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AUTHOR	Brooks, Gordon P.; Kanyongo, Gibbs Y.; Kyei-Blankson, Lydia; Gocmen, Gulsah
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ABSTRACT

Unfortunately, researchers do not usually have measurement instruments that provide perfectly reliable scores. Therefore, the researcher may want to account for the level of unreliability by appropriately increasing the sample size. For example, the results of a pilot study may indicate that a particular instrument is not as reliable with a given population as it has been with other populations. A series of Monte Carlo analyses were conducted to determine the sample sizes required when measurements are not perfectly reliable. The methods investigated were: (1) Pearson correlation; (2) Spearman rank correlation; and (3) analysis of variance (ANOVA). Using this information, a researcher can use the tables provided to determine an appropriate sample size for their study. Tables are also provided to illustrate the reduction in power from decreased reliability for given sample sizes. The computer program will be made available through the World Wide Web to help researchers determine the actual statistical power they can expect for their studies with less than perfect reliability. (Contains 1 figure, 6 tables, 6 charts, and 27 references.) (Author/SLD)

Power and Reliability for Correlation and ANOVA

Gordon P. Brooks

Gibbs Y. Kanyongo

Lydia Kyei-Blankson

Gulsah Gocmen

Ohio University

Paper presented at the annual conference of the American Educational Research Association, April 1-5, 2002, New Orleans, LA

Abstract

Unfortunately, researchers do not usually have measurement instruments that provide perfectly reliable scores. Therefore, the researcher may want to account for the level of unreliability by appropriately increasing the sample size. For example, the results of a pilot study may indicate that a particular instrument is not as reliable with a given population as it has been with other populations. Using this information, a researcher can use the tables provided to determine an appropriate sample size for their study. Tables are also provided that illustrate the reduction in power from decreased reliability for given sample sizes. Also, the computer program will be made available through the World Wide Web to help researchers determine what the actual statistical power they can expect for their studies with less-than-perfect reliability.

Power and Reliability for Correlation and ANOVA

Students of statistics usually become familiar with the factors that affect statistical power. For example, most students learn that sample size, level of significance, and effect size all determine the power of a statistical analysis. Additionally, some know that how effectively a particular design reduces error variance affects power, as does the directionality of the alternative hypothesis. However, many students do not realize that the reliability of measurements may also affect the statistical power (Hopkins & Hopkins, 1979). The purpose of this paper is (1) to explain the relationship between reliability and statistical power and (2) to provide sample size tables that account for reduced reliability. A series of Monte Carlo analyses were conducted to determine the sample sizes required when measurements are not perfectly reliable. Several statistical methods will be investigated, including (1) Pearson correlation, (2) Spearman rank correlation, and (3) analysis of variance.

Background

One of the chief functions of experimental design is to ensure that a study has adequate statistical power to detect meaningful differences, if indeed they exist (Hopkins & Hopkins, 1979). There is a very good reason researchers should worry about power a priori. If researchers are going to invest a great amount of money and time in carrying out a study, then they would certainly want to have a reasonable chance, perhaps 70% or 80%, to find a difference between groups if it does exist. Thus, a priori power (the probability of rejecting a null hypothesis that is false) will inform researchers how many subjects per group will be needed for adequate power.

Several factors affect statistical power. That is, once the statistical method and the alternative hypothesis have been set, the power of a statistical test is directly dependent on the sample size, level of significance, and effect size (Stevens, 2002). Often overlooked, however, is the relationship that variance has with power. Specifically, variance influences power through the effect size. For example, Cohen (1988) defined the effect for the *t* statistic as $\delta = (\mu_1 - \mu_0) / \sigma_X$. An applied example is that because variance is reduced, analysis of covariance is more powerful than analysis of variance when a useful covariate is utilized. Other variance reduction techniques include using a more homogeneous population and improving the reliability of measurements (Aron & Aron, 1997; Zimmerman, Williams, & Zumbo, 1993).

