 years in a five-annual panel evaluation group, leading to the adjusted Sheffield estimator (ASHE):

$$BAG_y^{DCASHE} = .5\left(BAG_{y+4}^{DCSHE} + BAG_{y+5}^{DCSHE}\right). \qquad (5)$$

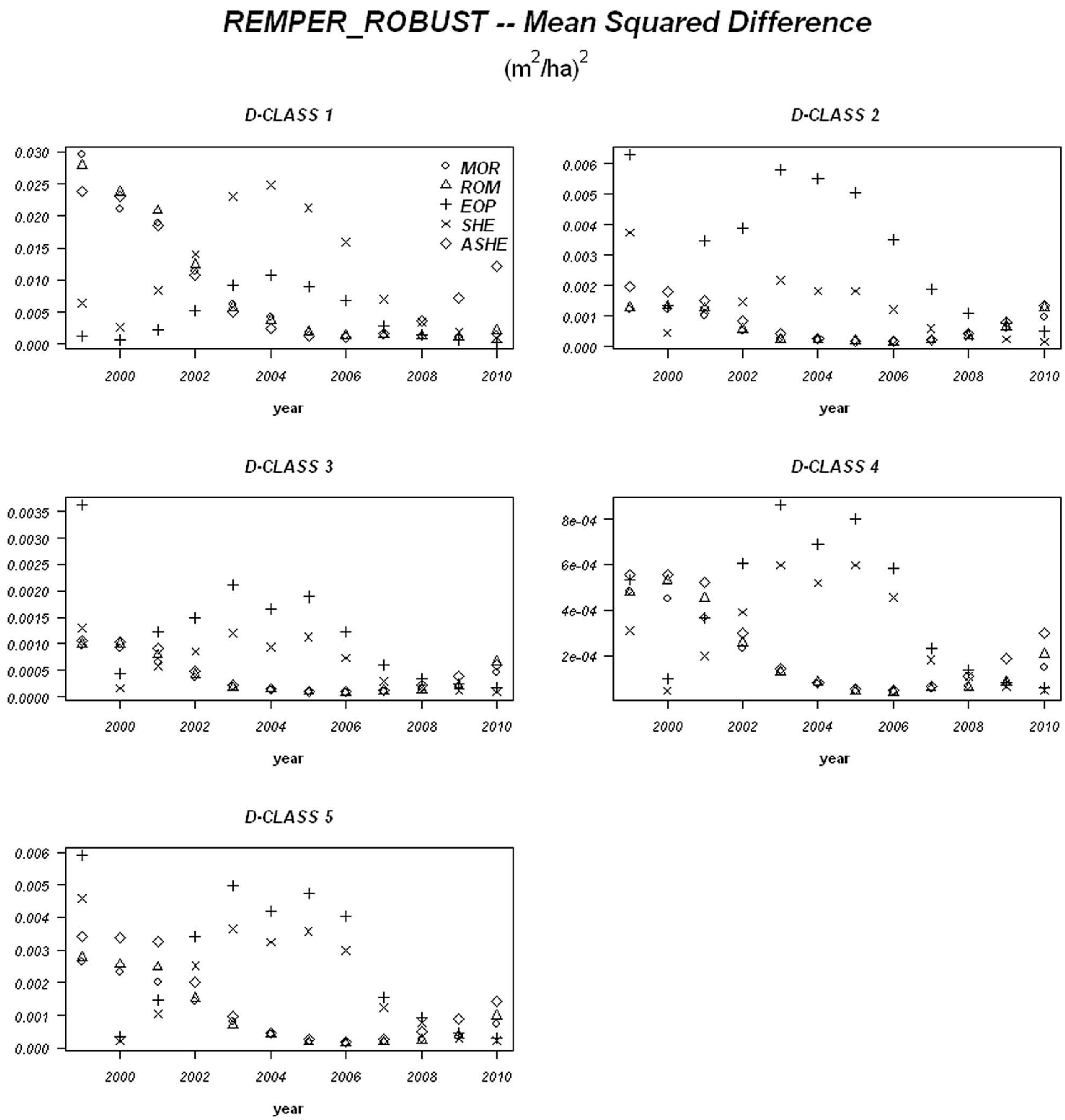

Figure 4. The mean squared difference (MSD), over the 1,000 iterations of 1,000 samples each, between each estimator and the corresponding large sample result from the target population for the same estimator, by diameter class and estimation year.

The derivations of the analogous estimators for different numbers of panels are left as an exercise for the reader. The estimator in Equation 5 would fit within FIA's current accounting system; however, it does not account for the fact that when individual trees grow through multiple diameter classes, they grow through the lower diameter classes first, as the estimators in Equations 1 and 2 do. Another significant difference between the estimators in Equations 1 and 2 and the estimators in Equations 3, 4, and 5 is that the former are based on actual measurement year, whereas the latter are based on "inventory year" or scheduled year of measurement, which, in later years is usually, but not necessarily always, the same as the actual year of measurement.

Methods

To motivate our discussion, in this application, we use data from FIA to estimate annual basal area growth of all living trees within the size classes given in Table 1 over a defined area (A) and temporal period. Although any classification system could be used, without loss of generality, for this example, the size classification shown in Table 1 is used. The diameters in Table 1 correspond to the metric equivalents of standard merchantability limits in the United States.

