Intergenerational Closure and Academic Achievement in High School: A New Evaluation of Coleman's Conjecture

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This article reexamines the conjecture of James S. Coleman that intergenerational social closure promotes student achievement in high schools, analyzing the best national data on academic achievement and social networks: the 2002 and 2004 waves of the Education Longitudinal Study. The results show that within the Catholic school sector, schools that are characterized by dense parental networks have substantially higher average student achievement. This association can be reduced but not eliminated by conditioning on available measures of student network structure and standard measures of family background. In contrast, in the public school sector, a similarly strong bivariate association between dense parental networks and student achievement can be attributed almost entirely to these basic conditioning variables. These results represent, at best, a mixed verdict for Coleman's predictions. Intergenerational closure in its currently observed form does not increase achievement in public schools, suggesting that parental monitoring of discipline does not outweigh some of the costs of parental closure. However, intergenerational closure may increase achievement in Catholic schools to a modest degree because Catholic schools are affiliated with religious communities that have appropriable norms.

Beginning with an analysis of the High School & Beyond survey in the early 1980s, James S. Coleman and his colleagues developed an important sociological literature on differences in achievement between public and private high schools (for overviews of the research, see Marsden 2005; Schneider 2000). In contrast to public schools, Coleman and his colleagues argued that the functional communities that surround Catholic schools provide social resources that help to compel students' motivation, maintain effective school organization, and increase students' learning (for the specific empirical research, see Coleman and Hoffer 1987; Coleman, Hoffer, and Kilgore 1982; Hoffer, Greeley, and Coleman 1985).

A primary resource at the disposal of these functional communities is the set of extant social ties that results from common affiliation with the Catholic church. Here, parents establish relationships for reasons other than the rearing of their children. These social bonds then allow networks of well-connected parents to win what Coleman depicted as a uniquely modern battle:

> A common complaint of modern parents is their defenselessness against pleas from a child, "A's mother lets her do X; it's not fair if

> I can't," or "All other guys' parents let them do Y, why can't I?" The absence of parental defense lies in the lack of knowledge about what the daughter's friend is allowed to do by her mother, or just what the "other guys' parents" will let them do. Children, knowing the absence of closure among their parents, exploit it, working back and forth to loosen parental rules. (Coleman 1987:188–89)

Coleman's concern with how best to control the antischool behavior of some adolescents has its origins in his 1961 book, *The Adolescent Society: The Social Life of the Teenager and Its Impact on Education.* In his later public-private schools research, Coleman recast this argument by considering the social networks between parents, characterizing communities in which the parents of schoolchildren know each other as rich in intergenerational social closure.

In spite of these benefits of parental networks, Coleman was well aware that network density alone is insufficient to produce a commitment to schooling. Instead, too much density can be harmful in some circumstances. In the same piece just quoted, Coleman characterized the potential harm of parental closure:

> Along with the benefits of a functional community with intergenerational closure come costs. . . . Corresponding to the richness of social texture within the community is a weakness of links to the outside. . . . At its worst, the separatist and exclusionary tendency of a tightly knit community imposes costs on the children of the community. They are not emancipated from the parochialism of their parents; they are not confronted with differing values and the freshness of view such differences can bring. (Coleman 1987:190–91)

Coleman's contrasting predictions about the consequences of parental closure for adolescents' achievement in secondary schooling have stimulated substantial evaluative research.

In a 1999 issue of the *American Sociological Review*, Morgan and Sørensen (1999a, 1999b) reported results from an analysis of the 1988 through 1992 waves of the National Education Longitudinal Study (NELS). They provided evidence that the network density of parents surrounding Catholic schools is conducive to students' learning, which they attributed to Coleman's conjecture that these parental networks create a norm-enforcing environment that effectively focuses students' effort on academic achievement. Yet within public schools, the relationship between parental networks and academic achievement differed. Schools that were surrounded by dense parental networks showed net lower levels of achievement, suggesting that these communities were reinforcing norms that were not as clearly linked to students' achievement. Accordingly, Morgan and Sørensen concluded that parental closure does not generate higher levels of students' achievement in the public sector, suggesting that the costs of intergenerational closure may outweigh its benefits for schools that are not embedded in religious communities with appropriable norms (see also Morgan 2000).

In an exchange in the same issue of the journal, Carbonaro (1999:685) replicated the results of Morgan and Sørensen and noted that "the NELS data do *not* support Coleman's theory of the positive effects of intergenerational closure" (see also Carbonaro 1998). However, Hallinan and Kubitschek (1999) offered reasonable objections to the way in which network associations with achievement were separated out across dimensions of network structure, arguing that Morgan and Sørensen had incautiously built interpretations around the sociological fiction that intergenerational closure can be decomposed into ties between friends and ties between their parents. Since this 1999 exchange, no consensus has emerged on the basis of the additional research of other scholars.[1]

In this article, we revisit this controversy and analyze more recent data from the 2002 and 2004 waves of the Education Longitudinal Study (ELS). Beyond their recency, the ELS data have advantages over the NELS data that have been used in most past evaluations. The ELS social network battery is better than that of the NELS, in part because participants in the earlier debates were asked to advise the technical review panel that designed the ELS. Most important, the NELS data had no information on the characteristics of named friends and was not focused on friendships in school, which Hallinan and Kubitschek (1999) argued represented a mismatch with Coleman's conjecture. Instead, the ELS data are focused on friendships in school and allow for adjustments for some types of friendships. Such adjustments were thought to be important by researchers who analyzed the NELS data, and the results we report in this article support that prediction.[2]

Beyond using a more recent and improved data source, we evaluate the dual nature of the closure conjecture in view of Coleman's alternative predictions about the trade-offs between parental support of collective discipline and the parochialism of isolation. To engage the norm-enforcing versus horizon-expanding interpretations of Morgan and Sørensen (1999a), we model differences in the relationship between parental closure and achievement that exist across public schools and Catholic schools (similar to what they thought were their most informative results in their Table 4). Finally, we estimate a wide range of models, varying the conditioning sets in each and offering alternative interpretations of the results that hold under alternative plausible assumptions about adjustment variables. Thus, although we offer our favored interpretations, we seek to leave other plausible interpretations on the table to guide further work.