Reliability and Effect Size

Cleary and Linn (1969) reported that "in the derivation and interpretation of statistical tests, the observations are generally considered to be free of error of measurement" (p. 50). From a classical test theory perspective, an individual's observed score (X) is the sum of true score (T) and error score (E); that is, $X = T + E$. Therefore, if there is no error of measurement, then the observations are the true scores. For a set of scores, measurements made without error occur only when the instruments provide perfectly reliable scores. Observed score variance, σ_X^2, is defined as the sum of true score variance, σ_T^2, and measurement error variance, σ_E^2. Because reliability, $\rho_{XX'}$, is defined as the ratio of true score variance to observed score variance, $\rho_{XX'} = \sigma_T / \sigma_X = 1 - \sigma_E / \sigma_X$, reliability can only be perfect (i.e., $\rho_{XX'} = 1.0$) when there is no measurement error (Lord & Novick, 1968).

Because σ_X can be written as $\sigma_T / \sqrt{\rho_{XX'}}$, the standardized effect size for the *t* test can be written as $\delta = (\mu_1 - \mu_0)\sqrt{\rho_{XX'}} / \sigma_T$ (Levin & Subkoviak, 1977; Williams &

Zimmerman, 1989). Consequently, reliability affects statistical power indirectly through effect sizes. Cohen (1988) reported that reduced reliability results in reduced effect sizes in observed data (ES), which therefore reduces power. That is, observed effect sizes, $ES = ESP * \sqrt{r_{XX'}}$, where ESP is the population effect size. When reliability is perfect, observed ES equals the true population ES; but when reliability is less than perfect, $ESP * \sqrt{r_{XX'}}$ is a value smaller than the true effect size. Therefore, effect sizes are reduced when measurement error exists. Some introductory statistics textbooks discuss this problem in reference to attenuation in correlation due to unreliability of measures (e.g., Glass & Hopkins, 1996).

Reliability and Power

Controversy surrounds the relationship between power and reliability (Williams & Zimmerman, 1989). For example, good statistical power can exist with poor reliability and a change in variance can be unrelated to reliability can change power. However, there are persuasive reasons to consider reliability as an important factor in determining statistical power.

There is no controversy that statistical power depends on observed variance. Zimmerman and Williams (1986) noted that when speaking of statistical power it is irrelevant whether the variance measured is true score variance or observed score variance; that is, "the greater the observed variability of a dependent variable, whatever its source, the less is the power of a statistical test" (p. 123). But because reliability is defined by observed variance in conjunction with either true or error variance, one cannot be certain which is changed when reliability improves. That is, if observed variance increases, we cannot be certain whether the increase is due to an increase in true score variance or a increase in error variance, or both. Or as Zimmerman, Williams, & Zumbo (1993) reported, power changes as reliability changes only if observed score variance changes simultaneously.

However, if we assume (1) that true variance is a fixed value for the given population and (2) that improved reliability results in less measurement error, then it follows that a change in reliability will result in a change in observed score variance. Indeed, statistical power is a mathematical function of reliability only if either true score variance or error variance is a constant; otherwise power and reliability are simply related (Cohen, 1988; Williams & Zimmerman, 1989). But improvement in reliability is usually interpreted as a reduction in the measurement error variance that occurs from a more precise measurement (Zimmerman & Williams, 1986). Therefore, a reduction in reliability that is accompanied by an increase in observed score variance will indeed reduce statistical power (Zimmerman, Williams, & Zumbo, 1993b). That is, if true score variance remains constant but lower reliability leads to increased error variance, then statistical power will be reduced because of the increased observed score variance (cf. Humphreys, 1993). It becomes apparent then that "failure to reject the null hypothesis with observed scores is obviously not equivalent to a failure to reject the null hypothesis with true scores" (Cleary & Linn, 1969, p. 50).

Based on such an assumption, for example, Light, Singer and Willett (1990) advised that when measurements are less than perfectly reliable, improving the power of statistical tests involves a decision either to increase sample size or to increase reliability—the researcher must compare the costs associated with instrument improvement to the costs of adding study participants (see also Cleary & Linn, 1969;

Feldt & Brennan, 1993). Researchers may encounter such a situation if an instrument does not perform as reliably in a given study as it has elsewhere, leading to increased variance in the current project. Assuming that the increased variance is not due to more heterogeneity in the population and that the true score variance of the population hasn't changed, the observed score variance will change as a consequence of the change in reliability.