We conducted two tests of the assumptions on which these estimators are based. Specifically, we strove to test the estimators' abilities to provide estimates of the average annual basal area growth within diameter classes over a 5-year period. A difficulty arises in actual inventory data because most plots are not actually measured at an exact temporal interval. The problem that this causes when one is attempting to estimate average annual growth was discussed in Roesch (2007b) (see the discussion in that article of Figures 2 and 3) and is often ignored in NFIs under the assumption that the "average annual growth" for all plots can be combined regardless of the diversity in remeasurement period lengths. We will dub this the REMPER assumption (after the name of the FIA remeasurement period variable, REMPER).

Test 1

To test the REMPER assumption, we started with a "super sample" of available data from a total of 120,143 remeasured FIA plots (both forested and nonforested) in 11

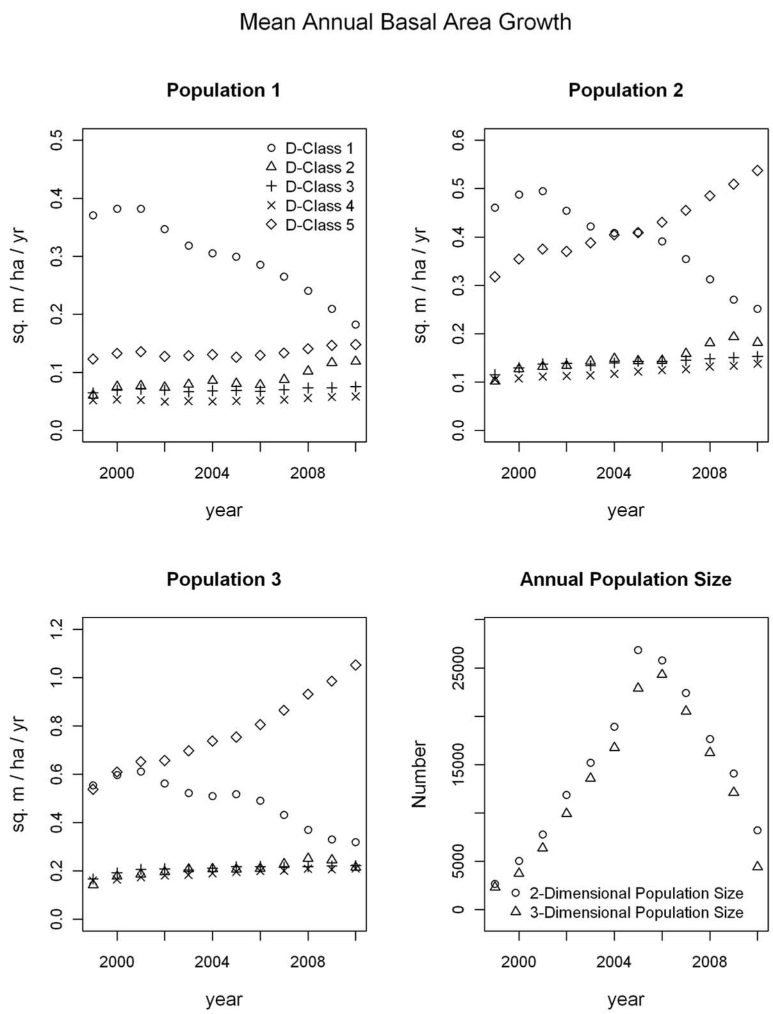

Figure 5. The population mean basal area growth for simulated populations 1, 2, and 3, by diameter class and estimation year. The two-dimensional and three-dimensional population sizes for each population by year are shown in the lower right-hand graph.

