DESIGN

&

ANALYSIS

for multiple-use studies of deer browse & timber production

by
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PURPOSE

A PRIMARY purpose of resource management on forested state game lands is usually to preserve or enhance the recreational value of the lands for hunting by producing an adequate and continuing supply of food and cover for wildlife. This objective is ordinarily obtained through sustained-yield management of the timber. Under this type of multiple-use management *(Society of American Foresters 1958)*, manipulation of the stand structure for wildlife values is planned, whenever possible, to provide for thinning or harvesting operations that are conducive to good timber yields. When this is done successfully, the combined values of hunting and timber production provided by a given piece of land should exceed the value that could be derived from a single use of the same land.

The purpose of this paper is to discuss the design and analysis of experiments for comparing the production of dormant woody deer browse under various intensities of cutting. The discussions are based on first-year results of an experiment that was begun in 1961 on State game land in northeastern Pennsylvania by the Pennsylvania Game Commission and the U. S. Forest Service.

This cooperative study was designed primarily to compare the effects of a wide range of cutting intensities in pole-sized hardwood stands on annual and total dormant browse production during the first 5-year period after treatment. Also, as a secondary objective, the growth and quality of residual timber and the abundance of sprout and seedling reproduction within each treatment will be compared 10 years after treatment and again 20 years after treatment.

The design of the experiment, together with suggestions for improving similar studies in the future, may be of value to others who deal with multiple-use research involving deer and timber. Also, the statistical procedures used to analyze first-year results may prove useful *(Morris et al. 1959)*.

Because no timber growth or quality data are available yet, this paper covers only the browse aspects of the study and preliminary results on deer and rabbit use.

STUDY DESIGN

The Area

The study was established on 65 acres of a 50-year-old second-growth northern hardwood stand of primarily sprout origin. Site, aspect, soil, and cover type were fairly uniform throughout the 65 acres. Basal area of all trees 4.6 inches d.b.h. and larger was 90 $\pm$ 4 square feet (1 standard error) per acre as determined with a basal-area angle gage at 30 random sampling points. Basal area of trees below 4.6 inches d.b.h. was approximately 20 square feet per acre throughout the study area.

Treatments

Five cutting treatments—reserving 0 (clearcut), 15, 30, 45, and 60 square feet per acre in the residual stand—and an uncut control (fig. 1) were established in two randomized blocks (fig. 2). In each cutting treatment plot, which was 2½ acres in size (330 x 330 feet), the smallest trees were cut to attain the desired basal area in the residual stand. Because of the uniformity of the stand, two randomized blocks seemed sufficient to represent the site. The size of the experimental area also limited the number of randomized blocks that could be used.

To promote acceptance of results among private landowners and sportsmen, treatments in block 1 were established adjacent to a gamelands road so they would be easily accessible as a demonstration area. This, of course, means, in a strict statistical sense, that the blocks represent *fixed effects* rather than *random effects*, which are desired; and that treatment results apply only within their respective blocks *(Snedecor 1959)*.

