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ABSTRACT

By employing a concomitant variable, block designs and analysis of covariance (ANCOVA) can be used to improve the power of traditional analysis of variance (ANOVA) by reducing error. If subjects are randomly assigned to treatments without considering the concomitant variable, an experiment uses a post-hoc approach. Otherwise, an a priori approach is used if the concomitant variable is utilized for assigning subjects to treatments. Traditionally, a priori has been considered the more powerful approach. This study compared ANOVA, block designs, and ANCOVA under various experimental conditions. The experimental conditions were 48 combinations of 4 levels of the number of treatments (T at 2, 3, 4, and 5), 3 levels of the number of subjects per treatment (n at 8, 40, and 72), and 4 levels of the correlation coefficient between the concomitant and dependent variables (p at 0.00, 0.28, 0.56, and 0.84). The optimal number of blocks to achieve maximum power was also investigated. Results indicated that a priori was not generally more powerful than post-hoc. For ANOVA, a priori became less powerful as T and p increased. For block designs, the preference depended on the experimental conditions. For ANCOVA, a priori was more powerful when T and n were small. Appendix A explores apparent imprecision, Appendix B presents two Statistical Analysis System computer programs, and Appendix C contains a power table. (Contains 9 tables and 3 references.) (Author/SLD)

A PRIORI VERSUS POST-HOC: COMPARING STATISTICAL POWER AMONG ANOVA, BLOCK DESIGNS, AND ANCOVA

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"A PRIORI VERSUS POST-HOC: COMPARING STATISTICAL POWER AMONG ANOVA, BLOCK DESIGNS, AND ANCOVA"

By employing a concomitant variable, block designs and ANCOVA can be used to improve the power of traditional ANOVA by reducing error. If subjects are randomly assigned to treatments without considering the concomitant variable, an experiment uses a post-hoc approach. Otherwise, an a priori approach is used if the concomitant variable is utilized for assigning subjects to treatments. Traditionally, a priori has been considered the more powerful approach. The purpose of this study is to compare statistical power of a priori and post-hoc approaches among ANOVA, block designs, and ANCOVA under various experimental conditions. The experimental conditions were 48 combinations of four levels of the number of treatments (T; 2, 3, 4, 5), three levels of the number of subjects per treatment (n; 8, 40, 72), and four levels of the correlation coefficient between the concomitant and dependent variables (ρ; .00, .28, .56, .84). The optimal number of blocks to achieve maximum power was also investigated.

Results indicated that a priori was not generally more powerful than post-hoc. For ANOVA, a priori became less powerful as T and ρ increased. For block designs, the preference depended on the experimental conditions. For ANCOVA, a priori was more powerful when T and n were small.

A PRIORI VERSUS POST-HOC: COMPARING STATISTICAL POWER AMONG ANOVA, BLOCK DESIGNS, AND ANCOVA

The most widely used procedures to harness the positive effects of a concomitant variable are block designs and ANCOVA. Whether to block or covary and how many blocks to use if a block design is chosen become crucial decisions. Wu and McLean (1993, November) and Wu (1994) provided an historical review of the problem, finding that some researchers favor block designs while others prefer ANCOVA. The most comprehensive studies on this topic were conducted by Feldt (1958) and Maxwell and Delaney (1984). Feldt analytically examined the problem using Apparent Imprecision as the criterion variable, while Maxwell and Delaney empirically examined the problem using the Type I error rate and power in addition to Apparent Imprecision as the criterion variables. Based on Apparent Imprecision, Feldt found the correlation coefficient between the concomitant and dependent variables is the factor in choosing blocking or ANCOVA. He also provided the optimal number of blocks to use if blocking is chosen. Feldt's findings have been cited by many research articles and texts discussing this work (cf., Maxwell & Delaney, 1984; Wu, 1994; Wu & McLean, 1993, November, 1994b).

The recommendation to consider the correlation in choosing blocking or ANCOVA is rejected by Maxwell and Delaney (1984); "instead, the two factors that should be considered are whether scores on the concomitant variable are available for all subjects prior to assigning any subjects to treatment conditions and whether the relationship of the dependent and concomitant variable is linear" (p. 136). Since Maxwell and Delaney suggested that power might provide a different perspective from Apparent Imprecision, the number of blocks used by them, which was based on Apparent Imprecision and recommendations by Keppel (1973) and Winer (1971), may not result in the optimal number of blocks to achieve maximum power. This potential limitation is magnified by the restricted experiment conditions used by them.

Wu and McLean (1994b) examined the blocking versus ANCOVA issue and estimated the optimal number of blocks to achieve maximum power using broader and more representative experimental conditions. They recommended that, when deciding among a completely randomized ANOVA, a block design, or ANCOVA, researchers should consider the assumptions of the procedures and weigh the magnitude of the power increase against the added cost of blocking or covarying. The Wu and McLean study described how Apparent Imprecision is similar to and different from statistical power, and why the correlation between the concomitant and dependent variables is considered the critical factor in choosing blocking or ANCOVA based on Apparent Imprecision. They also pointed out the potential problems with Apparent Imprecision and recommended power be used in preference to Apparent Imprecision. A comparison of power and Apparent Imprecision and a critique of Apparent Imprecision are provided in Appendix A. Generally, the Wu and McLean study supported the recommendation by Maxwell and Delaney (1984) to use ANCOVA in preference to blocking if the assumptions for ANCOVA can be met. Nevertheless, results of the Wu and McLean study concluded that ANCOVA is not always more powerful than blocking, as suggested by Maxwell and Delaney. The Wu and McLean study was limited to using only the post-hoc approach.

The Maxwell and Delaney study (1984) explored another dimension to the blocking versus ANCOVA issue by using the concomitant variable to assign subjects to treatments. If the concomitant variable is not considered when subjects are assigned to treatments, the experiment uses a post-hoc approach (Bonett, 1982; Keppel, 1973; Myers, 1979); otherwise an a priori approach is used. Maxwell and Delaney (1984) found that ANCOVA tends to be more powerful than blocking if the same approach is selected, and a priori tends to be more powerful than post-hoc if the same procedure is selected. The purpose of the present study is to compare the statistical powers of a priori and post-hoc approaches among ANOVA, block designs, and ANCOVA using broad, representative experimental conditions and varying the numbers of blocks based on statistical power.

