Estimating Landscape Pattern Metrics from a Sample of Land Cover

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Abstract

Although landscape pattern metrics can be computed directly from wall-to-wall land-cover maps, statistical sampling offers a practical alternative when complete coverage land-cover information is unavailable. Partitioning a region into spatial units and then selecting a subset (sample) of these units introduces artificial patch edge and patch truncation effects that may lead to biased sample-based estimators of landscape pattern metrics. The bias and variance of sample-based estimators of status and change in landscape pattern metrics were evaluated for four 120-km x 120-km test regions with land cover provided by the 1992 and 2001 National Land-Cover Data (NLCD) of the United States. Bias was generally small for both the estimators of status and estimators of change in landscape pattern, but exceptions to this favorable result exist and it is advisable to assess bias for the specific metrics and region of interest in any given application. A 10-km x 10-km sample block generally yielded larger biases but smaller variances for the estimators relative to a 20-km x 20-km sample block. Stratified random sampling improved precision of the estimators relative to simple random sampling. The methodology developed to determine properties of sample-based estimators can be readily extended to evaluate other landscape pattern metrics, regions, and sample block sizes.

Keywords: probability sampling, design-based inference, Horvitz-Thompson estimator, stratified sampling, land-cover change

Introduction

Development of measurements from maps (i.e., landscape metrics) is a defining characteristic of landscape ecological research (Wu and Hobbs 2002; Turner 2005). Landscape metrics were first introduced as a new area of ecological research (Krummel et al 1987; O'Neill et al 1988) to further explore linkages between spatial pattern and process. Software was developed to streamline calculation of landscape metrics (McGarigal and Marks 1995) which helped to further advance their use. Landscape metric development has included mathematical refinement (Li and Reynolds 1993; Riitters et al 1996), analysis of statistical correlations (Riitters et al 1995; Cain et al 1997), and analyses of sensitivity to pixel size (Wickham and Riitters 1995), map extent (O'Neill et al 1996), map format (raster versus vector) (Wickham et al 1996), map classification accuracy (Hess 1994; Hess and Bay 1997; Wickham et al 1997; Langford et al 2006; Shao and Wu 2008), map thematic resolution (Buyantuyev and Wu 2007), fuzzy representation of map classes (Arnot et al 2004), and gradients of land-cover composition (Neel et al 2004).

Despite some concern about the use and interpretation of landscape indicators (Wu and Hobbs 2002; Li and Wu 2004; Tischendorf 2001), there is a substantial literature demonstrating their utility. Browder et al (1989) showed that the land-water interface (i.e., contagion and edge), rather than wetland amount, was most strongly related to brown shrimp production. Hunsaker and Levine (1995) reported that forest edge, in addition to forest amount, was a significant factor in empirical water-quality models. Weathers et al (2001) has shown that forest edges intercept atmospheric pollutants and the meta-analysis by Harper et al (2005) identified several edge effects over a range of penetrating distances. Puth and Wilson (2001) and Levey et al (2005) have shown that edge is a corridor for the movement of some animals. Patch size is a well established and widely used metric in conservation planning (Hoctor et al 2001; Weber et al 2006; Opdam et al 2006; Hoctor et al 2008), and measures of fragmentation are indicators of forest condition (Noss 1999; Riitters et al 2004).

Empirical development of landscape metrics has not considered their behavior when they are estimated from a sample rather than derived from a wall-to-wall land-cover map. Concerns arising from using a sample to estimate landscape metrics do not exist when wall-to-wall land-cover maps are available. Still, sampling may yield significant cost savings and more accurate land-cover data. Boreal and tropical regions provide two clear examples. Consistent and extensive cloud cover and operational costs typically prevent wall-to-wall mapping of tropical forested regions, and sampling has been used to overcome this problem (Achard et al. 2002; Mayaux et al 2005). Sampling is also applicable to mapping protected natural areas (e.g., parks). Management of protected natural areas must consider both content (what is in the park) and context (what surrounds the park). Reliance on sampling may result from the need to integrate context and content (e.g., Townsend et al 2009). High spatial resolution imagery (e.g., IKONOS) may be needed to very accurately inventory park content, but the expense of this imagery may necessitate reliance on sampling to assess park context.

Hunsaker et al (1994, p. 207) emphasized the importance of developing sampling strategies for estimating landscape pattern in environmental monitoring programs. The possibility to monitor landscape pattern over large areas has been enhanced by the recent emergence of sample-based monitoring of land cover at national, continental, and global extents. For example, Achard et al (2002), Ridder (2007), and Hansen et al (2010) implemented sample-based forest cover monitoring studies that produced continental and global estimates, and Loveland et al (2002) implemented a sample-based monitoring strategy to estimate land-cover trends nationally for the United States. A common feature of these sample-based monitoring efforts is the use of a block sampling unit. The land cover data available from these sample blocks can be opportunistically used to estimate status and change in landscape pattern for no additional cost of data collection (Griffith et al 2003). Although these monitoring programs were not established with specific hypotheses concerning landscape pattern in mind, sample-based estimators of status and change in landscape pattern metrics derived from these sample data may be useful to detect key events and trends and generate hypotheses of importance to landscape ecology.

The objective of this research is to evaluate the bias and variance (i.e., precision) of sample-based estimators of landscape pattern metrics. The evaluation is undertaken for both status (i.e., one point in time) and change in the pattern metrics. Sampling methods are well-suited to estimate population parameters such as means, totals, proportions, and ratios (Cochran 1977), but not all landscape pattern metrics are expressible in these terms. Thus the question arises of whether such metrics can be estimated reliably from a sample of the landscape. We also evaluate the effect of sample block size on bias and precision and assess whether stratified random sampling improves precision of the estimates relative to simple random sampling.

In these analyses, bias and precision are evaluated according to their definitions in the design-based inference framework (Särndal et al 1992). In design-based inference, the observations on each sampling unit are regarded as fixed constants, not realizations of a random variable, and the randomization distribution resulting from the sampling design is the basis of inference. Bias and variance of an estimator of a population parameter are determined from the sample space, which is the set of all possible samples, and the sample space depends on the sampling design implemented. The bias and variance results do not require assuming that the landscape pattern metrics are normally distributed.

Methods

Description of the land-cover data, test regions, and landscape metrics

National Land Cover Data (NLCD) from 1992 (Vogelmann et al 2001) and 2001 (Homer et al 2007) were used to create populations to illustrate the properties of the sample-based estimators of landscape pattern. Both the 1992 and 2001 NLCD are 30-m pixel products. The NLCD 1992 and 2001 classes were aggregated to nine land-cover classes based on an Anderson Level I classification system (Anderson et al 1976): water, barren, shrubland, herbaceous upland natural/semi-natural vegetation, wetlands, developed, forested upland, non-natural woody, and herbaceous planted/cultivated (see Vogelmann et al 2001 and Homer et al 2007 for detailed NLCD class definitions). The nine landscape metrics evaluated were average patch size (APS), median patch size (MPS), patch density (PD), percentage like adjacencies (PLADJ), edge density (ED), total forest edge (TFE), Simpson's Diversity (SIDI), contagion, (CONT), and mean perimeter to area ratio (PARA). McGarigal and Marks (1995) provide the formula for each landscape metric. The nine landscape metrics chosen closely track the results reported by (Riitters et al 1995), who examined 55 metrics of landscape pattern and found that they could be reduced to six orthogonal dimensions. Four of the six orthogonal dimensions were represented by patch shape (e.g. perimeter-to-area ratio), contagion, average patch size, and patch density. These four orthogonal dimensions are represented in the metrics selected for evaluation. Because edge has been shown to be an important ecological indicator (e.g. Hunsaker and Levine 1995, Weathers et al 2001, Puth and Wilson 2001, Harper et al 2005, Levey et al 2005), three measures of edge, which were not explicitly represented in the analysis by Riitters et al (1995), were added.