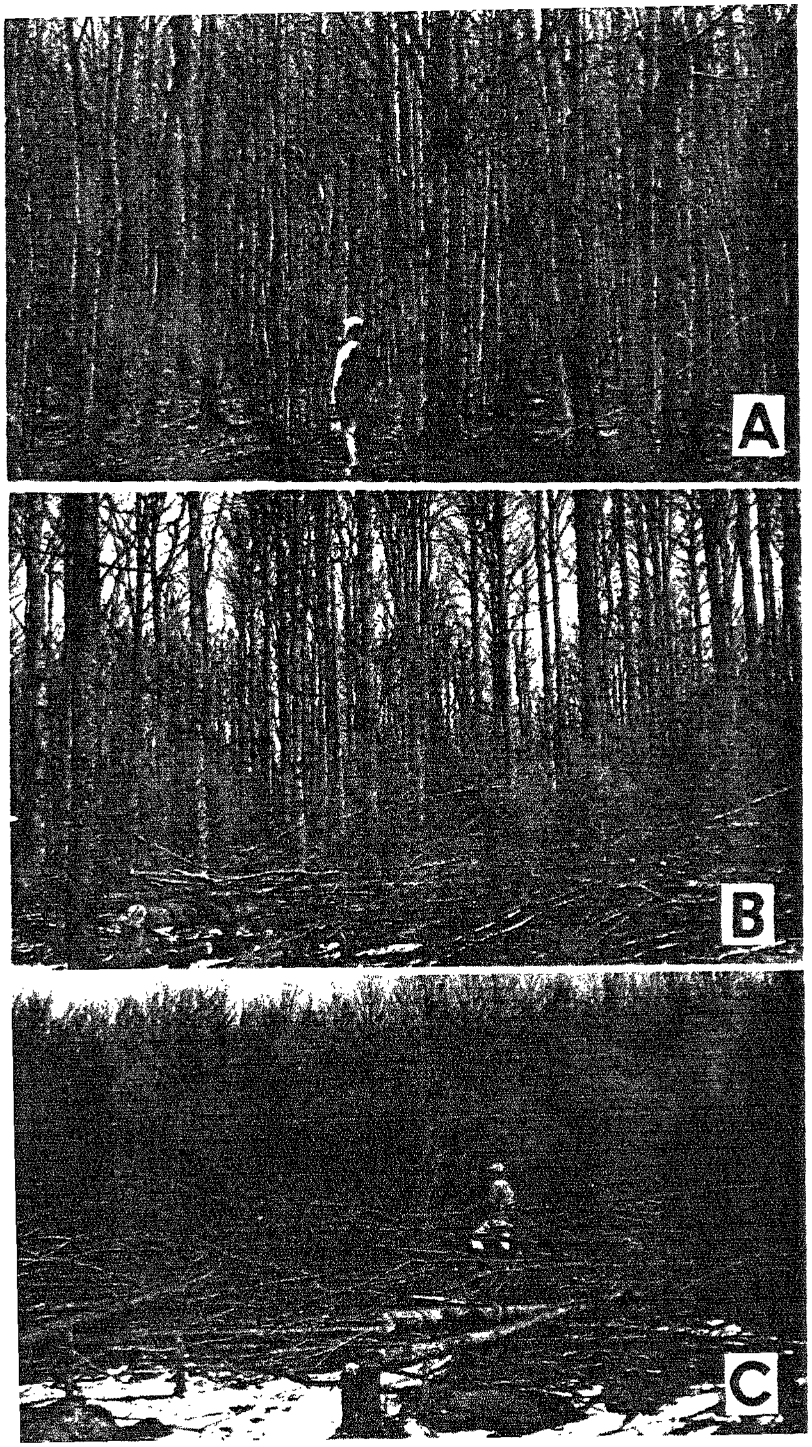
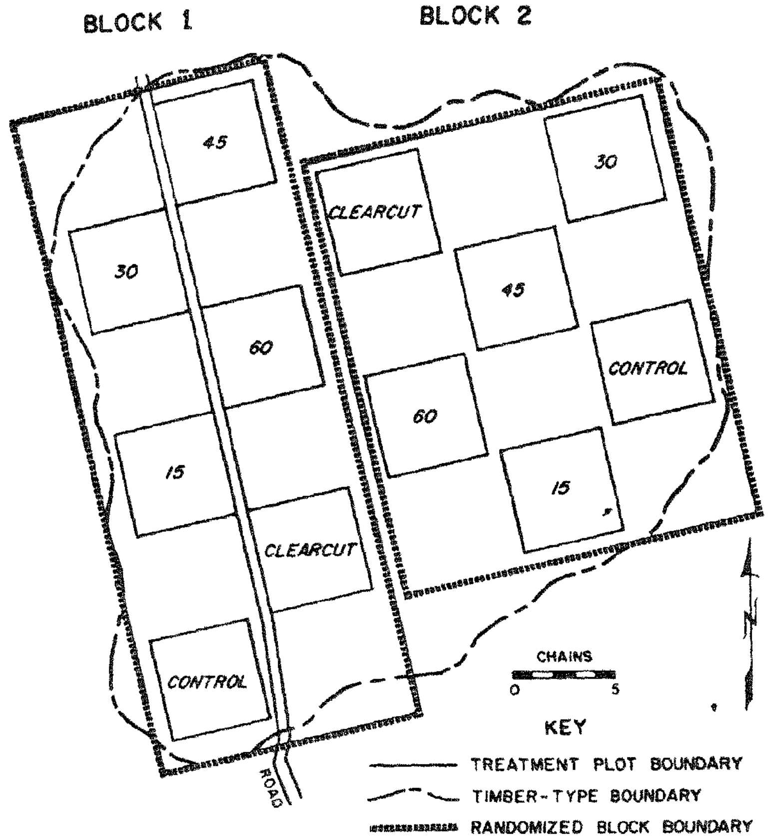


Figure 1.—Several of the treatments used in the multiple-use cutting experiment: *A*, control—no cutting. *B*, 45 square feet reserved per acre. C, clearcutting.

Figure 2.—Experimental design for the multiple-use cutting experiment. The basal area reserved per acre is shown within each 2½-acre treatment plot.



Furthermore, with a stand this small there was a problem of inserting two blocks within the stand. As a result, blocks are of unequal shape and theoretically cannot be considered statistically pure for representing random effects. For example, the control and 45-square-feet plots in block 1 are farther apart than any two plots in block 2.

Square $2\frac{1}{2}$-acre buffer strips of undisturbed vegetation were included between treatment plots to help equalize the effects of deer mobility, and of shade and seed dispersal from surrounding vegetation.

The most desired species reserved in each treatment were dominant and codominant sugar maple *(Acer saccharum* Marsh.), black cherry *(Prunus serotina* Ehrh.), and white ash *(Fraxinus americana* L.). Red maple *(Acer rubrum* L.) was favored over beech *(Fagus grandifolia* Ehrh.). Quaking aspen *(Populus tremuloides* Michx.) and gray birch *(Betula populifolia* Marsh.) were not reserved. A record of all reserved trees was maintained by 1-inch diameter classes.

In February 1961 all trees that were to be removed were cut and left where felled. In each cutting treatment (excluding the control), all trees below 4.6 inches d.b.h. were cut; thus, the residual basal areas were all in trees of 4.6 inches d.b.h. and larger (table 1).

In hardwoods, sprouting ability (and thus browse production) of stumps declined sharply with diameter increase above 8 to 10 inches *(MacKinney and Korstian 1932)*. Therefore, when browse production is the primary objective of thinning in young hardwood stands, all trees smaller than 4.6 inches d.b.h. may be cut to increase browse production.

