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Appendix D: The Econometric Analysis Of The Benefits Of School-Based Mentoring

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Appendix D: The Econometric Analysis of the Benefits of School-Based Mentoring By Amanda Bayer

Basic Regression Analyses

The results presented in Chapter V on the effects of match length on youth outcomes are based on regression analyses. This statistical technique allows us to isolate the effect of match length on individual outcomes by controlling for the effects of other variables, such as race and gender. In cases where the dependent variable is continuous (e.g., school liking, positive classroom behavior), ordinary least squares (OLS) regression was used as follows:

$$Y = a + b_1X_1 + b_2X_2 + \dots + b_KX_K + e$$

where: Y = value of the dependent variable (i.e., the follow-up value for the outcome measure of interest);
 X_k = value of k th explanatory variable, $k=1$ to K ;
 a, b_k = coefficients; and
 e = a stochastic disturbance term with a mean of zero and a constant variance.

In cases where the dependent variable is dichotomous (e.g., fighting in the month prior to the survey, principal's office visits, absences, tardies) logistic regression analysis was used, using maximum likelihood estimation by specifying a linear function for the logit (the logarithm of the odds) of having a positive response on the dependent variable as follows:

$$\log(p/[1-p]) = a + b_1X_1 + b_2X_2 + \dots + b_KX_K + e$$

where: p = the probability of having a positive response on the dependent variable (i.e., the follow-up value for the outcome measure of interest);
 $1-p$ = the probability of having a negative response on the dependent variable; and
 a, b, X and e are defined as in the OLS equation above.

All regressions include explanatory variables for ethnicity, gender, program, length of time between administration of the two surveys, the baseline level of the outcome measure (i.e., the value of the outcome measure at the beginning of the test period), and categorical variables indicating total duration of match (i.e., one indicating whether or not a match is at least nine months in duration, and a second indicating whether or not a match is six to nine months in duration). Regressions for the academic performance outcomes also include grade level.⁴ Analyses using teacher-reported outcomes are based on approximately 80 youth. Analyses using youth-reported outcomes are based on approximately 150 youth.

Table 1 summarizes the results of the regression analyses for the 24 outcome measures. The first two columns of data report the estimated coefficients and statistical significance for the two match-length variables (i.e., the six-to-nine-month and nine-or-more-month groups as compared to the zero-to-six-month group), while the third column records whether matches of six to nine months experience the same effect on outcomes as do longer matches. The last two columns report differences between the three participating programs and the Adjusted R^2 (pseudo R^2 in the case of dichotomous outcome variables), or goodness of fit, of each regression. The coefficients on the match-length variables indicate the additional change in the follow-up value of the outcome measure that youth in each of the two longer-matched groups experience relative to youth who were mentored for less than six months. This change was statistically significant for seven outcome measures, as recorded in the table and discussed in Chapter V.

Additional Analyses

In addition to these basic regression analyses, we also conducted analyses designed to test our hypotheses more rigorously and to compensate for limitations of the data. Specifically, we were concerned about two forms of bias:

Selection Bias. Youth with longer match lengths could differ from youth with shorter matches in ways that we could not account for but that could affect youth's receipt of benefits. For example, if teachers recommend their less motivated students earlier in the school year, these youth may have longer matches than better students. Alternatively, if the more motivated youth remain in the program for a longer period of time, it might appear that longer program participation leads to better outcomes, when in fact only the youth most able and motivated to improve over the school year decide to stay in the program and thus have longer matches. In either case, this type of bias could contribute to spurious associations between match length and benefits.

To help account for selection bias, we tried to use an additional statistical technique—two-stage least squares regression (Heckman, 1976)—that examines the extent to which results are affected by unobserved differences between participants. However, our results are inconclusive because the goodness of fit of the first-stage regressions was very poor; the data set did not have the information necessary to predict match length well enough for each child.

We also tested whether the effects of match length found in our basic results appear within our match-length subgroups (i.e., six to nine months, nine or more months). Our concern was that the positive behaviors demonstrated by youth with more than nine months of mentoring might not have been a result of that mentoring; rather, it could be that the youth who chose to remain in the program from one academic year to the next were more motivated to improve in some unmeasured way. If longer matches do directly lead to better youth outcomes, then we would expect our findings to replicate within the match-length subgroups. The effects of match length were only present for one of the six outcomes (i.e., school liking) in which we found effects using the entire sample, suggesting that selection bias may be affecting coefficient estimates in some of our basic regression analyses. However, these findings may be due, in part, to the small size of the subsamples: in four of the five remaining cases, regression coefficients for either or both of the subsamples were in the same direction and of a larger magnitude than statistically significant coefficients using the full sample.

We further investigated the possibility of selection bias by repeating our basic regression analyses, but eliminating those matches that ended before the follow-up survey for which we still have follow-up survey data. Youth who ended their match early may have traits that cause them to experience relatively poor gains over the test period, causing a spurious positive association between match length and changes in the outcome measures in the original analysis. On the contrary, removing these early-ending matches, about 20 percent of the original sample, actually yielded stronger positive correlations between overall match length and improvements in behavior, indicating that our initial estimates were relatively free of this particular form of selection bias.

