NBER WORKING PAPER SERIES

LOCAL VIOLENCE, EDUCATIONAL ATTAINMENT, AND TEACHER PAY

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Working Paper 6003

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 1997

I thank Nick Ronan, Steve Trejo, Jim Walker, and participants at the NBER Summer Institute for helpful comments. This paper is part of NBER's research program in Labor Studies. Any opinions expressed are those of the author and not those of the National Bureau of Economic Research.

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NBER Working Paper No. 6003
April 1997
JEL Nos. I2, J1
Labor Studies

ABSTRACT

Violence in and around schools has drawn increasing attention lately from both the public and policy makers. Despite the importance of the problem, however, research on this topic has been limited. In this paper I analyze how local violence affects high school graduation, college attendance, and teacher pay. Using data from the High School and Beyond survey, I find that local violence has important effects. Moderate levels of violence reduce the likelihood of high school graduation by 5.1 percentage points on average, and lower the likelihood that a student will attend college by 6.9 percentage points. They also raise teacher salaries by 2.4 percent.

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I. Introduction

School violence has drawn increasing attention lately from both the public and policy makers. Despite the importance of the problem, however, research on the topic has been limited. Most of the existing work has focused on determining simply how many students are victimized.[1] A smaller literature has analyzed the antecedents of school violence (Gottfredson and Gottfredson 1985; Menacker, Weldon, and Hurwitz 1990). Knowledge of its consequences, however, is quite limited. The principle findings to date are that students who fear attack at school, or who have been attacked, are more likely to stay at home for reasons other than illness (Pearson and Toby 1991, 1992; Lab and Whitehead 1992).

School violence easily could have more far-reaching consequences, however. Students concerned for their safety may find it difficult to concentrate. As a result, their achievement and advancement may suffer. Moreover, students who stay at home may fall behind, and students who fall behind are at greater risk of dropping out (Cairns, Cairns, and Neckarman 1989; Grissom and Shepard 1989).

Indeed the consequences of school violence may extend beyond the educational attainment of students. The theory of compensating differentials predicts that teachers would demand a wage premium in order to accept work in violent schools. Such violence-induced wage premia in turn may have important consequences for school finance. Taxpayers in violent school districts must pay for higher salaries either directly, in the form

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1 This includes National Institute of Education 1978; Gottfredson and Gottfredson 1985; Bastian and Taylor 1991; Collins, Messerschmidt, and Ringwalt 1992; Lab and Whitehead 1992; Pearson and Toby 1991,1992; Ralph et al. 1994.

of higher taxes, or indirectly, in the form of reduced expenditures for other educational inputs.

Ideally, one would like to know how school violence affects student performance and teacher pay. Estimating the effects of school violence raises several important identification issues, however, some of which are common to all studies of educational production functions, but some of which are unique to the study of school violence. First, a negative association across schools between the level of violence and the graduation rate may indicate merely that violent students are less likely to complete high school. This would not be a particularly interesting finding. By far the more interesting question involves the peer group effect: how violence on the part of one student affects the educational attainment of another.

If the violence-proneness, or type, of each student were observed, then it would be straightforward to estimate the peer group effect. One would simply regress educational performance on the level of violence in the school, while controlling explicitly for each student's type. Although the violence-proneness of students cannot be observed explicitly, the survey I analyze provides a number of indicators of behavior problems which collectively may provide an adequate proxy.

The second issue concerns the objectivity of the school violence measures. My violence data come from principal reports. At one extreme, one might fear that principals answer survey questions strategically, providing answers that rationalize their students' poor performance. Such strategic responses would cause regression estimates of the effects of school violence to be overstated. At the other extreme, principals may not want to reveal

the true level of violence in their schools. If so, then principal reports may lead to regression estimates that understate the effect of violence.

Another concern is that the level of violence in a school may be correlated with the overall level of disorder, or with a "bad school" effect more generally. Without controlling for the factors that contribute to the bad school effect, a regression model would attribute to school violence part of the effect actually due to those other factors, overstating the effects of school violence per se. My approach here is to control explicitly for a number of factors that contribute to a bad school effect.

Finally, there is the question of whether the effects of violence in the school can be distinguished from the effects of violence in the neighborhood. In a system of neighborhood schools, this distinction is essentially impossible to make. Students live near their schools, so violence in their schools in part may reflect violence in their neighborhoods. Without experimental data measuring how children from violent neighborhoods perform when sent to non-violent schools, its is hard to imagine how the two effects could be disentangled.