Treatments that differed by 15 square feet per acre were assumed to provide a good indication of cutting effects through-

Table 1.—*Summary of residual basal area within each cutting treatment (excluding the control). Basal area figures are for trees 4.6 inches d.b.h. and larger; all smaller trees were cut.*

Randomized block number	Basal area per acre		
	Reserved	Cut	Total
	Sq. ft.	*Sq. ft.*	*Sq. ft.*
1	0	87.1	87.1
2	0	106.6	106.6
1	15	75.4	90.4
2	15	73.0	88.0
1	30	62.2	92.2
2	30	71.5	101.5
1	45	52.8	97.8
2	45	44.6	89.6
1	60	33.9	93.9
2	60	24.2	84.2

out a wide range in basal area. The five levels of cutting were equally spaced at 15-square-foot intervals to facilitate regression analyses of the results, as outlined by Snedecor *(1959: 346-350)*, if treatments differed significantly in an analysis of variance. There was only a sparse amount of browse per acre in the undisturbed portions of the stand, so it seemed unnecessary to measure browse in the control plots or to plan for tests of differences in browse production among the cutting treatments and a control. However, control plots were necessary in the statistical design for comparing timber growth later among the treatments and a control.

Within each treatment, the butt logs of five residual trees per species of sugar maple, red maple, white ash, and black cherry in each 1-inch diameter class (9 inches and above) were graded according to standard log-grade specifications *(Ostrander et al. 1963)*. This was done to compare crop values between treatments after several years.

Subplots

A separate random design was used to establish forty 100-square-foot circular subplots within each 2½-acre treatment plot. Forty subplots were estimated to be necessary to obtain an average weight of dormant hardwood browse production per acre for the six major species combined that would be accurate within ±20 percent at the 95-percent probability level *(Shafer 1963)*. Thus, α was set at 0.05 — the risk that can be taken of deciding that a difference between treatment results exists when the difference really is zero.

Measurements

First-year measurements taken during April 1962 on each subplot included:

1. Dormant browse production and utilization by use of the twig-count method *(Shafer 1963)*.
2. A count of deer pellet groups since the date of last leaf-fall (November 1961) using the method described by Eberhardt *(1960)*.
3. The presence or absence of rabbit or hare pellets.

ANALYSIS OF BROWSE DATA

Population Parameters

The statistical observation was the weight of browse production (in grams) per subplot. Browse production was expressed in four ways: gross production, net production, green weight, and dry weight. Gross production was the combined weight of browsed and unbrowsed twigs. But only the analysis of green-weight gross production is discussed in detail (unless otherwise indicated); the inferences are generally the same for the other three measurements.

Preliminary analysis of the browse measurements indicated that the treatment variances were not homogeneous, as shown by the F_{max} test *(Walker and Lev 1953: 192)* at the bottom of table 2. The ratio of the estimated standard deviation to the mean ($s/\bar{x}$) was constant enough to justify the assumption that in the population σ/u was a constant; that is, the standard deviation varied directly as the mean.

One reason why variances among treatments differed significantly is because treatment effects probably were not additive. By additive, we mean that if X_1 is the value of a given observation (browse weight) under the control conditions, then under a treatment condition

$$X_2 = X_1 + a$$

where *a* represents a constant treatment effect such as the number of deer using each treatment or the rate of browse growth under different overstory conditions. In the present experiment, treatments probably had some sort of multiplicative effect, which is expressed by:

$$X_2 = X_1 a$$

When the analysis of variance technique *(Steel and Torrie 1960)* is used to compare treatment results as in this experiment, additivity is the most important requirement and homogeneity of variance the next most important *(Snedecor 1959)*. Initial data in our experiment exhibited non-additivity and non-uniform variance, so it was necessary to transform the data.

Table 2.—*Gross browse production (green weight) before and after transformation of data for each subplot (X) to log (X + 2).*

Randomized block number	Basal area per acre reserved	Original data: Average weight of browse per subplot	Original data: $\frac{s}{\bar{x}}$ [1]	Transformed data: Average weight of browse per subplot	Transformed data: Standard deviation s
	Sq. ft.	*G.*		*G.*	
1	0	28.86	0.8932	1.291	0.4832
2	0	24.84	1.4496	1.076	.5803
1	15	24.21	.9586	1.226	.4450
2	15	25.06	1.0726	1.197	.4989
1	30	10.76	1.0167	.920	.4300
2	30	32.76	.8092	1.407	.3742
1	45	17.56	1.1788	1.043	.4904
2	45	17.12	1.0169	1.079	.4541
1	60	5.00	1.3860	.678	.3665
2	60	18.34	1.3418	1.005	.5439
		$F_{max.} = \frac{1296.61}{48.01} = 26.9$ [2]		$F_{max.} = \frac{.3368}{.1343} = 2.51$ [2]	
		$P\{F_{max.} > 2.99\} = 0.05$		$P\{F_{max.} > 2.99\} = 0.05$	

[1] Standard deviation per average weight of browse.

[2] $F_{max.} = s^2_{max.} / s^2_{min.}$

Transformation of Data

The logarithmic transformation described in Snedecor *(1959: 320)* was used. This transformation is necessary for unbiased tests when the standard deviation is proportional to the mean. Gross weight of browse per subplot (X) was transformed to log (X + 2); this made all subplot values positive and greater than zero. Homogeneous variance was obtained by this transformation as indicated by an F_{max} test (table 2), and additivity existed in the transformed data as evidenced by Tukey's test of additivity *(Snedecor 1959)*.

