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Type II Error Problems in the Use of Moderated Multiple Regression for the Detection of Moderating Effects of Dichotomous Variables

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Monte Carlo simulation procedures were used to assess the power of moderated multiple regression (MMR) to detect the effects of a dichotomous moderator variable under conditions of: *(1) between-group differences in within-group relationships between two variables (i.e., $|\rho_{XY(1)} - \rho_{XY(2)}|$ = .20, .40, .60); (2) different combined sample sizes for the two groups ($N_1 + N_2 = N_T$ =* 30, 60, 90, 180, 300)*; and (3) differing proportions of cases (P_i) in the two groups (i.e., P_1 =* .10, .30, .50). *Results showed that, consistent with our a priori predictions, the power of MMR increased as: (1) total sample size (NT) increased; (2) the difference between within-group correlation coefficients increased; and (3) the difference between the proportion of cases in each group decreased. Moreover, the simulation showed that these three variables had interactive effects on power. The major implication of our findings is that in cases where tests of moderating effects are conducted with MMR and the proportion of cases in each group differs greatly, inferences of no moderating effect may be erroneous: Such inferences may be the result of low statistical power rather than the absence of a moderating effect.*

Researchers in industrial and organizational psychology, organizational behavior, human resources management, and a host of other disciplines are often interested in testing for the existence of moderating effects, i.e., interactive effects of two variables (cf. Stone, 1988; Zedeck, 1971). In recent years, attempts to detect such effects have relied increasingly on moderated multiple regression (MMR; Cohen & Cohen, 1983; Stone, 1988; Zedeck, 1971). MMR has been

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used to detect moderating effects for moderator variables that are measured on both continuous (e.g., age) and dichotomous (e.g., gender) scales. In personnel psychology, for instance, researchers have used MMR to determine if pre-employment selection tests differentially predict job performance for two groups of applicants (e.g., male, female; white, non-white). Unfortunately, tests of moderating effects for dichotomous moderator variables may not always provide valid information about the existence of such effects. One reason for the failure to find effects in studies that attempt to show differences in predictor-criterion relationships (e.g., correlation coefficients, regression slopes) for two groups (e.g., male, female) is that the relative sizes of the samples of the two groups (N_1, N_2), and thus the proportions (p_i) of individuals in the groups (p_1, p_2) may differ markedly from one another. As a consequence of differences between p_1 and p_2 there will be limits on the magnitudes of relationships (e.g., zero-order correlation coefficients) between the moderator (Z) and (1) X, the other predictor variable (e.g., test scores); and (2) Y, the continuously measured criterion variable (e.g., job performance) (cf. Cohen & Cohen, 1983; Nunnally, 1978).

The effect of $p_1 - p_2$ differences on relationships can be deduced from the following formula for the point-biserial correlation coefficient (cf. Cohen & Cohen, 1983; Nunnally, 1978):

$$r_{pb} = [\,(M_1 - M_2\,)\,/\,\sigma\,][\,p_1 \cdot p_2\,]^{1/2} \tag{1}$$

where: M^1 = mean score of Group 1 on the continuous variable
M_2 = mean score of Group 2 on the continuous variable
p_1 = proportion of cases in Group 1
p_2 = proportion of cases in Group 2
σ = standard deviation of the continuous variable

As Equation 1 illustrates, assuming a fixed difference between the means of Group 1 and Group 2 on a continuous variable and a constant standard deviation for this variable, the greater the discrepancy between p_1 and p_2, the lower will be the value of r_{pb} (Cohen & Cohen, 1983, pp. 66-67; Nunnally, 1978, pp. 145-146). In the limiting case, when either p_1 or p_2 equals 0, r_{pb} will equal zero.