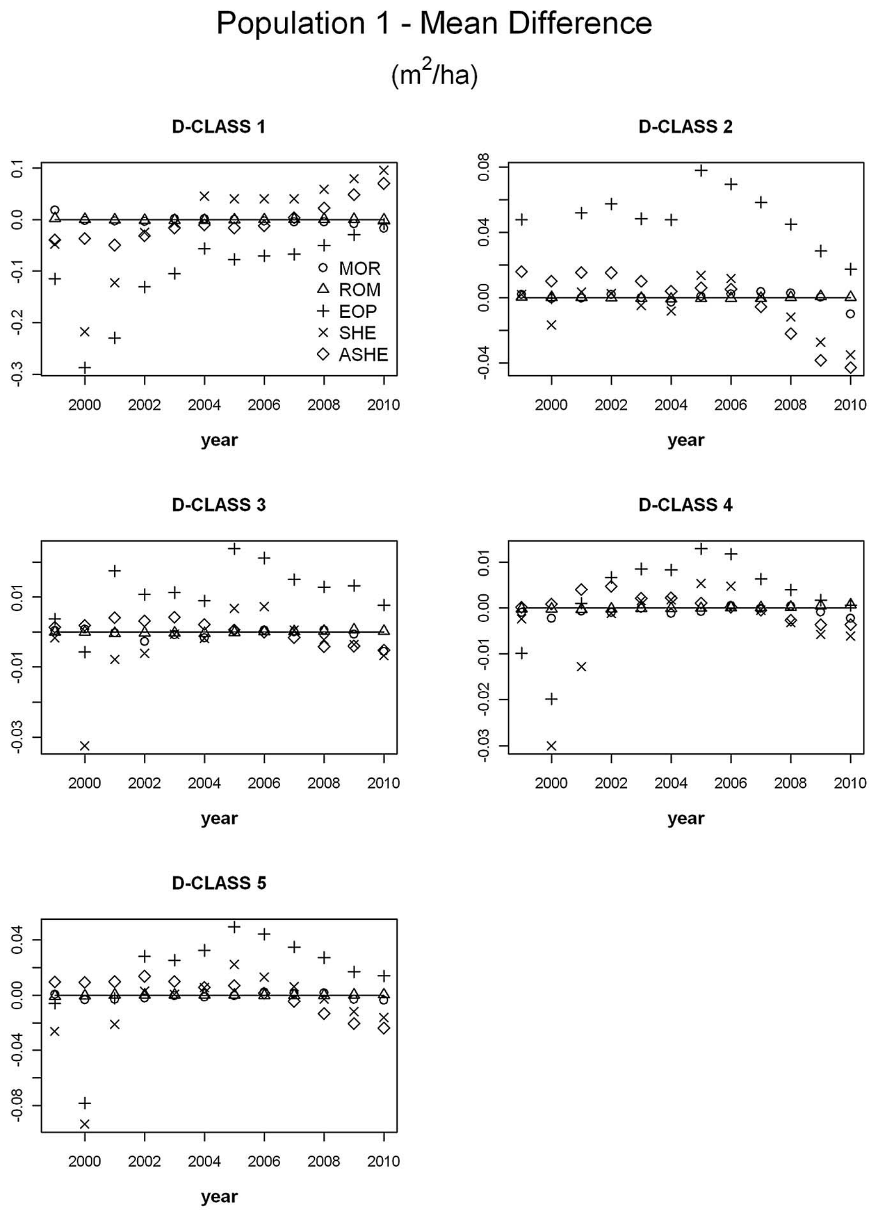

Figure 6. The MD, over the 1,000 iterations of 1,000 samples each, between each estimator and the corresponding population 1 mean, by diameter class and estimation year.

states in the southern United States. Each tree observation on each plot has both a time 1 diameter (PREDBH) and a time 2 diameter (DBH) measurement. These states were Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, North Carolina, South Carolina, Tennessee, Texas, and Virginia, with remeasurement (time 2) dates from 1999 to 2011. The remeasurement interval lengths of plots in this super sample vary widely. The overall distribution of these remeasurement interval lengths is given in Figure 1. Earlier years in this study had a greater diversity in remeasurement interval lengths than later years because the transition from earlier designs to the panelized design, described above, occurred in the earlier years. We give the distribution of remeasurement interval lengths by remeasurement year in Figure 2.

We created a large sample of 23,679 plots from the target population by selecting all plots with a remeasurement period between 4.5 and 5.5 years from the super sample. We set this (apparently) small, nonzero tolerance on the target length of 5 years because the sample from the target population would have been extremely small if we had set a tolerance of 0. We then calculated all of the estimators from this large sample for the target population for the years 1999–2010. We treated these results as the population values to be estimated. If the REMPER assumption is valid and the actual remeasurement period is independent of any variable of interest, then the estimates from a small random sample of the super sample should have the same expected value as the estimates from the large sample of the target population.

In the simulation, we sampled 1,000 plots from the super sample (without replacement) for each of 1,000 iterations. For each year, we calculated the mean difference (MD) and the mean squared difference (MSD), over the 1,000