This transformation also provided homogeneous variance among treatments for *net* browse production in terms of both green and dry weight, except for the 60-square-foot treatment in block 1. There may have been either of two reasons for this discrepancy in the latter treatment:

1. The 60-square-foot treatment in block 1 resulted in the lowest browse production of all treatments (table 2): average gross production of browse was only 5 grams per subplot, and approximately half of this was utilized. Browsing on this small amount of browse during the growing season may have altered the growth pattern so that the variance observed under this treatment no longer belonged to the same population of variances as the other treatments.

2. This discrepant observation may have been an unusual member of the population being sampled.

Because no proved explanation could be found for this discrepancy, the net browse observations in the 60-square-foot treatment plot of block 1 were used in the analysis of variance, but with three precautions in mind: first, by pooling this discrepant variance estimate with the others, the standard errors of the other treatment means may have been underestimated; second, if this outlying value had been rejected, the result might have been an overestimation of errors; and third, the possibly low estimate of pooled sampling variance may have increased the likelihood of committing an alpha-type error in the analysis—that is, rejecting a true hypothesis.

Analysis of Variance

The analysis of variance (table 3), which used a fixed-effects model with sampling *(Steel and Torrie 1960: 144)*, indicated that cutting treatments did not differ significantly in terms of browse production. However, the residual error mean square was significantly larger than the sampling error.

The residual error mean square was computed from observations that normally are used — if treatments are replicated within blocks — to compute a block x treatment-interaction mean square. Interaction occurs if the difference between treatment effects depends upon individual blocks. Because treatments were not replicated within blocks, it is impossible to state explicitly whether or not there was a significant block x treatment interaction. However, several factors in the design indicate that interaction probably would have been significant.

Interaction measures the failure of treatment effects to be the same for each block. Some degree of interaction is illustrated by figure 3 where the difference between blocks for the 30-square-

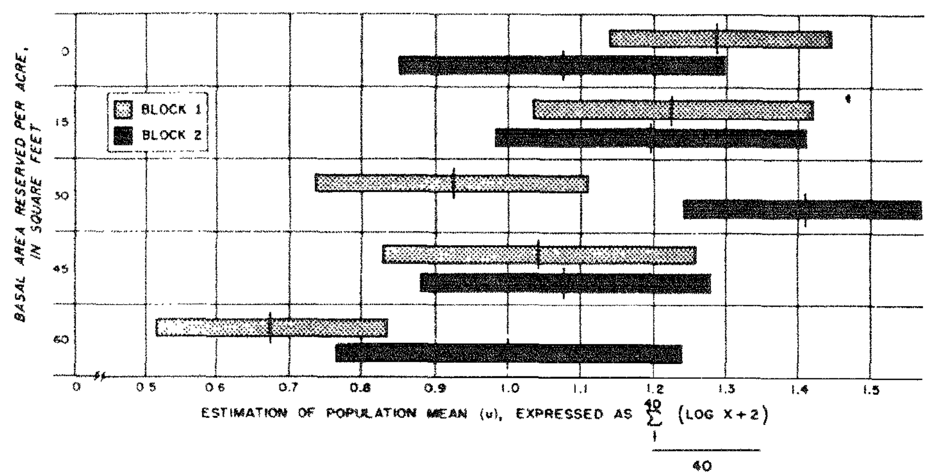


Figure 3. — Interval estimates (probability of 0.95) of browse population mean weights (green weight, gross production) — after logarithmic transformation of subplot data—for each of five cutting treatments in two randomized blocks. Mean of each sample is shown as a vertical line.

Table 3.—Analysis of variance for gross browse production (green weight) in terms of grams per subplot after logarithmic transformation, for 5 cutting treatments on 2 randomized blocks with 40 subplots per cutting treatment plot.

Source of variation	Degrees of freedom	Sum of squares	Mean squares	F	F 0.05	Parameters estimated
Blocks	1	1.5588	—	—	—	—
Treatments	4	7.2175	1.8044	1.08	6.59	$\sigma_s^2 + 40\sigma_E^2 + 80K_T^2$
Residual error	4	6.6573	1.6643	6.86*	2.39	$\sigma_s^2 + 40\sigma_E^2$
Sampling error	390	94.5224	.2424	—	—	σ_s^2

$$[40(s_E^2) + s_s^2] - s_s^2 = 1.6643 - 0.2424 = 1.4219$$

$$s_E^2 = \frac{1.4219}{40} = 0.0355$$

* Indicates significance.

foot treatment, in particular, is inconsistent with the block differences for other treatments.

Factors Influencing Results

Although an equal basal area per acre was reserved in both blocks for any one treatment, the total number, basal area per acre, diameter-class distribution, and species distribution of the trees that were cut for any one treatment differed considerably between blocks. Even though the overall condition of the experimental area was fairly uniform, considerable experimental variation was introduced when the area was divided into ten 2½-acre treatment plots. Before the cutting treatments were applied, the randomized blocks differed significantly in terms of average basal area per acre for a main treatment plot. Covariance analyses, which attempted to relate browse production to initial basal areas, and basal areas cut within each treatment, did not prove successful.