Procedures

Experimental Conditions

This Monte Carlo study compares the statistical powers among ANOVA, block designs, and ANCOVA under 48 experimental conditions with both post-hoc and a priori approaches. The 48 experimental conditions are combinations of four levels of the number of treatments (T; 2, 3, 4, 5), three levels of the number of subjects per treatment (n; 8, 40, 72), and four levels of the correlation coefficient between the concomitant and dependent variables (ρ; .00, .28, .56, .84). The levels of experimental conditions were selected to achieve equal intervals and to be representative of real world situations. The four levels of the number of treatments represent the most commonly used numbers of treatments; the three levels of the number of subjects per treatment represent small, medium, and large sample sizes; and the four levels of the correlation coefficient represent zero, low, moderate, and high correlation.

Method of Assignment

For the a priori approach, the required total number of subjects were randomly selected and ranked by the concomitant variables. The highest ranked k subjects formed the first block; the second highest ranked k subjects formed the second block; and so on until the lowest ranked k subjects formed the nth block, where k is the number of treatments and n is the number of subjects per treatment. The subjects in each block then were randomly assigned to treatments. This method was chosen because of its simplicity and its ability to form the most homogeneous blocks. With this method of assignment, the concomitant variable should be classified as continuous according to Maxwell and Delaney (1984). For the post-hoc approach, subjects were randomly assigned to treatments without considering the concomitant variable.

Method of Analysis

For ANOVA, the concomitant variable was not considered in the analysis. For block designs, subjects in each treatment were blocked by their ranks on the concomitant variable. The block analyses included all possible numbers of blocks to achieve equal numbers of subjects in each block. For example, with 72 subjects per treatment, analyses were conducted with 2, 3, 4, 6, 8, 9, 12, 18, 24, 36, and 72 blocks. For ANCOVA, the concomitant variable was treated as the covariate in the analysis.

Post-hoc ANOVA (Completely Randomized ANOVA) as the Control Group

The experiment controlled the power of the post-hoc ANOVA (completely randomized ANOVA) at .50 using the effect sizes reported by Wu (1994). This allows the power of the other procedures to increase or decrease as a function of experimental conditions and to make comparisons of the analysis procedures more meaningful with the post-hoc ANOVA serving as a control group.

Computer Simulation System

The computer simulation system initialized by Wu and McLean (1993, November) and used by Wu (1994) and Wu and McLean (1994b) was modified for this study. This computer simulation system has been demonstrated to be capable of generating data that meet predetermined specifications and carrying out accurate simulations; also, the computer programs can be modified easily for many other studies (Wu & McLean, 1994a). For this study, paired data were generated from two linearly correlated normal populations. Data generated in this nature will meet the assumptions of ANOVA and ANCOVA, but will not completely satisfy the assumptions of block designs. Robustness of block designs under the circumstances has been illustrated analytically by Feldt (1958) and empirically by Maxwell and Delaney (1984).

The computer programs used by Wu (1994) and Wu and McLean (1994b) were used for the post-hoc approach in this study. The specific computer codes and a detailed description of the simulation procedures can be found in Wu. The same seed numbers used by the two studies were used in this study. The results demonstrated Wu's statement that experiments are replicable using the same seeds. The computer programs for the a priori approach are slightly different from the post-hoc approach and are listed in Appendix B. The same seeds used for the post-hoc approach were used for the a priori approach in order to compare the two approaches based on analyzing the same sets of data.

Results

The resulting power values under each experimental condition are listed in Appendix C. Each power is based on 3,000 analyses with α preset at .05.

Optimal Number of Blocks

Results show that the optimal number of blocks to achieve maximum power increases as the correlation, the number of treatments, and the number of subjects per treatment increase. The optimal number of blocks for the a priori approach is essentially the same as that for the post-hoc approach. As adjacent numbers of blocks often yield very close power values, the optimal number of blocks has no clear-cut boundary. In fact, the power values are almost the same as the number of blocks approaches its optimal number. This is important for researchers because the optimal number of blocks may not be as crucial as it has been regarded, although theoretically there exists an optimal number of blocks. The results of this study support the recommendation by Wu and McLean (1994b) that researchers may select from a wide range of numbers of blocks if they avoid using small numbers of blocks when the correlation, the number of treatments, and the number of subjects per treatment are large, and vice versa. The optimal numbers of blocks are listed in Table 1. The purpose of this table is to show the trend that the optimal number of blocks increases as the correlation, the number of treatments, and the number of subjects per treatment increase, rather than provide strict numbers of blocks for researchers to follow.

Table 1

The optimal number of blocks to achieve statistical power

Correlation Coefficient*	Number of Treatments	Number of Subjects per Treatment		
		8	40	72
.28	2	2	10	18
	3	4	10	24
	4	4	20	24
	5	4	20	24
.56	2	4	10	24
	3	4	20	36
	4	8	20	36
	5	8	20	36
.84	2	4	20	36
	3	8	20	36
	4	8	40	72
	5	8	40	72

*Between concomitant and dependent variables.

Post-hoc ANOVA (Completely Randomized ANOVA) as the Control Group

When the correlation coefficient is zero, both a priori and post-hoc ANOVAs are as powerful as or more powerful than blocking and ANCOVA. They are more powerful when the number of subjects per treatment (n) and the number of treatments (T), especially n, are small. This is plausible because blocking and ANCOVA achieve no advantage over a completely randomized ANOVA when the correlation is zero and using blocking or ANCOVA loses degrees of freedom for error. Thus, for block designs, power is diminished when a larger number of blocks is used. The loss of degrees of freedom has little impact when the sample size is large, but causes significantly more power loss as the sample size becomes small.

The power of post-hoc ANOVA (completely randomized ANOVA) is controlled at .50 under all experimental conditions. The power of a priori ANOVA is also controlled at .50 when the correlation is zero. This is also plausible because a priori ANOVA is no different from the completely randomized ANOVA when the correlation is zero. But the power of a priori ANOVA drops as ρ and T increase. For example, a priori ANOVA loses .27 power with T=5, n=72, and ρ=.84. The magnitudes of the power loss under each experimental condition are listed in Table 2. Loss of less than .02 are omitted for clarity.

Table 2

Power difference between a priori and post-hoc ANOVA

Number of Subjects per Treatment	Correlation Coefficient*	Number of Treatments			
		2	3	4	5
8	.28	†	†	†	-.03
	.56	†	-.04	-.07	-.09
	.84	†	-.11	-.21	-.24
40	.28	†	†	†	-.03
	.56	†	-.03	-.06	-.10
	.84	†	-.12	-.21	-.26
72	.28	†	†	†	-.02
	.56	†	-.04	-.07	-.08
	.84	†	-.11	-.21	-.27

* Between concomitant and dependent variables.
† Denotes difference is less than .02.