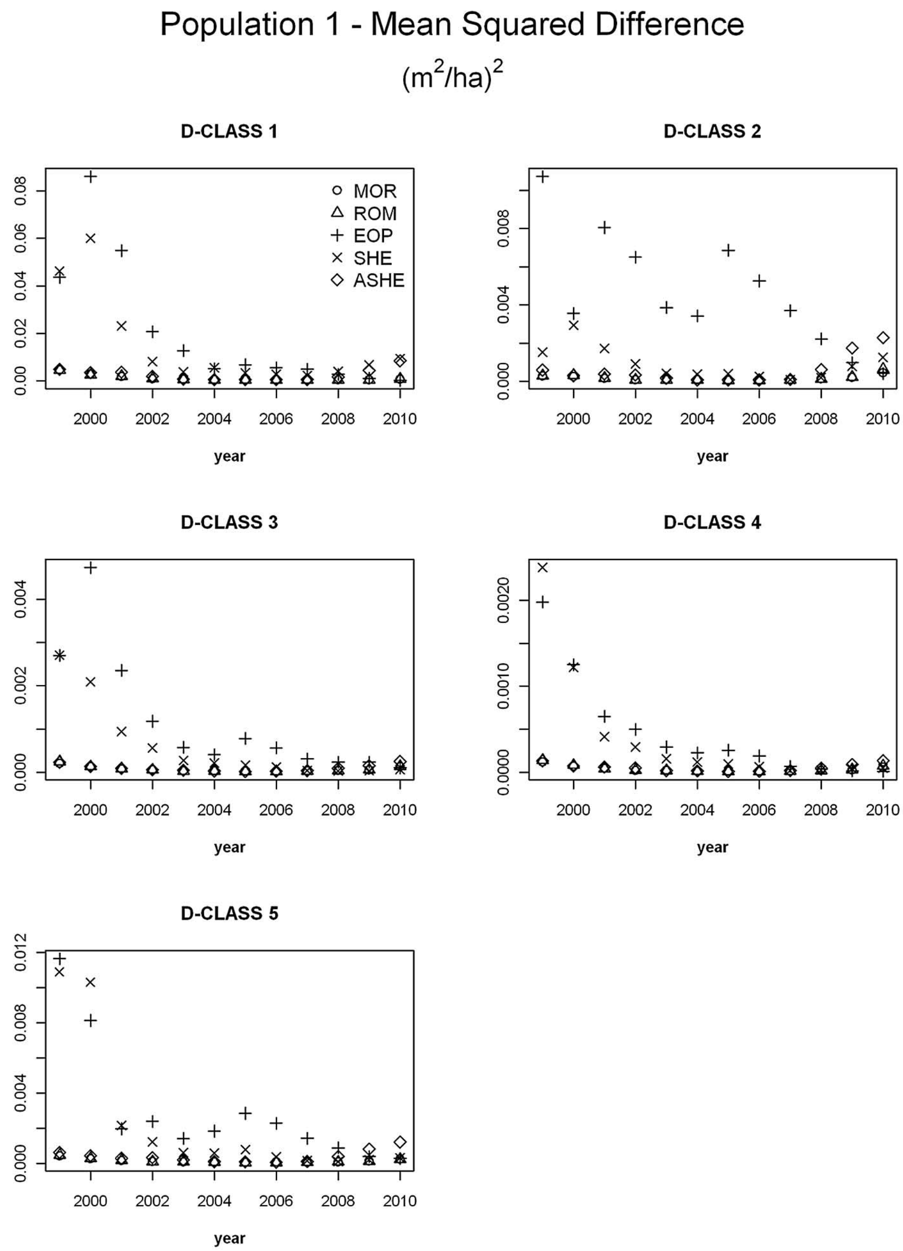

Figure 7. The MSD, over the 1,000 iterations of 1,000 samples each, between each estimator and the corresponding population 1 mean, by diameter class and estimation year.

iterations, between each estimate and the corresponding estimate from the large sample of the target population for the same estimator. That is,

$$MD = \frac{1}{1000} \sum_{i=1}^{1000} (\hat{x}_i^S - \hat{x}^T),$$

where $\hat{x}_i^s$ is the sample estimate of estimator x, for the iterate i sample from the super sample, and $\hat{x}^T$ is the large sample estimate for estimator x for the target population. Likewise,

$$MSD = \frac{1}{1000} \sum_{i=1}^{1000} (\hat{x}_i^S - \hat{x}^T)^2.$$

This tests the robustness of each estimator to the REMPER assumption. If we get empirical results that are large in MD or MSD, they will suggest that the underlying population of the super sample is different from that of the target population, rendering the REMPER assumption tenuous. The results from this test do not give an indication of the quality of the estimates derived from each estimator but do suggest how consistent and robust the estimators are when applied to temporally diverse intervals.

Test 2

Test 2 addresses the temporal indifference assumption, as well as the relative performance of the estimators with respect to the assumption. To do this, we built three simulated populations, by first selecting all plots from the super sample with a remeasurement period of exactly 5 years, resulting in 3,119 plots (set 1). Construction of each of the

