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Efficiency of Type 2 Modified Augmented Designs in Soybean Variety Trials

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ABSTRACT

Replicated designs are not practical when large numbers of test lines with limited seed supplies must be evaluated. Therefore modified augmented design (Type 2) was used to assess yield and other agronomic variables in a large number of soybean [*Glycine max.* (L). Merr.] strains in Ottawa. Thirty-three experiments, each involving 100 to 500 test lines, were studied to determine the relative efficiency (RE) of adjusted versus unadjusted observations. There were seven groups of experiments in which the same set of test lines were used, and these were further investigated to determine RE in replicated conditions. Thus, RE was defined in two different ways; for nonreplicated (single) experiments the RE was measured by the ratio of variances between the unadjusted and the adjusted values of the subplot controls, and for the replicated experiments the RE was measured by the ratio of the mean squares of the "Replication × Test line" (based on a randomized block design) between the unadjusted and the adjusted values. Adjustment was made for fertility variation by using the row and the column effects of control plots (Method 1), and the regression coefficient of the test plots on the control (Method 3). The results show that, for yield, the average RE by Method 3 was 220% for the nonreplicated and 167% for the replicated experiments, and 160 and 147% for Method 1 for the corresponding experiments. The adjustment was most effective for yield and plant height, somewhat less effective for protein and oil, and not very effective for maturity and 100-seed weight.

IN THE EARLY stages of a variety development program a plant breeder often has a large number of test lines, but only a small amount of seed available of each line. Yield evaluation must either be postponed to a later generation when sufficient seed is available for a replicated experiment or must risk biased estimation due to uncontrolled fertility differences in the field. To cope with this dilemma, Lin and Poushinsky (1983) proposed a modified augmented design (MAD) for nonreplicated experiments. Two types of MAD were developed; Type 1 for a square plot (or nearly so), and Type 2 for a rectangular plot (Lin and Poushinsky, 1985). The design is structured as a split plot with whole plots arranged in rows and columns. The subplots are grouped either 3 × 3 (Type 1) or a 5 × 1 (Type 2) to form a whole plot. The center subplot of each whole plot is designated as a control plot to which a check variety is allocated. In addition, about 10 whole plots are randomly chosen to which two subplot controls are assigned. The test lines are then randomly allocated to the remaining plots. The basic idea is to use the control plots, which are systematically allocated over the entire test area, to adjust for soil heterogeneity and to use the subplot controls to measure the efficiency of adjustment. Three methods of adjustment, row-column (Method 1), index (Method 2), and regression (Method 3), were studied using simulation for both Type 1 MAD (Lin et al., 1983) and Type 2 MAD (Lin and Poushinsky, 1985). The results indicated that Method 2 is clearly inferior and has the greatest chance for making over-adjustment, whereas Method 1 and Method 3 are both effective depending on whether the fertility variation is directional (Method 1) or spotty (Method 3). Similar conclusions were also reached by Schaalje et al. (1987), who used a Type 1 MAD for a potato breeding project.

The purpose of this paper is to study the effectiveness of Type 2 MAD under field condition. Data from 33 soybean variety trials, conducted at Ottawa, Ontario from 1984 to 1986, were used to examine the relative efficiency (RE) of Methods 1 and 3. The RE of the design when replicated was also investigated.

MATERIALS AND METHODS

Lin and Poushinsky's (1985) Type 2 MAD was used for all experiments. The number of rows and columns differed

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for each experiment, but the number of subplots within a whole plot was always five. Subplots were 5 m in length and 1.6 m wide. Each subplot consisted of four rows, with 40 cm between rows and between plots. Before harvest the plots were end-trimmed to 4 m. All four rows were combine harvested (no border row). Three check varieties were used: Check A as whole plot controls in all whole plots; and checks B and C as subplot controls assigned randomly to 7 to 14 whole plots. The checks were not necessarily the same for each experiment. The scope of each experiment is shown in Table 1. Experiments numbered 2000 to 5000 were composed of Maturity Group 000 and 00 strains (2300 to 2500 Corn Heat Units), and those numbered from 6000 to 9000 were composed of Maturity Group 0 strains (2500 to 2700 Corn Heat Units). Experiments numbered from 4000 to 7000 were composed of F3 and F4 bulks, while other experiments consisted of F5 and F6 strains. In some of these experiments the same set of test lines was used. They are indicated as "a", "b," or "c" after the test number of the corresponding experiments. These data sets can be considered as replicated experiments and were subjected to further analysis of RE based on a randomized block design. Note that the experiments of replicated groups were all conducted in the different fields with contrasting soil types (clay vs. loam).