Four test regions each 120-km x 120-km were selected within the United States to capture a variety of landscape patterns: northern New York State (mainly the Adirondacks), western Oregon, northern Florida, and eastern Iowa (Figure 1). The New York and Iowa regions were strongly dominated by forest and cropland, respectively, whereas the Oregon and Florida regions were comprised of a mix of land cover with no single class dominating either region (Table S1, supplementary information).

The four test regions were then partitioned into 144 non-overlapping 10-km x 10-km blocks and also into 36 non-overlapping 20-km x 20-km blocks. A sample constitutes a subset of the blocks for a given partition. The sample can be selected by numbering the blocks from 1 to 144 (for the 10-km x 10-km blocks) or 1 to 36 (for the 20-km x 20-km blocks) and applying a simple random or stratified random sampling protocol to the list of blocks (Lohr 2010). In practice, it is unlikely that the region of interest will be rectangular. In such cases, the rectangle of minimum area that circumscribes the region can be partitioned into the blocks that form the list of potential sample units. The two block sizes evaluated were chosen because this range of block size has been used in several applications of sampling to estimate status and change of land cover (e.g., Loveland et al 2002; Griffith et al 2003; Mayaux et al 2005; Ridder 2007; Hansen et al 2010). The standard errors of the estimators obtained from the two block sizes were compared to determine which block size is preferable.

<< Figure 1 about here >>

FRAGSTATS (McGarigal and Marks 1995) was used to compute the landscape pattern metrics. All land-cover classes in the test region or sample block were used to calculate each metric, except for TFE which was based on only the forest class. The value for each landscape metric computed for the entire region (not partitioned into blocks) was the target parameter (Table 1) to be estimated by the sampling approach. To evaluate the sample-based estimators, the landscape metrics were then computed for each block and bias and precision were calculated using these block values (see next section). In practice the target parameter would be estimated by computing the pattern metric separately for each sample block using off-the-shelf software such as FRAGSTATS, and then averaging the sample block data to produce the estimator.

<< Table 1 about here >>

We assume that the landscape metrics measured within the 120-km x 120-km blocks are ecologically relevant, but depending on the species or process of interest, metrics computed for smaller extents may be more relevant. In the latter case, the sampling design and estimation issues would need to be re-assessed for the smaller extent appropriate to the specific application. The sampling theory and the general methodology we develop for evaluating bias and precision would still be applicable to the smaller extent, but the specific results quantifying bias and precision at the 120-km x 120-km extent would not necessarily generalize to a smaller extent.

Estimating landscape pattern metrics from a sample

Bias

Let θ denote the value of a landscape pattern metric computed from complete coverage land-cover data for the entire region of interest. The target parameter for estimation is θ. Let z_u denote the value for the metric computed for block u, u=1, 2, …, K, where K is the number of blocks in a complete partition of the region of interest (i.e. each 120-km x 120-km test region was partitioned into K = 36, 20-km x 20-km blocks, and also partitioned into K = 144, 10-km x 10-km blocks). The parameter defined by the mean of all K blocks is

$$\theta^* = \frac{1}{K}\sum_{u=1}^{K} z_u \tag{1}$$

where the subscript u indexes an element of the full set of K units. For example, if z_u is patch density in block u, the parameter θ^* represents the mean patch density for the universe of K blocks. θ^* is not necessarily equal to the target parameter θ, but θ^* is readily estimated from the sample.

Suppose $\hat{\theta}^*$ is a sample-based estimator of θ^*. For a simple random sample of k blocks, $\hat{\theta}^*$ is the sample mean

$$\hat{\theta}^*_{SRS} = \frac{1}{k}\sum_{u=1}^{k} z_u \tag{2}$$

where the subscript u indexes an element of the sample. For stratified random sampling, $\hat{\theta}^*$ becomes

$$\hat{\theta}^*_{STR} = \frac{1}{K}\sum_{h=1}^{H} K_h \bar{z}_h \ , \tag{3}$$

where K_h is the number of blocks and $\bar{z}_h$ is the sample mean of the z_u values in stratum h. Estimating TFE requires the slight modification of multiplying the estimators in (2) and (3) by K to convert the mean forest edge per block to an estimator of total forest edge.

In the Results section, we establish that an unbiased estimator of θ^* exists for any probability sampling design. Because complete land-cover information is available for the four study regions, it is straightforward to compute θ^* and θ for each landscape metric in each region and to compute the bias of $\hat{\theta}^*$ as an estimator of θ. Because $\hat{\theta}^*$ is an unbiased estimator of θ^*, the bias is θ^*- θ, and relative bias, 100%(θ^*- θ)/θ, re-scales bias relative to the target parameter of interest. It is not necessary to select samples or to use simulation because bias and relative bias of $\hat{\theta}^*$ can be computed exactly using the complete coverage land-cover data available for all K blocks in the region (population) of interest. The bias associated with the sample-based estimators is distinct from the bias in landscape pattern metrics attributable to map classification error (e.g. Wickham et al 1997, Langford et al 2006).

Precision of sample-based estimators

Stratified sampling is commonly used to reduce standard errors relative to simple random sampling. For simple random sampling, the standard error of the estimator of θ^* is

$$SE(\hat{\theta}^*_{SRS}) = S/\sqrt{k} \ , \tag{4}$$

where S is the population standard deviation of z_u (i.e. the standard deviation of z_u over the K blocks) and k is the number of blocks sampled. For stratified random sampling (assuming H strata), the standard error is

$$SE(\hat{\theta}^{*}_{STR}) = \sqrt{(1/K^2)\sum_{h=1}^{H} K_h^2 S_h^2 / k_h}\ , \qquad (5)$$

where K_h is the number of blocks in stratum h, k_h is the number of blocks sampled in stratum h, and S_h is the population standard deviation of z_u within stratum h. The finite population correction factors (1-k/K) for SRS and (1-k_h/K_h) for stratified random sampling are omitted from equations (4) and (5) because in most practical applications, the sampling fractions k/K and k_h/K_h will be close to 0 and the finite population correction factor will be close to 1.

Stratification is effective when blocks within a stratum have similar values of the pattern metric but the stratum means differ from one another. To assess the potential benefit of stratified sampling, two options using different landscape metrics to construct the strata were evaluated for the 10-km block size: 1) Two strata created by splitting the population of K blocks at the median value of SIDI and PD; 2) Four strata created by splitting the population of blocks at the first, second, and third quartiles of SIDI and PD. The total sample size was fixed at n=36, and the sample was allocated equally to each stratum, $k_1 = k_2 = 18$ for the two-strata option, and $k_1 = k_2 = k_3 = k_4 = 9$ for the four-strata option. The standard errors (equations 4 and 5) were then calculated for the estimators of APS, PARA, ED, and CONT. The maximum potential benefit of stratification would occur when the target variable for estimation is the same as the variable used to construct the stratification. Therefore, SIDI was included in the landscape metrics evaluated when SIDI was used to construct strata, and PD was included in the evaluation when PD was used to construct strata. In practice, it is not possible to stratify by the target estimation variable, but we evaluate that case here to provide a theoretical upper bound on the benefit of stratification.

Block size comparison

The block size chosen for the sampling design affects both precision and bias of the estimators. The precision resulting from the two block sizes was compared for simple random sampling (see equation 4) by examining the ratio of the standard deviations, S_{10}/S_{20}, where S_{10} and S_{20} are the population standard deviations for each landscape pattern metric for the 10-km and 20-km block sizes. A sample of 4k 10-km blocks has the same total area sampled as a sample of k 20-km blocks. Thus for an equal total area sampled, the standard error resulting from sampling 4k 10-km blocks is less than the standard error resulting from sampling k 20-km blocks if $(S_{10} / \sqrt{4k}) < (S_{20} / \sqrt{k})$, and this inequality is true when $S_{10}/S_{20}<2$.