If the experimental error had been estimated with 80 subplots rather than the original 40, its mean square would have been:

$$0.2424 + 80\ (0.0355) = 3.0824$$

instead of 1.6643. Thus, the variance of a treatment mean ($s_{\bar{x}}^2$) would have been:

$$3.0824/160 = 0.019$$

instead of the present value of

$$1.6643/80 = 0.021$$

This is a trivial decrease in variance to compensate for doubling the sampling rate. Any material decrease in the standard error of the treatment mean would have to be sought by increasing the number of randomized blocks, or by replicating treatment within blocks. This would provide a means of detecting block x treatment interaction if it existed.

In our experiment the standard error of a treatment mean ($s_{\bar{x}}$) was:

$$s_{\bar{x}} = \sqrt{\frac{0.0355}{2} + \frac{0.2424}{2(40)}}$$

$$= 0.142$$

It should be noted that the computed $s_{\bar{x}}$ probably is large because the value of 0.0355 may contain a block x treatment-interaction effect. A treatment mean, expressed as

$$\frac{\sum_{1}^{80} \text{Log } (X + 2)}{80}$$

was correct to within $(\pm 1.99)(0.142) = \pm 0.284$ with 95 percent confidence.

RECOMMENDED CHANGES IN DESIGN

Preliminary Decisions

First, it is necessary to consider the nature of the test of significance for any future experiment. Generally, one tests a null hypothesis (H_0) against an alternative hypothesis (H_1). Usually, the null hypothesis (H_0) is formulated as

$$H_0: m_i = m_j,$$

where m_i and m_j represent the mean weights of browse production for any two treatments *i* and *j*. As a result the alternative hypothesis (H_1) usually is

$$H_1: m_i \neq m_j$$

Second, the question arises, how large a difference *d* needs to exist between m_i and m_j before one decides from a managerial standpoint that they are significantly different? Because the many other food sources besides woody browse were not considered in the study and because deer management is based on approximate numbers of deer, we suggest that in any future study m_i should differ from m_j by at least ± 20 percent of m_i before a difference *d* is labelled significant. However, for certain experimental purposes *d* conceivably could be as small as ± 10 percent of m_i, and still be important enough to detect.

The average *transformed* weight of browse per subplot throughout all treatments in this study was 1.09 grams (table 2). Thus, in future experiments, our best estimate of a 10-percent difference between any two treatment means is 0.109 grams; and for a 20-percent difference it is 0.218 grams (0.10 x 1.09 = 0.109; and 0.20 x 1.09 = 0.218). It should be stressed that these quantities apply to transformed data.

A third item we need to consider in future studies is the power of the test of significance, as defined by Walker and Lev *(1953: 162)*:

Power = 1 — Probability of a type II error

where a type II error (or B error) is the probability of *not* rejecting the null-hypothesis when it is false. Thus, the power of a test of significance is the probability of rejecting the null hypothesis when it should be rejected.

One way to increase the power of a test is to increase α, which was 0.05 in the present study. The α term is the risk one takes of deciding that $m_i \neq m_j$, when the difference is really zero. An α of 0.05 seems reasonable for most timber-browse production experiments, and therefore, we suggest $\alpha = 0.05$ for future studies.

Another way to increase the power of the test is to decrease the B term. Remember that B is the risk one takes of deciding that $m_i = m_j$, when the difference between m_i and m_j really is as large as a predetermined value *d*. We suggest that B be somewhere between 0.10 and 0.40 depending upon the importance to be attached to the results.

Required Sample Size per Treatment

Having decided upon the preliminary specifications for future browse-timber production experiments, we calculated the number of subplots (N_2) required within any treatment plot for such experiments (table 4). Subplots required were computed by using the sample-size formula for a two-tailed test in Walker and Lev *(1953: 165)* and inserting in the formula:

1. An estimate of the population variance $\sigma^2 = 0.2424$ (from table 3).
2. The appropriate value for an $\alpha = 0.05$.

Table 4.—Number of randomized blocks (N_1) and subplots (N_2) per main cutting treatment plot needed to reject H_o:$m_i = m_j$ when some alternative is true.[1] *The α error is 0.05.*[2]

B error[3]	The difference (d) between m_i and m_j that needs to be detected if it exists			
	± 10 percent of m_i[4]		± 20 percent of m_i[4]	
	N_2	N_1	N_2	N_1
0.10	212	12	54	3
.20	162	12	41	3
.30	124	13	32	4
.40	99	13	25	4

[1] m_i and m_j are the mean weights of the logarithmic transforms of browse per subplot for treatment i and j respectively.

[2] The risk one takes of deciding that $m_i \neq m_j$, when the difference is really zero.

[3] The risk one wants to take of deciding that $m_i = m_j$ when the difference between m_i and m_j really is as large as a predetermined difference (d).

[4] The best estimate we have of m_i is 1.09 which is the grand mean; thus 10 percent of 1.09 = 0.109, and 20 percent of 1.09 = 0.28.

3. Combinations for the prescribed values of B and *d*: that is, for B = 0.10, 0.20, 0.30, or 0.40; and *d* = 0.109 or 0.218.

Required Number of Randomized Blocks

If a new experiment consists of N_1 blocks and N_2 subplots per treatment, the predicted variance of a treatment mean ($s_{\bar{x}}^2$) would be (table 3):

$$s_{\bar{x}}^2 = \frac{0.0355}{N_1} + \frac{0.2424}{N_1 N_2} \qquad (1)$$

However, for any prescribed *d* and an $\alpha = 0.05$, the value of $s_{\bar{x}}^2$ can be computed and inserted in equation 1. For the present data, when N_2 is calculated for d = 0.109, $s_{\bar{x}}$ is 0.054; and when N_2 is calculated for d = 0.218, $s_{\bar{x}}$ is 0.109. An $s_{\bar{x}}$ of ±0.054 provides confidence limits of ± 10 percent, whereas an $s_{\bar{x}}$ of 0.109 provides confidence limits of ± 20 percent at the 0.95 probability level. For example, (± 1.99) (0.109) = ±0.217; and 0.217/1.09 = ± 20 percent.