METHODS

Data and Analysis Sample

Data were drawn from the 2002 base-year and 2004 follow-up waves of the ELS—a nationally representative sample of students in public and private high schools that was collected by the National Center for Education Statistics (NCES). From among all base-year ELS participants (N = 15,360), we restricted the analysis to respondents who were enrolled in either a Catholic school or a public school during the 2001–02 school year. The resulting analysis sample included 1,918 students who were sampled from 95 Catholic schools and 12,025 students who were sampled from 580 public schools (for a total N = 13,943).

For models in which we analyzed 12th-grade achievement, we then limited the analysis sample to respondents who did not transfer between schools and who were enrolled in the 12th grade at the time of the 2004 survey. This restriction resulted in a 12th-grade analysis sample of 1,660 Catholic school respondents and 8,842 public school respondents (for a total N = 10,502). Because these 10,502 respondents are a nonrandom subset of the 13,943 respondents in our base-year analysis sample, our 12th-grade results incorporate a model-based adjustment for attrition patterns, as we discuss in the next section.

We did not analyze private non-Catholic schools for two related reasons. First, the category is heterogeneous, and the ELS data do not offer enough information to model that heterogeneity effectively. Second, because we could not model the heterogeneity effectively, we could not examine Coleman's alternative predictions about the effectiveness of parental closure in these types of schools (i.e., elective communities that come together narrowly because students and parents have similar educational goals versus traditional functional communities in which closure exists for reasons other than educational strategy). Nonetheless, we recognize that readers may want to know how our main results turn out for non-Catholic private schools. We provide a set of models for private non-Catholic schools in a Supplementary Appendix (on the website of the first author: ftp://hive.soc.cornell.edu/slm45/webpage/Morgan&Todd2009SOEAppendix.pdf), which we summarize in the concluding section of this article.

Modeling Strategy

After offering descriptive results on differences in school sector, we then use two-level hierarchical models to estimate the putative causal effect of parental closure on math test scores in the 10th and 12th grades (see Gelman and Hill 2007; Raudenbush and Bryk 2002). We analyze math test scores for two reasons. First, they are the same outcome variable used in the past literature that has evaluated this conjecture because they are the most reliable measure of achievement that is comparable across schools (see Carbonaro 1999; Morgan and Sørensen 1999a, 1999b). Second, math tests are the only tests that are available for both the 10th and 12th grades in the ELS data.

Our models are straightforward for the 10th-grade data because the base-year analysis sample is a full cross section of high school sophomores in public and Catholic schools in 2002. When the data are weighted by the base-year poststratification weight, the coefficients can be given standard interpretations that generalize to the national target population.

For the 12th-grade results, however, the modeling challenges are more complex. First, our social network measures are available for the 10th grade only. Thus, the 10th- and 12th-grade data cannot be used to set up any version of a classic pretest–posttest design. Rather, both the 10th- and 12th-grade tests are best regarded as two separate posttests, with the difference between them serving as a gain in posttests between 2002 and 2004. Moreover, the achievement tests were not administered in 2004 to students who transferred out of their 2002 base-year school. As a result, our dependent variable is censored for those who switched schools (as well as for students who dropped out, graduated early, or otherwise left the sample). To enable an analysis that incorporates 12th-grade test scores, a supplemental adjustment is needed for those factors that lead some students to exit from the stable group of students for whom we have fully informative data—the 10,502 students out of 13,943 students who remained in the same school between 2002 and 2004 and who were enrolled in the 12th grade in 2004.

As we describe in the Supplementary Appendix, we estimated a logit model from which we then extracted, for each respondent, the predicted probability of being in the 12th grade and in the same school in 2004 as when initially sampled in 2002. We then formed a direct-adjustment weight (see Rosenbaum 2002) that is the poststratification weight from the base-year data multiplied by the inverse probability of being on track in the 12th grade and at the same school in 2004 as in 2002 (see also Morgan and Todd 2008). Under a propensity score-weighting justification (see Imbens 2004 for a review), our 12th-grade models give disproportionately more weight to individuals who were least likely to remain in our analysis sample between 2002 and 2004. Thus, conditional on the suitability of the underlying logit estimation of the odds of remaining in the analysis sample, our 12th-grade results are interpretable as generalizable estimates of what the patterns would have been in the 12th grade if all sophomores had stayed in the same school and progressed to the 12th grade between 2002 and 2004 (and all else remained the same). We present additional details of this procedure in the Supplementary Appendix, where we also describe our usage of best-subset linear and logistic regression for the imputation of item-specific missing data.

RESULTS

Characteristics of Students and Schools by Sector

Table 1 presents the means and standard deviations of math test scores and other variables, separately for students in Catholic schools and public schools. As is shown in the first row, the mean of the math test score was substantially higher for Catholic school students in the 10th grade. The difference of 7.31 points is equal to .52 standard deviations of the math test score among public school students. Moreover, the gap between Catholic and public school students increased between the 10th and 12th grades, as shown in the third row of Table 1. These patterns are consistent with prior results from earlier data (see Hoffer et al. 1985; Morgan 2001).

The characteristics of students' and parents' social networks that we use for our analysis were taken from the base-year sophomore survey. On the student questionnaire, each respondent was asked to answer questions about each of their three closest friends in their present school.[3] The primary explanatory variable, which we label *parents know parents*, is the mean response across up to three nominated friends of whether or not students indicated that their parents knew their nominated friends' parents. As Table 1 shows, Catholic school students indicated that more than two-thirds of their nominated friends' parents were known by their own parents. For public school students, the mean was slightly lower, at .61 rather than .67.

The remaining panels of Table 1 present other variables that we use to adjust the bivariate relationship between parents know parents and math achievement. Student net-

Table 1. Means and Standard Deviations of the Primary Variables

Variable	Catholic Mean	Catholic *SD*	Public Mean	Public *SD*
Math Test Scores				
IRT estimated number right (10th grade)	48.99	12.02	41.68	13.97
IRT estimated number right (12th grade)	56.08	12.80	47.64	15.05
Gain Score (12th–10th-grade IRT estimated number right)	6.66	6.06	4.66	6.48
Parents Know Parents				
(Mean across nominated friends)	.67	.33	.61	.34
Student Network Structure				
Number of friends nominated	2.81	.65	2.73	.78
Same sex (Mean across nominated friends)	.89	.20	.82	.23
Grade below (Mean across nominated friends)	.04	.14	.08	.19
Grade above (Mean across nominated friends)	.08	.18	.18	.27
Female	.48		.50	
Race (White is the reference category)				
Native American	.00		.01	
Asian	.04		.04	
Black	.06		.15	
Hispanic	.11		.16	
Multiracial	.04		.04	
Urbanicity (Urban is the reference category)				
Suburban	.41		.51	
Rural	.01		.21	
Region (Midwest is the reference category)				
Northeast	.31		.18	
South	.23		.34	
West	.16		.23	
Size of 10th-Grade Enrollment	192.28	96.06	377.79	199.21
Learning Disability (as reported by parents)	.05		.09	
Family Background				
Mother's education (in years)	14.77	2.22	13.45	2.32
Father's education (in years)	15.25	2.57	13.59	2.59
SEI score of mother's occupation in 2002 (GSS 1989 coding)	50.55	12.85	44.98	12.86
SEI score of father's occupation in 2002 (GSS 1989 coding)	49.81	11.71	44.15	11.70
Family income (natural log)	11.23	.90	10.60	1.09
Two-parent family	.84		.75	