Power is a function of level of significance, sample size, and effect size only under the assumption of no measurement error, but our measures in the social sciences are typically not measured perfectly (Cleary & Linn, 1969; Levin & Subkoviak, 1977). Indeed, the implicit assumption that our measures are perfectly reliable is not justified in practice (Crocker & Algina, 1986; Sutcliffe, 1958). Measurement error in the dependent variable should be considered a priori for sample size and post hoc for power (Subkoviak & Levin, 1977).

Unfortunately, there are few easy ways to account for reliability when determining sample sizes. The tables found in Cohen (1988) do not provide the option to vary reliability. Computer programs such as SamplePower and PASS 2000 also assume perfect reliability. Along the same lines of work done by Kanyongo, Kyei-Blankson, and Brooks (2001), this paper will report on the impact of reliability on power as well as provide tables to assist researchers in finding sample sizes necessary with fallible measures.

Method

Two Monte Carlo programs, MC2G (Brooks, 2002) and MC3G (Brooks, 2002) written in Delphi Pascal, were used to create normally distributed but unreliable data and perform analyses for several statistical methods, including Pearson correlation, Spearman rank correlation, and analysis of variance (ANOVA) with three levels. The programs were used to create power and sample size tables for these tests. Reliability was varied from .70 to 1.0 in increments of 0.05. For power tables, power rates will vary from .70 to .90 by .10. Population effect sizes were varied from small to large using Cohen's (1988) conventional standards. Specifically, for correlations, a small effect was set at $r = .10$, medium was $r = .30$, and a large effect was set to be $r = .50$; for ANOVA, a small standardized difference effect was set at $f = .10$, medium was $f = .25$, and large was $f = .40$.

Statistical power tables for given sample sizes are based on empirical Monte Carlo results of 100,000 iterations; the sample size tables were based on 10,000 simulated samples. For the power tables, the sample sizes were obtained under the assumption of perfect reliability. That is, the sample sizes were fixed at the values needed to achieve power levels of .70, .80 and .90 when reliability was 1.0. The remaining values in the power tables were determined by systematically varying the reliability with that given sample size. For the sample sizes tables, power was fixed, reliability was varied, and sample sizes were tried until the required power was achieved.

Data Generation

For each analysis, the researchers entered appropriate information into the program. For example, the values for large effect size of $r = .50$ and reliability or .90 were provided as input to the program (see Figure 1). The programs generate uniformly distributed pseudorandom numbers to be used as input to the procedure that will convert them into normally distributed data. For each sample, the appropriate statistical analysis is performed. The number of correct rejections of the null hypothesis is stored and reported by the program. These procedures were repeated as necessary for each sample condition created.

The L'Ecuyer (1988) generator was chosen for the programs. Specifically, the FORTRAN code of Press, Teukolsky, Vetterling, and Flannery (1992), was translated into Delphi Pascal. The L'Ecuyer generator was chosen because of its large period and because combined generators are recommended for use with the Box-Muller method for generating random normal deviates, as will be the case in this study (Park & Miller, 1988). The computer algorithm for the Box-Muller method used in this study was adapted for Delphi Pascal from the standard Pascal code provided by Press, Flannery, Teukolsky, and Vetterling, 1989. Extended precision floating point variables were used, providing the maximum possible range of significant digits. Simulated samples were chosen randomly to test program function by comparison with results provided by SPSS for Windows version 10.1.

The programs generate normally distributed data of varying reliability based on classical test theory. That is, reliability is not defined using a particular measure of reliability (e.g., split-half or internal consistency); rather it is defined as the proportion of raw score variance explained by true score variance, σ_T/σ_X, or equivalently $1 - \sigma_E/\sigma_X$. Each raw score generated is taken to be a total score. The program user enters (1) the expected true score variance for the population and (2) a reliability estimate. Consequently, as reliability decreases, raw score variance increases as compared to the given true score variance. For correlation analyses, the same reliability was used for both measures.

Monte Carlo Simulations

The number of iterations for the study is based on the procedures provided by Robey and Barcikowski (1992). Significance levels for both tests on which Robey and Barcikowski's method is based were set at $\alpha = .05$ with $(1-\beta) = .90$ as the power level; the magnitude of departure was chosen to be $\alpha \pm .2\alpha$, which falls between their intermediate and stringent criteria for accuracy. The magnitude of departure is justified by the fact that at $\pm.2\alpha$, the accuracy range for $\alpha = .05$ is $.04 \leq \alpha \leq .06$. Based on the calculations for these parameters (this set of values was not tabled), 5422 iterations would be required to "confidently detect departures from robustness in Monte Carlo results" (Robey & Barcikowski, 1992, p. 283), but applies to power studies also (Brooks, Barcikowski, & Robey, 1999). However, to assure even greater stability in the results, a larger number of simulations was chosen for each type of analysis. Specifically, 100,000 samples were used for the power tables, but because the determination of sample sizes is a much slower process, only 10,000 simulated samples were used in creating those tables.