Power of Block Designs and ANCOVA

The completely randomized ANOVA is the best choice and there is no need to block or covary when the correlation is zero. When the correlation is not zero, blocking and ANCOVA become more powerful than the completely randomized ANOVA as ρ, n, and T increase. Neither one procedure nor one approach is uniquely most powerful. ANCOVA is not generally more powerful than blocking and the a priori approach is not generally more powerful than the post-hoc approach. The relative merits of the procedures are complicated and the choice of the optimal procedure varies depending on the experimental conditions. The power increases for both blocking and ANCOVA are listed in Table 3 and 4. Note that, under each experimental condition, all procedures analyze the same sets of data with the post-hoc ANOVA (completely randomized ANOVA) serving as the control group. The effect size of treatments is set at a specific value under each experimental condition to control the power of the completely randomized ANOVA at .50. Thus, the increase is calculated by subtracting the power of the completely randomized ANOVA from the powers of the optimal blocking procedure and ANCOVA under each experimental condition. When the correlation is low (ρ =. 28), the increases do not exceed .05 for either approach. When the correlation is moderate (ρ =. 56), the increases range from .10 to .21. When the correlation is high (ρ = .84), the increases are as high as .24 to .49.

Table 3

Power increase using optimal blocking and ANCOVA for the a priori approach

n*	ρ**	Number of Treatments							
		2		3		4		5	
		Optimal Block	ANCOVA	Optimal Block	ANCOVA	Optimal Block	ANCOVA	Optimal Block	ANCOVA
8	.28	†	.02	†	.02	†	.04	†	.02
	.56	.13	.17	.12	.16	.14	.17	.15	.17
	.84	.37	.46	.43	.48	.43	.46	.45	.48
40	.28	.04	.04	.03	.03	.04	.04	.03	.03
	.56	.16	.17	.18	.18	.17	.17	.17	.17
	.84	.45	.46	.46	.47	.46	.46	.48	.48
72	.28	.04	.03	.05	.05	.04	.04	.03	.03
	.56	.16	.16	.16	.16	.17	.16	.18	.18
	.84	.44	.44	.46	.46	.46	.47	.48	.48

* Denotes number of subjects per treatment.
**Denotes correlation coefficient between concomitant and dependent variables.
† Denotes difference is less than .02.

Table 4

Power increase using optimal blocking and ANCOVA for the post-hoc approach

n*	ρ**	2: Optimal Block	2: ANCOVA	3: Optimal Block	3: ANCOVA	4: Optimal Block	4: ANCOVA	5: Optimal Block	5: ANCOVA
8	.28	†	†	.02	†	.03	†	.04	†
	.56	.10	.13	.12	.14	.16	.17	.17	.18
	.84	.24	.43	.32	.46	.36	.45	.40	.48
40	.28	.03	.03	.03	.03	.05	.04	.05	.03
	.56	.13	.16	.16	.17	.19	.17	.21	.18
	.84	.31	.46	.38	.47	.41	.46	.45	.48
72	.28	.03	.03	.04	.04	.05	.03	.05	.03
	.56	.14	.16	.16	.15	.20	.19	.21	.17
	.84	.30	.44	.37	.46	.42	.46	.45	.49

* Denotes number of subjects per treatment.
** Denotes correlation coefficient between concomitant and dependent variables.
† Denotes difference is less than .02.

Table 5

Power differences between optimal blocking and ANCOVA for the a priori approach

n*	ρ**	Number of Treatments: 2	3	4	5
8	.28	†	†	†	†
	.56	.04	.04	.04	.02
	.84	.09	.04	.03	.02
40	.28	†	†	†	†
	.56	†	†	†	†
	.84	†	†	†	†
72	.28	†	†	†	†
	.56	†	†	†	†
	.84	†	†	†	†

* Denotes number of subjects per treatment.
** Denotes correlation coefficient between concomitant and dependent variables.
† Denotes difference is less than .02.

Comparing Blocking and ANCOVA

The power differences between optimal blocking and ANCOVA for both approaches are listed in Tables 5 and 6.

Table 6

Power differences between optimal blocking and ANCOVA for the post-hoc approach

n*	ρ**	Number of Treatments			
		2	3	4	5
8	.28	†	†	†	-.02
	.56	.03	†	†	†
	.84	.19	.14	.10	.08
40	.28	†	†	†	-.02
	.56	.03	†	†	-.03
	.84	.14	.09	.05	.03
72	.28	†	†	†	-.02
	.56	†	†	†	-.04
	.84	.15	.09	.04	.04

* Denotes number of subjects per treatment.
** Denotes correlation coefficient between concomitant and dependent variables.
† Denotes difference is less than .02.

For the a priori approach, ANCOVA is more powerful than the optimal blocking procedure when the number of subjects per treatment is small and the correlation is moderate or high. For the post-hoc approach, ANCOVA is more powerful when the correlation is high while the optimal blocking procedure is slightly more powerful when the correlation is low or moderate and the number of treatments are large. These findings are different from those based on Apparent Imprecision that suggest ANCOVA is consistently better than blocking as the correlation increases (Feldt, 1958). Rather, the results support Maxwell and Delaney's (1984) statement that "the recommendation of most experimental design texts to consider the correlation between the dependent and concomitant variables in choosing the best technique for utilizing a concomitant variable is incorrect" (p. 136).

Comparing A Priori and Post-hoc Approaches

Overall, the a priori approach yields little advantage over the post-hoc approach. The power means of the a priori and post-hoc approaches of all the analysis procedures over all the experimental conditions except those with zero correlation are listed in Table 7. Tables 8 and 9 show the power differences between a priori and post-hoc approaches for optimal blocking and ANCOVA.

Table 7

Mean powers of analysis procedures for a priori and post-hoc approaches

Design	Approach	
	a priori	post hoc
ANOVA	.432	.500
2 blocks	.630	.629
3 blocks	.679	.665
4 blocks	.689	.671
5 blocks	.705	.684
6 blocks	.708	.690
8 blocks	.703	.680
9 blocks	.715	.696
10 blocks	.717	.695
12 blocks	.717	.698
18 blocks	.719	.700
20 blocks	.721	.699
24 blocks	.720	.701
36 blocks	.721	.702
40 blocks	.719	.696
72 blocks	.720	.700
ANCOVA	.723	.717

Table 8

Power differences between a priori and post-hoc approaches for optimal blocking

Number of Subjects per Treatment	Correlation Coefficient*	Number of Treatments			
		2	3	4	5
8	.28	†	†	†	-.03
	.56	.03	†	-.03	-.02
	.84	.13	.12	.07	.05
40	.28	†	†	†	-.02
	.56	.04	†	-.02	-.04
	.84	.14	.08	.05	.03
72	.28	†	†	†	-.02
	.56	.02	†	-.04	-.03
	.84	.14	.09	.05	.04

* Between concomitant and dependent variables.
† Denotes difference is less than .02.