Source: Education Longitudinal Study of 2002 (2002 and 2004 waves)

Note: N = 1,918 students enrolled in Catholic school and N = 12,025 students enrolled in public school for all variables except 12th-grade math test scores and math gain scores. For these two variables, N = 1,660 students enrolled in Catholic school and N = 8,842 students enrolled in public school. Data are weighted by the NCES poststratification weight BYSTUWT for all variables except 12th-grade math test scores and gain scores. For these two variables, we constructed a weight as BYSTUWT multiplied by the inverse probability of remaining in the same school and not falling behind grade, as estimated from the logit model described in the main text.

work structure is measured in four ways: the number of friends nominated, the proportion of nominated friends who are of the same sex as the student, the proportion of nominated friends in grades below the student (i.e., freshman or lower), and the proportion of nominated friends in grades above the student (i.e., junior or senior). Although these measures capture only four features of students' networks, they are more detailed than those for the NELS data that were analyzed in past evaluations. For Catholic and public school students, these features of network structure suggest that Catholic school students nominated a slightly higher number of friends, at 2.81 versus 2.73. Moreover, Catholic school students were slightly more likely to have best friends of the same sex and who were enrolled in the same grade.

The other sector differences that are shown in Table 1 are also consistent with the literature. In addition to having higher levels of parental education, parental occupational attainment, and family income, students in Catholic schools are more likely to be raised in two-parent families. They are less likely to have a self-identified race other than white, and they are more likely to attend schools with smaller enrollments and located in Northeastern urban areas.

Parental Closure and Math Achievement

Table 2 presents five models that predict math test scores in the 10th grade from variables that were also measured in the 10th grade. Each model is a two-level linear regression, where students are nested in schools. The models are estimated separately by school sector for clarity and in recognition of Coleman's position on the distinctive characteristics of Catholic schools. Later, and in the Supplementary Appendix, we offer coefficient estimates that can be obtained from pooled regression models, where all variables except the parental closure variable are constrained to have common associations with test scores regardless of school sector.

Consider Model 1, where the math test score is regressed on the school-level mean of parents know parents as well as individual-level departures of parents know parents from these school-level means. For the Catholic school variant of Model 1, the school-level mean of parents know parents has a substantial positive association with test scores, as indicated by its estimated coefficient of 18.28. Across Catholic schools, each standard deviation of parental closure is associated with .44 standard deviations on the math test (i.e., 18.28 * .14 / 5.78 = .44, where .14 is the standard deviation of parents know parents across Catholic schools and 5.78 is the standard deviation of the math test scores across Catholic schools). For the public school variant of Model 1, the analogous coefficient is slightly smaller at 16.29. Across public schools, each standard deviation of parental closure is associated with .31 standard deviations on the math test (i.e., 16.29 * .13 / 6.80 = .31).

On average, for individuals within schools, these associations are much weaker at only .06 standard deviations for Catholic school students and only .04 standard deviations for public schools students (i.e., 2.04 * .30 / 10.53 = .06 and 1.56 * .31 / 12.27 = .04). Thus, even though it is hard to compare such associations across levels, especially with recognition that the variance of parental closure is inflated more at the student level by random measurement error than at the school level, it seems fair to conclude that the associations between parental closure and achievement are substantial only at the school level. This pattern is consistent with Coleman's public-good perspective on the benefits of social capital, especially when represented by a collective social network. It is also consistent with results obtained from prior analyses of the NELS data, where net individual-level associations were small in comparison to school-level associations (see Carbonaro 1999:Table 1; Morgan and Sørensen 1999b:Table 1).

Beyond substantive size, statistical inference should be considered as well. The school-level coefficients are significant at conventional levels, having *p*-values less than .05 for two-tailed tests. Thus, conclusions that are based on substantive size and statistical inference are largely in agreement at the school level for Model 1.

At the student level, and using the same cutoff value for statistical significance, the effect of within-school differences in closure is not significant for Catholic school students but is significant for public school students. One may be tempted to argue, therefore, that

Table 2. Coefficients from the Multilevel Regression Models of 10th-Grade Math Test Scores on Network Characteristics of School Communities and Students Within Schools