Results

Table 1, Table 3, and Table 5 show the relationship between statistical power and reliability for the Pearson product-moment correlation, Spearman rank-order correlation, and ANOVA, respectively. There is a relatively linear relationship between the two when sample size is fixed (variations are due to the Monte Carlo sampling process). Chart 1, Chart 3, and Chart 5 show graphical representations of these relationships. This relationship is roughly the same for all tests at all effect sizes. When reliability changes, the observed score variance changes, and any change in reliability that increases observed score variance reduces statistical power. Similarly, increasing reliability increases power.

For example, Table 1 shows that when statistical power is chosen to be .80 for a Pearson correlation, 28 cases are required when perfect reliability is assumed and a large effect size (a correlation of .50) is expected. When reliability was changed to .90, the actual statistical power was observed to be .70. Reliability set at .80 resulted in observed statistical power of .58. Finally, actual power was .46 when reliability was set at .70. Such depreciation of power occurs also with t-tests and their nonparametric alternatives.

Table 2, Table 4, and Table 6 show the change in sample size required for analyses in order to maintain a given power level when reliability is less than perfect. Again, there are relatively linear relationships for all tests at all power levels. Chart 2, Chart 4, and Chart 6 show that sample sizes must increase much more dramatically for smaller effect sizes. For example, Table 2 shows that when the desired statistical power level is set at .80 and a large effect size (a correlation of .50) is expected, the use of 28 cases results in power of .80 when reliability is 1.0; but when reliability is reduced to .90, 36 cases are required. If reliability is .80, then the study needs 46 participants. Finally, 61 cases must be used to achieve power of .80 when reliability is .70.

Conclusions

In social sciences, few things are measured perfectly (Subkoviak & Levin, 1977). However, by making judicious design decisions, one can improve the quality of his or her measurements. To begin with, the researcher needs to understand what influences measurement quality or helps to reduce measurement error. There are three main sources of errors: (a) flaws in the instrument and its administration, (b) random fluctuations over time in subjects measured, and (c) disagreement among raters or scores (Light, Singer & Willet, 1990). Knowing what the sources of error are and how they get into measurements helps in improving the quality of measurement.

Researchers should make an effort to minimize the effects of measurement error. There are several strategies that have been developed for minimizing the effects of measurement error and increasing reliability. These include revising items, increasing the number of items, lengthening item scales, administering the instrument systematically, timing of data collection and use of multiple raters or scores (Light, Singer & Willet, 1990). Effect of measurement fallibility on power and on sample size is most dramatic for small effect size.

Before one chooses a final sample size, the possibility of measurement error should be considered. To determine sample sizes "without simultaneously considering errors of measurement is to live in a 'fool's paradise'" (Levin & Subkoviak, 1977, p. 337). If one suspects that measurement error exists and there is no viable means to reduce it, sample size should be increased accordingly. Researchers can identify potential problems with measurement error through pilot studies or previous research. Where reliability information is lacking, the researcher should use cautious estimates, with a preference toward more conservative values, when deciding sample sizes in the presence of less-than-perfect reliability (Levin & Subkoviak, 1977). Light, Singer, and Willett (1990) provided tables to illustrate the point. Unfortunately, their tables provide only a very few situations and are therefore limited in their usefulness. The present study extends their tables and provides such information for additional statistical methods.