Table 9

Power differences between a priori and post-hoc approaches for ANCOVA

Number of Subjects per Treatment	Correlation Coefficient*	Number of Treatments			
		2	3	4	5
8	.28	.03	†	†	†
	.56	.04	.02	†	†
	.84	.02	†	†	†
40	.28	†	†	†	†
	.56	†	†	†	†
	.84	†	†	†	†
72	.28	†	†	†	†
	.56	†	†	-.03	†
	.84	†	†	†	†

* Between concomitant and dependent variables.
† Denotes difference is less than .02.

A priori blocking is more powerful than post-hoc blocking when the correlation is high. Post-hoc blocking is slightly more powerful than a priori blocking when the correlation is low or moderate and the number of treatments is large. A priori ANCOVA is more powerful than post-hoc ANCOVA when the number of subjects per treatment and the number of treatments are small. These results do not support Maxwell and

Delaney's (1984) conclusion that the a priori approach is generally more powerful than the post-hoc approach. However, this study does support their findings for similar experimental conditions. But, Maxwell and Delaney used much narrower experimental conditions. Specifically, the power values in the upper-left three cells (T=2, n=8, and ρ=.28, 56, and 84) of Tables 8 and 9 actually support the results reported by Maxwell and Delaney. The pattern of the magnitudes of power differences is analogous to that of the Maxwell and Delaney study, where the magnitudes of differences are generally small except the one between a priori and post-hoc blocking when the correlation is high and the number of subjects per treatment and the number of treatments are small.

Discussion and Recommendations

No one procedure or single approach is uniquely more powerful. Although the most powerful technique to employ a concomitant variable varies depending on the experimental conditions, most of the magnitudes of the power differences are not large enough to be practically significant. It is recommended that researchers utilize the tables provided in this study to help select the best technique when employing a concomitant variable.

The problems concerning utilizing a concomitant variable become complicated when considering a variety of experimental conditions, methods of assignment, and assumptions of the analysis procedures. Despite these complications, the results of this study show that choices in research practice may have little impact because many of the power differences are small. Based on practical significance, this study suggests the simplest rule to follow is <u>use regular ANCOVA (post-hoc ANCOVA) if its assumptions can be met</u>. The rationale for preference of blocking over ANCOVA reported by earlier experimental design texts, such as ease of calculation and ability to test simple effects, seems to have faded away recently. With modern computer statistical packages, ANCOVA becomes at least as easy to compute as block designs. Using regular ANCOVA, researchers need not consider questions such as whether the concomitant variable is available before the experiment, how to assign subjects, what is the magnitude of the correlation, and how many blocks should be used, and still gain power though not necessarily achieve maximum power. The power differences between post-hoc ANCOVA and the optimal procedure are of little practical significance under most experimental conditions, and do not exceed .04 even in the most extreme cases.

When the correlation is zero, the waste of degrees of freedom due to blocking and ANCOVA may result in the reduction of power. Wasting degrees of freedom has little influence on power when T and n are large but has an effect when T and n are small. For example, the power drops from .50 to .43 when using post-hoc 8 block analysis procedure for $\rho=.00$, $T=2$ and $n=8$. Because this study uses a minimum n of 8, the loss of one degree of freedom using ANCOVA, especially a priori ANCOVA, when the correlation is zero, seems to have little effect on the loss of power. However, one should be cautious that the loss will increase as n becomes smaller. Much of the criticism of the post-hoc approach is based on the ease with which researchers can block or covary in a post-hoc manner. Myers (1979) pointed out the danger of abusing post-hoc block designs by demonstrating that reordering scores within each treatment does not change the treatment means but generally reduces the error variance, resulting in significant Fs which "merely reflect the reduction in error variance due to blocking rather than any variability due to treatments" (p. 155). However, Myers did not consider the loss of degrees of freedom with block designs. Wasting degrees of freedom on some nonsense concomitant variable would simply decrease power. Nevertheless, the caution urged by Myers should be considered. It is often too easy to peek at the data, play with several concomitant variables, or try several analysis procedures to achieve significant results. However, these should be considered ethical problems rather than problems of the post-hoc approach per se. Researchers should neither block nor covary unless they can justify the concomitant variable before the analysis. If researchers would always consider practical as well as statistical significance, these problems could be avoided as none of these analysis techniques affect effect sizes.

One of the most interesting findings of this study is the problem with a priori ANOVA. Maxwell and Delaney (1984) questioned this method and detected minor Type I error rate problems with it. But the Type II error rate problem with a priori ANOVA was not detected in their study. This is because their study was limited to two treatments. Future research could investigate the Type I error rate using broader experimental conditions. A follow-up Monte Carlo study comparing a priori and post-hoc ANOVA by examining the sample distributions of the variances showed that the power loss of a prior ANOVA was due to a decrease of treatment variance and an increase of error variance while stratified instead of randomized assignment is used. The Type II error rate problem with a priori ANOVA may provide the best example of how power can be different from

Apparent Imprecision; the precision of a priori ANOVA is higher than the completely randomized ANOVA but results in lower power. This may also explain why the a priori approach is not generally more powerful than the post-hoc approach. A priori does achieve more homogeneous blocks and ensures more equal covariates across treatments. But, the advantages are reduced by the loss of power due to stratified rather than random assignment. Stratified assignment is still a common practice. It is believed to guarantee fairness of treatments and avoid preexisting differences. Some suggest that stratified assignment reduces error and increases power. Based on the results of this study, if stratified assignment is used, the concomitant variable should not be ignored in the analysis. Since stratified assignment loses power due to non-random sampling but gains power because of providing more homogeneous blocks for analysis, future research could investigate whether there is an optimal combination of the number of blocks used in assignment and the number of blocks used in analysis.

Wu and McLean (1994b) suggested four reasons that Maxwell and Delaney's (1984) results differed from theirs: (1) limited experimental conditions, (2) not including the optimal number of blocks, (3) including the interaction in the effects model, and (4) inaccuracy of the computer simulation. The results of this study show that not including the optimal number of blocks causes only minor power loss, and the inaccuracy of this computer simulation and that of the Maxwell and Delaney study is unlikely because results of the two studies support each other for similar experimental conditions. Among the four factors, the restriction of experimental conditions and including the interaction in the effects model should contribute the most to the different findings. Conclusions based on restricted conditions may limit generalization. As to the interaction factor, the two studies complement each other's findings as some researchers suggest pooling the interaction variance with the error variance when the interaction is non-significant while other researchers do not. Note that Maxwell and Delaney did not specify that the interaction was included in the effects model. Our conclusion is based on the statement "perform a two-way analysis of variance (ANOVA) utilizing levels of the concomitant variable as a factor in the design" (p. 138).