Moreover, to the extent that $p_1 - p_2$ differences affect estimates of the magnitudes of zero-order correlations between (*a*) a moderator variable (Z) and another continuous predictor (X), and (*b*) a moderator variable and a criterion variable (Y), the results of MMR analyses involving the dichotomous predictor variable will be affected. The reason for this is that there are upper limits on the magnitudes of squared semi-partial correlations involving dichotomous moderator variables (cf. Cohen & Cohen, 1983, pp. 89-90). The important implication of the foregoing is that such limits will influence the ability of MMR to detect moderating effects: The greater the $p_1 - p_2$ difference, the lower will be the power of an MMR-based test of a moderating effect for Z. Stated differently, the greater the $p_1 - p_2$ discrepancy, the greater will be the risk of the researcher committing a Type II statistical error in the search for moderating effects (i.e., falsely concluding that there is no moderating effect).

A recent study by Hattrup and Schmitt (1990) provides an illustration of this problem. In the study, MMR was used to test for differential predictor-criterion relationships in groups formed on the basis of subject gender (male vs. female) and race (white vs. non-white). Their MMR analyses used data from samples that differed substantially from one another in terms of the relative sizes of each subgroup, leading to substantial $p_1 - p_2$ differences. More specifically, in tests for race-based moderating effects the respective proportions of whites and nonwhites were .831 and .169, and in tests for gender-based moderating effects the respective proportions of males and females were .899 and .101. Their MMR analyses failed to show moderating effects for either race or gender, leading them to conclude that neither race nor gender were moderators of the predictor-criterion relationships considered by their study.

Another recent study that provides an illustration of the low statistical power problem in tests of the effects of dichotomous moderator variables is Cortina, Doherty, Schmitt, Kaufman, and Smith (1992). In the study MMR was used to test for the moderating effects of race on relationships between personality predictors and several criterion variables (e.g., performance ratings). Minority group members comprised only 31% of the sample. Results of 12 separate MMR analyses showed that there was no moderating effect for race, leading the researchers to conclude that the tests were fair to both majority and minority groups.

As a consequence of statistical power problems, we believe that the conclusions of Hattrup and Schmitt (1990), Cortina et al. (1992), and a host of others may not be valid: More specifically, we believe that the inference of no moderating effect that stems from the use of MMR with data sets for which there is a large difference between p_1 and p_2 levels may be a function of Type II error. At present, however, there is no direct evidence on the extent to which differences in p_1 and p_2 levels influence the power of MMR to detect moderating effects. Therefore, we conducted a Monte Carlo simulation to assess the power of MMR to detect the effects of a dichotomous moderator variable under conditions of differing p_1 and p_2 levels. Three independent variables were manipulated in our simulation: (1) the overall size of the sample ($N_T = N_1 + N_2$) upon which the MMR analysis was based; (2) the proportions of individuals in two groups for which differential prediction was tested (p_1, p_2); and (3) the zero-order correlations between the continuous predictor (X) and the criterion (Y) in the population from which cases were sampled.

Method

Simulation Design and Procedures

Monte Carlo simulation procedures were used to determine the power of MMR to detect the moderating effects of a dichotomous moderator variable, Z, on the relationship between two other variables, X and Y. A QuickBASIC

4.5 program was used for the simulation. Prior to using the program to conduct the simulation, we conducted a number of checks to insure its accuracy.

The simulation design was a 4 ($\rho_{XY(1)}$ = population correlation between variables X and Y in Group 1) X 4 ($\rho_{XY(2)}$ = population correlation between variables X and Y in Group 2) X 5 (N_T = total sample size) X 3 (N_1 / N_T = proportion of cases in Group 1): (1) The Group 1 (G_1) correlation between the continuous predictor (X) and the criterion (Y) in the population ($\rho_{XY(1)}$) was set at values of .2, .4, .6, and .8; (2) The Group 2 (G_2) correlation between the continuous predictor (X) and the criterion (Y) in the population ($\rho_{XY(2)}$) was set at levels of .2, .4, .6, and .8; (3) The total sample size (N_T) was set at levels of 30, 60, 90, 180, and 300; and (4) The proportions of individuals in the two groups (p_1 and p_2) for which differential prediction was tested were varied by setting p_1 at .1, .3, and .5.