Results

Bias of estimators of landscape pattern

Theoretical results

The assessment of bias of sample-based estimators of landscape pattern metrics depends on the key result that there exists an unbiased estimator of θ^* (equation 1), the population mean of a landscape pattern metric computed for the region partitioned into K blocks. The general estimator of θ^* depends on the inclusion probability π_u for each block sampled, where π_u is the probability that block u would be included into the sample. Different sampling designs result in different inclusion probabilities. For any probability sampling design of sample size k, the Horvitz-Thompson estimator (Horvitz and Thompson 1952)

$$\hat{\theta}^* = \frac{1}{K}\sum_{u=1}^{k} z_u / \pi_u \ , \tag{6}$$

is an unbiased estimator of θ^* (but not necessarily an unbiased estimator of the target parameter θ). Equations (2) and (3) are special cases of the Horvitz-Thompson estimator applied to simple random and stratified random sampling, so the estimators shown in (2) and (3) are unbiased estimators of θ*.

The Horvitz-Thompson estimator ensures that it is possible to construct an unbiased estimator of θ* for any probability sampling design. Consequently, the bias of a sample-based estimator is θ*- θ (i.e., the bias depends on whether θ* is equal to the target parameter θ). Because the Horvitz-Thompson estimator provides an unbiased estimator of θ* for any probability sampling design, the bias evaluation based on θ*- θ is applicable to any probability sampling design and therefore it is not necessary to re-evaluate bias for each different sampling design.

The bias evaluation also readily extends to sample-based estimators of change in a landscape pattern metric over time. When estimating change in pattern metrics between 2001 and 1992, let $\theta_c = \theta_{01} - \theta_{92}$ be the difference between the pattern metrics computed from the 1992 and 2001 land-cover data, and $\theta_c^* = \theta_{01}^* - \theta_{92}^*$ be the difference in the pattern metrics computed from the K blocks that partition the region, where θ_{01}^* and θ_{92}^* are computed using equation (1) applied to the 2001 and 1992 data. An unbiased estimator of θ_c^* is $\hat{\theta}_c^* = \hat{\theta}_{01}^* - \hat{\theta}_{92}^*$, where the estimator for each year is a Horvitz-Thompson estimator (equation 6). The bias of $\hat{\theta}_c^*$ is then $\theta_c^* - \theta_c$.

Empirical results for estimating status

For the three patch metrics APS, MPS, and PD, the sample-based estimators had small relative biases with the direction of the bias generally consistent across all four regions and both years (Figure 2). APS had a negative bias (underestimate), whereas PD had a positive bias (overestimate). The relative bias for MPS was near 0 for Iowa, Oregon, and Florida, but a relative bias of -10% was found for New York for both block sizes in 1992 and a positive relative bias occurred for New York in 2001. The two edge metrics ED and TFE were estimated with small relative bias (Figure 3). The direction of bias for ED and TFE varied across regions and years, but usually both were underestimated. The sample-based estimator of PLADJ typically also underestimated the target parameter at both sample block sizes in both years. The estimators of SIDI and CONT had the largest relative biases of the nine metrics estimated (Figure 4). The direction of the bias of SIDI (overestimate) and CONT (underestimate) remained consistent across test regions, block sizes, and years. The estimator of PARA had a small relative bias tending to underestimate the true parameter (Fig 4). In general, relative biases of the estimators were higher for the 10-km block size compared to the 20-km block size.

<< Figures 2, 3, and 4 about here >>

Empirical evaluation of bias for estimating change in landscape pattern

The differences between the true change in each pattern metric and the change estimated from the sample indicate that the bias of $\hat{\theta}_c^*$ was generally small for all four test regions and block sizes (Table 2). For some of the landscape metrics, the sample-based estimators of change improved upon the bias of the single date estimators. For example, the relative bias of the estimator of SIDI was high for each date (Fig 4), but the estimator of change in SIDI was nearly unbiased (Table 2). The bias of $\hat{\theta}_c^*$ was generally smaller for the 20-km block size compared to the 10-km block size.

<< Table 2 about here >>

Precision of estimators

Comparison of block size (simple random sampling)

Precision resulting from the two block sizes was compared using the ratio of the standard deviations S_{10}/S_{20} with a ratio less than 2 indicating that the 10-km blocks yielded a smaller standard error than the 20-km blocks (equal total area sampled). With the exception of APS in Florida in 2001, all of the ratios S_{10}/S_{20} were smaller than 2, and often considerably smaller (Fig 5). For estimating change in landscape pattern, S_{10} and S_{20} represent the standard deviations of change for the population of blocks. The ratios S_{10}/S_{20} for the change in each pattern metric were almost always less than 2 indicating that the 10-km blocks yielded better precision than the 20-km blocks for estimating change (Fig 5). The exceptions all occurred in Iowa, where the ratio S_{10}/S_{20} exceeded 2 for TFE, SIDI, and CONT.

<< Figure 5 about here >>

Comparing precision of simple random to stratified random sampling

The precision comparison of stratified random to simple random sampling was based on relative error (RE), defined as the standard error for the stratified design divided by the standard error for simple random sampling. Because usually RE<1 for the landscape metrics evaluated, stratified sampling generally outperformed simple random sampling (Table 3). The improvement in precision achieved by stratification could be substantial as RE was often less than 0.80 (RE=0.80 represents a 20% reduction in standard error relative to simple random sampling).

<<Table 3 about here >>

The benefit of stratification depended on the relationship between the variable used to construct the strata and the landscape metric being estimated. Stratifying by SIDI produced very little or no reduction in standard error for estimating PARA and only a slight improvement in precision for estimating APS. But SIDI was highly effective as a stratification variable for improving precision of the estimators of ED and CONT, and the improvement for estimating

SIDI was generally very great. Stratifying by PD improved precision when estimating APS. This was expected because PD and APS are inversely related. The PD stratification generally produced only minor improvements in precision for PARA, although for Oregon 1992 the improvement was substantial. For estimating ED, stratifying by PD was usually better than stratifying by SIDI, sometimes substantially better (e.g. Florida 2001). Stratifying by PD generally yielded meaningful gains in precision for estimating CONT, but SIDI was a better stratification variable for CONT in most cases (New York 1992 being a notable exception).

Discussion

Sampling design and estimation

Hunsaker et al (1994) evaluated the bias of sample-based estimators of the proportion of area of each land-cover class and estimators of landscape pattern metrics for systematic sampling of hexagonal spatial units. Because the Horvitz-Thompson estimator provides an unbiased estimator of the proportion of area of each land-cover class for any probability sampling design (including systematic sampling), empirical investigation of the bias of estimators of land-cover proportions is not necessary. Hunsaker et al's (1994) evaluation of bias of other pattern metrics focused on a single sampling design whereas our bias evaluation is applicable to any probability sampling design. Hunsaker et al (1994) examined how well a single systematic sample performed in each of 86 spatial regions of about 120-km by 180-km in size. In contrast, the bias and precision results we report represent the performance of the estimators over all possible samples, in keeping with the basic tenets for evaluating estimator properties in design-based inference (Särndal et al 1992).