Two adjustment methods were used to adjust for soil variation: row-column (Method 1) and regression (Method 3). Let $y_{ij(k)}$ be the observed value of the kth test line in the whole plot of the ith row and the jth column, and let X_{ij} be the observed value of check A in the control plot of the corresponding whole plot. Then the adjusted value for the kth line (Y_k by Method 1 is,

$$Y_k = y_{ij(k)} - \bar{X}_{i\cdot} - \bar{X}_{\cdot j} + 2\bar{X}_{\cdot\cdot};$$

and by Method 3 is,

$$Y_k = y_{ij(k)} - b(X_{ij} - \bar{X}_{\cdot\cdot}),$$

where

$$b = \sum_i \sum_j (\bar{y}_{ij(\cdot)} - \bar{y}_{\cdot\cdot})(X_{ij} - \bar{X}_{\cdot\cdot}) / \sum_i \sum_j (X_{ij} - \bar{X}_{\cdot\cdot})^2.$$

Note that $\bar{y}_{ij(\cdot)}$ is the mean of the four test plots (test lines and/or subplot controls) within the ijth whole plot. In previous papers (Lin and Poushinsky, 1983,1985), b was calculated using the data from only those whole plots, which included subplot controls (see discussion).

Soil heterogeneity is investigated by the analysis of variance (ANOVA) in which row, column, and row × column interaction (whole plot error) effects were calculated from the (whole plot) controls, and subplot error was calculated from the subplot controls plus the controls of the selected whole plots. Assuming soil variation is random, the row and column can be tested against the interaction (whole plot error), and the interaction can be tested against the subplot error. The ANOVA for this type of design is not intended to provide a rigid test of significance, but to provide an overall view of the variation and, thus, indicates what adjustments, if any, are required for the experiment. The primary decision rules are; (i) if row, column, and the interaction effects are not substantially greater than the subplot error, the soil variation is considered to be homogeneous and adjustment is unnecessary; (ii) if either row or column effects, or both are substantially larger than the subplot error, but the interaction is not significant, then soil variation is adequately described by an additive model and Method 1 is used; (iii) if the interaction is significantly greater than the subplot error then the soil variation follows a non-additive model and Method 3 is used (Lin and Poushinsky, 1985). Decisions made based on the ANOVA can be further checked by RE of the subplot controls. The RE is defined in one of two ways; for nonreplicated experiments, RE is defined as the ratio of the variance (pooled mean squares within checks) based on the unadjusted and adjusted data of subplot controls, and for the replicated experiments, it is the ratio of the ordinary randomized block error (Replication × Test line) mean squares (MS) based on the unadjusted and adjusted data.

Table 1. Experimental set-up of 33 experiments.

Experiment	Test lines	Row × Col.	Area (m × m)	Selected whole plots†
1984				
1 (T2100)	202	8 × 7	48 × 51	11
2 (T4000)	376	9 × 11	54 × 94	10
3 (T4500)	304	8 × 10	48 × 85	8
4 (T7000a)‡	308	14 × 6	84 × 51	14
5 (T7000b)	308	14 × 6	84 × 51	14
6 (T8000a)	314	6 × 14	36 × 119	11
7 (T8000b)	322	8 × 11	48 × 94	15
8 (T3000a)	574	15 × 10	90 × 85	13
9 (T3000b)	574	15 × 10	90 × 85	13
1985				
10 (T9100a)	108	10 × 3	60 × 25	6
11 (T9100b)	108	10 × 3	60 × 25	6
12 (T9100c)	108	10 × 3	60 × 25	6
13 (T2100a)	124	9 × 4	54 × 32	10
14 (T2100b)	124	9 × 4	54 × 32	10
15 (T3000)	206	11 × 5	66 × 40	7
16 (T4700)	268	12 × 6	72 × 48	10
17 (T7000)	380	20 × 5	120 × 40	10
18 (T8000a)	268	12 × 8	72 × 48	10
19 (T8000b)	268	12 × 8	72 × 48	10
20 (T4000-1)	412	18 × 6	108 × 48	10
21 (T4000-2)	412	18 × 6	108 × 48	10
22 (T4000-3)	412	18 × 6	108 × 48	10
23 (T4000-4)	412	18 × 6	108 × 48	10
24 (T4000-5)	412	18 × 6	108 × 48	10
25 (T4000-6)	412	18 × 6	108 × 48	10
1986				
26 (T6000)	204	7 × 8	64 × 42	10
27 (T8500)	204	7 × 8	64 × 42	10
28 (T5000)	204	8 × 8	64 × 48	10
29 (T4100)	204	7 × 8	64 × 42	10
30 (T9600)	204	7 × 8	64 × 42	10
31 (T8300a)	100	5 × 6	48 × 30	10
32 (T8300b)	100	5 × 6	48 × 30	10
33 (T7500)	102	5 × 6	48 × 30	9