For each of the 240 resulting cells of the design 2,000 samples were drawn and for each such sample MMR was used to test for the moderating effect of Z on the relationship between X and Y. The existence of such an effect was assessed by testing the statistical significance of the regression weight for β_3 in the following model:

$$Y = \beta_0 + \beta_1 X + \beta_2 Z + \beta_3 X \cdot Z \qquad (2)$$

where: $X \cdot Z$ is a product term that carries information about the interaction between X and Z. These MMR analyses and the associated statistical tests yielded information about the power of MMR to detect true (population) moderating effects for samples that differed in terms of the three manipulated variables.

Results

Tables 1-5 show results of the simulation. More specifically, the tables show the percentage rejection rates of the null hypothesis that $\beta_3 = 0$. Rejection of this hypothesis signals the existence of a moderating effect of Z on the relationship between X and Y.

The results presented in Tables 1-5 show that, consistent with our *a priori* predictions, the power of MMR to detect true, between-group differences in within-group correlation coefficients increased as the proportion of cases in each of the groups (p_1 and p_2) approached .5. In many cases power dropped substantially as the $p_1 - p_2$ difference increased. For example, for $N_T = 90$, and a .60 difference between the population correlation coefficients, power reached .925 for $p_1 = .50$, but dropped to .341 for $p_1 = .10$. The magnitude of these substantial changes in power is illustrated in Figure 1 which shows rejection rates of the null hypotheses of $\beta_3 = 0$ for $N_T = 90$ and differing levels of the other two manipulated variables.

The results in Tables 1-5 also show that our other *a priori* expectations were confirmed by the findings of the simulation. More specifically, the results showed that power increased as a function of both: (1) the magnitude of the

Table 1. Power to Detect a Moderating Effect as a Function of the Strength of the Effect in the Population and the Proportion of Cases In Group 1 for Total Sample Size of 30

		Proportion of cases in Group 1		
$\rho_{XY(1)}$	$\rho_{XY(2)}$	*.1*	*.3*	*.5*
.2	.2	.048	.052	.061
.2	.4	.079	.085	.083
.2	.6	.149	.208	.191
.2	.8	.338	.452	.443
.4	.2	.043	.069	.083
.4	.4	.047	.045	.046
.4	.6	.084	.091	.096
.4	.8	.229	.292	.244
.6	.2	.044	.147	.193
.6	.4	.040	.068	.087
.6	.6	.058	.048	.046
.6	.8	.137	.134	.118
.8	.2	.058	.273	.432
.8	.4	.028	.147	.254
.8	.6	.022	.067	.099
.8	.8	.056	.048	.052

Note: $\rho_{XY(1)}$ = zero-order correlation between Y and X for Group 1 and $\rho_{XY(2)}$ = zero-order correlation between Y and X for Group 2.

Table 2. Power to Detect a Moderating Effect as a Function of the Strength of the Effect in the Population and the Proportion of Cases In Group 1 for Total Sample Size of 60

		Proportion of cases in Group 1		
$\rho_{XY(1)}$	$\rho_{XY(2)}$	*.1*	*.3*	*.5*
.2	.2	.042	.046	.045
.2	.4	.092	.112	.120
.2	.6	.216	.342	.366
.2	.8	.497	.733	.752
.4	.2	.055	.101	.133
.4	.4	.053	.050	.049
.4	.6	.118	.143	.138
.4	.8	.338	.481	.494
.6	.2	.099	.275	.377
.6	.4	.043	.114	.147
.6	.6	.054	.045	.056
.6	.8	.174	.188	.183
.8	.2	.177	.634	.774
.8	.4	.077	.338	.469
.8	.6	.032	.116	.186
.8	.8	.046	.054	.048

Note: $\rho_{XY(1)}$ = zero-order correlation between Y and X for Group 1 and $\rho_{XY(2)}$ = zero-order correlation between Y and X for Group 2.