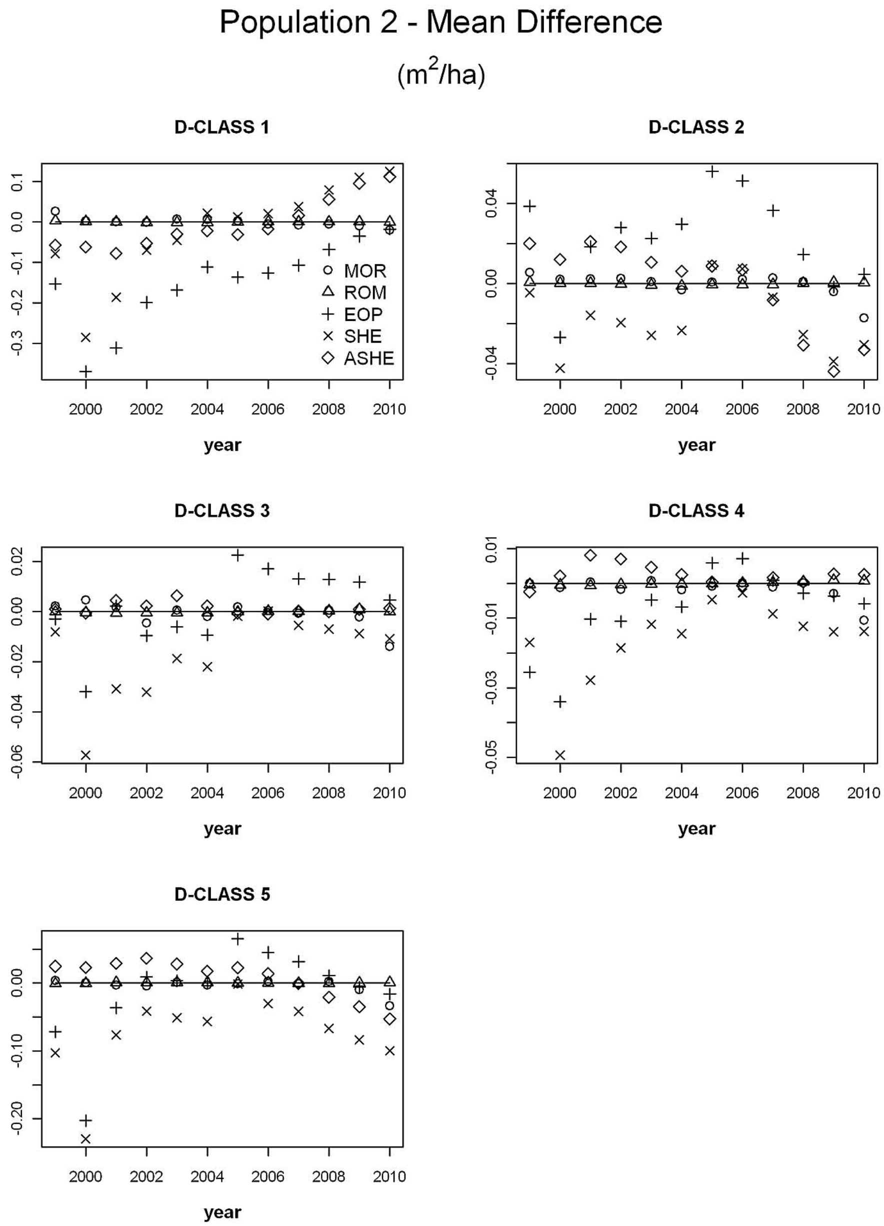

Figure 8. The MD, over the 1,000 iterations of 1,000 samples each, between each estimator and the corresponding population 2 mean, by diameter class and estimation year.

three populations started with 10 copies of set 1 (set 2). That is, set 2 has 31,190 plots. Variance was introduced into set 2, as follows, to ensure population variation.

For population 1, we multiplied each time 2 tree diameter in set 2 by a random variate from a uniform (0.9, 1.1) distribution. For population 2, a nonlinear trend was introduced by multiplying each time 2 diameter in population 1 by a function

$$T1_t = [1 + .05 \ln(y_i - 1998)],$$

where y_i is the year of remeasurement for tree i. For population 3, a stronger nonlinear trend was introduced by multiplying each time 2 diameter in population 1 by a function

$$T1_t = [1 + .10 \ln(y_i - 1998)],$$

The mean basal area growth by diameter class and remeasurement year for each of the three populations is given in 5, as well as the two-dimensional and three-dimensional annual population sizes.

For 1,000 iterations, we sampled 1,000 plots from each population and calculated all of the estimators for the years 1999–2010. For each iterate, for each year, we calculated the MD and MSD, over the 1,000 iterations, between each estimator and the true population values.

Results

Figures 3 and 4 give the results by diameter class for test 1. Figure 3 gives the MD, an indicator of relative bias, over the 1,000 iterations of 1,000 samples each from the super population, between each estimator and the same estimator's result from the large sample of the target population.

Figure 4 gives the MSD, an indicator of relative mean squared error, over the 1,000 iterations, between each estimator and the corresponding result from the target population for the same estimator. In Figure 3, almost all of the values are negative, and they are all negative where the data are the most complete with respect to coverage of the growth interval being estimated (from 1999 to 2007). Because of the timing of the conversion by FIA to the five-panel system, the period between 2003 and 2007 has a distribution of temporal diversity (Figure 2) that is most like what can be expected to continue for full data sets (for all five panels) into the future, under current practices. Figure 3 shows that during this period, over all diameter classes, the estimators that appear to be the least affected in terms of empirical bias are the MOR, ROM, and ASHE estimators. However, it is clear that the REMPER assumption is bias inducing.

Figure 5 gives the population mean basal area growth for simulated populations 1, 2, and 3, by diameter class and estimation year. The two-dimensional and three-dimensional population sizes for each population by year are shown in the lower right-hand graph of the figure.

Figures 6–11 give the results by diameter class for test 2. Figures 6, 8, and 10 give the MDs, over the 1,000 iterations, between each estimator and the corresponding true population values for populations 1, 2, and 3, respectively. Figures 7, 9, and 11 give the MSDs, over the 1,000 iterations, between each estimator and the corresponding true population values for populations 1, 2, and 3, respectively.