The sample-based estimators we constructed were based on first computing the landscape pattern metrics for each sample block and then averaging the block values to obtain an estimate for the entire region. This would be a practical and convenient approach requiring only repeated application of software such as FRAGSTATS to each sample block. An alternative approach is to aggregate the sample block information into a single spatial conglomerate (Hunsaker et al 1994), in effect treating the aggregate of sample blocks as an entire landscape. It would be useful to examine this alternative for those metrics such as SIDI and CONT that did not perform well in our study. For example, computing SIDI from such an aggregate sample area would likely diminish the bias of the estimator of SIDI because it is more likely that all land-cover classes present in the region would be represented in the conglomerate sample of blocks. However, this approach of adjoining sample blocks that are not in reality contiguous will introduce problems when estimating metrics such as percent like adjacencies (PLADJ) because artificial adjacencies will be created. Another limitation of the aggregate sample analysis is that the estimation and standard error formulas assume simple random sampling. Consequently, estimators appropriate for sampling designs that are not equal probability (e.g. stratified sampling with equal allocation of sample size to strata) would need to be derived.

Bias of estimators of status of landscape pattern metrics

The sample-based estimators of the edge metrics (ED, TFE, PLADJ, PARA) were the least biased, followed by the patch-based metrics (APS, MPS, PD), with the diversity metrics (SIDI and CONT) being the most biased. Thus an inverse relationship appears to exist between relative bias and the amount of information used to calculate the landscape pattern metric. Edge metrics are based on pixel-level adjacencies and therefore draw on a large number of observations (pixels). Consequently, a substantial amount of pixel-level change from one sample block to the next would need to exist to produce large deviations between the sample and the population values. The bias of patch-based metrics was intermediate because such metrics draw on an amount of information that is intermediate between edge and diversity metrics. Patch-based metrics are calculated by treating adjacent and like-classified pixels as individual objects, and there tend to be fewer patches than pixels but more patches than land-cover classes.

Diversity metrics (SIDI and CONT) are based on the least amount of information (i.e. the land-cover classes) and their sample-based estimators were the most biased of the nine pattern metrics examined. It is well known from field ecology manuals (e.g. Brower and Zar 1977) that SIDI is sensitive to the number of classes and the relative proportions of those classes. The number of classes present in any individual block must be less than or equal to the number of classes present in the entire region because some of the land-cover classes may be absent from any particular block. Therefore, the individual block SIDI values would tend to be less than SIDI for the unpartitioned region, and when all the block values are averaged to construct the partitioned universe parameter θ*, this parameter will be smaller than the true SIDI, θ. The results confirmed this as the sample-based estimator of SIDI underestimated the true parameter. Like SIDI, contagion (CONT) is sensitive to changes in the number of classes and their relative proportions from one sample to the next. Wickham et al (1997) tested the sensitivity of several landscape metrics to land-cover misclassification, and found contagion to be the most sensitive.

The degree of spatial uniformity in the landscape will also affect bias in the estimation of landscape metrics simply because each sample would be an exact replica of the population if perfect spatial uniformity were present. The magnitude of sample bias should decline as spatial uniformity increases. The “grain” (i.e. average patch size) of the area being sampled is also an important factor. Other factors being equal, biases for estimating patch-based metrics should decline as patch size declines because it is more likely that the sample block will encompass the full extent of the patches when they are small (O’Neill et al 1996).

When sample block boundaries intersect land-cover patches, additional artificial patches of smaller sizes will be created resulting in a negative bias (underestimate) for APS and an associated positive bias (overestimate) for PD. The observed results confirmed these anticipated biases for APS and PD (Figure 2). Because total forest edge (TFE) is a population total, the estimator of TFE would be unbiased if for each sample block, true forest edges (i.e. edge between forest and another land-cover class) are distinguished from edges created when a forest patch is intersected by a block boundary. In the FRAGSTATS calculation of TFE for each block, sample block boundaries were not included as landscape edges. Consequently, forest edges that happened to coincide with block boundaries (e.g. a square forest clearcut aligns with a block boundary) would be omitted thereby decreasing TFE for that sample block. Overall, the omission of such forest edges would lead to underestimation of TFE. However, the degree of underestimation would depend on the amount of forest edge that coincided with block boundaries. In most cases, it would be unlikely to have forest edges align closely with block boundaries so the bias of the estimator of TFE is expected to be small. For similar reasons, ED should also be estimated with small bias. Edges between all land-cover classes are considered in ED, so there will be more total length of edge omitted than occurs with just forest edge because of coincident true edge with a block boundary. Patch truncation and edge effects evidently had a negligible effect on estimation of PARA. If only a small proportion of all land-cover patches are impacted by a block boundary, then the bias of PARA would be anticipated to be small. Further, the sample block boundaries would likely intersect land-cover patches randomly so that on average, there is no tendency for the patch truncations to increase or decrease PARA.

Bias of estimators of change in landscape pattern metrics

Generally the biases of the estimators of change in landscape pattern metrics were small. Because change was estimated by the difference between the two single date (status) estimates, bias of estimated change will tend to reflect bias of the individual status estimates. However, under some circumstances the biases of the two status estimates will cancel. The estimator of change for SIDI (Simpson's diversity) is a good example. The relative bias of the two single date estimators of SIDI was high (Figure 4), but the high positive bias cancels out when subtracting the two estimators to obtain the estimator of change in SIDI (Table 2). The bias in the single date SIDI estimators attributable to the absence of some of the land-cover classes in each sample block is roughly the same for the two dates and so the bias is greatly diminished when subtracting the two single date estimates to estimate change in SIDI.

Change in APS provides another illustration of the nature of bias in change estimation. The bias of the single date estimator of APS (Figure 2) is attributable to patches being truncated by sample block boundaries. If little change occurs in the landscape between the two dates, the sample block patch truncation effect would be expected to be of the same magnitude at both dates, and the estimated change in landscape pattern would be unaffected (i.e. the estimated change would be near zero for the truly unchanged landscape). Conversely, if substantial change in landscape pattern occurs, then the differential bias for the estimates from the two dates will be manifested in the bias of the change estimate. For example, if a large negative bias exists for the second date and no bias exists for the first date, the underestimate at the second date will be reflected in the bias of the change estimate.

Block size

O'Neill et al (1996) studied the relationship between sample unit size and landscape grain and found that the sample unit should be 2 to 5 times larger than the landscape grain to avoid bias. Our results are consistent with those of O'Neill et al (1996) in that biases were generally smaller for the 20-km x 20-km block compared to the 10-km x 10-km block. The larger biases associated with the smaller block size were expected because a smaller block generates substantially more total length of block boundary. For the 120-km by 120-km regions used in this study, the total block boundary length (excluding the perimeter of the region of interest) is 1200 km for the 20-km x 20-km block partition and 2640 km for the 10-km x 10-km block partition. The addition of 1640 km of block boundary for the 10-km blocks will lead to more land-cover patches being intersected by block boundaries exacerbating effects attributable to patch truncation or artificial edge on the estimators of the pattern metrics. Although the 20-km blocks resulted in smaller bias than the 10-km blocks, the latter would be preferred in terms of precision (on an equal total area sampled basis).

Bias and precision are obviously both important factors in determining which block size to use. Because the bias and precision criteria lead to a conflicting recommendation for block size, the smaller cost per unit area to obtain land-cover data for larger block sizes becomes relevant. When an equal total area is sampled, the overall cost of obtaining and processing land-cover data would typically be smaller for the 20-km blocks than the 10-km blocks, so the 20-km size would generally be preferable on this basis.

Stratified sampling

The improvement in precision gained by stratification is determined by how strongly the variable of interest for estimation is related to the variable used to construct the strata. For example, stratifying by a patch metric (e.g. PD) improved precision of the estimates of other patch metrics (e.g. APS), but did not improve precision of metrics less related to PD such as SIDI. Stratification could also be used for estimating change, assigning blocks to low, medium, and high land-cover change strata, where the intervals defining these categories of change will be dependent on how much change is present for a specific application.