Truncation Bias. Another potential limitation of the data stems from the numerical scales used to record teacher and youth reports of outcomes. The highest possible score for many of our outcome measures is “4” or “5.” Thus, a child who starts with a fairly high score at the beginning of the school year could not show a large increase by the end of the year. In this way, one might expect to see smaller positive changes for children who start with better behavior and bigger changes for youth starting out with lower scores. If teachers match their most needy students (i.e., youth scoring lower on our measures) earlier in the year, then we would see a spurious positive association between match length and improvements. If, on the other hand, youth scoring higher at baseline have longer match lengths, then we would see a spurious negative association between match length and improvements.

To explore the extent to which truncation bias exists in our results we took several approaches. First, we ran a series of regressions using only those observations for which the outcome measures were not at extreme values at baseline. Second, we conducted analyses using standard methods of working with limited dependent variables, namely tobit regression and ordered logit. These approaches reinforced our original results: for all of the outcome measures with statistically significant match-length effects in the basic analysis, the size and statistical significance of the effects were at least as large in these additional analyses. Moreover, these methods revealed that improvements in an additional outcome measure, emotional disposition in the classroom, were associated with longer mentoring matches.

Conclusions

Our assessment of the effectiveness of SBM programs presented in Chapter V withstands more rigorous testing as reported in this Appendix. While selection bias remains a concern, we could produce no strong evidence that the basic results are biased; this lack of definitive evidence, however, is largely a result of data limitations. Truncation bias is likely muting our results, and the analyses suggest that the effects of mentoring are even stronger than portrayed by the basic analysis. In sum, we must use extreme caution in interpreting the results reported here, and we recommend that future projects utilize a random-assignment design to determine the impacts of school-based mentoring.

Appendix D: Table 1
Coefficient Estimates for Match Length Variables for 24 Outcome Measures

Outcome	Effect Of Match Length			Program Effects	Adjusted R ²
	6-9 months	9+ months	6-9 months vs. 9+ months		
Social Skills and Networks					
Peer social network	0.082	0.314**			.48
Social skills	0.284	0.482**			.29
Relationships with Adults					
Adult social support	0.162	-0.100			.14
Relationship with parent	0.179	0.383			.14
Parent tells youth school is important	0.325	1.480			.12
Parent involvement in school	0.171	0.079			.55
Perception of teacher's attitude toward child	-0.226	-0.169		Prog. 1 > Prog. 3**	.23
Academic Performance					
Study skills	0.156	0.274		Prog. 3 > Prog. 1*	.59
Language	-0.145	-0.048		Prog. 3 > Prog. 1*** Prog. 3 > Prog. 2**	.45
Social Studies	0.373	0.362			.40
Math	0.598	0.524			.36
Science	0.811	0.510		Prog. 3 > Prog. 2*	.26
Percentage of in-class assignments not completed	0.117	0.103			.21
Percentage of homework assignments not completed	0.111	0.066			.12
Classroom Behavior and Attitude					
Fought in last 4 weeks	0.692	-0.728	9+<6-9*		.21
Positive classroom behavior	0.235	0.592***	9+>6-9*	Prog. 3 > Prog. 2*	.55
Principal's office visit in last 4 weeks	-0.373	-2.318**	9+<6-9**	Prog. 3 < Prog. 2*	.24
Classroom effort	0.115	0.182		Prog. 3 > Prog. 2*	.43
Academic engagement	0.199	0.350*		Prog. 3 > Prog. 2**	.49
Classroom emotional disposition	0.042	0.214		Prog. 3 > Prog. 1**	.44
School liking	0.350**	0.386**			.39
Attendance					
Absence in last 4 weeks	-0.553	-1.024			.10
Tardy in last 4 weeks	-0.021	-0.426			.13
Hygiene					
Hygiene/Appearance	-0.078	0.225		Prog. 3 > Prog. 2*	.42

Notes:

* Estimated coefficient is statistically different from zero at $p < .10$ significance level.

** Estimated coefficient is statistically different from zero at $p < .05$ significance level.

*** Estimated coefficient is statistically different from zero at $p < .01$ significance level.

Appendices Endnotes

- 1 Youth whose teachers completed surveys at the second time point differed from youth without a teacher survey in only three ways: they were less likely to have parents who helped them with their school work, they had case managers who spoke more often with their parent or guardian, and they felt slightly less close to their mentor at the second time point.
- 2 Youth whose mentors completed the survey differed from youth without a mentor survey in the following ways: they were more likely to be female, had met with their mentor longer during the test period and, at the second time point, reported lower levels of adult support and perceived that their teachers had a less positive attitude toward them. Case managers reported that, relative to youth without mentor surveys, these youth had closer relationships with their mentors. Their mentors enjoyed spending time with them more, engaged in more positive behaviors toward the youth and participated more often in agency events. Case managers also reported that these youth had less direct supervision from the agency but benefited from more communication between the case manager and their parents. These youth did not differ in age, grade, ethnicity, single-parent status or any other outcome of interest in the study.
- 3 This is a measure (ranging from 0 to 1.00) of how well a set of variables reflects a single unidimensional construct. In this case, these alphas (or “reliability coefficients”) reflect how well the three items listed intercorrelate to measure “classroom emotional disposition.”
- 4 In an initial set of regressions, we included grade level in all analyses. These analyses revealed significant effects for this variable only when predicting academic performance. Thus, it was only retained in this subset of analyses.