The EOP estimator usually shows the most bias in

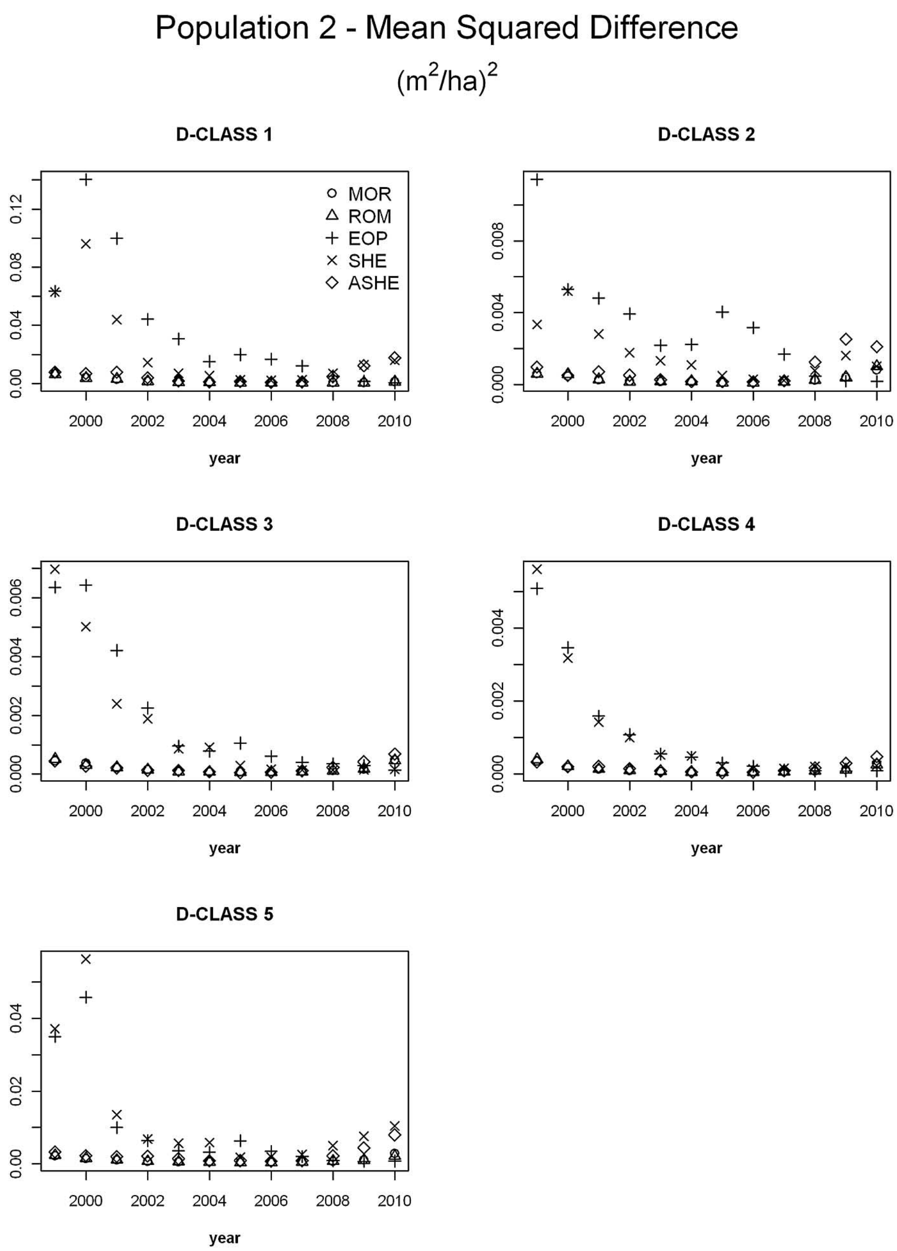

Figure 9. The MSD, over the 1,000 iterations of 1,000 samples each, between each estimator and the corresponding population 2 mean, by diameter class and estimation year.