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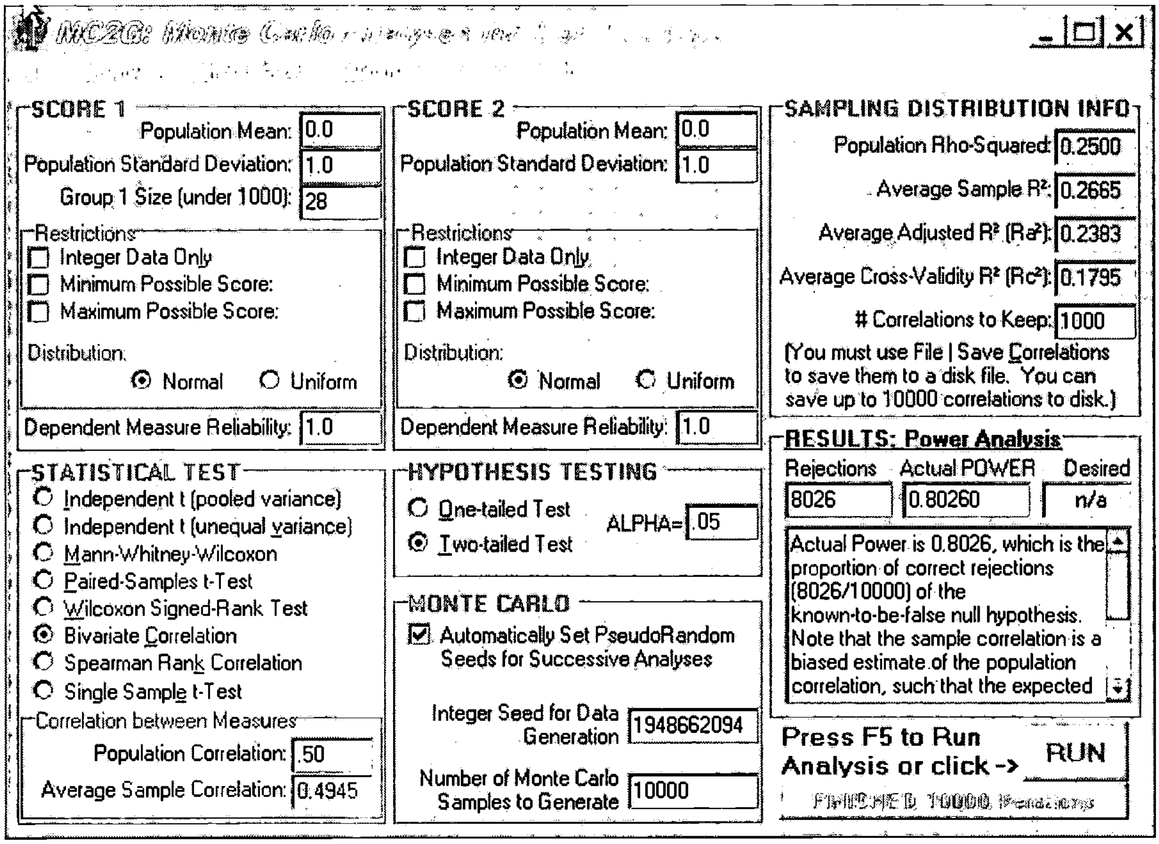
Figure 1

Example screen for the MC2G program



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Pearson Product-Moment Correlation

Table 1
Actual Power at Different Reliability Values at two-tailed $\alpha = .05$ (100,000 iterations)

Effect Size	N	Reliability						
		1.0	.95	.90	.85	.80	.75	.70
Large (r = .5)	23	.71	.65	.60	.55	.49	.43	.38
	28	.80	.75	.70	.63	.58	.52	.46
	37	.90	.86	.82	.77	.71	.65	.58
Mediu m (r = .3)	66	.70	.65	.60	.55	.50	.45	.40
	84	.80	.75	.71	.66	.60	.54	.49
	112	.90	.87	.83	.78	.73	.67	.61
Small (r = .1)	615	.70	.65	.61	.56	.51	.46	.41
	787	.80	.76	.71	.67	.61	.55	.50
	1021	.90	.86	.82	.78	.73	.67	.61

Table 2
Sample Sizes Required at Different Reliability Values at two-tailed $\alpha = .05$ (10,000 iterations)

Effect Size	Power	Reliability						
		1.0	.95	.90	.85	.80	.75	.70
Large (r = .5)	.70	23	25	29	35	37	43	49
	.80	28	32	36	41	46	54	61
	.90	37	42	47	54	61	72	81
Mediu m (r = .3)	.70	66	75	83	95	104	120	138
	.80	84	95	105	119	132	151	172
	.90	112	128	140	158	175	205	235
Small (r = .1)	.70	615	663	756	838	945	1095	1293
	.80	787	918	973	1169	1251	1386	1709
	.90	1021	1211	1292	1515	1694	1922	****