	Catholic					Public				
Independent Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 1	Model 2	Model 3	Model 4	Model 5
Fixed Effects										
Constant	49.04	49.02	48.39	48.42	48.36	41.79	41.77	41.56	41.58	41.58
School-Level Variables										
Parents know parents	18.28*	16.90*	8.10*	6.46	3.87	16.29*	13.93*	.56	.84	.04
	(4.13)	(4.04)	(3.69)	(3.57)	(5.23)	(2.40)	(2.22)	(1.82)	(1.79)	(1.84)
Number of friends nominated		-1.33	-.49	-.06	-1.56		.08	-.42	-.30	-.49
		(1.30)	(.87)	(.84)	(.98)		(.97)	(.58)	(.54)	(.47)
Same sex		-2.83	-2.55	-1.89	-6.99		-7.07	2.39	3.33	3.47
		(6.08)	(5.74)	(5.65)	(4.88)		(5.31)	(3.24)	(3.08)	(2.99)
Grade below		-29.28	-6.97	-6.06	.36		-30.85*	-2.66	-2.50	1.51
		(14.99)	(10.10)	(9.92)	(11.95)		(5.29)	(3.50)	(3.43)	(3.00)
Grade above		-4.80	2.82	4.43	4.77		-21.30*	-7.11*	-6.91*	-3.82
		(8.62)	(7.40)	(6.91)	(6.51)		(3.54)	(2.32)	(2.21)	(2.09)
Student-Level Variables										
Parents know parents	2.04	2.03	1.62	1.61	1.00	1.56*	1.51*	.66	.65	-.09
	(1.35)	(1.30)	(1.24)	(1.24)	(1.19)	(.47)	(.45)	(.41)	(.41)	(.37)
Number of friends nominated		-.14	.18	.17	.18		.70*	.43*	.44*	.14
		(.47)	(.42)	(.42)	(.34)		(.18)	(.16)	(.16)	(.16)
Same sex		.29	.48	.48	.30		-1.66*	-1.21*	-1.21*	-.57
		(2.40)	(2.08)	(2.08)	(1.65)		(.66)	(.58)	(.58)	(.53)
Grade below		-3.88*	-2.90	-2.90	-2.61		-7.38*	-5.21*	-5.20*	-3.92*
		(1.94)	(1.93)	(1.93)	(1.87)		(.79)	(.67)	(.67)	(.65)
Grade above		-3.61*	-2.36	-2.36	-1.56		-5.15*	-3.60*	-3.60*	-2.27*
		(1.61)	(1.65)	(1.65)	(1.43)		(.58)	(.52)	(.52)	(.49)
Sex, Race, SES, Learning Disability, Urbanicity, School Size			✓	✓	✓			✓	✓	✓
Region				✓	✓				✓	✓
Behavior, Educational Expectations, Factors in Choosing College, Tracking, Parental Involvement and Attitudes					✓					✓
Random Effects										
School-level variance	22.88	22.64	8.21	7.92	8.39	34.89	29.54	9.26	8.62	7.21
Student-level variance	116.66	116.24	104.74	104.75	96.89	158.96	155.41	125.07	125.10	109.21
Number of schools	95	95	95	95	95	580	580	580	580	580
Number of students	1,918	1,918	1,918	1,918	1,918	12,025	12,025	12,025	12,025	12,025

Source: Education Longitudinal Study of 2002 (2002 and 2004 waves)
Note: Data are weighted by the base-year poststratification weight (see note to Table 1). Student-level variables are centered around their respective school means. Standard errors are in parentheses below each coefficient.
* $p < .05$ (two-tailed test).

there is evidence of an individual-level effect in public schools but not in Catholic schools. We do not offer such an interpretation. The sizes of the estimated coefficients are virtually the same, and both are small at .06 and .04 standard deviations of math achievement for each standard deviation of net within-school variation in parental closure. This pattern of statistical significance is almost entirely a consequence of the imbalance in the number of students across sectors—1,918 Catholic school students and 12,025 public school students—which yields an estimated standard error that is nearly three times larger for Catholic school students than for public school students.

Because of situations such as this one, and mindful of the broad literature on the pitfalls of conventional hypothesis testing against point-null hypotheses of zero (see Gelman and Stern 2006 and Gill 1999 for particularly clear statements of the primary issues), we rely mostly on substantive size of estimated coefficients in this article for our interpretations. We use an implicit Bayesian logic, which is consistent with the multilevel modeling tradition (see Gelman and Hill 2007), even though all our most crucial results are statistically significant by conventional standards anyway.[4]

The more important issue, in our view, is causal rather than statistical inference. The bivariate association between parental closure and 10th-grade math achievement estimated for Model 1 is not a warranted causal effect by the standards that prevail in sociology and in this particular literature in the sociology of education. Figure 1(a) depicts a plausible causal diagram in which the effect of parental closure on test score can be identified only by adjusting for the variables background and student network structure that generate back-door associations between parental closure and test score (see Pearl 2000 for the usage of graphs to represent causal relationships). In the absence of such adjustments, the unconditional association between parental closure and test scores cannot be given a causal interpretation because that association is generated, in part, by the dependence of parental closure and test scores on common causes.

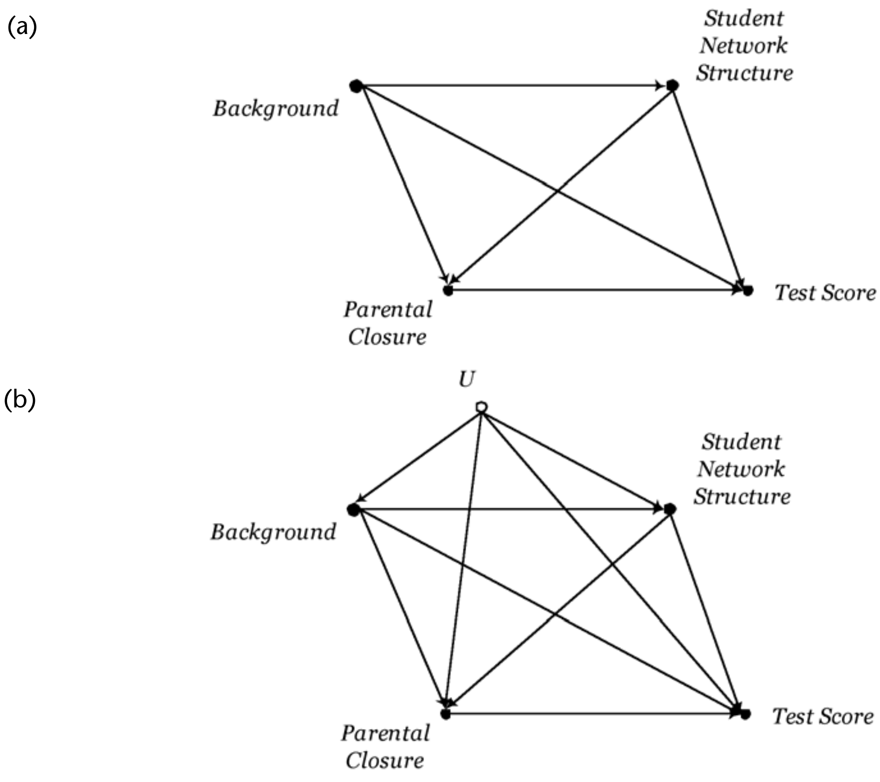

Figure 1. Alternative Assumed Causal Graphs in Which (a) the Effect of Parental Closure Is Identified and (b) the Effect of Parental Closure Is Not Identified

Models 2–4 offer separate attempts to identify and estimate the causal effect of parental closure by back-door conditioning. First, for Model 2, the four variables measuring students' network structure from Table 1 are added to the model, specified analogously at the school level and student level.[5] For Model 3, variables for gender, self-identified race, urbanicity, size of the 10th-grade class, learning disability, family structure, and the five dimensions of socioeconomic status are added to the model. For Model 4, the three dummy variables for region are then added. For these three models, our primary interest remains the relationship between parental closure and 10th-grade math achievement.