Implementing a stratified design requires having available at the planning stage complete coverage information that can be used to form the strata. The *a priori* requirement for implementing stratification makes its logic seem circular and thus its use hypothetical because having *a priori* information of landscape pattern seems unlikely; there is no need for stratified sampling if information on landscape pattern is already available. The need for information on content and context surrounding protected areas (see Townsend et al 2009) provides a real situation where stratified sampling would be a genuine consideration. Determining content within and around protected areas might require information from high-resolution imagery such as Quickbird or IKONOS, but the cost of these data (approximately 30 US dollars per km^2, which translates to more than 1 million US dollars to collect data for an area the size of one Landsat scene) would likely prevent wall-to-wall collection of IKONOS data. Classified Landsat imgery or existing land-cover data from programs such as NLCD (Homer et al 2007) could be used to generate the information needed to develop the stratification underlying a sampling approach employing IKONOS images as the sampling unit (Fig. 6).

<< Figure 6 about here >>

Although our results demonstrate that stratified sampling can decrease standard errors relative to simple random sampling, further investigation is needed to determine effective stratification options when the objectives require estimation of several landscape metrics or estimation of change in landscape pattern. Poststratified estimation offers a viable alternative to stratified sampling when a single stratification attribute cannot be identified to reduce standard errors for more than a few of the pattern metrics of interest. Instead of employing strata in the sampling design, poststratified estimation uses the strata at the analysis stage (Särndal et al 1992). The choice of strata in the poststratified analysis can target estimation of a specific pattern metric to take advantage of strata that best enhance precision for that metric. The flexibility offered by poststratified estimation to use different strata for different metrics is advantageous over the single stratification that would be implemented at the sampling design stage.

Extended evaluation of sample-based estimators of landscape pattern

The bias of a sample-based estimator of a landscape pattern metric is likely the result of a complex and difficult to predict relationship between the specific metric of interest and the characteristics of the landscape. The fundamental theory and methods developed in this article provide a foundation for extending the evaluation to other pattern metrics and block sizes. Specifically, the Horvitz-Thompson estimator ensures the existence of an unbiased estimator of θ^*, the mean of the pattern metric over the K blocks forming a partition of the region. The assessment of bias then reduces to comparing θ^* to the true value of the pattern metric θ computed from the unpartitioned region of interest. Existing wall-to-wall land-cover products such as the NLCD 1992 and NLCD 2001 provide an ideal source of test populations to evaluate any landscape pattern metric of interest and different sample block sizes. This type of evaluation should be conducted prior to initiating any application in which sampling of land cover will be used to estimate status or change of landscape pattern metrics.

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Figure Captions

Figure 1. Locations of the four test regions within the United States.

Figure 2. Relative bias of the estimators of average patch size (APS), median patch size (MPS), and patch density (PD) for NLCD 1992 and 2001 and 10-km and 20-km square blocks.

Figure 3. Relative bias of the estimators of total forest edge (TFE), edge density (ED), and percent like adjacencies (PLADJ) for NLCD 1992 and 2001 and 10-km and 20-km square blocks.

Figure 4. Relative bias of estimators of Simpson's diversity index (SIDI), contiguity (CONT), and perimeter to area ratio (PARA) for NLCD 1992 and 2001 and 10-km and 20-km square blocks.

Figure 5. Evaluation of precision resulting from different sample block sizes based on ratios of the standard deviation for 10-km blocks divided by the standard deviation for 20-km blocks, S_{10}/S_{20} (ratios below 2 indicate that the estimator has a smaller standard error for the 10-km unit than for the 20-km unit when the total area for all sample blocks combined is equal).

Figure 6. State of Wyoming with Landsat images overlaid in gray (one image in yellow) and 30 Quickbird images (red). The 30 Quickbird images could be thought of as a realization of a stratification based on landscape indicators derived from classifying the Landsat imagery.

Table 1. Description of landscape pattern parameters by region. Values shown are θ_{92}= value of the pattern metric computed from the full (unpartitioned) region using the 1992 NLCD, θ_{01} = value computed from the full region using the 2001 NLCD, and $\theta_c = \theta_{01} - \theta_{92}$.

	New York			Iowa			Oregon			Florida		
Metric (units)	θ_{92}	θ_{01}	θ_c	θ_{92}	θ_{01}	θ_c	θ_{92}	θ_{01}	θ_c	θ_{92}	θ_{01}	θ_c
APS (ha)	18.6	15.6	-3.0	5.8	66.7	60.9	4.9	21.7	16.8	2.0	6.9	4.9
MPS (ha)	0.2	0.7	0.5	0.18	0.90	0.72	0.09	1.08	0.99	0.18	0.72	0.54
PD (per 100 ha)	5.4	6.4	1.0	17.4	1.5	-15.9	19.7	4.6	-15.1	52.5	14.6	-37.9
PARA (m^{-1})	1057.3	780.6	-276.7	1092.3	760.4	-331.9	1131.8	630.6	-501.2	1113.1	792.3	-320.8
PLADJ (%)	97.6	90.1	-7.5	92.4	93.3	0.9	89.9	88.1	-1.8	77.3	81.5	4.2
ED (m/ha)	30.3	66.0	35.7	50.7	44.8	-5.9	67.3	78.9	11.6	151.1	122.9	-28.2
TFE (km)	40,655	84,215	43,560	13,348	3,481	-9,867	60,525	64,186	3,661	103,453	98,058	-5,395
SIDI (none)	0.233	0.359	0.126	0.164	0.208	0.044	0.602	0.708	0.106	0.798	0.789	-0.009
CONT (%)	80.2	72.9	-7.3	82.1	82.2	0.1	63.0	52.8	-10.2	38.1	41.1	3.0

Table 2. Evaluation of estimators of change in landscape pattern*.

State	APS(ha)	PD(100ha)	PARA(m^{-1})	PLADJ(%)	ED(m/ha)	TFE(km)	SIDI	CONT(%)
New York								
True	-3.0	1.0	-276.7	-7.50	35.7	43560	0.126	-7.3
20km	-4.8	1.2	-282.8	-5.40	36.0	43993	0.120	-7.5
10km	-7.8	1.3	-275.3	-5.42	36.1	43927	0.121	-8.2
Iowa								
True	60.9	-15.9	-331.9	0.90	-5.9	-9867	0.044	0.10
20km	67.5	-15.9	-329.7	0.89	-5.9	-9855	0.043	0.22
10km	67.7	-15.8	-334.6	0.91	-6.0	-10101	0.044	0.14
Oregon								
True	16.6	-15.1	-494.7	-1.80	11.6	3661	0.105	-10.2
20km	15.2	-15.0	-480.1	-1.74	11.6	3670	0.123	- 9.3
10km	14.2	-15.1	-471.8	-1.80	11.2	2870	0.116	- 9.6
Florida								
True	4.9	-37.9	-320.8	-28.2	-28.2	-5395	-0.009	3.02
20km	5.3	-38.0	-330.0	-28.1	-28.1	-5380	-0.005	2.19
10km	5.5	-37.9	-327.4	-27.6	-27.6	-4919	0.004	0.03