Nevertheless, it still may be possible to estimate approximately how school violence and neighborhood violence together affect student performance and teacher pay. Moreover, depending on the types of remedial policy one envisions, their joint effect may be of substantive interest. For example, a finding that local violence adversely affects educational attainment would bolster arguments in favor of school choice policies, which may permit students both to attend a non-violent school and to escape a violent neighborhood, at least for part of the day.

The results from this study contribute to three branches of research, the first of which is the substantial literature on the effects of school quality. Betts' (1996) survey includes several papers that have analyzed the effects of various school inputs, including peer groups, on students' educational attainment. In general, he finds little relationship between school inputs and student performance. None of the studies in his survey include estimates of the effects of school violence, however, possibly because school violence is a relatively recent phenomenon.

My results also contribute to the literature on compensating differentials in the labor market. In their study, Antos and Rosen (1975) found broad support for the notion that teachers earn wage premia for dealing with more difficult students. Their study did not consider the effects of violent students, however, in all likelihood because the problem was perceived as minor at the time.

Finally, this study also can be viewed as an extension of the growing literature on the economic consequences of crime. Much of this research has attempted to determine how involvement with the criminal justice system affects the labor market opportunities of criminals (Bound and Freeman 1992; Freeman 1992, 1994; Grogger 1992, 1995a, 1995b; Waldfogel 1994). This study, in contrast, seeks to estimate the consequences of local violence on students. If local violence affects youths' human capital acquisition, then it affects their lifetime earnings potential as well. Viewed this way, violence in and around schools may be a particularly costly form of crime.

II. Data

I analyze data from two surveys. The High School and Beyond (HSB) study provides data on students' educational attainment and principals' reports of school violence. The HSB Sophomore Cohort includes roughly 15,000 students who were tenth-graders in 1980, drawn from 1000 schools nationwide. It is a stratified sample, with a particularly large representation of Hispanics. The Administrator and Teacher Survey (ATS), a 1984 augmentation of the HSB, provides data on teacher salaries.

A. Dependent Variables

In the HSB's 1984 and 1986 follow-up surveys, survey respondents answered questions about higher education and employment. From their responses I construct two measures of educational attainment: a dummy variable equal to one if the student graduated from high school, and another dummy variable equal to one if the student attended a four-year college. The graduation dummy excludes GED recipients. Although it might be useful to analyze GED receipt as well as traditional graduation, in this sample such a measure would be incomplete: nearly 40 percent of GED recipients obtain the credential after age 24 (Murnane, Tyler, and Willett 1996), whereas the 1986 HSB follow-up was conducted when the survey respondents were 22.

The college dummy equals one for students who matriculated at a four-year college at any time within the first four years after completing high school. Although 74 percent started "on time", that is, by the end of 1982, extending the matriculation window to 1986 accomplishes two objectives. First, it captures students who began college at two-year institutions but ultimately transferred to a four-year school. Second, it captures students

who interrupted their schooling with a break after high school. To the extent that students interrupt their schooling as a result of violence in their high school, omitting these students by imposing a short matriculation window could lead to exaggerated estimates of the effects of school violence on eventual college attendance.

The teacher salary data come from the Administrator and Teacher Survey (ATS). In 1984, the ATS interviewed roughly 10,000 teachers in approximately 450 of the original HSB schools. In addition to salary data, the ATS collected information about teachers' education, their past experience, their current duties, and their salaries.

B. Measuring School Violence

The school violence measures come from the 1980 HSB School File. This file contains principals' responses to many questions about school conditions, covering such typical items as enrollment, attendance, class sizes, and curriculum emphases. These files also contain three valuable measures of school violence. Principals were asked to indicate the extent to which their school suffered from: (1) fights among students; (2) conflicts between students and teachers; and (3) students bringing weapons to school. Thus the survey provides violence measures that may reflect different levels of severity.

Table 1 provides estimates of the prevalence of violence in U.S. schools and its distribution by race. Although this information was obtained from school principals, I have merged the principal responses onto the student records, so the unit of observation here is the student. I have also used the sampling weights in these calculations, so the figures can be interpreted as nationally representative student-weighted means. The principals indicated

whether, in their schools, each type of violence was serious, moderate, minor, or non-existent. Panel A presents the distribution of violence based on these principal reports.