† Number of randomly chosen whole plots in which the subplot controls are allocated.

‡ "a", "b", or "c" indicate that the same set of test lines were used for the corresponding experiments.

RESULTS

The ANOVA (Table 2) for yield indicates that soil heterogeneity existed for all experiments except Exp. 27, 29, and 33, and adjustments are necessary. The adjustment method chosen, based on the decision rules described above, is indicated in the column labeled "ANOVA" of Table 2, and that based on the RE of subplot controls is shown in the column labeled "SUB". (Note that, although in this analysis we follow the customary 5% significance level, it may in many instances be more appropriate to use the 10% or even 25% level since the ANOVA is intended as a guide to which adjustment to make, rather than a test of line differences). Among 33 experiments, only 15 show good agreement between the two assessments. This probability is slightly less than that obtained in the simulation study (Lin et al., 1983). It seems that soil heterogeneity patterns observed in a real field are more complex than those of the contour maps used in the simulation.

Table 2. The combined ANOVA (kg ha^{-1})† based on controls and subplot controls for yields of 33 experiments.

	Row		Column		R × C		Error		Best method assessed by	
Experiment	MS‡	df	MS	df	MS	df	MS	df	ANOVA	SUB§
1984										
1	471*	7	976*	6	50*	42	11	20	3	3
2	3309*	8	184*	10	76*	80	21	18	3	3
3	141*	9	73*	7	34*	63	7	14	3	3
4	126*	13	82	5	59	65	41	26	1	3
5	147*	13	139*	5	58	65	48	26	1	3
6	971*	5	54	13	105*	65	35	20	3	3
7	300*	7	339*	10	57	70	58	28	1	3
8	42	14	394*	9	34	126	50	24	1	1
9	871*	14	455*	9	71	126	62	24	1	3
1985										
10	142*	9	162*	2	37*	18	10	10	3	3
11	84	9	132	2	92*	18	28	10	3	3
12	69*	9	114*	2	17	18	12	10	1	3
13	50	8	130*	3	40	24	25	18	1	3
14	94*	11	97*	2	14	22	17	18	1	1
15	58	10	1223*	4	116*	40	42	12	3	1
16	195*	11	162*	5	59*	55	19	18	3	3
17	286*	19	255*	4	74	76	70	18	1	3
18	150*	11	758*	5	52*	55	19	18	3	3
19	745*	11	454*	5	47	55	38	18	1	1
20	104*	17	90*	5	26*	85	11	18	3	3
21	97*	17	58*	5	20	85	16	18	1	3
22	116	17	59	5	108*	85	17	18	3	3
23	36*	17	174*	5	9	85	5	18	1	3
24	52*	17	31	5	20	85	11	18	1	3
25	105*	17	87*	5	15	85	20	18	1	3
1986										
26	68	6	166*	7	47	42	73	18	1	U¶
27	48	6	123	7	39	42	65	18	U	U
28	1370*	7	253	7	106	49	364	18	1	1
29	20	6	60	7	56	42	126	18	U	3
30	94*	6	25	7	37	42	37	18	1	U
31	148	4	404	5	354*	20	83	18	3	1
32	92*	4	112*	5	32	20	45	18	1	U
33	50	4	52	5	40	20	27	16	U	3

* Significant at the 0.05 level of probability. Row and column effects were tested against R × C, and the interaction was tested against subsample error.
† divided by 1000.
‡ MS = Mean squares.
§ "SUB" implies subplot control.
¶ "U" indicates unadjusted.