Table 3. Power to Detect a Moderating Effect as a Function of the Strength of the Effect in the Population and the Proportion of Cases In Group 1 for Total Sample Size of 90

		Proportion of cases in Group 1		
$\rho_{XY(1)}$	$\rho_{XY(2)}$	*.1*	*.3*	*.5*
.2	.2	.056	.049	.056
.2	.4	.096	.148	.153
.2	.6	.263	.491	.536
.2	.8	.629	.891	.915
.4	.2	.066	.124	.167
.4	.4	.056	.050	.052
.4	.6	.138	.184	.187
.4	.8	.402	.621	.653
.6	.2	.170	.404	.526
.6	.4	.060	.143	.180
.6	.6	.047	.052	.056
.6	.8	.197	.260	.245
.8	.2	.341	.820	.925
.8	.4	.153	.509	.643
.8	.6	.056	.173	.247
.8	.8	.052	.049	.049

Note: $\rho_{XY(1)}$ = zero-order correlation between Y and X for Group 1 and $\rho_{XY(2)}$ = zero-order correlation between Y and X for Group 2.

Table 4. Power to Detect a Moderating Effect as a Function of the Strength of the Effect in the Population and the Proportion of Cases In Group 1 for Total Sample Size of 180

		Proportion of cases in Group 1		
$\rho_{XY(1)}$	$\rho_{XY(2)}$	*.1*	*.3*	*.5*
.2	.2	.055	.039	.057
.2	.4	.145	.263	.283
.2	.6	.477	.790	.822
.2	.8	.843	.991	.999
.4	.2	.118	.234	.288
.4	.4	.049	.050	.040
.4	.6	.184	.316	.343
.4	.8	.634	.877	.924
.6	.2	.350	.741	.836
.6	.4	.111	.268	.351
.6	.6	.050	.051	.041
.6	.8	.295	.450	.458
.8	.2	.706	.993	.999
.8	.4	.363	.862	.930
.8	.6	.130	.361	.458
.8	.8	.058	.048	.058

Note: $\rho_{XY(1)}$ = zero-order correlation between Y and X for Group 1 and $\rho_{XY(2)}$ = zero-order correlation between Y and X for Group 2.

Table 5. Power to Detect a Moderating Effect as a Function of the Strength of the Effect in the Population and the Proportion of Cases In Group 1 for Total Sample Size of 300

		Proportion of cases in Group 1		
$\rho_{XY(1)}$	$\rho_{XY(2)}$	*.1*	*.3*	*.5*
.2	.2	.044	.049	.056
.2	.4	.199	.384	.434
.2	.6	.675	.942	.971
.2	.8	.958	1.000	1.000
.4	.2	.180	.382	.442
.4	.4	.054	.050	.060
.4	.6	.251	.487	.513
.4	.8	.809	.973	.994
.6	.2	.574	.921	.968
.6	.4	.186	.422	.523
.6	.6	.053	.043	.046
.6	.8	.380	.635	.681
.8	.2	.950	1.000	1.000
.8	.4	.644	.978	.991
.8	.6	.207	.577	.681
.8	.8	.049	.056	.049

Note: $\rho_{XY(1)}$ = zero-order correlation between Y and X for Group 1 and $\rho_{XY(2)}$ = zero-order correlation between Y and X for Group 2.

difference between the population correlations ($|\rho_{XY(1)} - \rho_{XY(2)}|$); and (2) the overall size of the sample (N_T). These effects can be clearly seen in Figure 1. The same figure also illustrates how the manipulated variables interacted with one another in influencing power.

In order to better determine the nature of the main and interactive effects of the manipulated variables, we conducted a MMR analysis in which we regressed statistical power estimates derived from our simulation (i.e., the proportion of times that the null hypothesis of $\beta_3 = 0$ was correctly rejected) on variables representing the main and interactive effects of the manipulated parameters (i.e., differences in proportions, overall sample size, and the absolute difference between the Fisher's Z equivalents of the population correlation coefficients). Because the dependent variable was statistical power, data from only the 180 cells of the design for which the manipulated moderating effect was not equal to zero (i.e., $\rho_{XY(1)} \neq \rho_{XY(2)}$) were used in this MMR analysis. At step one of the analysis, the main effects of the three predictors (manipulated variables) were entered into the equation. At step two, the three two-way interaction terms were entered. Finally, at step three, the three-way interaction term was entered. Because linearity was assumed, the actual values of the variables rather than dummy codes were used in the MMR analysis.