Models 2–4 reveal a substantial sector difference in the partial association between parental closure and achievement. While the measures of students' network structure reduce the association to a similar degree in public and Catholic schools in Model 2, the additional background variables added in Model 3 reduce the association in Catholic schools but eliminate the association in public schools.

In particular, the coefficient for school-level parents know parents in Catholic schools decreases from 18.28 in Model 1, to 16.90 in Model 2, and then to 8.10 in Model 3. The association that remains in Model 3 is substantively meaningful, since it is equal to .20 standard deviations of achievement for each standard deviation of parental closure across Catholic schools (i.e., 8.10 * .14 / 5.78 = .20). Even across only 95 schools, this effect is still statistically significant by conventional standards because its *p*-value is less than .05.

In contrast, for students in public schools, the same school-level coefficient declines from 16.29 in Model 1, to 13.93 in Model 2, to only .56 in Model 3. This coefficient implies that the association has declined to only .01 standard deviations of achievement for each standard deviation of parental closure across public schools (i.e., .56 * .13 / 6.80 = .01). And even though the estimated standard error is less than half as large as the standard error for Catholic schools (because there are 580 public schools in the sample compared to only 95 Catholic schools), this coefficient is not close to conventional statistical significance.

Figure 2 presents partial plots of the school-level parents know parents coefficient from Model 3, separately for Catholic schools and public schools.[6] The pattern of residuals (in this case, empirical Bayes residuals) demonstrates that the results are not being driven by any unduly influential cases, and the substantive difference across school sectors is clearly revealed by the differential slopes of the two lines of predicted values.

For Model 4, three dummy variables for region are added to the specification for Model 3. The coefficient for Catholic schools declines from 8.10 to 6.46, which is not quite significant (with a *p*-value of .07, exceeding the conventional cutoff value of .05). In standard deviation units, the effect in Model 4 suggests that .16 standard deviations of achievement result from each standard deviation of parental closure across Catholic schools (i.e., 6.46 * .14 / 5.78 = .16). Inspection of the coefficients for the dummy variables for region suggest that Catholic schools in the Midwest have both slightly higher levels of parental closure and slightly higher academic achievement. As a result, an adjustment for region reduces the net association between closure and achievement for Catholic schools. Because we have no evidence or reason to believe that there is anything inherently better about Catholic schools in the Midwest (especially net of all else already in Model 3), we do not regard Model 4 as inherently better than Model 3. It may simply be a matter of chance that the best Catholic schools in the sample happen to be from the Midwest, and therefore adjusting for region may deprive the genuine causal effect of some of its magnitude.

For public schools, however, the coefficient for school-level parents know parents is straightforward in Model 4 as well. It remains small and nonsignificant, suggesting that only .02 standard deviations of achievement result from each standard deviation of parental closure across public schools (i.e., .84 * .13 / 6.80 = .02). We conclude, on the basis of Models 3 and 4, that there is no support for the existence of a genuine effect of parental closure on math achievement in public schools. The strong associations suggested by Models 1 and 2 are eliminated by adjustments for demographic and family background characteristics.

Nonetheless, there may be some evidence that an effect exists within the Catholic school

(a)

(b)

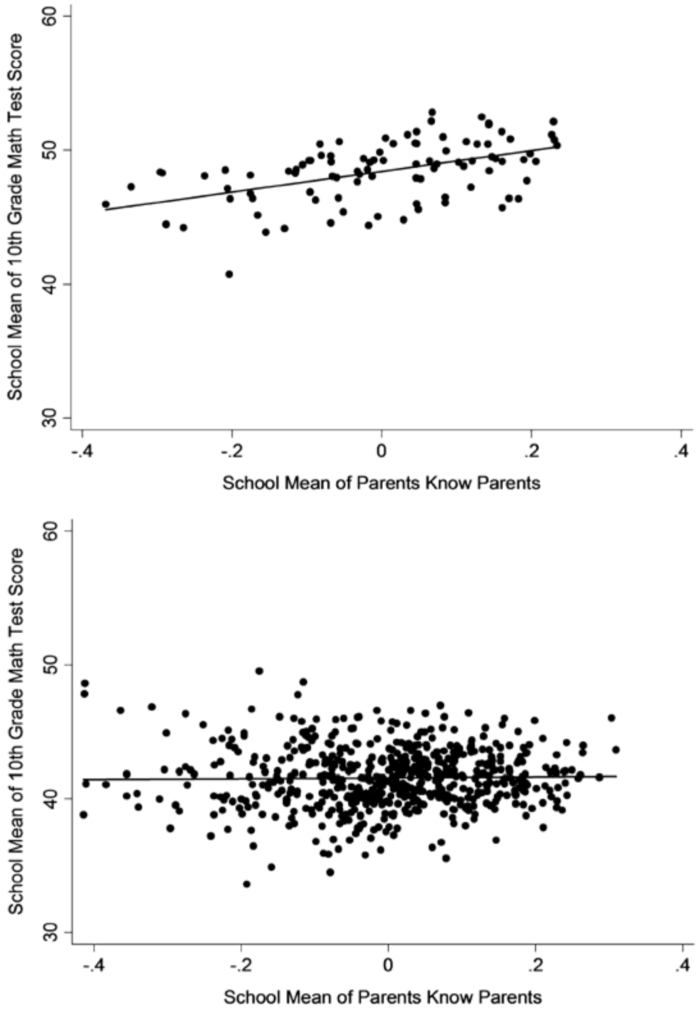

Figure 2. Partial Plots of School Achievement by School Level of Social Closure for (a) Catholic Schools and (b) Public Schools (from Model 3 in Table 2, using EB residuals for each school). Note: Parents know parents is centered around its mean value in each school sector, .67 and .61, respectively.

sector, as Coleman predicted. If the causal model in Figure 1(a) is accepted as valid for either of the conditioning sets in Models 3 or 4, then the results can be treated as evidence of a genuine causal effect within the Catholic school sector. In our view, caution is warranted for two reasons. First, the measures of background and (especially) the measures of student network structure are limited (although better than for the NELS data analyzed in past research on this question). It is therefore possible that the backdoor paths in Figure 1(a) remain unblocked even after conditioning in the way that we have parameterized these regression models. Second, there may be additional unblocked backdoor paths generated by additional common causes of parental closure, students' network structure, and test scores, as would be the case in Figure 1(b).