Furthermore, with regard to equation 1, acceptable values of N_2 have already been calculated in a previous section of this paper (table 4). Therefore, the only unknown value in equation 1 is the number of blocks (N_1), which can be computed as:

$$N_1 = \frac{(N_2 \cdot 0.0355) + 0.2424}{N_2 (s_{\bar{x}}^2)} \qquad (2)$$

Values of N_1 were computed for future experiments that use the previously calculated number of subplots (N_2) for various combinations of B and *d* (table 4).

In our study, the average establishment cost per 2½-acre treatment plot was $230 when tree-cutting costs were included, and $70 when they were omitted. Installation and annual measurements per subplot were calculated to be $5 for a 5-year experiment. Thus, from these figures, costs can be estimated for any combination of blocks and subplots (table 4). For example, if 212 subplots per treatment plot and 12 blocks (table 4) are too expensive, one may revise his choice of B or *d*.

We recommend 54 subplots per treatment plot and 3 blocks, when blocks seem necessary, for most future studies. This arrangement of subplots and blocks permits detection of a 20-percent difference between any two treatment means, and provides a test of significance with a power of 0.90 (table 4).

Randomized Blocks Verses a Completely Randomized Design

In our experiment, a completely randomized design would have been just as efficient as the randomized-blocks design *(Snedecor 1956: 302)*. Therefore, in future experiments, if stands are as uniform as the one used in this study, we recommend using a completely randomized design. However, the number of treatment replications in a completely randomized layout should be equal to the number of blocks required for selected *d* and B terms in a comparable randomized-block design (table 4).

Treatment Description

On the basis of initial results from our experiment, we suggest that future studies of browse production under various cutting practices should incorporate wide differences between cutting treatments in order to detect significant differences of practical value. Three cutting intensities recommended for similar experiments in this kind of stand are: 0 (clearcut), 45, and 90 square feet of basal area reserved per acre. However, one undesirable feature might be in not learning about the true shape of the curve of response for intermediate basal-area treatments. When timber production is the primary research interest and wildlife habitat is the secondary consideration, an experiment of this type might profitably include more than three residual densities and possibly some different variables as well.

Perhaps variation in browse data could be reduced by assigning treatments in terms of a fixed basal area to be cut within certain diameter classes and restricting cutting to two or three species such as beech, red maple, and sugar maple. However, this procedure probably would increase the variation in timber-growth data.

Large cuttings (more than 2½ acres) spaced farther apart might give different browse production-utilization results. Future experiments of this kind should have larger treatment plots—perhaps 20 to 50 acres—to help eliminate the possible effects of deer use in one treatment on deer use in another. It may be difficult to find large areas with uniform site and stand conditions and similar local deer populations, but multiple co-variance analysis techniques may help to solve this problem.

Replication of Treatments within Blocks

If randomized blocks are used, treatments should be replicated at least once within each block so that the effect of block x treatment interaction can be measured (table 5). This design also will provide a more appropriate error term for testing treatment effects. If interaction mean square is significant (table 5), then it is necessary to analyze treatment effects within each block as a separate experiment and to treat each block as a completely randomized design *(Steel and Torrie 1960: 99-100)*.

Table 5.—*Analysis of variance model for a randomized block experiment with treatments replicated within each block and subsampling of treatment plots. Components of variance for fixed effects.*

Degrees of freedom	Source of variation	Parameters estimated[1]
Blocks	(b-1)	$\sigma_s^2 + sr\sigma_E^2 + tsr\,K_B^2$
Treatments	(t-1)	$\sigma_s^2 + sr\sigma_E^2 + bsr\,K_T^2$
Blocks x treatment	(b-1) (t-1)	$\sigma_s^2 + sr\sigma_E^2 + sr\,K_{BT}^2$
Experimental error	bt (r-1)	$\sigma_s^2 + sr\sigma_E^2$
Sampling error	btr (s-1)	σ_s^2

[1] b = number of blocks, t = number of treatment, r = number of replications, and s = number of subplots.

Effect of a Location Factor

The objective of future multiple-use experiments for a given timber type may be to compare the effects of various cutting intensities at different locations so that inferences can be made from experimental results that will apply to a timber type throughout a region rather than to a specific location within that region. Usually treatments would be considered as fixed effects. Locations and blocks would be considered as random effects if specific locations and the blocks within each location were selected at random from a population of locations and blocks. The analysis-of-variance model for these conditions is outlined in table 6. An example of the calculations involved, without subsampling, is given by Snedecor *(1959: 374-375)*.