A randomized complete block design is defined as a block design in which each block within each treatment has only one observation. Lindquist (1953) used the term, treatments-by-levels design, which consists of more than one subject in a cell, to differentiate it from the randomized complete block design. The treatments-by-levels design is also called the treatments-by-blocks design (Kennedy & Bush 1985). While the randomized complete block design usually uses an additive model because there is only one observation per cell, the treatments-by-blocks design can either use an additive or nonadditive model by excluding or including the interaction term in the effects model. The additive model is used in this study because the interaction does not exist in the population. Which model to be used should be based on researchers' subject matter knowledge and should be justified before the experiment. For example, suppose the concomitant variable is an IQ score, the dependent variable is a Scholastic Achievement Test (SAT) score, and the treatments are some teaching methods. If researchers can justify that the correlation between the IQ score and the SAT score should not be influenced by the teaching methods, the additive model should be used. When an additive model is used, the concomitant variable is treated as a nuisance variable and the variance accounted for by the concomitant variable is nuisance variance, which is out of the researcher's interest and is to be extracted to reduce error and increase power. But, if the dependent variable is a computer attitude measure and the researcher cannot justify that high IQ students usually have better attitudes toward computers, disregarding the teaching methods used, the nonadditive model should be used. Under these circumstances, the concomitant variable is no longer a nuisance variable; rather, it is a factor of interest because the researcher would want to test if a teaching method is better for low IQ than high IQ students. In this case, the block design in form, in analysis, and in interpretation is undistinguishable from a factorial design. The difference is that in a factorial design subjects are assigned to each combination (cell) of factors, while in block designs subjects in each block level are assigned to treatments and the block factor is usually intrinsic in the subjects themselves.

The question of should nonadditive block designs be categorized as block or factorial designs adds some difficulty to the nomenclature of experimental designs. The alternative uses of terms such as blocking, factorial, and stratification by researchers certainly add some confusion. Which model to use and whether to adhere to the original model or revise it during the analysis process should be justified in advance. In many instances, the researcher includes the interaction for convenience. If the interaction is non-significant, some researchers pool the interaction variance with the error variance in order to increase power. The issue of pooling and non-pooling is still disputed. If the interaction does not exist in the population, pooling the interaction variance with the error variance should provide a better estimate of the error term and increase power due to an increased number of degrees of freedom for the error term.

Using nonadditive model suggests another question: Should the block levels formed by ranking be treated as random or fixed? If random, the interaction should be used as the error term to test the treatment effect (see Kirk, 1982, p. 240-241); if fixed, the within cell variance should be used as the error term (see Feldt, 1958; Maxwell & Delaney, 1984). Levels based on the rank of sample subjects seem to be more fixed, though not completely fixed, than random because ranking is deterministic instead of random. Calculating expected mean squares seems to be the most appropriate way to obtain the error terms, which is beyond the scope of this discussion. However, future Monte Carlo studies could block the population to obtain completely fixed block levels. Boundaries for block levels can be set based on two principles: equal proportion or equal interval (Feldt, 1958). In research practice, blocking a population is not feasible in most cases. An alternative method is to randomly select subjects, then fit them into corresponding levels. This would most likely result in unequal numbers of subjects in the blocks. To obtain an equal number of subjects in each block, researchers could keep on randomly selecting subjects until the required number of subjects fit in each block level and discard those exceeding the required number of subjects.

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Appendix A

Examining Apparent Imprecision may provide an insight of how it can be similar to or different from power. Apparent Imprecision is defined as the product of True Imprecision and an adjustment factor based on the degrees of freedom for error:

$$I_a = \frac{ave\ var}{\min\ var} \times \frac{df_e+3}{df_e+1},$$

where, ave var is the average variance of the treatment mean difference from sample to sample; and min var is the theoretical minimum variance of the treatment mean difference. According to Feldt (1958), the minimum variance is the variance of the dependent variable at a fixed value of the covariate given the assumption of homoscedasticity. For block designs, Apparent Imprecision (AI_{BD}) is computed with the following formula:

$$AI_{BD} = \frac{\frac{2\sigma_y^2}{n}[1-\rho^2(1-\frac{\overline{\sigma_x^2}}{\sigma_X})]}{\frac{2\sigma_y^2}{n}(1-\rho^2)} \times \frac{f_{e_y}+3}{f_{e_y}+1} = \frac{[1-\rho^2(1-\frac{\overline{\sigma_x^2}}{\sigma_X})]}{(1-\rho^2)} \times \frac{f_{e_y}+3}{f_{e_y}+1},$$

where Y represents the dependent variable; X, the concomitant variable; ρ, the correlation coefficient between X and Y; n, the number of subjects per treatment; $\overline{\sigma}_x^2$, the average variance of X over all blocks; and f_{e_y}, the degrees of freedom for error in Y. For ANCOVA, Apparent Imprecision is computed using the following formula:

$$AI_{ANCOVA} = \frac{\frac{2\sigma_y^2}{n}(1-\rho^2)(1+\frac{1}{f_{e_x}-2})}{\frac{2\sigma_y^2}{n}(1-\rho^2)} \times \frac{f_{e_y}+3}{f_{e_y}+1} = \frac{f_{e_x}-1}{f_{e_x}-2} \times \frac{f_{e_y}+3}{f_{e_y}+1},$$

where f_{e_x} stands for the degrees of freedom for error in X.

For block designs, the average variance of the covariate over all blocks decreases and the True Imprecision approaches 1.00 as the number of blocks increases. Theoretically, if an infinite number of blocks could be used, the values of the concomitant variable would be the same in each block, and True Imprecision would be exactly 1.00. This does not mean that employing a larger number of blocks always decreases Apparent Imprecision, because as larger numbers of blocks are used, larger degrees of freedom for error are lost, and, based on the adjustment factor, Apparent Imprecision increases. Therefore, there is an optimal number of blocks that minimizes True Imprecision, yet, on the other hand, minimizes the increase of Apparent Imprecision due to the loss of degrees of freedom for error. This phenomenon of Apparent Imprecision is analogous to that of power. For the same reason, there is an optimal number of blocks such that the higher the degree of the correlation between the concomitant and dependent variables, the more homogeneous the values of the dependent variable are in each block, the more variance that is extracted from the error term, the higher the power. At the same time, the power is decreased due to the loss of degrees of freedom for error. To optimize power, one must find a balance between these two forces. Apparent Imprecision is similar to power in this regard.