Additional Examination of the Parental Closure Effect

To examine the empirical support for the parental closure conjecture further, we offer two additional pieces of analysis. We first demonstrate that the remaining association across Catholic schools declines substantially when additional variables are introduced into the analysis. We also show that parental closure predicts substantial growth in achievement between the 10th and 12th grade for students in Catholic schools but much less so for students in public schools.

For Model 5 in Table 2, we introduce an additional 25 predictors. These variables, presented in Table A1, include such variables as parental involvement in school, parental attitudes about school, educational expectations of students and of their parents, patterns of past grade retention, behavioral problems in the 10th grade, and the within-school distribution of students across alternative curricula.

Across Catholic schools, the coefficient for parents know parents declines to 3.87 for Model 5. Thus, these 25 additional variables can account for 40 percent of the net association that was estimated for Model 4. This reduction in the coefficient pushes it undisputedly to nonsignificance, suggesting that the remaining association could well result from sampling error alone. Finally, across public schools, the already small coefficients from Models 3 and 4 decline to .04 for Model 5.

As with much past research in this tradition, no straightforward interpretation of the results of Model 5 is available. The complication is that many of the variables listed in Appendix Table A1 have no unambiguous place in a causal model. Having high educational expectations, for example, could predispose one to attend a better school, endowed with substantial intergenerational closure. In this case, an adjustment for educational expectations would be appropriate. On the other hand, if the closure effect is genuine, then it is reasonable to assume that it should come about, in part, by increasing some proximate motivational determinants of learning that the literature has suggested lead to higher educational expectations. In this case, an adjustment for educational expectations would be inappropriate because such a model would deprive the true causal effect of part of its impact.

Coleman and his colleagues confronted a similar situation when they analyzed the possibility that Catholic schools outperform public schools in general. Describing their final model specifications, they wrote that achievement was regressed

> on a large number of background variables that measure both objective and subjective differences in the home. Some of these subjective differences may not be prior to the student's achievement, but may in part be consequences of it, so that there may be an overcompensation for background differences. It was felt desirable to do this so as to compensate for possible unmeasured differences in family background; but of course the results may be to artificially depress the resulting levels of background-controlled achievement. (Coleman et al. 1982:147)

As a result, appropriate conclusions based on Model 5 depend on alternative plausible assumptions about these additional covariates. If one assumes that they are best interpreted as additional family background causes of achievement, then Model 5 weakens the case for the existence of a causal effect in the Catholic school sector. If one assumes instead that they are best interpreted as mediators of the causal effect, then Model 5 strengthens the case for the existence of a causal effect of closure on achievement in the Catholic school sector. In fact, it would be hard to maintain that the purported effect is causal in the way that Coleman proposed if these variables could not account for at least some portion of the net association between parental closure and achievement. Fortunately, for public schools, no conditional conclusions are required. The small association remains small in Model 5.[7]

There is one other way to assess whether the data support Coleman's conjecture. Although we do not have a parental closure variable for the 12th grade, we do have a 12th-grade math test score to analyze. If the 10th-grade parental closure effect is important, it should not disappear by the 12th grade. Indeed, it should also generate differential growth in achievement between the 10th and 12th grade.[8]

Table 3 presents school-level coefficients for parents know parents, drawn from 16 multilevel models that use 12th-grade test scores to evaluate these predictions. The coefficients are drawn from models with the four specifications of predictor variables as in Models 1–4 in Table 2, estimated separately for Catholic schools and public schools, and estimated separately for two outcome variables—the 12th-grade math test scores in the top panel and the difference between the 12th- and 10th-grade test scores in the bottom panel. In addition, the sample for these 16 models is restricted, as was discussed in the Methods section, to those students who remained in the same school in 2002 and 2004 and who progressed from the 10th grade to the 12th grade between 2002 and 2004. Because the analysis sample declines by about 25 percent, the models reported in Table 3 must be weighted by the direct-adjustment weight described in the Methods section to generate results that are comparable across the two tables.

The pattern of results for 12th-grade test scores in the top panel is similar to those observed for the 10th-grade test scores. The coefficients are larger in Catholic schools than in public schools and decline much more substantially between Models 2 and 3 for public schools than for Catholic schools. The sector difference in the apparent closure effect is smaller in Models 3 and 4 than for 10th-grade test scores, but the same patterns reported in Table 2 hold.

The pattern of results for the math gains models in the bottom panel of Table 3 is also supportive of the general conclusions offered based on Table 2. For Catholic schools, the parents know parents coefficient for Model 1 is 3.72 in comparison to only .37 for public schools. With the inclusion of students' network structure and then background variables in Models 2–4, the school-level coefficients for parents know parents fluctuate for both Catholic schools and public schools. Yet, the coefficients for school-level parents know parents remain substantially larger for Catholic schools.

Table 3. Coefficients for the School-Level Variable Parents Know Parents from Multilevel Regression Models with 12th-Grade Math Test Scores and Math Gains Scores as the Outcome Variables, Using the Same Specifications of Predictor Variables as in Table 2 for Models 1–4

	Outcome Variable: 12th-Grade Test Scores							
	Catholic				Public			
School-Level Fixed Effect	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Parents know parents	21.04*	20.88*	9.07*	6.67	14.42*	14.13*	2.71	3.20
	(4.23)	(4.59)	(3.80)	(4.20)	(2.32)	(2.26)	(1.81)	(1.77)
	Outcome Variable: Math Gains Between the10th and 12th Grade							
	Catholic				Public			
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Parents know parents	3.72*	4.76*	3.32	2.66	.37	.39	1.14	1.22
	(1.50)	(1.80)	(1.75)	(1.98)	(.69)	(.70)	(.85)	(.85)

Source: Education Longitudinal Study of 2002 (2002 and 2004 Waves)

Note: Data are weighted by the base-year poststratification weight (see the note to Table 1) multiplied by the inverse probability of remaining in the same school and not falling behind grade (see the Supplementary Appendix). The Catholic school models are estimated for 1,660 students nested in 95 schools, and the public school models are estimated for 8,842 students nested in 579 schools. Models 1 through 4 have the same specifications of predictor variables as in Table 2. More complete results, reported analogously to those in Table 2, are available in Tables S8 and S9 in the Supplementary Appendix.

* $p < .05$ (two-tailed test).