Spearman Rank-Order Correlation

Table 3
Actual Power at Different Reliability Values at two-tailed α = .05 (100,000 iterations)

Effect Size	N	Reliability						
		1.0	.95	.90	.85	.80	.75	.70
Large (r = .5)	26	.70	.65	.70	.54	.49	.42	.38
	33	.80	.76	.71	.65	.60	.53	.47
	43	.90	.87	.83	.77	.72	.65	.60
Mediu m (r = .3)	75	.70	.65	.61	.55	.51	.45	.41
	94	.80	.76	.71	.66	.60	.55	.49
	128	.90	.88	.84	.79	.74	.68	.62
Small (r = .1)	680	.70	.66	.61	.56	.51	.46	.41
	827	.80	.74	.70	.65	.59	.54	.48
	1148	.90	.87	.83	.78	.73	.68	.62

Table 4
Sample Sizes Required at Different Reliability Values at two-tailed α = .05 (10,000 iterations)

Effect Size	Power	Reliability						
		1.0	.95	.90	.85	.80	.75	.70
Large (r = .5)	.70	26	30	33	37	41	48	54
	.80	33	36	41	47	53	60	68
	.90	43	48	52	62	67	79	91
Mediu m (r = .3)	.70	75	82	92	104	118	129	153
	.80	94	105	116	131	149	169	197
	.90	128	137	156	176	198	222	252
Small (r = .1)	.70	680	753	841	954	1075	1235	1387
	.80	827	941	1044	1254	1345	1589	1740
	.90	1148	1212	1512	1593	1685	1826	****

Analysis of Variance (three independent samples)

Table 5
Actual Power at Different Reliability Values at two-tailed α = .05 (100,000 iterations)

Effect Size	N per group	Reliability						
		1.0	.95	.90	.85	.80	.75	.70
Large (f = .40)	17	.70	.67	.65	.63	.60	.56	.53
	21	.80	.78	.75	.73	.71	.67	.64
	28	.91	.89	.87	.85	.83	.80	.77
Medium (f = .25)	41	.70	.67	.65	.62	.60	.57	.54
	51	.80	.78	.75	.73	.70	.67	.64
	66	.90	.88	.86	.84	.82	.79	.76
Small (f = .10)	269	.71	.68	.65	.62	.60	.57	.54
	333	.80	.78	.75	.73	.70	.67	.64
	441	.90	.89	.87	.85	.82	.80	.77

Table 6
Sample Sizes Required at Different Reliability Values at two-tailed α = .05 (10,000 iterations)

Effect Size	Power	Reliability						
		1.0	.95	.90	.85	.80	.75	.70
Large (f = .40)	.70	17	18	19	20	21	22	24
	.80	21	22	23	25	26	28	30
	.90	28	29	30	32	34	36	39
Medium (f = .25)	.70	41	44	45	48	50	54	58
	.80	51	54	56	61	65	68	73
	.90	66	70	75	78	83	88	95
Small (f = .10)	.70	269	288	300	314	332	356	382
	.80	333	353	374	395	419	451	482
	.90	441	464	488	516	551	583	619

Pearson Product-Moment Correlation

Chart 1
Statistical power and reliability (for N based on Power = .80)