Nevertheless, Apparent Imprecision could suggest different results from power. For block designs, the average variance and the minimum variance decrease as the correlation increases. But, the average variance would never decrease as much as the minimum variance unless an infinite number of blocks were used. Therefore, the Apparent Imprecision of a block design usually increases as the correlation increases. For ANCOVA, the correlation terms in the numerator and denominator are canceled out because the average variance is a function of the minimum variance. Notice that the minimum variance is the ideal variance based on the covariance model. Therefore, the Apparent Imprecision of ANCOVA is the same for all values of the correlation. This is basically why block designs are found to consistently become less precise than ANCOVA as the correlation increases and why the correlation has been regarded as the critical factor in choosing block designs or ANCOVA. The correlation being negative in a block design and irrelevant in ANCOVA based on Apparent Imprecision is different from what most texts and this study have found about the positive effect of higher correlation in reducing error and increasing power.

A simple rule to follow when evaluating a criterion variable is to determine if it provides a direct measure of the variable of interest. For example, if a new brand of bulbs is to be evaluated, it is better to check how long the bulbs last rather than to analyze the precision of the components of the bulbs. The precision of the components of the bulbs would be a good criterion if it could determine a useful property; for example, the more precise the element is, the longer the bulbs last, the less power the bulbs consume, or the less eye strain the bulbs cause. A theoretical framework merits less if the degree it can be related to the physical property of interest is low. For example, the theory of the imagined number $\sqrt{-1}$ would not have been valuable if it could not be used to predict the behavior of electronic circuits. Based on the rule, the Type I error rate and statistical power should be considered a good criterion in evaluating a research design.

Maxwell and Delaney (1984) also illustrated how Apparent Imprecision might be different from power:

> Suppose an experimenter plans to conduct a two-group comparison of means using an alpha level of 0.05. If the population difference in means is .5 standard deviation units, and 150 subjects are randomly and independently assigned to groups, the power for an independent groups t test is 0.99. Suppose that a concomitant variable were available that correlated .6 with the dependent variable. The power of an ANCOVA would still be 0.99 (or, actually, 1.00 if rounded to two decimal places). From the standpoint of power, the ANCOVA offers no gain over the t test. On the other hand, it can be shown that the apparent imprecision of the t test here is 1.573, whereas for ANCOVA the apparent imprecision is 1.010, demonstrating that the estimated magnitude of the treatment effect is much more precise when ANCOVA is used rather than the t test. (p. 137)

One might interpret, facially, the above demonstration as a benefit of using Apparent Imprecision as the criterion variable. However, it should be noticed that the powers of the t test and the ANCOVA have both reached the ceiling point because of the large sample size that was used. Based on the rule, this illustration, indeed, offers an example of power as a favorable criterion. If one can achieve a .99 power with a t test, what is the advantage of spending money on collecting concomitant data? For example, administering a pretest or IQ test, to gain an impractical .001 power. Eventually, almost all analysis will become statistically significant if a large enough size is used. What is the use of a new teaching method that claims to increase students' SAT scores by 1 point? Practical significance would also need to be considered when evaluating the results of an analysis.

Appendix B

Executable File

```
/*    */
ADDRESS COMMAND
"ERASE PVALUE DATA A"
NUMERIC DIGITS 10
TIME = 1
DO WHILE TIME < 1001
SEED = 2132560 + (TIME -1) * 2147483
"EXECIO 1 DISKW" NEWSEED DATA A "(STRING" SEED
"EXEC SAS T57284"
"ERASE NEWSEED DATA A"
TIME=TIME+1
END
"EXEC SAS T57284P"
```

First SAS Program (T57284 SAS A)

```
CMS FILEDEF INDATA DISK NEWSEED DATA A;
CMS FILEDEF PVALUE DISK PVALUE DATA A (LRECL 306
BLKSIZE 306 RECFM FBS;
CMS FILEDEF SASLIST DISK T57284 LISTING A;
DATA BIVNORM;
   INFILE INDATA;
      INPUT SEED;
   DO I=1 TO 360;
      X=RANNOR(SEED);
      Y=.84*X+SQRT(1-.84**2)*RANNOR(SEED);
      OUTPUT;
   END;
PROC SORT;
   BY X;
DATA BIVNORM;
   SET BIVNORM;
   B72=CEIL(_N_/5);
   B2=CEIL(B72/36);B3=CEIL(B72/24);
   B4=CEIL(B72/18);B6=CEIL(B72/12);
   B8=CEIL(B72/9);B9=CEIL(B72/8);
   B12=CEIL(B72/6);B18=CEIL(B72/4);
   B24=CEIL(B72/3);B36=CEIL(B72/2);
```

```
PROC SORT;
   BY B72 I;
DATA BIVNORM (DROP=SEED I);
   SET BIVNORM;
   GROUP=MOD(_N_,5); IF GROUP=0 THEN GROUP=5;
   IF GROUP=2 THEN Y=0.0951+Y;
   IF GROUP=3 THEN Y=0.1903+Y;
   IF GROUP=4 THEN Y=0.2854+Y;
   IF GROUP=5 THEN Y=0.3805+Y;
PROC PRINT;
PROC SORT;
   BY GROUP;
PROC CORR DATA=BIVNORM;
   VAR X Y;
   BY GROUP;
PROC ANOVA;
   CLASS GROUP;
   MODEL Y=GROUP;
PROC ANOVA;
   CLASS GROUP B2;
   MODEL Y=GROUP B2;
PROC ANOVA;
   CLASS GROUP B3;
   MODEL Y=GROUP B3;
PROC ANOVA;
   CLASS GROUP B4;
   MODEL Y=GROUP B4;
PROC ANOVA;
   CLASS GROUP B6;
   MODEL Y=GROUP B6;
PROC ANOVA;
   CLASS GROUP B8;
   MODEL Y=GROUP B8;
PROC ANOVA;
   CLASS GROUP B9;
   MODEL Y=GROUP B9;
PROC ANOVA;
   CLASS GROUP B12;
   MODEL Y=GROUP B12;
PROC ANOVA;
   CLASS GROUP B18;
   MODEL Y=GROUP B18;
PROC ANOVA;
   CLASS GROUP B24;
   MODEL Y=GROUP B24;
PROC ANOVA;
   CLASS GROUP B36;
   MODEL Y=GROUP B36;
PROC ANOVA;
   CLASS GROUP B72;
   MODEL Y=GROUP B72;
PROC GLM;
   CLASS GROUP;
   MODEL Y=GROUP X/SS3;
DATA;
   INFILE SASLIST;
   INPUT WORD1 $ WORD2 $ @;
   FILE PVALUE MOD;
   IF WORD1 = 'X' AND WORD2 ='72' THEN DO;
      INPUT MEAN STDDEV;
      PUT MEAN 6.4 STDDEV 6.4 @;
      INPUT Y $ N MEAN STDDEV;
      PUT MEAN 6.4 STDDEV 6.4 @;
      END;
   ELSE  IF WORD1="X" AND WORD2 = '1.00000' THEN
   DO;
      INPUT CORR;
      PUT CORR 6.4 @;
      END;
   ELSE  IF WORD1="GROUP" AND WORD2 = '4' THEN DO;
      INPUT SS MS F PR;
      PUT PR 6.4 @;
      INPUT BLOCK $ DF SS MS F PR;
      PUT PR 6.4 @;
      END; */
```