In the present sets of data Method 3 appears to be more effective than Method 1 in two out of three cases for yield (Table 3). Although the superiority of a method depends on variability patterns, Method 3 is generally safer than Method 1, since for yield, nine out of 33 experiments were over-adjusted (RE < 100) by Method 1, but none by Method 3. On the average, the gain in RE for yield is about 60% by Method 1 and about 120% by Method 3. The mean CV% over all experiments was 13.7% with no adjustment, and 9.9% after adjustment by Method 3 (Table 4). Among the six variables studied, adjustment was most effective for yield and plant height, and somewhat less effective for protein and oil. In contrast, the adjustment for maturity and 100 seed weight were not effective, except for maturity in Exp. 28 and 31, and 100 seed weight in Exp. 18.

To demonstrate how adjustment works, yields of Exp. 1 (the most effectively adjusted experiment) are used as an example. The 8 × 7 control plots are shown in Table 5. Since the same check was used on all these control plots, Table 5 can be regarded as a reflection of soil variation in the test field. Unadjusted and adjusted yields for the 22 subplots controls grown in the 11 selected whole plots are shown in Table 6. Sample variances for each check are substantially reduced with adjustment. The CV% of the adjusted values is about 1/3 of that of the unadjusted for both checks.

For the seven groups of replicated experiments, MAD also were effective in reducing the experimental error, except in one case (Exp. 31 and 32 combined). The average gain in RE for yield (Table 7) is about 47% by Method 1 and 67% by Method 3. To facilitate comparison of methods all experiments within the same group in Table 7 were adjusted by the same method. In practice, however, a user should use the best adjustment for each experiment independently. It is important to remember that RE measured by subplot controls is based on small samples (about 20 observations per experiment), while the RE measured by replicated sets is based on all the test lines (>100), so the latter estimates are usually more accurate.

DISCUSSION AND CONCLUSIONS

The MAD is based on the assumption that subplots within a whole plot are homogeneous with respect to soil variability, so the adjustment is on a whole plot basis. To make certain that this basic assumption holds in practice and the adjustment is effective, the design

Table 3. Relative efficiency (%) measured by subplot controls for methods 1 (M1) and 3 (M3).

	Yield		Height		Maturity		Seed wt.		Protein		Oil	
Experiment	M1	M3	M1	M3	M1	M3	M1	M3	M1	M3	M1	M3
1984												
1	645	1032	190	161	60	63	186	141				
2	120	201	143	248	62	100	198	259				
3	84	261	238	194	95	108	71	102				
4	155	196	216	331	88	100	84	93				
5	75	105	120	118	105	101	81	99				
6	157	257	163	290	99	104	96	97				
7	152	165	105	106	66	54	171	173				
8	265	180	105	126	100	101	99	100				
9	144	165	139	258	17	123	118	115				
1985												
10	135	293	205	263	72	104	71	108	107	107	72	101
11	161	631	88	125	77	100	73	103	125	288	126	230
12	117	142	160	118	110	104	94	102	153	142	92	102
13	172	193	125	104	75	105	70	103	101	116	144	150
14	240	202	150	119	79	101	100	109	160	133	165	120
15	319	259	114	171	130	104	99	118	153	195	126	130
16	135	137	196	186	81	115	92	103	74	101	70	100
17	173	226	286	265	95	144	126	113	218	331	202	247
18	159	279	124	203	143	223	148	309	99	116	106	108
19	215	209	275	212	132	264	105	113	154	184	127	215
20	101	323	209	457	85	104	113	143				
21	120	232	147	131	123	130	133	143				
22	74	162	256	293	47	136	98	133				
23	220	286	147	205	74	102	159	102				
24	115	127	87	130	60	101	122	108				
25	75	101	90	103	107	103	82	102				
1986												
26	74	100	80	125	88	202						
27	124	101	116	112	119	123						
28	136	133	387	323	292	250						
29	80	106	116	165	81	102						
30	71	101	141	141	8	101						
31	258	154	122	237	353	444						
32	71	101	117	107	97	100						
33	92	207	90	117	75	102			103	136	152	134
Mean	159	223	159	189	100	131	112	128	132	168	126	140