Table 6.—*Analysis of variance model for a series of randomized blocks experiments over several locations. Treatments replicated within each block and subsampling of treatment plots. Components of variance for mixed model; treatments fixed, locations and blocks random.*

Source of variation	Degrees of freedom	Parameters[1] estimated
Treatments	(t-1)	$\sigma_s^2 + bsr\sigma_{TL}^2 + bcsrK_T^2$
Locations	(c-1)	$\sigma_s^2 + tsr\sigma_{B(L)}^2 + tbsr_L^2$
Treatments x Locations	(t-1) (c-1)	$\sigma_s^2 + bsr\sigma_{TL}^2$
Blocks in location, B(L)	c(b-1)	$\sigma_s^2 + tsr\sigma_{B(L)}^2$
Pooled error	c(t-1) (b-1)	σ_s^2

[1] b = number of blocks in each location, t = number of treatments, c = number of locations, r = number of replications, and s = number of subplots.

ANALYSIS OF DEER USE

Pellet Group Counts on Subplots

The average number of deer-pellet groups per subplot are summarized by treatments in table 7. These average values serve as an estimate of deer population levels within each treatment during the winter months (November to April). Eberhardt *(1960)* pointed out that: "In open areas, or under coniferous cover, it becomes necessary to estimate the age of pellet groups." This was not done in our study, so deer use may have been overestimated in the clearcut, 15-, and 30-square-foot treatments. The forested area surrounding these treatment plots may have

Table 7.—*Average number of deer-pellet groups before and after transformation of each subplot count (X) to $\sqrt{X}$.*

Randomized block number	Reserved basal area per acre	Original data: Average number of pellet groups per subplot	Original data: $\frac{s}{\bar{x}}$[1]	Transformed data: Average number of pellet groups per subplot	Transformed data: Standard deviation s
	Sq. ft.	*No.*		*No.*	
1	0	2.12	1.09	1.12	0.95
2	0	1.70	1.32	.90	.96
1	15	1.57	1.49	.80	.98[2]
2	15	1.35	1.20	.87	.78
1	30	1.00	1.58	.65	.76
2	30	2.22	.92	1.20	.85
1	45	.85	1.38	.60	.71
2	45	.80	.139	.58	.68[3]
1	60	.17	.265	.16	.39[4]
2	60	1.00	1.20	.69	.71
		$F_{max.} = \frac{5.43}{0.20} = 27.15$[5]		$F_{max.} = \frac{0.96}{0.47} = 2.04$[5]	
		$P\{F_{max.} > 2.99\} = 0.05$		$P\{F_{max.} > 2.99\} = 0.05$	

[1] Standard deviation per average number of pellet groups.
[2] Maximum standard deviation.
[3] Minimum standard deviation.
[4] Omitted data.
[5] $F_{max.} = s^2_{max.} / s^2_{min.}$

contributed a certain amount of leaf cover but probably not enough to eliminate completely this source of error. However, the analysis of this data is presented here to show the procedures and models that were used.

Homogeneous treatment variances—except for the 60-square-foot treatment in block 1—were obtained by transforming the count of pellet groups per subplot (X) to $\sqrt{X}$. The hypothesis of additivity for the transformed data was accepted by means of Tukey's test of additivity.

Relation Between Cutting Intensity and Deer Use

Basal area cut per acre (X) was related to the average of the square-root-transformed number of deer-pellet groups per subplot (Y) within that treatment. The regression

$$Y = 0.168 + 0.009X$$

which was significant at the 0.01 level, accounted for about 58 percent of the total variation in the dependent variable (fig. 4).

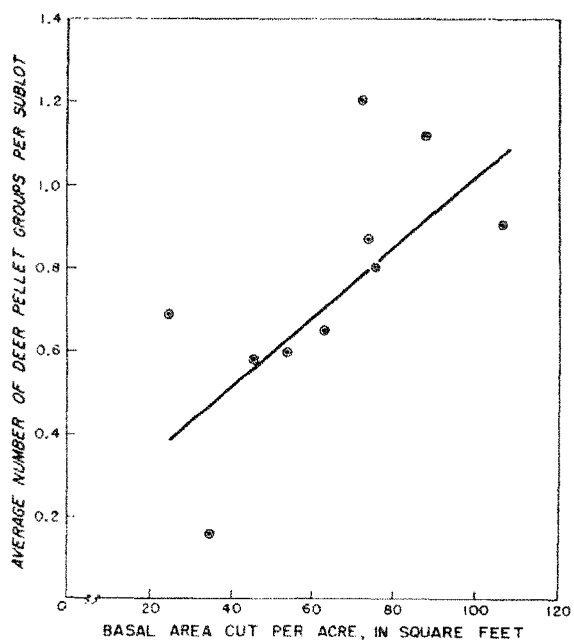


Figure 4. — Regression of deer use during the winter period (expressed as the average .number of deer pellet groups per subplot, after the square-root transformation of subplot data) and basal area cut per acre.

This is a model I (fixed X) after Snedecor *(1959: 126)*. Regression model IA *(Snedecor 1959: 153)* was not appropriate, even though the standard deviation was proportional to the mean in the original data, because the logic of the situation did not permit the assumption that the regression passes through zero. The slope of the line probably would have been less if the age of the pellet groups had been estimated in the heavier cuttings.

ANALYSIS OF RABBIT USE

The percentage of subplots within each treatment and block that contained rabbit pellets (not aged) is shown in table 8. Analysis of variance of the arcsin transformed values (table 9) indicated that the 45- and 60-square-foot treatments differed significantly. Furthermore, the clearcutting of 2 ½-acre areas did not result in a greater rabbit frequency—during the first year after cutting—than the average of the 15-, 30-, 45-, and 60-square-foot basal-area reserve cuttings. The same inference can be made concerning the 15-square-foot basal-area reserve treatment vs. the average of the 30-, 45-, and 60-square-foot treatments, and the 30-square-foot reserve treatment vs. the average of the 45 and 60. One possible explanation may be that increased overstory density offers protection from winged predators and offsets the protective advantages of slash density in heavily cut areas—up to a 45-square-foot reserve cut. However, these results may also be related to other variables not measured in this study.