Table 6 shows the results of this analysis. As can be seen in the table, both the two-and three-way interaction terms were statistically significant. The mean rejection rates (i.e., power levels) showed the following pattern: (1) In a number

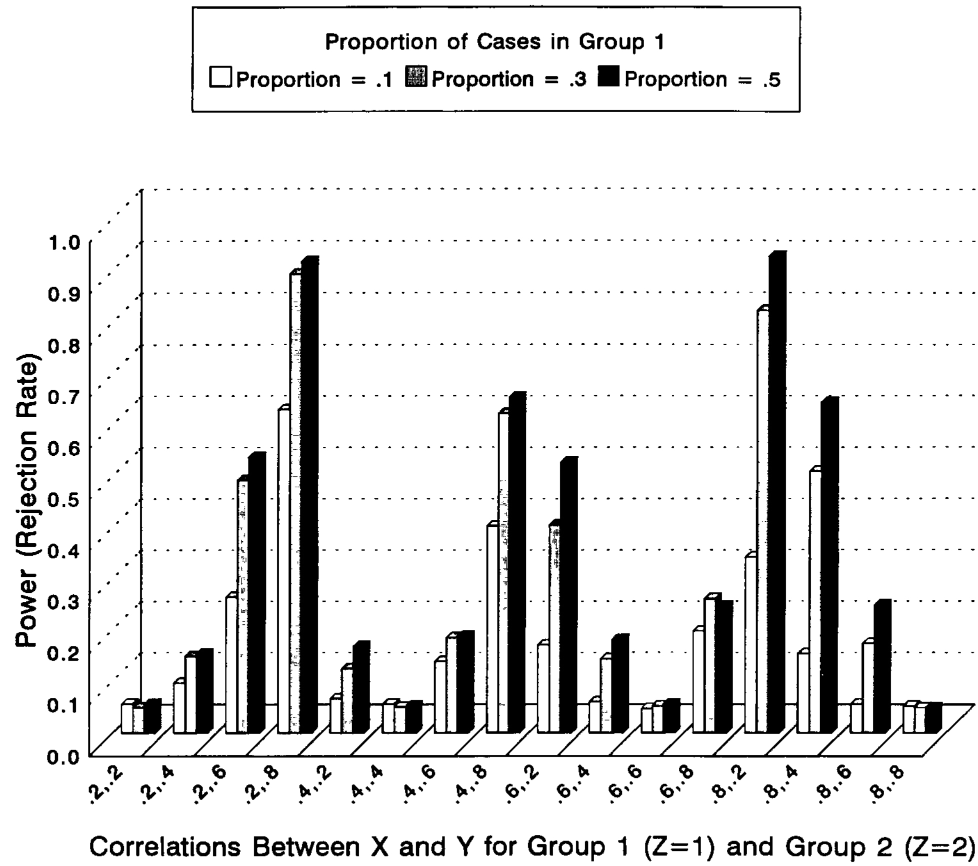

Figure 1. Effects of differences in (a) correlation coefficients for two groups and (b) proportions of cases in the groups on the power of moderated multiple regression to detect moderating effects.

of conditions power was relatively low and increased only slightly as p_i (i.e., the proportion of cases in Group 1 or Group 2) increased from .10 to .50 . Examples of this are the condition for which $N_T = 30$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .40$, and the condition for which $N = 90$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .20$; (2) In several other conditions power was relatively low and increased moderately as p_i rose from .10 to .30, but showed a lesser degree of increase as p_i shifted from .30 to .50. Two examples of this are the condition for which $N_T = 30$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .60$, and the condition for which $N_T = 180$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .20$. (c) In yet other cases power was moderately high for $p_i = .10$, increased markedly as p_i rose to .3, and reached very high levels at $p_i = .50$. Two conditions show this pattern (i.e., that for which $N_T = 90$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .60$, and that for which $N_T = 180$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .40$; and (4) In still other conditions power was very high at $p_i = .1$ and increased only