uses two devices; arranging the subplots in such a way that the distance between the center of each subplot and the corresponding control plot in that whole plot is approximately equal, and making the whole plots nearly square, and then arranging them in a row and column design. The first device ensures that an equal adjustment can be applied to all subplots within a whole plot, in order to avoid a weighted adjustment by distance, which is frequently found to be unsatisfactory in practice (Lin and Poushinsky, 1983). The second device ensures that no matter how the design is allocated in the field, it will not be affected by the magnitude and the direction of soil variation. To be more specific, the design uses the square shape of a whole plot to ensure the equal weight of adjustment for both row and column directions. Also, by arranging whole plots in rows and columns (two-way) the design resolves the orientation problem of experimental layout. A one-way design, such as a randomized block, is sometimes ineffective in field experimentation. The reason seems to be that effective blocking requires that blocks be laid down perpendicular to the direction of soil variation, and this, in practice, is often unobtainable. A two-way design avoids this problem.

A general conclusion from the present study is that the MAD is effective in controlling variation in field experiments for both replicated and nonreplicated trials. In a previous simulation study (Lin et al., 1983) a MAD was found to be as effective as a lattice square in replicated experiments when the soil variation between squares was large. The present results reinforce the view that MAD may also be useful for a replicated experiment when the number of test lines is large (say, > 100). However, the practicality of the MAD in replicated experiments requires further study, because other statistical techniques such as nearest neighbor (Besag and Kempton, 1986; Binns, 1987), or a generalized lattice design (Patterson and Hunter, 1983), can be more effective.

From a practical point of view, the most attractive features of MAD are; its flexibility to accommodate different numbers of test lines and whole plot arrangements, its ease of field layout, management and harvest (Schaalje et al., 1987), and its convenience in the field for the observation of soil variation and for line comparisons. However, one should also keep in mind the remarks (Lin and Poushinsky, 1983) that "the present design is prepared for the limited experimental conditions of a nonreplicated experiment with many test lines. Although the design has managed to overcome some of these experimental limitations by use of control plots as a sort of built-in uniformity trial, and by use of subplot controls to obtain some measure of the efficiency of the adjustment methods, those plots provide only a fraction of the observations; care should be taken when drawing conclusions from them."

Table 4. The CV (%) for unadjusted and adjusted measured by subplot controls by Method 3 (M3).

	Yield		Height		Maturity		Seed wt.		Protein		Oil	
Experiment	Unadj	M3	Unadj	M3	Unadj	M3	Unadj	M3	Unadj	M3	Unadj	M3
1984												
1	18.5	5.6	13.5	10.3	1.6	2.0	5.9	5.0				
2	11.5	9.3	10.5	7.4	1.3	1.3	8.4	5.3				
3	6.8	4.4	10.7	7.9	1.7	1.7	4.2	4.2				
4	11.3	8.0	17.2	9.4	1.3	1.3	5.0	5.2				
5	7.8	7.7	9.7	8.9	1.4	1.4	4.3	4.3				
6	14.1	8.5	19.9	12.4	1.8	1.8	4.8	4.9				
7	12.9	9.8	6.7	6.4	1.5	2.1	6.2	4.6				
8	15.8	11.3	13.9	12.3	1.5	1.5	4.9	4.9				
9	15.0	11.4	16.9	10.2	0.8	6.9	5.9	5.5				
1985												
10	8.8	5.2	11.9	7.3	1.6	1.6	5.1	4.9	1.4	1.4	1.9	1.9
11	22.0	8.8	11.9	10.6	1.8	1.8	5.7	5.7	3.1	1.8	2.8	1.8
12	6.1	5.2	11.1	8.6	1.2	1.2	4.7	4.7	2.3	1.9	3.6	3.5
13	13.9	10.0	10.0	9.8	1.7	1.6	4.0	3.9	2.5	2.3	3.1	2.5
14	14.1	9.9	12.7	11.6	1.2	1.2	9.0	8.6	4.1	3.6	4.5	4.1
15	23.0	14.3	14.3	11.0	1.9	1.9	9.1	8.3	2.6	1.9	3.4	3.0
16	9.2	7.9	10.2	7.4	0.6	0.5	4.8	4.7	1.5	1.5	1.4	1.4
17	22.0	14.6	18.0	11.0	1.8	1.5	6.9	6.4	3.5	1.9	4.1	2.6
18	11.3	6.8	7.1	5.0	0.8	0.5	8.5	4.8	2.4	2.3	2.1	2.2
19	18.7	13.0	15.2	10.4	1.5	0.9	4.1	3.9	3.1	2.3	3.2	2.2
20	17.9	9.9	13.0	6.1	1.2	1.1	5.4	4.5				
21	14.2	9.3	5.3	4.7	1.0	0.9	6.5	6.5				
22	15.0	11.8	15.6	9.1	1.0	0.9	6.0	5.2				
23	12.5	7.4	11.3	7.9	1.6	1.6	5.7	5.7				
24	9.5	8.4	7.3	6.4	1.3	1.3	5.8	5.6				
25	6.9	6.9	3.9	3.8	1.1	1.1	6.3	6.3				
1986												
26	11.6	11.6	7.4	6.6	1.1	0.7						
27	10.2	10.2	8.9	8.4	1.2	1.1						
28	33.9	29.4	15.6	8.7	3.5	2.2						
29	12.1	11.8	9.2	7.2	0.7	0.7						
30	6.5	6.5	11.0	9.3	0.6	0.6						
31	17.3	13.9	11.2	7.3	4.4	2.1						
32	8.9	8.9	8.2	8.0	2.5	2.5						
33	11.3	7.8	11.2	10.3	2.2	2.2			1.6	1.4	2.5	2.2
Mean	13.7	9.9	11.5	8.5	1.5	1.6	5.9	5.3	2.6	2.0	3.0	2.5