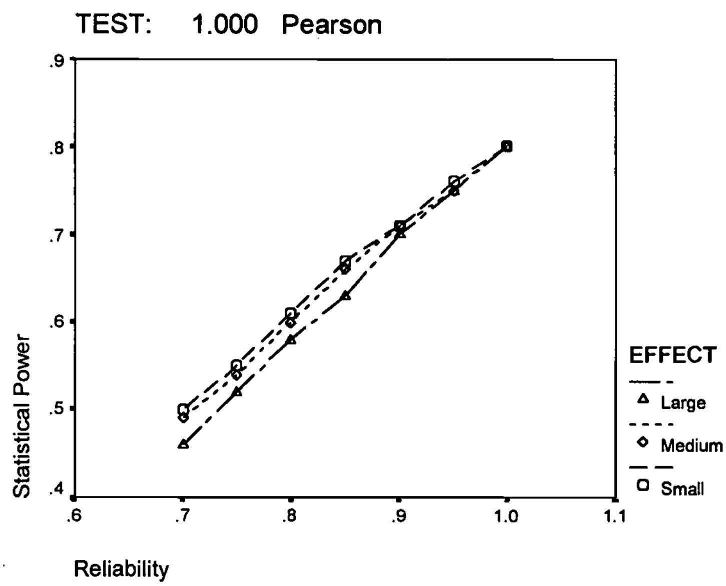
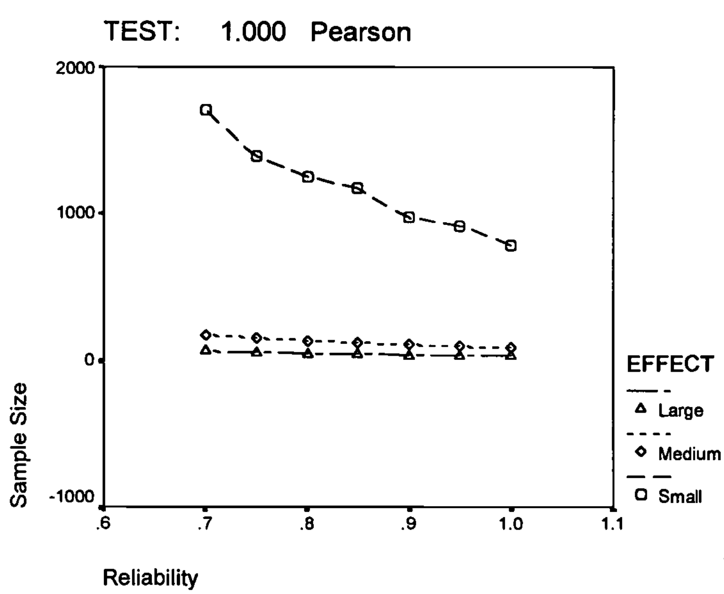


Chart 2
Reliability and sample size at power = .80



Spearman Rank-Order Correlation

Chart 3
Statistical power and reliability (for N based on Power = .80)

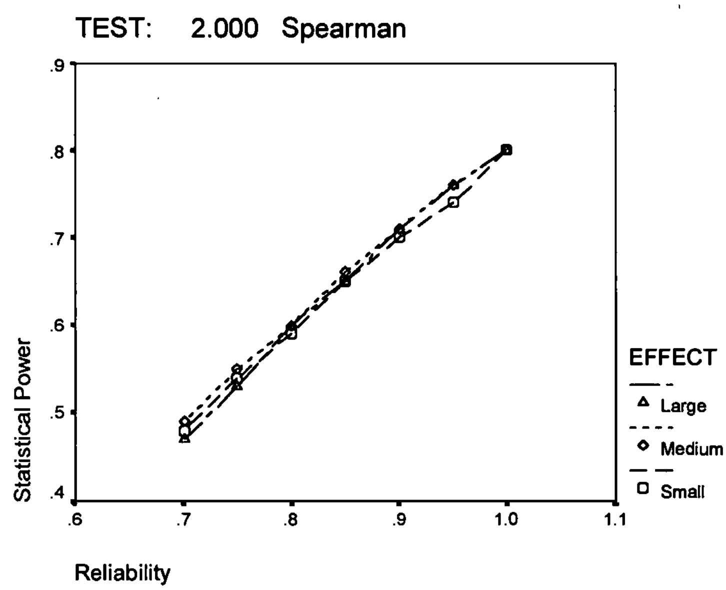
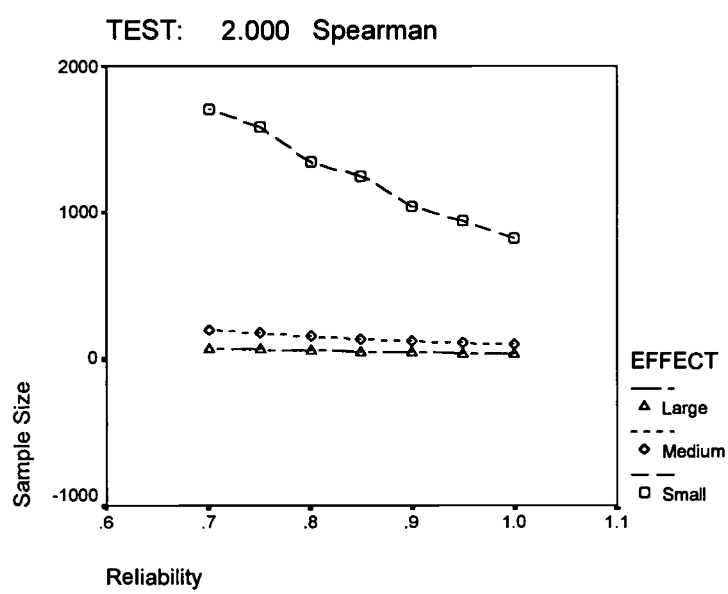