The first two columns provide estimates for the total population of students, disaggregated by public and privates schools.[2] The distribution of violence varies quite sharply between the two types of schools. Almost 1 in 10 public school students attends a school in which fights among students present a moderate to serious problem, whereas there are no private schools in these categories. Minor fighting problems are so prevalent as to be commonplace in public schools, affecting three-fourths of all public-school students. Moderate to serious conflicts between students and teachers are slightly rarer in public schools, although minor conflicts between students and teachers are nearly as common as fighting among students. Minor conflicts between teachers and students are surprisingly common in private schools, which may result from the wording of this particular questionnaire item.[3] Moderate to serious weapons problems are more unusual, affecting only 1.1 percent of all public school students. Nevertheless, because there were roughly 13 million students enrolled in public high schools in 1982, this figure indicates that about 143,000 students attended public schools where weapons constituted a substantial problem. Minor weapons problems are surprisingly prevalent, affecting 39.7 percent of all

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2 Private schools here refer to both Catholic and non-sectarian schools. Preliminary analyses revealed the distribution of violence across these two types of schools to be quite similar.

3 School questionnaire item 56G asked principals to rate the severity of problems in his/her school involving "physical conflicts among students," whereas the next item, 56H, referred to problems involving "conflicts between teachers and students." The absence of an explicit reference to physical conflict may have introduced some ambiguity into this item. It is possible that public school principals, most of whom had answered the previous question affirmatively, interpreted the question as referring to physical conflicts, whereas private school principals, having answered the previous question negatively, interpreted the question more broadly.

public school students. In contrast, weapons are effectively absent from private schools, with only 4.4 percent of private schools reporting even minor weapons problems.

Although these questionnaire items provide some insight into the prevalence of school violence, each item captures only part of the problem. It seems desirable to construct an index of violence that incorporates and summarizes information from all three questionnaire items. If one could rank the severity of the various questionnaire responses on some a priori basis, then an index could be constructed from that ordering. Unfortunately, it is difficult to establish such an a priori ranking. For example, are moderate weapons problems more or less severe than serious fighting among students? Given the lack of prior information, it seems desirable to let the data suggest how to combine the three questionnaire items into a more comprehensive index of the level of violence in each school.

Some preliminary analyses revealed that student attainment and teacher pay were best explained by different types of school violence. For this reason, I constructed two indexes which give different ranks to different types of violence. I summarize my procedures here and explain them in detail in Appendix A.

Given the small number of responses in the "serious" category, it was first necessary to collapse the "serious" and "moderate" responses into a single category, which I refer to as "substantial". These three collapsed responses to the three violence questions provide a total of 27 possible response sequences. The question is whether these response sequences indeed reflect 27 distinct levels of violence, or whether their dimension can be reduced in an interpretable manner.

To construct an index of violence that explains student performance, I constructed dummy variables for each of the 19 observed response sequences (8 of the 27 cells were empty), then regressed the college attendance dummy on the full set of response sequence dummies, controlling for a number of other factors as well. There was substantial clustering among the coefficients, indicating that the dimensionality of the violence index indeed could be reduced. In grouping the response sequences, I attempted to balance the goals of explanatory power and interpretability. For example, of the nine sequences that reflect substantial weapons problems, all but one had large, negative coefficients. If I were interested only in minimizing the residual sum of squares, I would have grouped these eight response sequences together into one category. Because the resulting index would have been difficult to interpret, however, I instead grouped all nine response sequences that reflect substantial weapons problems. This group makes up the category labeled as "serious" on the index that I use to explain student performance (which I term the student index).

The next ranking, which I label as "moderate", goes to schools that do not have substantial weapons problems, but in which fights among students pose a substantial problem. The remaining schools are classified as having "minor" or "minimal" problems with violence, where the minor classification goes to schools for which the principal reported both minor fighting problems among students and minor conflicts between students and teachers.

For the index that I use to explain teacher salaries, which I term the teacher index, I followed a similar procedure, regressing teacher pay on the full set of response sequence

dummies. The most serious ranking on the teacher index goes to schools whose principals reported substantial conflicts between students and teachers. Among the remaining schools, schools were ranked as having a "moderate/minor" problem if the principal indicated that there were weapons problems of any kind. All remaining schools were ranked as having "minimal" problems.[4]

It is important at this point to clarify some possible misconceptions about these violence indexes. One possible criticism is that these violence indexes are essentially guaranteed to be significant in the educational attainment regressions. This is not the case.