The following are some comments which may be useful in practice. One problem that arises from the small sample size for control plots and the subplot controls is that the conclusions drawn from the ANOVA and from the RE of subplot controls may not always agree. Under such circumstances it is necessary to examine the data of the control plots and subplot controls, as in Tables 5 and 6, and then to investigate whether the disagreement is due to uneven distribution of subplot controls or to outliers. Depending on the results of the investigation, one then decides which analysis is more appropriate. If no apparent reason is found, our advice is to use the ANOVA to decide whether the data should be adjusted or not, and then to use the RE of the subplot controls to choose the method. If the results are still inconclusive (borderline case), one might use Method 3 because this method seldom overadjusts.

In the present paper the regression coefficient of Method 3 is calculated from the entire data set, rather than from selected whole plots. The latter approach has the advantage of b being independent of test lines, but it gives relatively poor estimation because of the small numbers of observations. In comparison, the

Table 5. Yields (kg ha^{-1}) of control plots (check A) in Exp. 1.

	Column							
Row	1	2	3	4	5	6	7	Ave.
1	3189	2636	2400	1660	1329	1687†	1921	2117
2	3302†	3099	2827	2364	2291†	2144	2486†	2645
3	3039	3158†	2741	3254	2273	2648	2576	2813
4	3132	3042	3310	3024	2427	2677	2331†	2849
5	3179	3200	2763	2810	2337	2507	2446†	2749
6	3052	3094	2894	2985	2539	2564	2197	2761
7	3085	2984	3146†	2640	2408	2441†	2518	2746
8	3263	3173	3408	3027†	2498	2597†	2989	2994
Ave.	3155	3048	2936	2721	2263	2408	2433	2709

† Indicates 11 randomly chosen whole plots in which the subplot controls are allocated.

Table 6. Unadjusted and adjusted yield (kg ha^{-1}) by Method 1 (M1) and by Method 3 (M3) for subplot controls in Exp. 1.

Whole plot		Check B			Check C		
Row	Col.	Unadj.	M1	M3	Unadj.	M1	M3
1	6	1181	2074	2111	2277	3170	3207
2	1	2842	2460	2176	3948	3566	3282
2	5	1784	2295	2117	2800	3311	3133
2	7	2399	2740	2540	3379	3720	3520
3	2	2804	2361	2281	3611	3168	3088
4	7	2089	2225	2383	2903	3039	3197
5	7	1881	2117	2061	2968	3204	3148
7	3	2509	2245	1998	3579	3315	3068
7	6	1769	2033	1954	3068	3332	3253
8	4	2727	2431	2333	3720	3424	3326
8	6	2209	2226	2240	3238	3255	3269
Mean		2199	2292	2200	3226	3319	3227
SD		520	202	176	484	193	127
CV %		23.6	8.8	8.0	15.0	5.8	3.9

Table 7. The relative efficiency (%) of Methods 1 (M1) and 3 (M3) as compared to the unadjusted for the yield of the replicated experiments.