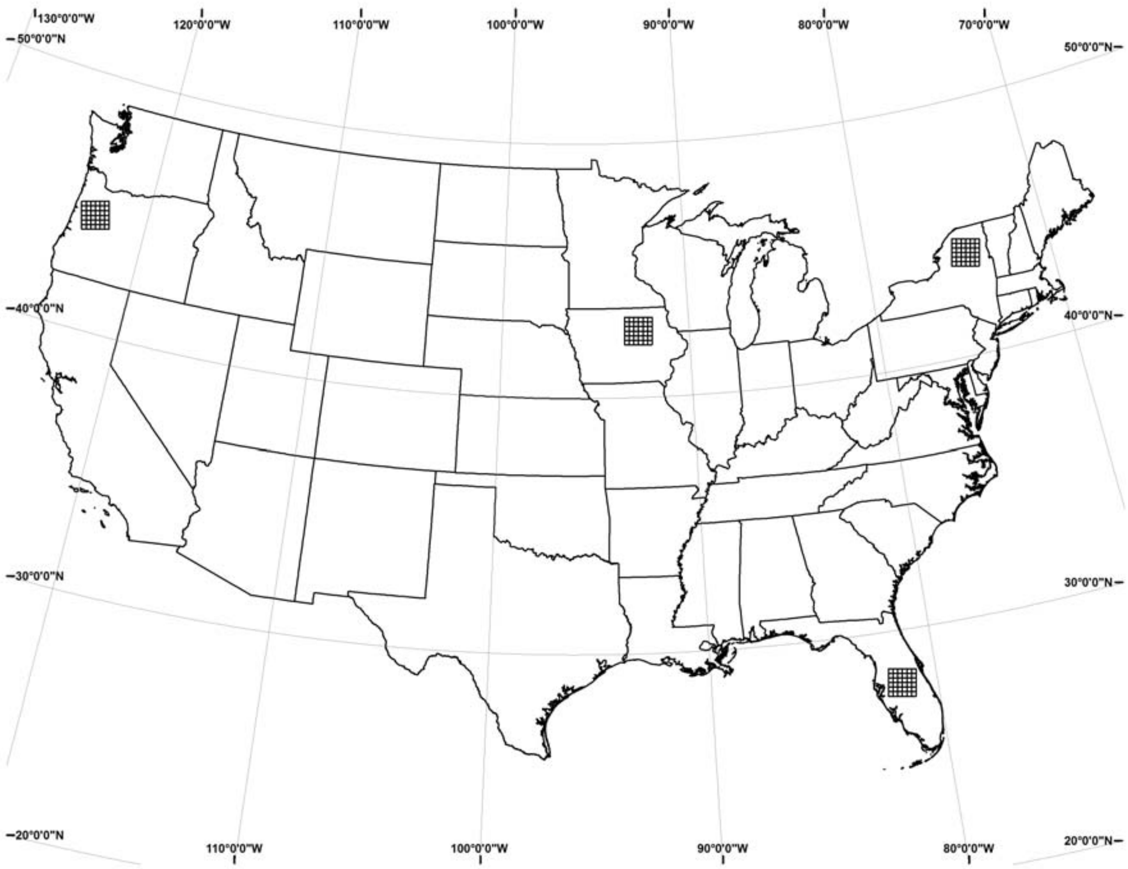
* The row labeled “True” is the actual value of the parameter, and the values shown for the 20-km and 10-km blocks are the expected value of the sample-based estimator. The bias for an estimator is the difference between the expected value and the “True” value.

Table 3. Relative error (RE) of stratified random sampling for estimating status of landscape pattern using the 10-km block size. RE<1 indicates that the standard error of stratified random sampling is smaller than the standard error of simple random sampling.

		SIDI		PD	
Test Region	**Metric**	**2 strata**	**4 strata**	**2 strata**	**4 strata**
New York	APS 1992	1.00	1.01	0.77	0.58
	2001	0.88	0.81	0.85	0.70
	PARA 1992	1.00	1.01	0.94	0.92
	2001	0.99	0.98	0.96	0.95
	ED 1992	1.00	1.00	0.74	0.57
	2001	0.65	0.53	0.61	0.48
	CONT 1992	1.00	1.01	0.85	0.73
	2001	0.59	0.38	0.66	0.58
	SIDI 1992	0.47	0.47	--	--
	2001	0.56	0.31	--	--
	PD 1992	--	--	0.59	0.35
	2001	--	--	0.56	0.32
Iowa	APS 1992	0.98	0.98	0.98	0.98
	2001	0.71	0.60	0.54	0.33
	PARA 1992	1.00	1.00	1.01	1.01
	2001	0.98	0.96	0.98	0.98
	ED 1992	0.72	0.53	0.72	0.47
	2001	0.75	0.57	0.72	0.53
	CONT 1992	0.68	0.43	0.76	0.56
	2001	0.72	0.51	0.78	0.63
	SIDI 1992	0.80	0.80	--	--
	2001	0.75	0.52	--	--
	PD 1992	--	--	0.69	0.46
	2001	--	--	0.67	0.44
Oregon	APS 1992	0.84	0.69	0.81	0.60
	2001	0.81	0.78	0.70	0.52
	PARA 1992	0.92	1.10	0.91	0.88
	2001	1.00	0.99	0.97	0.95
	ED 1992	0.70	0.31	0.68	0.47
	2001	0.74	0.68	0.69	0.61

	CONT 1992	0.57	0.39	0.65	0.45
	2001	0.56	0.39	0.70	0.63
	SIDI 1992	0.56	0.28	--	--
	2001	0.51	0.27	--	--
	PD 1992	--	--	0.74	0.53
	2001	--	--	0.58	0.35
Florida	APS 1992	0.96	0.88	0.88	0.76
	2001	0.97	0.90	0.87	0.77
	PARA 1992	0.99	0.98	1.00	0.99
	2001	1.00	1.01	0.93	0.85
	ED 1992	0.90	0.80	0.64	0.48
	2001	0.95	0.86	0.72	0.59
	CONT 1992	0.72	0.51	0.77	0.65
	2001	0.78	0.66	0.91	0.81
	SIDI 1992	0.56	0.56	--	--
	2001	0.73	0.57	--	--
	PD 1992	--	--	0.60	0.36
	2001	--	--	0.62	0.38

Figure 1. Four 120-km x 120-km test regions for evaluating properties of sample-based estimators (NY = Adirondack region of New York in the northeast US, FL = Florida in the southeast US, IA = Iowa in the central US, and OR = Oregon in the northwest US). Each test region is shown partitioned by the 20-km x 20-km sampling blocks.