If violence had no explanatory power for student attainment, then it would be impossible to construct a significant index. What is true is that, due to the pre-testing procedure used to construct the indexes, the significance levels of the violence indexes in the college attendance and teacher pay regressions will be overstated. The important point, however, is that the 19 response sequence dummies on which the indexes are based were themselves highly significant in the original regressions used to construct the indexes. As one can see in Appendix table A1, all but one of the coefficients were negative, and eight were significant at the 5 percent level. The joint F-statistic was 3.07, which is significant at any conventional level. Thus the indexes are simply a convenient means of summarizing a

4 It proved impossible to rank 15 public schools, which account for 357 teachers and 442 students, and several private schools. To construct the teacher index, I first constructed 27 dummy variables, where each dummy corresponded to a unique combination of values from the three principal questionnaire items. I then fit a teacher salary regression with these 27 dummies, and collapsed the 27 categories into three on the basis of their regression coefficients. In four cases, one of the original 27 cells contained only one school, and in five cases, a cell contained only 2 or 3 schools. In these cases, it is (essentially) impossible to distinguish the effect of school violence from a pure school effect. I therefore drop these schools from all analyses involving the teacher index. This problem did not arise in constructing the student index, for which a larger sample of schools was available.

significant relationship between college attendance and the original, rather unwieldy, set of 19 violence dummies.

Furthermore, any remaining concerns one might have regarding the reported significance levels of the school violence coefficients should be restricted to the college attendance and teacher salary regressions, since these were the only dependent variables used to construct the indexes. In other words, the violence coefficients in the high school graduation regressions are unaffected by pre-test estimation bias. Their significance levels can be interpreted in the usual way.[5]

Returning now to substantive issues, we see in Panel B of Table 1 that, according to both ranking schemes, relatively few public schools, and no private schools, have serious violence problems. However, large numbers of schools experience either moderate or minor levels of violence. Both of the violence indexes also show that, within public schools, the burden of serious school violence falls disproportionately on racial and ethnic minorities. The racial distribution of lesser levels of violence varies between the two indexes, however. According to the student index, minor violence problems within public schools are nearly invariant to race. According to the teacher index, however, blacks are more likely than whites or Hispanics to attend public schools with minor to moderate levels of violence. This difference across indexes reflects the different rankings that the indexes assign to the specific violence questions. Whereas intermediate levels of fighting among students and conflicts between students and teachers are essentially independent of race, blacks are much more likely to attend public schools with moderate levels of weapons-related violence.

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5 McManus et al. (1983) used a similar approach to reduce the dimensionality of a number of language proficiency questions in analyzing the relationship between English proficiency and wages.

Finally, it would be useful to know whether these violence measures reflect only the level of violence faced by students in their sophomore year, or whether they reflect violence that is more persistent. It is only possible to provide a limited answer to this question. The HSB administered a second school survey in 1982, when the students in the Sophomore Cohort were seniors. Unfortunately, with only one exception, the violence-related questions were worded differently in 1982 than in 1980. The exception is the question about weapons. Table 2 provides a school-level cross-tabulation that provides some evidence on the persistence of weapons-related violence over the Sophomore Cohort's high school career. It shows that, among schools reporting any weapons-related violence in 1980, 72 percent also reported weapons-related violence in 1982. Similarly, among schools reporting no weapons-related violence in 1980, 70 percent reported no such violence in 1982. Thus the weapons component of the violence indexes, at least, reflects violence that is fairly persistent over the duration of the sample members' time in high school.

III. Estimation

Although the descriptive comparisons above between public and private schools reveal some interesting facts about the distribution of school violence, for the remainder of the paper, I restrict attention to public schools. I do this for three reasons. First, the vast majority of American children attend public schools. Second, private schools may be able to take measures to exclude violent children that are unavailable to public schools. Finally, the number of students in the private school subsample of the HSB is too small for meaningful analyses.

A. Student Performance

Consider now the problem of estimating the effects of school violence on the performance of students who attend public schools. I begin with a model in which school and neighborhood violence appear separately, in order to analyze the consequences of omitting neighborhood violence from the model. The regression model is given by:

$$(1) \qquad y_{ij} = X_{ij}\beta + V_j^S\gamma^S + V_j^N\gamma^N + C_j\delta + u_{ij}, \quad i = 1,\ldots,n_j; j = 1,\ldots,N.$$

The variable y_{ij} represents the educational attainment of the ith student in the jth school. The vector X_{ij} includes individual-specific regressors. The vector V_j^S contains the measures of violence in the jth school, the vector V_j^N contains measures of neighborhood violence pertaining to the jth school, and the vector C_j contains all other school-specific factors. The term u_{ij} is an unobservable disturbance term, assumed to have zero mean and to be independent across students in different schools.[6] The number of individuals in the jth school is n_j; there are N schools in the sample. The terms β, γ^S, γ^N, and δ are the regression coefficients to be estimated. In particular, γ^S gives the effect of school violence on student performance, and γ^N gives the effect of neighborhood violence, controlling for all the variables included in X_{ij} and C_j.