Replicated experiments	df	Error MS† for unadjusted	RE‡ of M1	RE‡ of M3
1984				
4 and 5	307	96	117	112
6 and 7	313	195	140	141
8 and 9	573	103	124	141
1985				
10, 11 and 12	214	116	135	147
13 and 14	123	67	137	154
18 and 19	267	208	282	382
1986				
31 and 32	99	266	95	94

† Error MS = MS for (Rep. × Test line) divided by 1000.
‡ RE = Error MS for unadjusted/Error MS for adjusted × 100.

present approach generally gives better adjustment, provided that the test lines have been randomized.

When preparing MAD the user should work out the degrees of freedom for respective sources of error to ensure that they are not too small (not less than 15). One way to increase the number of subplot controls for estimating RE is to use more than one subplot for those test lines that have enough seeds for duplicated plots. The practice of mixing single or duplicated test lines is acceptable for this design.

Modified augmented design requires 1/5 to 1/3 of the total plots for controls and subplot controls. This is one disadvantage of the design, but is unavoidable. This constraint can be relaxed to some extent for Type 2 MAD by increasing the number of subplots within a whole plot to seven or nine (the number which is equivalent to Type 1). Lin and Poushinsky (1985) suggested that the number can be adjusted, based on the ratio of width and length of the subplot, in such a way that the resulting whole plot is nearly square. However, an increase in the number of subplots may also decrease the sensitivity of adjustment.

The primary objective of the MAD is to estimate the genetic values of the test lines for selection (top 10%) and not for critical assessment of line differences. Such a test will eventually become necessary when selection progresses to the later stages. However, by that time the number of selected lines is substantially reduced and sufficient material may be available for a replicated experiment, therefore, there would be no need for MAD. Having said that, if a test is really needed, formulae based on the expected value of the MS are given in the appendix. Note that since these variance estimates are based on partial data, the resulting tests are only approximations. For practical purposes, use of the pooled error MS within checks, based on the adjusted subplot controls, to estimate the standard error of differences should be adequate.

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APPENDIX

Let the expected mean squares for the ANOVA be written as follows:

Source	df	EMS
Row	$p - 1$	$\sigma_w^2 + q\,\sigma_r^2$
Column	$q - 1$	$\sigma_w^2 + p\,\sigma_c^2$
R × C (whole plot error)	$(p - 1)(q - 1)$	σ_w^2
Subplot error	$(3 - 1)m$	σ_s^2

where p and q are numbers of rows and columns respectively, m is the number of whole plots in which subplot controls are allocated, and σ_r^2, σ_c^2, σ_w^2, σ_s^2 are variance components for row, column, whole plot, and subplot errors respectively. [Note that we assume two random components of error associated with each subplot: one which differs from subplot to subplot (σ_1^2), and one which is the same for all subplots within a whole plot (σ_2^2). Thus, $\sigma_w^2 = \sigma_1^2 + \sigma_2^2$, and $\sigma_s^2 = \sigma_1^2$, (Lin and Poushinsky, 1983)].

Let $\sigma^2 = \sigma_w^2 + \sigma_c^2 + \sigma_r^2$, and let r be the simple correlation coefficient between the control plots and the means of corresponding subplots. Then the squared standard error of the difference between two test lines can be summarized as follows:

Two lines in the	Method 1	Method 3
1) same whole plot	$2\hat{\sigma}_s^2$	$2\hat{\sigma}_s^2$
2) same row	$\frac{2(p-1)}{P}\hat{\sigma}_w^2 + \frac{4}{P}\hat{\sigma}_s^2$	$2(1-r^2)(\hat{\sigma}^2-\hat{\sigma}_r^2)$
3) same column	$\frac{2(q-1)}{q}\hat{\sigma}_w^2 + \frac{4}{q}\hat{\sigma}_s^2$	$2(1-r^2)(\hat{\sigma}^2-\hat{\sigma}_c^2)$
4) different row and column	$\frac{2(pq-p-q)}{pq}\hat{\sigma}_w^2 + \frac{4(p+q)}{pq}\hat{\sigma}_s^2$	$2(1-r^2)\hat{\sigma}^2$

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