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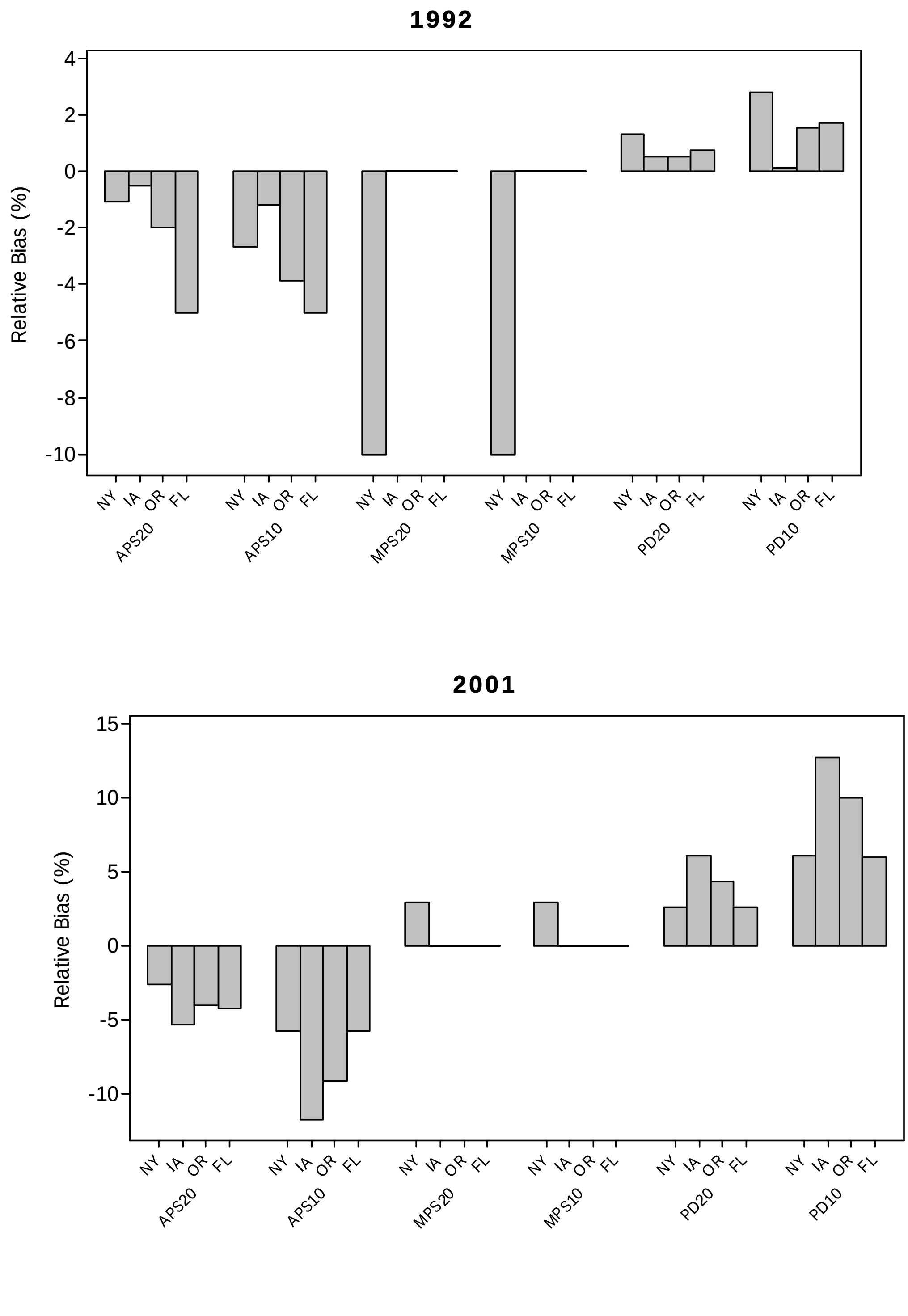

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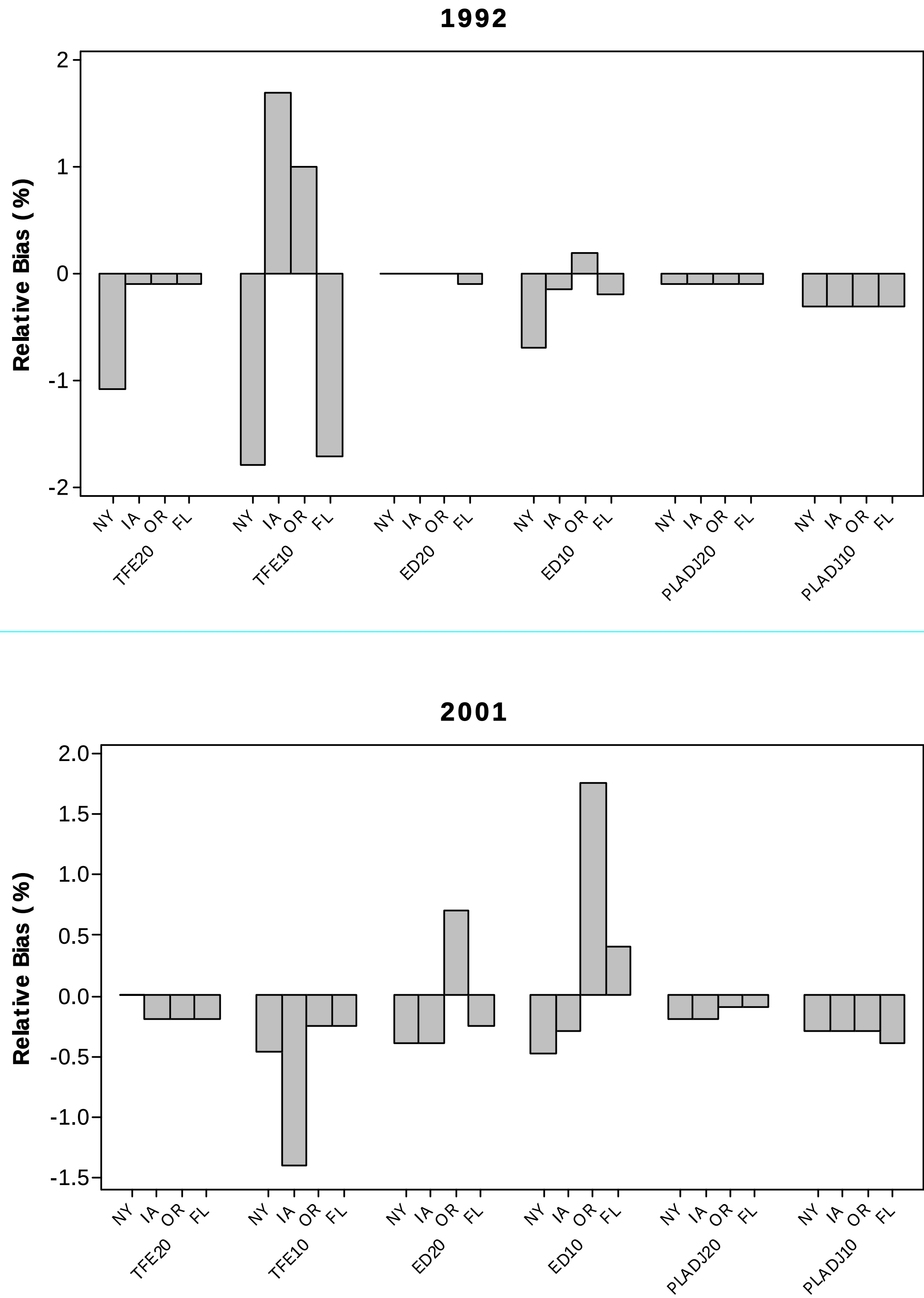
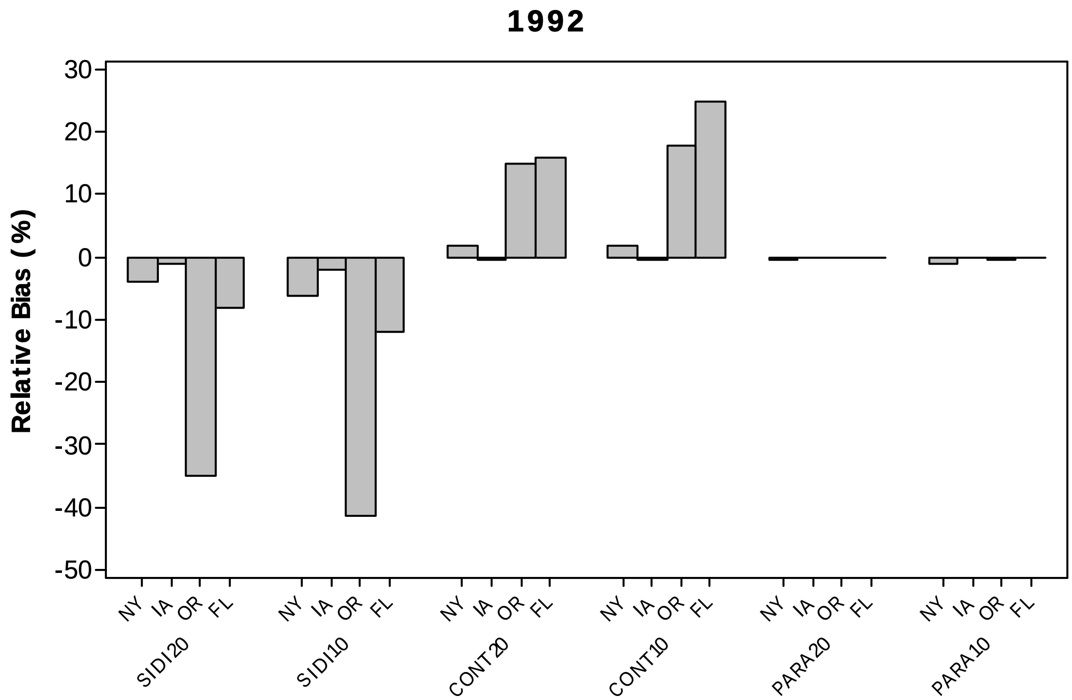
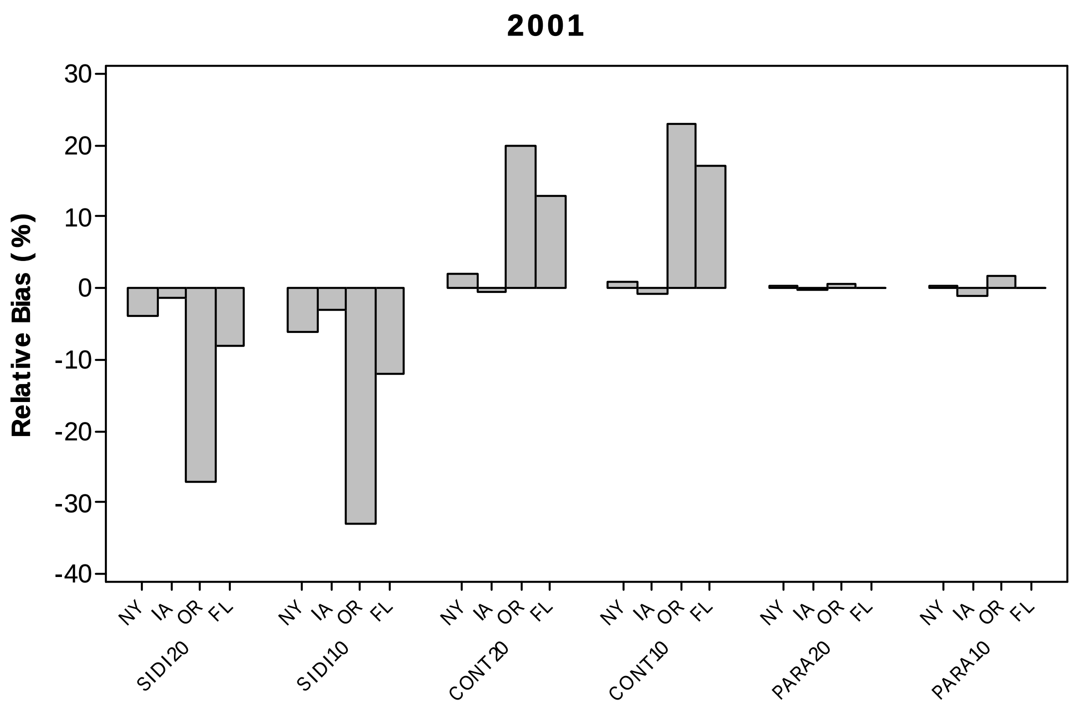