Table 6. Regression of Empirically Derived Power Levels on Variables Manipulated in the Simulation

Variables entered at step	*b*	*B*	*F*	R^2	ΔR^2
1. Main effects				.83223**	.83223**
p_1	-.46	-.25	5.40*		
N_T	-6.23×10^{-4}	-.20	2.28		
$\Delta\rho$	.09	.07	.48		
2. Two-way interactions				.8652**	.0330**
$N_T \times \Delta\rho$	4.14×10^{-3}	.84	30.05**		
$\Delta\rho \times p_1$	1.64	.61	20.41**		
$N_T \times p_1$	4.98×10^{-3}	.65	17.04**		
3. Three-way interaction				.8734**	0082**
$N_T \times \Delta\rho \times p_1$	-7.38×10^{-3}	-.57	11.13**		

Notes: * $p < .01$
** $p < .001$
b = unstandardized regression coefficient
B = standardized regression coefficient
N_T = total sample size
p_1 = proportion of cases in Group 1
$\Delta\rho$ = absolute difference between Fisher's Z equivalents of $\rho_{XY(1)}$ and $\rho_{XY(2)}$
b_0 = .007088 for the raw score regression equation

slightly or not at all as p_i rose to levels of .3 and .5. A condition that serves as an example of this is that for which $N_T = 300$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .60$. Overall, the results in Tables 1 to 5 show that, in general, high levels of power (i.e., power $\geq .90$) are only assured when three conditions are simultaneously satisfied, i.e., sample size is large (i.e., ≥ 180), the difference between correlation coefficients is also large (i.e., $| \rho_{XY(1)} - \rho_{XY(2)} | \geq .60$), and the smallest of the p_i levels is .30 or greater.

It deserves noting that a $p_1 - p_2$ difference leads to a relatively low drop in power when overall sample size is relatively low (e.g., $N_T = 30$). However, the decrease in power is substantial when overall sample size is relatively high (e.g., $N_T = 180$ or 300). Thus, while a relatively large N_T may do much to enhance the odds of rejecting the null hypothesis that the squared multiple correlation coefficient in the population (Ψ^2) = 0, the power to detect interaction effects can suffer dramatically if there are marked $p_1 - p_2$ differences.

Discussion

Overall, our findings show that the power of MMR to detect moderating effects when the moderator variable is dichotomous is very much a function of the relative sample sizes of the groups for which the strength of predictor-criterion relationships is being compared. Specifically, as the difference between the proportions p_1 and p_2 increases, statistical power decreases.

These findings suggest that the failure of researchers to find moderating effects (e.g., differential prediction) with MMR may be attributable to low statistical power. One example of this is the earlier cited study by Hattrup and Schmitt (1990). Their regression analyses were based upon an overall sample size of about 300. In the case of the test for race-based moderating effects, approximately 10 percent of the subjects were members of the minority group. Assuming the existence of a true difference between the validity coefficients for the minority and white subgroups as large as .20 (e.g., $\rho_{XY} = .20$ in one group and .40 in the other) for the subjects studied by Hattrup and Schmitt, the power of MMR to detect this effect would only be .199 (cf. Table 5). Assuming a lower effect size, power would be even lower. Low statistical power would also seem to be a problem in the study by Cortina et al. (1992) in which numerous MMR analyses failed to show evidence of race-based moderating effects. With a sample size of approximately 300 and assuming a true difference in validity coefficients of .20 (e.g., $\rho_{XY} = .20$ in one group and .40 in the other) the power to detect race-based moderating effects would only be about .38.