<u>Second SAS Program (T57284P SAS A)</u>

```
CMS FILEDEF INDATA DISK PVALUE DATA A;
DATA PVALUE;
INFILE INDATA;
INPUT (G1XMEAN G1XSD G1YMEAN G1YSD G1CORR
       G2XMEAN G2XSD G2YMEAN G2YSD G2CORR
       G3XMEAN G3XSD G3YMEAN G3YSD G3CORR
       G4XMEAN G4XSD G4YMEAN G4YSD G4CORR
       G5XMEAN G5XSD G5YMEAN G5YSD G5CORR
       GROUP1B BLOCK1B GROUP2B BLOCK2B GROUP3B
       BLOCK3B GROUP4B BLOCK4B GROUP6B BLOCK6B
       GROUP8B BLOCK8B GROUP9B BLOCK9B GROUP12B
       BLOCK12B GROUP18 GROUP18B BLOCK18B GROUP24B
       BLOCK24B GROUP36B BLOCK36B GROUP72B
       BLOCK72B GROUPANC BLOCKANC) (51* 6.4);
     G1BSG=0;
     B1BSG=0;
     G2BSG=0;
     B2BSG=0;
     G3BSG=0;
     B3BSG=0;
     G4BSG=0;
     B4BSG=0;
     G6BSG=0;
     B6BSG=0;
     G8BSG=0;
     B8BSG=0;
     G9BSG=0;
     B9BSG=0;
     G12BSG=0;
     B12BSG=0;
     G18BSG=0;
     B18BSG=0;
     G24BSG=0;
     B24BSG=0;
     G36BSG=0;
     B36BSG=0;
     G72BSG=0;
     B72BSG=0;
     GANCSG=0;
     BANCSG=0;
     TOTAL=1;
     IF GROUP1B <= 0.05 THEN G1BSG=1;
     IF BLOCK1B <= 0.05 THEN B1BSG=1;
     IF GROUP2B <= 0.05 THEN G2BSG=1;
     IF BLOCK2B <= 0.05 THEN B2BSG=1;
     IF GROUP3B <= 0.05 THEN G3BSG=1;
     IF BLOCK3B <= 0.05 THEN B3BSG=1;
     IF GROUP4B <= 0.05 THEN G4BSG=1;
     IF BLOCK4B <= 0.05 THEN B4BSG=1;
     IF GROUP6B <= 0.05 THEN G6BSG=1;
     IF BLOCK6B <= 0.05 THEN B6BSG=1;
     IF GROUP8B <= 0.05 THEN G8BSG=1;
     IF BLOCK8B <= 0.05 THEN B8BSG=1;
     IF GROUP9B <= 0.05 THEN G9BSG=1;
     IF BLOCK9B <= 0.05 THEN B9BSG=1;
     IF GROUP12B <= 0.05 THEN G12BSG=1;
     IF BLOCK12B <= 0.05 THEN B12BSG=1;
     IF GROUP18B <= 0.05 THEN G18BSG=1;
     IF BLOCK18B <= 0.05 THEN B18BSG=1;
     IF GROUP24B <= 0.05 THEN G24BSG=1;
     IF BLOCK24B <= 0.05 THEN B24BSG=1;
```

```
     IF GROUP36B <= 0.05 THEN G36BSG=1;
     IF BLOCK36B <= 0.05 THEN B36BSG=1;
     IF GROUP72B <= 0.05 THEN G72BSG=1;
     IF BLOCK72B <= 0.05 THEN B72BSG=1;
     IF GROUPANC <= 0.05 THEN GANCSG=1;
     IF BLOCKANC <= 0.05 THEN BANCSG=1;
PROC FREQ;
     TABLE G1BSG -- BANCSG;
PROC SUMMARY DATA=PVALUE;
     VAR G1XMEAN -- BANCSG;
     OUTPUT OUT = DESCRIPT;
PROC PRINT DATA=DESCRIPT;
PROC UNIVARIATE DATA=PVALUE PLOT NORMAL;
     VAR G1XMEAN -- BLOCKANC;
```

Appendix C

Power Table

				ANO	B02	B03	B04	B05	B06	B08	B09	B10	B12	B18	B20	B24	B36	B40	B72	COV
T2	n08	C00	A	.505	.498		.489			.449										.498
			P	.497	.491		.481			.430										.457
		C28	A	.506	.519		.513			.472										.527
			P	.505	.510		.511			.470										.501
		C56	A	.500	.592		.616			.584										.658
			P	.490	.565		.589			.554										.623
		C84	A	.484	.773		.872			.870										.958
			P	.501	.680		.740			.730										.934
	n40	C00	A	.502	.498		.501	.501		.500		.500			.498			.483		.501
			P	.503	.503		.503			.502										.506
		C28	A	.508	.526		.535	.537		.533		.536			.535			.525		.539
			P	.501	.521		.529	.529		.532		.532			.527			.521		.533
		C56	A	.499	.603		.648	.655		.660		.663			.661			.652		.671
			P	.499	.583		.613	.614		.622		.623			.624			.621		.657
		C84	A	.498	.820		.908	.919		.939		.943			.949			.948		.953
			P	.498	.693		.769	.779		.797		.802			.812			.805		.954
	n72	C00	A	.502	.503	.502	.503		.502	.503	.500		.501	.500		.500	.500		.493	.503
			P	.513	.511		.511			.509										.507
		C28	A	.505	.521	.526	.530		.532	.533	.535		.535	.538		.537	.536		.539	.531
			P	.501	.522	.527	.529		.533	.531	.532		.533	.532		.531	.532		.529	.535
		C56	A	.495	.598	.622	.634		.643	.649	.649		.653	.656		.660	.660		.657	.660
			P	.499	.579	.606	.614		.624	.627	.628		.630	.629		.636	.633		.628	.655
		C84	A	.496	.819	.884	.910		.930	.939	.940		.943	.948		.947	.948		.947	.950
			P	.510	.694	.748	.769		.788	.794	.799		.805	.807		.808	.809		.806	.954

continued)