Figure 4. Relative bias of estimators of Simpson's diversity index (SIDI), contiguity (CONT), and perimeter to area ratio (PARA) for NLCD 1992 and 2001 and 10-km and 20-km square blocks.





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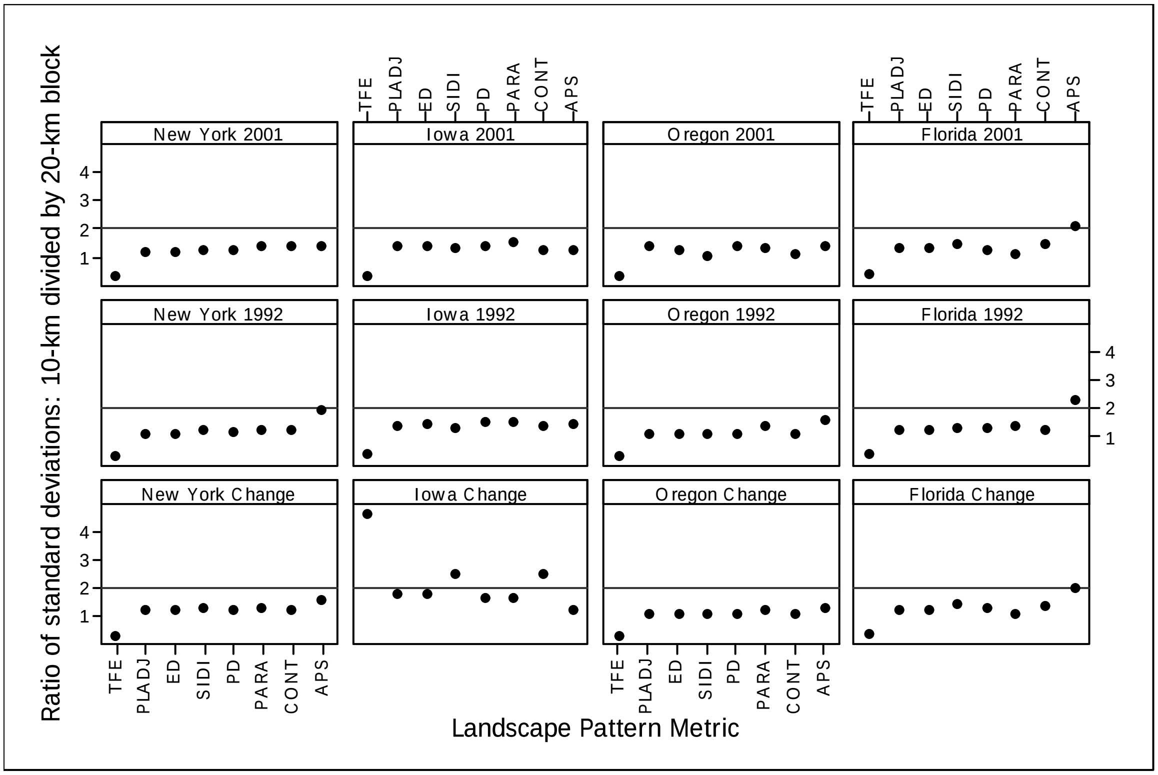

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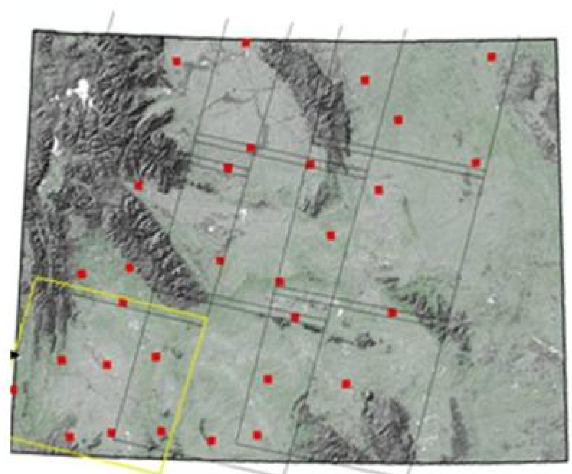

Figure to replace Table 3. This figure is offered for reviewers' consideration of whether the figure would be a better representation of the results currently presented as Table 3. We not include both the figure and the table, but one or the other based on reviewer preferences.

Relative error (RE) of stratified random sampling relative to simple random sampling for estimating landscape pattern metrics (NLCD 1992 data). The reference line at 1 demarcates where stratified random sampling is better than simple random (RE<1 shows stratified is better). A second figure would be needed to show the 2001 results.