Supplementary Analysis of Non-Catholic Private Schools

We noted earlier that we do not believe that the ELS data (like their predecessor the NELS data) are informative enough to enable effective modeling of the heterogeneous non-Catholic private school sector. Nonetheless, we offer, in the Supplementary Appendix, a set of tables that are equivalent to the results in Tables 1, 2, 3, and A1 in the main text but for the 76 non-Catholic private schools in the ELS data.

Estimating our basic models in Tables 2 for non-Catholic private schools reveals the following pattern. Models analogous to Models 1 and 2 from Table 2 yield coefficients of 11.43 and 7.67. These coefficients suggest a positive association between social closure and achievement that is substantial (but smaller than for Catholic and public schools and more imprecisely estimated, given large standard errors of 10.31 and 8.37, respectively). When adjustment variables are added, as in Models 3–5, that are analogous to those in Table 2, these coefficients drop precipitously to -6.15, -3.94, and -13.19 (again, with large standard errors of 6.29, 6.23, and 9.43, respectively).

One may conclude from these results that after conditioning on basic background characteristics of the student populations, higher levels of social closure among non-Catholic private schools do not increase achievement. We favor the weaker conclusion that because this category of schools is relatively small and heterogeneous, the results are not very informative.

CONCLUSIONS

Across Catholic schools, parental closure has a substantial association with math achievement in the 10th and 12th grades and with the gains in math achievement between the 10th and 12th grades. This relationship remains substantial after adjustments are performed for the most important dimensions of family background and for all the dimensions of students' network structure that are measured for our data source. However, as much as 40 percent of the remaining net association in our preferred 10th-grade models can then be attributed to additional covariates that can be interpreted as either confounders or mediators. Overall, these patterns weakly support Coleman's conjecture that students' learning may be facilitated by parental closure in the Catholic school sector, which Coleman argued was one plausible explanation for the existence of the most highly effective Catholic schools.

In spite of this limited and qualified support within the Catholic school sector, our results suggest a different set of relationships across public schools. Here, associations between parental closure and math achievement are easily accounted for in our 10th-grade models by student network structure and differences in family background without resort to any of the variables that may be argued to be genuine mediators. Taken together, the findings suggest that parental closure in its form observed in the ELS data is mostly ineffective in the residential communities that surround public schools but may be effective in the functional communities that surround Catholic schools.

DISCUSSION

The findings we have presented in this article are similar to those of Morgan and Sørensen (1999a), but there are substantial differences beyond their comparative recency. On the one hand, we also found some limited support for the existence of a parental closure effect on learning within the Catholic school sector. On the other hand, we did not find a net negative association between parental closure and learning within the public school sector. Instead, we found only a trivially small association between parental closure and achievement in the public school sector after basic adjustments for students' network structure and family background were performed.

How can our findings on the noneffect of closure in public schools be reconciled with the negative effect of closure in public schools presented in Morgan and Sørensen (1999a)? We see at least three possibilities: (1) differences in model specification, (2) changes in the effectiveness of some types of public schools, and (3) revisions to the data collection instrument. We cannot determine definitively which of these three possibilities is most likely, although we suspect that the third possibility is the most compelling.

First, there are some slight differences in model specification that could have led to different point estimates of coefficients.[9] The favored model presented in Morgan and Sørensen (1999a) included a variable "parents have an adequate say in setting school policy." Instead, we used more direct measures of parental involvement (see the five variables listed under parental involvement in Appendix Table A1). Furthermore, we used these parental involvement variables only in Model 5 in Table 2, not in our favored Models 3 and 4 that include adjustments only for student network structure, family background, and other demographic characteristics. Thus, it is possible that Morgan and Sørensen (1999a) obtained a negative effect across public schools because they adjusted for a consequence of the causal effect itself.[10]

Second, it is possible that in the 12 years that passed between when the NELS respondents were sophomores in 1990 and the ELS respondents were sophomores in 2002, patterns of achievement changed within the public school sector. It is possible that the worst public schools are no longer characterized by higher-than-average levels of parental closure (or the converse that the best public schools are no longer characterized by lower-than-average levels of parental closure). We have no evidence to assess this possibility because we do not have a consistent measure of parental closure to rank public schools in 1990 and 2002, respectively.

Third, it is possible that the difference lies instead in the different survey instruments that were used for the NELS and ELS, even though they were both designed by the same agency within the Department of Education and even though the ELS was modeled on the NELS. As we noted earlier, the friends' name generator was placed on the parent questionnaire for the NELS, where it asked parents to list up to five of their child's friends, regardless of whether or not those friends attended the child's school. Given the way the models of Morgan and Sørensen (1999a, 1999b) were parameterized, the lowest performing public schools in the NELS data were those schools for which parents knew up to five of the parents of their child's friends *and* schools in which many of the child's friends did not attend the same school.

For the more recent ELS data analyzed in this article, the name generator was placed on the student questionnaire and prompted students to list only up to three names. Moreover, the questionnaire asked students to list only best friends currently attending the same school. If the out-of-school friends that were not captured by the ELS student questionnaire are more likely than not to have lower average attachment to schooling (as would be the case if they were more likely to be school dropouts, for example), then it is possible that the same negative effect of parental closure on achievement would have shown up in the public school sector for the ELS data. Again, we have no basis for choosing from among these three possible sources of the differences, or others we have not yet thought of, but this third candidate is the most convincing to us.

Finally, our results have a variety of implications for school choice and voucher initiatives, albeit weak ones. Building on Coleman's claims that Catholic schools provide a superior education using fewer financial resources, advocates of school choice have argued that public education can be improved by giving incentives to students and parents to choose from among a broader range of schools (see Chubb and Moe 1990; Howell and Peterson 2002). Not only have advocates of school choice argued that many students would benefit from shifting from public to Catholic schools, they have also argued that the primary benefits of Catholic schooling can be fostered within the public school system itself. One argument, which was first laid out by Coleman himself, is that elective communities of choice may be more likely to become functional communities than may the preexisting residential communities that typically surround public schools. The claim here is that like-minded adults are more likely to be able to agree on how to monitor students and schools to compel students' learning.