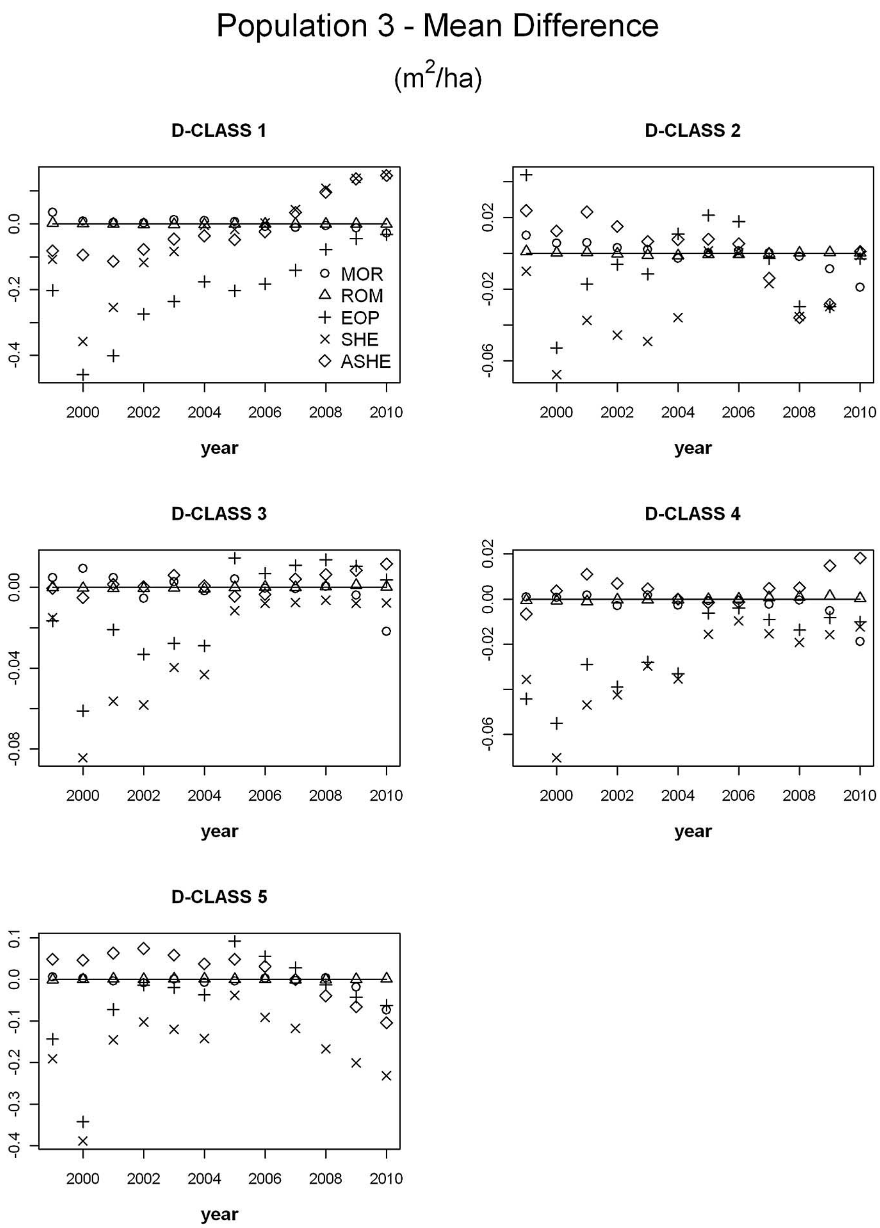

Figure 10. The MD, over the 1,000 iterations of 1,000 samples each, between each estimator and the corresponding population 3 mean, by diameter class and estimation year.

Figures 6, 8, and 10, for each of the three populations over all diameter classes. The Sheffield estimator (SHE) usually shows the second highest empirical bias. In addition, the bias for these estimators tends to increase for corresponding graphs from populations 1–3, as the simulated nonlinear trend increases. The ASHE estimator usually shows less empirical bias than the SHE estimator does. This finding suggests that much of the bias in both the EOP estimator and the SHE estimator, in the absence of remeasurement period variance, is due to lag bias, which can be eliminated by adjusting for the lag, as we did for the ASHE estimator.

Nevertheless, the ASHE estimator usually shows some empirical bias, because of its reliance on the temporal indifference assumption. The ROM and MOR estimators almost always show no empirical bias, with the MOR estimator sometimes showing a small amount of bias during the years with very small annual population sizes (as shown in Figure 5). Comparing Figures 7, 9, and 11 with Figures 6, 8, and 10, respectively, we note that bias squared is often the greatest contributor to mean squared error. An exception to this is seen when both are very small, such as when Figures 7, 9, and 11 show almost identical values for the ROM, MOR, and ASHE estimators, while the corresponding values in Figures 6, 8, and 10 show bias in the ASHE estimator, but not in the ROM and MOR estimators, indicating higher variance but lower bias in the latter two estimators. When this is the case and mean squared errors are about equal, we would usually prefer the least biased estimators.

Contrasting test 1 and test 2, we note that variance in the remeasurement interval length, present in test 1 but not in test 2 induces a high cost, in terms of bias.