Chart 4
Reliability and sample size at power = .80



<u>Analysis of Variance (three independent samples)</u>

Chart 5
Statistical power and reliability (for N based on Power = .80)

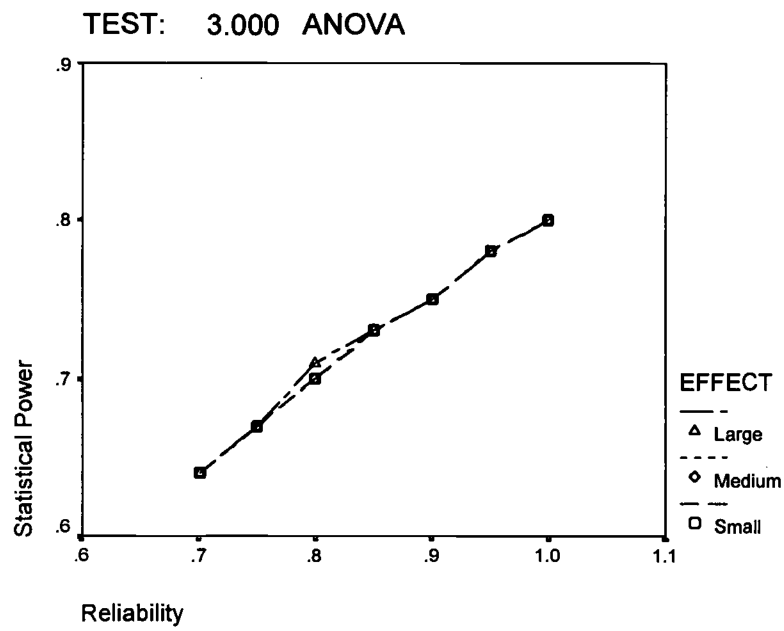
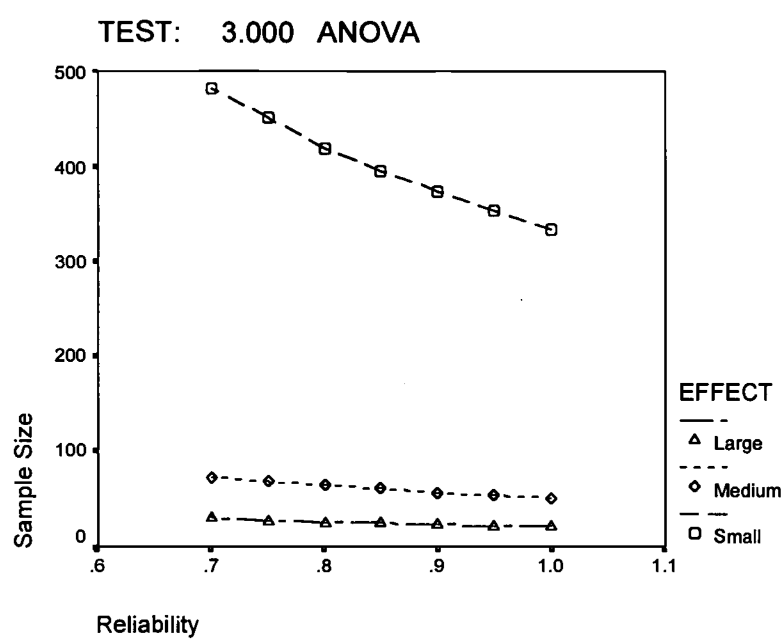


Chart 6
Reliability and sample size at power = .80



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Organization/Address: McCracken Hall, Ohio University, Athens, OH 45701	Telephone: 740-593-0880	Fax: 740-593-0477
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