				ANO	B02	B03	B04	B05	B06	B08	B09	B10	B12	B18	B20	B24	B36	B40	B72	COV
T3	n08	C00	A	.498	.495		.491			.471										.492
			P	.508	.508		.495			.481										.485
		C28	A	.483	.505		.506			.495										.516
			P	.493	.514		.516			.501										.512
		C56	A	.463	.584		.621			.619										.658
			P	.498	.591		.617			.607										.634
		C84	A	.376	.762		.888			.924										.966
			P	.490	.720		.797			.809										.952
	n40	C00	A	.499	.499		.498	.500		.497		.498			.498			.498		.500
			P	.503	.501		.501			.500										.498
		C28	A	.493	.525		.531	.535		.538		.536			.534			.534		.539
			P	.508	.526		.533	.535		.537		.537			.539			.537		.540
		C56	A	.469	.598		.650	.654		.671		.669			.676			.670		.678
			P	.500	.602		.641	.647		.656		.658			.662			.658		.667
		C84	A	.378	.800		.913	.927		.943		.949			.954			.956		.965
			P	.493	.744		.831	.843		.864		.864			.872			.870		.960
	n72	C00	A	.486	.486	.487	.487		.488	.485	.486		.487	.488		.484	.482		.485	.486
			P	.504	.505		.506			.504										.506
		C28	A	.494	.524	.533	.533		.536	.538	.538		.538	.538		.538	.536		.537	.544
			P	.490	.516	.523	.525		.527	.529	.528		.531	.533		.534	.533		.531	.530
		C56	A	.466	.589	.622	.636		.650	.654	.656		.658	.659		.663	.663		.661	.666
			P	.508	.610	.638	.648		.661	.665	.666		.669	.672		.670	.671		.671	.658
		C84	A	.395	.815	.902	.929		.945	.952	.953		.957	.961		.961	.964		.964	.964
			P	.504	.742	.807	.834		.851	.858	.863		.867	.871		.875	.875		.873	.968

(continued)

				ANO	B02	B03	B04	B05	B06	B08	B09	B10	B12	B18	B20	B24	B36	B40	B72	COV
T4	n08	C00	A	.504	.505		.497			.484										.502
			P	.497	.496		.497			.477										.485
		C28	A	.488	.516		.519			.510										.536
			P	.501	.529		.532			.522										.518
		C56	A	.427	.572		.619			.632										.669
			P	.495	.616		.651			.657										.666
		C84	A	.305	.752		.897			.935										.966
			P	.510	.764		.853			.865										.964
	n40	C00	A	.509	.508		.508	.506		.507		.507			.504			.503		.510
			P	.505	.507		.506			.504										.500
		C28	A	.479	.516		.524	.525		.528		.526			.529			.529		.532
			P	.494	.523		.536	.536		.540		.536			.541			.535		.532
		C56	A	.438	.581		.639	.648		.656		.659			.668			.668		.674
			P	.502	.625		.662	.669		.679		.681			.688			.685		.672
		C84	A	.291	.787		.922	.940		.953		.958			.962			.964		.969
			P	.505	.782		.875	.888		.905		.908			.913			.914		.968
	n72	C00	A	.503	.503	.501	.502		.499	.502	.501		.500	.502		.503	.501		.504	.502
			P	.506	.508		.508			.508										.508
		C28	A	.483	.510	.517	.520		.521	.524	.525		.524	.528		.525	.526		.524	.528
			P	.493	.529	.533	.537		.539	.539	.541		.540	.541		.541	.543		.543	.527
		C56	A	.428	.578	.611	.630		.641	.651	.655		.657	.661		.661	.663		.661	.660
			P	.498	.626	.662	.677		.686	.693	.695		.695	.698		.699	.701		.699	.692
		C84	A	.303	.811	.900	.930		.954	.959	.961		.965	.965		.967	.969		.970	.973
			P	.508	.788	.849	.877		.901	.912	.915		.920	.923		.923	.925		.924	.969

(continued)

				ANO	B02	B03	B04	B05	B06	B08	B09	B10	B12	B18	B20	B24	B36	B40	B72	COV
T5	n08	C00	A	.515	.512		.511			.496										.509
			P	.508	.503		.502			.488										.486
		C28	A	.471	.507		.515			.514										.522
			P	.502	.532		.540			.533										.518
		C56	A	.421	.584		.646			.658										.679
			P	.510	.633		.678			.680										.689
		C84	A	.252	.757		.906			.947										.971
			P	.494	.791		.875			.893										.970
	n40	C00	A	.504	.504		.505	.503		.500		.501			.502			.502		.504
			P	.509	.509		.511			.513										.506
		C28	A	.479	.515		.526	.530		.530		.532			.529			.530		.537
			P	.504	.537		.549	.551		.555		.555			.554			.552		.532
		C56	A	.413	.582		.641	.652		.666		.668			.678			.678		.680
			P	.508	.645		.690	.702		.710		.711			.714			.712		.685
		C84	A	.238	.781		.926	.942		.960		.966			.973			.973		.979
			P	.495	.814		.901	.913		.931		.934			.940			.940		.973
	n72	C00	A	.502	.503	.502	.501		.502	.500	.501		.500	.503		.501	.502		.501	.502
			P	.498	.498		.497			.495										.500
		C28	A	.474	.510	.517	.522		.524	.526	.528		.528	.529		.529	.528		.526	.531
			P	.498	.532	.541	.544		.547	.547	.549		.548	.548		.549	.547		.545	.528
		C56	A	.420	.583	.627	.645		.666	.671	.673		.676	.679		.681	.683		.681	.685
			P	.504	.642	.674	.690		.703	.706	.711		.711	.714		.714	.715		.717	.675
		C84	A	.227	.781	.888	.920		.949	.961	.964		.968	.972		.972	.973		.974	.976
			P	.492	.807	.870	.895		.918	.923	.927		.930	.934		.936	.937		.936	.981

Note. Tx represents a number of treatments of x. Nxx represents a number of subjects per treatment of xx. Cxx represents a correlation coefficient between the concomitant and dependent variables of .xx. ANO represents ANOVA. Bxx represents a block design with xx blocks. COV represents ANCOVA. A represents the a priori approach. P represents the post-hoc approach.