Table 8.—*Percentage of 40 subplots per treatment and block containing rabbit pellets.*

Randomized block	Basal area per acre reserved, square feet				
	0 (clearcut)	15	30	45	60
1	17	12	27	30	2
2	20	5	20	20	5

Table 9.—*Analysis of variance for arscin transformed values of percentage of subplots with rabbit pellets.*

Source of variation[1]	Degrees of freedom	Sum of squares	Mean square	F	F 0.05
Block	1	13.96	—	—	—
Treatment:	4	565.56	141.39	9.35*	6.39
1 vs. 2, 3, 4, 5	1	25.36	25.36	1.68	7.71
2 vs. 3, 4, 5	1	63.77	63.77	4.22	—
3 vs. 4, 5	1	101.62	101.62	6.72	—
4 vs. 5	1	374.81	374.81	24.80*	—
Error	4	60.45	15.11	—	—

[1] The treatment numbers from 1 through 5 denote, respectively, the reserve basal area treatments of 0 (clearcut), 15, 30, 45, and 60 square feet per acre.

* Indicates significance.

ANALYSIS OF SPROUT GROWTH

Although sprout growth usually is considered less desirable than seedling reproduction from the timber manager's point of view, sprout growth is analyzed here because:

- It is important for wildlife food and cover.
- The procedures used to compare sprout growth under the various cutting treatments are applicable to seedling reproduction measurements.
- The data may prove useful to timber managers for prediction purposes or in deciding how to avoid excessive sprout growth.

A stocked subplot was considered as one that contained at least one unbrowsed terminal leader of hardwood sprout growth. Treatment results are summarized in table 10. Two analyses of variance were made (table 11) using the arcsin transformed values of the percentages of subplots stocked with: sprout growth of red maple, sugar maple, black cherry, or white ash; or sprout growth of any of the above species or beech, in the 1.0- to 4.9-foot height class.

The results indicated that, when species other than beech were considered, the clearcutting treatment resulted in a significantly greater percentage of stocked subplots the first year after

Table 10.—Percentage of subplots in each treatment and block stocked with spout growth of (1) red maple, sugar maple, black cherry, or white ash, and (2) any of the above or beech in the 1.0- to 4.9-foot height class.

Type of sprout growth	Randomized block	Basal area per acre reserved, square feet				
		0	15	30	45	60
Red maple, sugar maple, black cherry or white ash	1	30	30	10	12	7
	2	20	12	10	10	5
any of the above species or beech	1	37	35	42	30	10
	2	35	20	32	17	20

Table 11.—Analysis of variance for arcsin transformed values of percentages of subplots stocked with (1) red maple, sugar maple, black cherry, or white ash, and (2) any of the above species or beech, in the 1.0- to 4.9-foot height class.

Source of variation[1]	Degrees of freedom	Sum of squares	Mean square	F	F 0.05
RED MAPLE, SUGAR MAPLE, BLACK CHERRY, OR WHITE ASH					
Blocks	1	56.83	—	—	—
Treatments:	4	331.64	82.91	14.04*	6.39
1 vs. 2, 3, 4, 5	1	167.07	167.07	10.70*	7.71
2 vs. 3, 4, 5	1	133.43	133.43	8.53*	—
3 vs. 4, 5	1	3.84	3.84	.24	—
4 vs. 5	1	27.30	27.30	1.75	—
Error	4	62.40	15.60	—	—
ANY OF THE ABOVE SPECIES OR BEECH					
Blocks	1	30.96	—	—	—
Treatments	4	304.64	76.16	1.03	6.39
Error	4	294.27	73.57	—	—

* Indicates significance.

[1]The treatment numbers from 1 through 5 denote, respectively, the reserve basal area treatments of 0 (clearcut), 15, 30, 45, and 60 square feet per acre.

cutting than the average of the other cutting treatments. And the 15-square-foot reserve cutting resulted in a greater percentage of stocked subplots than the average of the 30-, 45-, and 60-square-foot treatments.

When all unbrowsed sprout growth, including beech, was considered, none of the treatments differed significantly in terms of stocking. Beech browse was low-preference deer food within the study area, which may account for an abundance of unbrowsed beech for all treatments.

LIMITATIONS OF THE DATA

If the procedures or the results outlined in this paper are used in the design or analysis of future timber-wildlife experiments, at least three limitations of the data need to be considered:

1. By using the variance of one year's browse-measurement data across five cutting intensities, statistical inferences have been made concerning the appropriate design for future studies of browse-timber production. These inferences assume that timber-growth data, which is not yet available for this study, will contain about the same, or less, variation than the browse-measurement data.
2. The stand in which the experiment was conducted was fairly homogeneous with respect to age, basal area per acre, species composition, aspect, and soil type. Experiments on less uniform areas would very likely produce greater variances than were reported for this study.
3. The confidence limits and designs recommended in this paper provide the basis for a series of yearly analyses over the lifetime of the experiment. Any analysis based upon sums or means over several years should provide relatively more accurate estimates than those obtained for any one year.

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