Our MMR analysis in which power was the dependent variable showed that not only is power affected by a $p_1 - p_2$ difference, but that the effect of the $p_1 - p_2$ difference on power varies across levels of sample size and the magnitude of the absolute difference between the correlation coefficients (i.e., $| \rho_{XY(1)} - \rho_{XY(2)} |$). For example, if one of the p_i levels is .10, power will be below .10 when $N_T = 30$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .20$, and will rise to only .48 when $N_T = 90$ and $| \rho_{XY(1)} - \rho_{XY(2)} | = .60$. The important implication of this is that when the proportions of cases in two groups differ markedly from one another, there will generally be very low power to detect moderating effects for the levels of sample sizes and correlation differences that are typically found in organizational research. In view of this, under conditions where statistical power is low, researchers should be quite cautious about concluding that there is no moderating effect.

Tables 1-5 show that the rejection rates varied as a function of whether the smallest group had the largest correlation coefficient. For example, Table 3 shows that when $\rho_{XY(1)}$ and $\rho_{XY(2)}$ were .2 and .6, respectively, the rejection rate for $p_1 = .10$ was .263. However, for the same proportion value (i.e., $p_1 = .10$), the rejection rate for $\rho_{XY(1)} = .6$ and $\rho_{XY(2)} = .2$ was .170. This difference in power for constant levels of N_T and $| \rho_{XY(1)} - \rho_{XY(2)} |$ is consistent with the findings of a simulation by Alexander, DeShon, and Govern (1993). Their study assessed the relative power of: (1) the F-test used to test for moderating effects in MMR; and (2) the χ^2 test of the equality of correlation coefficients in two groups. In their simulation different sample sizes and effect sizes (i.e., ρ values within each of two groups) were paired. The χ^2 test was used as the benchmark against which the F-test was compared. Results of their simulation showed that when the smallest group had the smallest correlation coefficient (ρ), the F-test was overly liberal. However, when the smallest group had the largest correlation coefficient, the F-test for moderating effects was overly conservative. This difference in power was attributable to the fact that the standard error of the estimate for the regression equation was greatest when the largest sample size was paired with the smallest correlation coefficient.

It deserves noting that our simulation showed that rejection rates of the null hypothesis of $\beta_3 = 0$ decreased as p_1 decreased, irrespective of whether the p_1 value was accompanied by the largest or smallest correlation coefficient.

The important implication of this is that the ability to find moderating effects using MMR will suffer when sample sizes differ across two groups, resulting in different p_i levels.

On the basis of the findings of Alexander et al. (1993) it seems clear that, in some instances, the χ^2 test should be used in testing for moderating effects since it will be more powerful than the F-test used in MMR. More specifically, the χ^2 test is preferable to the F-test except when the group with the largest sample size has the smallest error variance. Note, however, that this conclusion only applies in instances when the moderator variable is a natural dichotomy. It does not apply when the χ^2 test is used to test for the moderating effect of a variable that was initially continuous but was later converted (i.e., transformed) to a dichotomous variable. Stated differently, power will be greater when MMR is used to test for the moderating effects of a moderator variable that is measured on a continuous scale than it will be for the χ^2 test of the difference between correlation coefficients for a moderator variable that is artificially dichotomous (cf. Stone-Romero & Anderson, in press).

If MMR is used to test for moderating effects, the results in Table 6 should provide researchers with a convenient method for estimating statistical power. Researchers need only use the b-weights in Table 6 in conjunction with their estimates of total sample size, the magnitudes of within-group correlation coefficients, and the proportion of cases in one of the two groups. One possible limitation of this procedure, however, is that our results are based on a simulation that had limited range on all of the predictors. For example, our simulation limited $| \rho_{XY(1)} - \rho_{XY(2)} |$ to .60. Given this, estimates of power derived from the use of our equation should be conservative. This inference is based upon the results of a simulation by Aguinis (1993) that showed that the power to detect moderating effects is attenuated when predictor variables have restricted range.

In summary, our results suggest that researchers should be cautious in their interpretation of null findings when using MMR to test for moderating effects in cases where they are dealing with: (1) a dichotomous moderator variable; and (2) differing proportions of cases in two groups. In such instances, low statistical power (i.e., Type II error) may very well serve as a viable explanation for the failure of MMR to detect moderating effects.

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