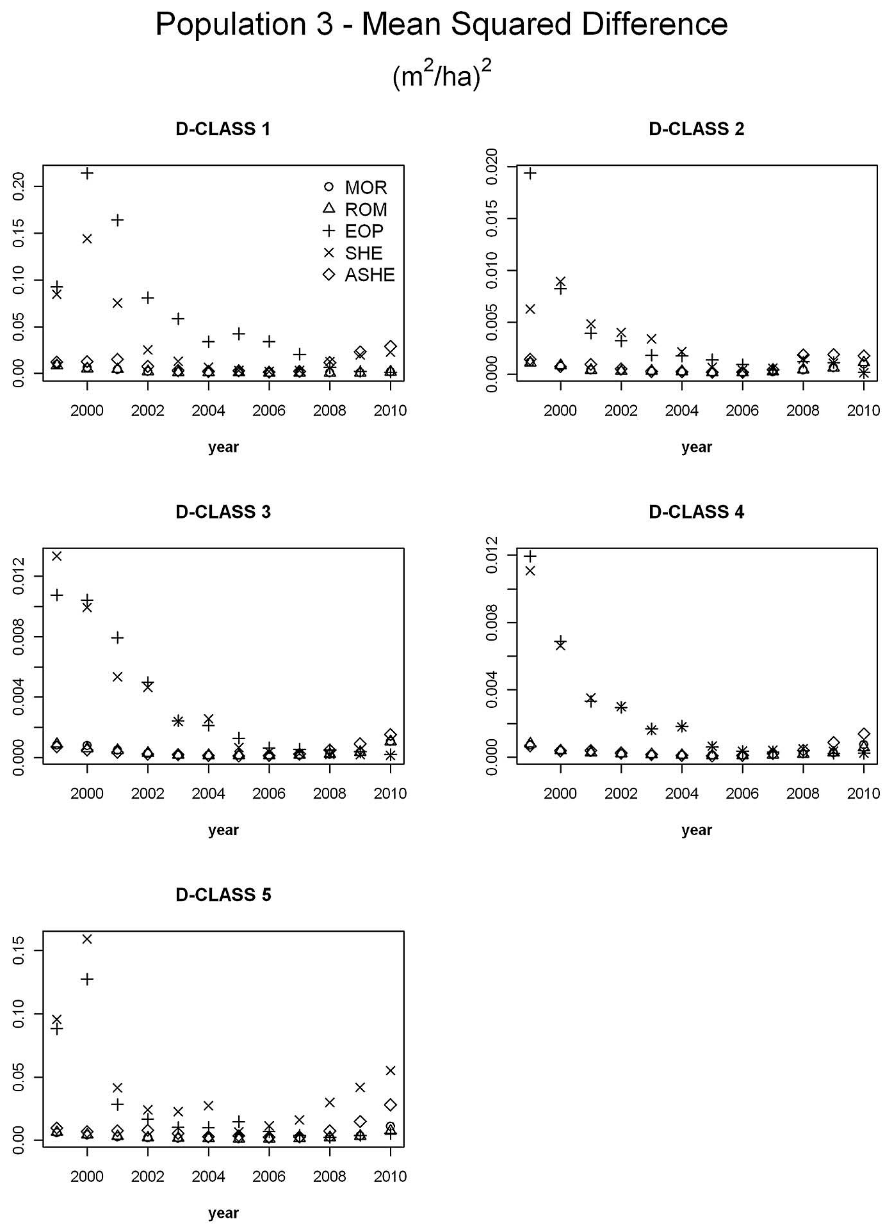

Figure 11. The MSD, over the 1,000 iterations of 1,000 samples each, between each estimator and the corresponding population 3 mean, by diameter class and estimation year.

Discussion and Conclusions

The field of statistics gives us many estimation tools to bolster analyses. All five estimators discussed here use external information to various degrees to make annual estimates. In estimators 1 and 2, the external information has a clear relationship to the annual estimates of interest, because the observations span the years estimated. It is clear in the formulation of the end-of-period estimators (EOP and SHE), in Equations 3 and 4, that much of the external information used in those estimators does not span the time estimated.

Under a nonstringent condition, the MOR and ROM estimators in Equations 1 and 2 are unbiased. A linear trend for the intervals covering the year of interest is sufficient for unbiasedness. Note also that these two estimators gave almost the same results, which are quite different from those for the EOP and SHE estimators. For the latter two estimators (EOP and SHE) to be unbiased, a flat line trend (i.e., linear with a slope of 0) over all years used in the estimators would have to exist. With a 5-year cycle, a flat line trend must have been true for the 10 years before any annual EOP estimate of growth. The conditions under which estimator 5 is unbiased lie between those necessary for estimators in the set containing Equations 1 and 2 and the set containing Equations 3 and 4.

In general, test 1 indicates that the two EOP estimators are the least robust to the REMPER assumption during the period from 2003 to 2007. In the later years, for which Figure 2 shows less diversity in remeasurement period lengths, the EOP estimators use much more of the data from earlier years than do the other three estimators, bringing their results in Figures 3 and 4 closer to the results of the other three estimators. Comparison of Figures 3 and 4 suggests that the REMPER assumption contributes more to an increase in the bias of the estimates than to an increase in the variance. If the REMPER assumption was valid and the actual remeasurement period is independent of any variable of interest, then Figure 3 should have shown no perceptible empirical bias in any of the estimators. That is, the empirical results in Figure 3 show large mean differences, suggesting that the underlying population of the super sample, containing a greater diversity of remeasurement interval lengths, is different from that of the target population. Overall, test 1 appears to invalidate the REMPER assumption. This is a significant finding because the remeasurement period assumption is implicit in NFI systems worldwide. That is, plots in NFI systems are never remeasured on exact temporal intervals, and sometimes there is little effort made to restrict the distribution of temporal interval lengths. The results of test 1 do show that the problem is reduced greatly in these data as the distribution of temporal interval lengths becomes more restricted in the later years of the investigation. Further research is needed to determine what restrictions should be placed on the distribution of temporal intervals to achieve specific objectives.

Test 2, using populations based on plots measured on exact 5-year temporal intervals, shows that the MOR and ROM estimators 1 and 2 always perform well, and the ASHE estimator 5 usually performs well, whereas the end-of-period estimators (EOP and SHE) often perform poorly. The temporally indifferent method is a smoothing function that has the tendency to obfuscate temporal trends and delay recognition of those trends. A judicious application of the three-dimensional view of this design can negate the necessity of the temporal indifference assumption and its associated problems. The results from test 2 with respect to the ASHE estimator do suggest, however, that preliminary estimates based on the intuitively unappealing temporal indifference assumption can be applied in a way to reduce greatly the bias seen in the EOP and SHE estimators.

The description of continuous forest inventories as a sample of a three-dimensional population is uniquely informative. It arose from the recognition of the importance of the time of observation on the outcome of the sample, and it is useful for putting temporally ordered observations into perspective while formulating model-unbiased estimators of growth and trend.

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