In this article, we have not attempted to identify the Catholic school effect on learning, and thus we have not examined the most important policy legacy of Coleman's research on public-private schools (but see Morgan and Todd 2008). Our results do have some implications for these policy proposals because they suggest that the highest performing Catholic schools are embedded in dense functional communities. If one accepts Coleman's position that these dense networks are public goods, then shifting a small number of public school students into high-performing Catholic schools may boost achievement for these students without otherwise affecting the schools that accept them.[11]

Nonetheless, our results provide no evidence to support the claim that elective communities will emerge with effective and dense parental networks if school-choice programs are unleashed at a large scale. Such communities could emerge, but one would have to assume a rather dramatic shift in the way that parents relate to each other in the public school sector. It would certainly be unwarranted to assume that functional communities will necessarily arise when it is clear that Catholic schools have close ties to a major norm-reinforcing institution that has no clear counterpart in the public sector.

NOTES

1. For results that provide some support for the closure hypothesis, see Bankston and Zhou (2002) and McNulty and Bellair (2003). For research that has generally been critical of the closure hypothesis, see Horvat, Weininger, and Lareau (2003); John (2005); Pribesh and Downey (1999); and Sandefur, Meier, and Campbell (2006).

2. Nonetheless, the ELS data are still limited. The survey does not ask students or their parents to report directly on tie-formation processes, the depth of contact associated with ties between parents, or the types of cooperation made possible by ties between parents. The National Longitudinal Study of Adolescent Health does have comparably rich network data, but it has no high-quality achievement tests.

3. Students were instructed: "Please write down the names of your best friends at your present school. Please fill in up to three names. If you have fewer close friends, provide less than three names. Then for each friend you named, answer questions 25a through 25g."

4. At the request of reviewers and the editor, we included asterisks in our tables to signify p-values < .05 for coefficients from two-tailed tests with 0 as the null hypothesis. See Leahey (2005) for an explanation and critique of the usage of asterisks in sociological research.

5. We hesitate to interpret the coefficients for the adjustment variables for student network structure because our models are specified with the goal of estimating the causal effects of parental closure. If we were to attempt to estimate the causal effects of out-of-grade friendships on learning, for example, we would need to specify alternative models explicitly for that purpose, first beginning with a model, such as Model 1, with this variable alone and then specifying appropriate adjustment variables that are based on the literature on friendship formation, as well as the literature on school achievement.

6. In the Supplementary Appendix, we provide partial plots for all 10 models reported in Table 2.

7. The models in Table 2 can be estimated by pooling Catholic and public schools. If all two-way interactions with sector are parameterized, then the same coefficients presented in Table 2 can be generated. However, the pooled model offers one additional possibility, which we report here because it may be of interest to readers. While allowing the parents know parents coefficients to vary across school sector, the association between achievement and all other predictor variables can be constrained to be the same across sectors. Such a parameterization is, in fact, closer to the sort of pooled models reported in Morgan and Sørensen (1999a, 1999b) and Carbonaro (1999). We report five such models in Table S7 of the Supplementary Appendix, corresponding to the 10 separate models reported in Table 2, and they support the same substantive interpretations offered in the main text. For Models 1–5, the estimated coefficients for the school-level main effect of parents know parents are 16.28, 13.98, .75, 1.07, and .20, respectively. The estimated coefficient for the interactions between school-level parents know parents and Catholic school are 2.01, 1.70, 5.09, 5.84, and 5.56. Thus, as in Table 2, the difference across sectors increases between Models 2 and 3 (when the interaction jumps from 1.70

to 5.09) and then fluctuates slightly for Models 4 and 5.

8. Of course, these predictions would hold only if it is reasonable to assume that parental closure does not generate a countervailing suppressor effect that eliminates the increases in achievement modeled for the 10th grade in Table 2.

9. There is one notable difference in model specification between our results and those of Morgan and Sørensen (1999a) that does not explain the difference. Morgan and Sørensen reported gain score models with a lagged variable for 10th-grade achievement as a predictor variable. For our similar models summarized in Table 3, we did not include a lag. We determined that this difference is inconsequential for our interpretations in two ways. First, Morgan and Sørensen provided a supplementary appendix, in which they showed, in Table S-2, that they arrived at the same basic conclusions as in their main text when they removed the lag variable, which makes their specification similar to ours in this article. Symmetrically, we provide in our Supplementary Appendix a set of models in Table S10 that adds the 10th-grade lag variable to our models. We obtained slightly different estimates, but the same pattern of results prevails.

10. Nonetheless, they noted in their footnote 8 (p. 670) that this is not likely to have occurred.

11. Note our "small number" qualification. School-choice programs could backfire in this regard if too many public school students enroll in these high-performing Catholic schools in response to the introduction of a voucher program. Average levels of parental closure could then decline precipitously with the introduction of additional parents into the school community who have no ties to the dense functional community created by a common affiliation with the Catholic church.

APPENDIX

Table A1. Means and Standard Deviations of Additional Variables for Model 5 in Table 2

	Catholic		Public	
Variable	Mean	*SD*	Mean	*SD*
Behavior				
Number of times suspended this year	.08	.57	.37	1.23
Number of times on probation this year	.09	.57	.22	.92
Ever held back prior to this year	.05		.13	
Repeated 4th grade	.00		.00	
Educational Expectations for Student (in years)				
Student	17.43	1.77	16.47	2.25
Mother	17.16	1.81	16.50	2.24
Father	17.15	1.86	16.41	2.28
Factors Important in Choosing Future College				
Curriculum important	1.22	.42	1.19	.37
Athletics important	2.46	.65	2.26	.72
Low crime rates important	1.14	.37	1.13	.34
Academics important	1.15	.36	1.20	.39
Tracking Characteristics of School				
Percentage college prep	89.11	23.44	55.47	28.69
Percentage remedial reading	1.59	2.80	5.43	7.77
Percentage remedial math	3.30	7.19	7.01	8.99
Parental Involvement in School Organizations				
Parent belongs to parent-teacher organization	.40		.22	
Parent attends parent-teacher organization meetings	.44		.34	
Parent takes part in parent-teacher organization activities	.50		.26	
Parent volunteers at school	.52		.25	
Parent attends other organization	.41		.28	
Parental Involvement and Attitudes About School				
Parents invest in community	.85		.75	
School assigns too little homework	1.91	.60	2.19	.59
Children challenged at school	3.17	.57	2.84	.56
Child works hard	3.11	.66	2.92	.64
School prepares students for college	3.43	.53	2.89	.58
Number of Years Parents Lived in the Community	12.90	8.21	10.56	8.00

Source: Education Longitudinal Study of 2002 (2002 and 2004 waves)

Note: N = 1,918 students enrolled in Catholic school and N = 12,025 students enrolled in public school. Data are weighted by the base-year poststratification weight (see the note to Table 1).

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