Without data on neighborhood violence, it becomes impossible to estimate the effects of school and neighborhood violence separately. Under the assumption that u_{ij} is uncorrelated with V_j^S, V_j^N, X_{ij}, and C_j, and assuming for simplicity that V_j^S and V_j^N are

6 Because the violence measures only vary between schools, however, it is particularly important to allow for groupwise dependence among observations within the same school in computing standard errors. See Moulton (1986).

measured as scalars, the probability limit of $\hat{\gamma}^S$ when V_j^N is omitted from equation (1) is given by

(2) $$plim\ \hat{\gamma}^S = \gamma^S + \gamma^N \theta_{NS}$$

where θ_{NS} is the coefficient on V^S in the population regression of V^N on V^S, X, and C. I refer to the term on the right-hand side of equation (2) as the effect of local violence. In the case where V^N and V^S are (conditionally) perfectly correlated, the estimate of γ^S identifies the sum of the effects of school and neighborhood violence on the outcome. In the more likely case where school and neighborhood violence are positively but imperfectly correlated, the regression identifies a hybrid parameter which may be greater or less than the sum of the two effects. If the (conditional) variance in school violence exceeds the (conditional) variance in neighborhood violence, so $\theta_{NS} < 1$, then the estimated parameter will be less than the sum of the two effects. Intuitively, this condition will hold if the distribution of violence across schools includes more extremes, both large and small, than the distribution of violence across neighborhoods.

Data on the other explanatory variables in the model, included in X_{ij} and C_j, come from various sources. The HSB student survey provides a number of student background characteristics such as family structure, parental education, and income. The family background measures I include in X_{ij} are a dummy variable indicating that the student did not live with his father, dummy variables indicating the education level of each parent (the categories are high school dropout, high school graduate, more than high school but less than a college degree, and college degree or higher), and a dummy variable indicating that the student's family income was less than $8000 per year.

The survey also contains many valuable proxies for each student's own proneness to violence. Students reported on whether they had ever been suspended from school; whether and to what extent their peers viewed them as troublemakers; whether they had been in trouble with the law; and whether they had had discipline problems in school. These are admittedly imperfect controls, because students may have disciplinary problems or run afoul of the law for reasons unrelated to violence. Nevertheless, since fighting and weapons possession are often causes for strong disciplinary (or legal) action, these indicators of student behavior problems may provide reasonably good controls for the violence-proneness of the student.

The HSB School File contains data on many school characteristics, including class sizes, school enrollment, expenditures per pupil, and the level of racial segregation. It also includes measures of the extent of vandalism problems in the student's school. The vandalism data are reported by the principal, and coded in the same way as the reports of fighting and weapons. I include this measure as an attempt to distinguish the effects of school violence per se from the more general effect of school disorder. All of these factors may be important determinants of student performance. Since they may also be correlated with the level of violence in the school, it is particularly important to include these variables in the C_j vector in the regression model.

Because the HSB includes multiple students per school, I can also control for average characteristics of the student body. For example, in addition to conditioning directly on the student's own family structure, I can condition on the proportion of students in the school who live in single-parent families. In other words, by averaging over all

students within the school, this approach allows me to estimate various other types of peer group effects in addition to those related to violence. The specific regressors I include are the proportion of students officially classified as disadvantaged, the proportions of poor and wealthy students, and the fraction of the student body living in fatherless families. Table 3 provides summary statistics for many of the variables used in the student attainment regressions.

Finally, the HSB permits one to identify the state and urbanicity of each school in the sample.[7] I interact a full set of state dummies with the three-way urbanicity measure (urban, suburban, rural) and include these state/urbanicity interactions in the regression. These dummy variables provide controls for any factors that might vary by state and by urbanicity within state. These include the general level of crime, which may be important for the educational attainment regression, and the cost of living, which may be important for the teacher salary models.

B. Teacher Salaries

With some re-interpretation, equation (1) also serves for estimating the effect of school violence on teacher salaries. The dependent variable y_{ij} now represents the logarithm of the salary of the ith teacher in the jth school. It should be noted that salaries are interval-coded in the ATS, so rather than observing the actual salary, I observe a range in which the teacher's salary falls. For all intervals except the highest, I assigned each teacher the midpoint of the salary interval. For the top, open-ended category, which indicated a salary

7 Although the survey does not actually identify states, Ganderton (1992) and Hanushek and Taylor (1990) have noted that, by tracking the post-secondary educational institutions attended by all students in a high school, it is straightforward to infer the state in which most schools were located.

above $40,000, I assigned the value $40,000. Some experimentation revealed that the estimates were not particularly sensitive to reasonable changes in this value.[8]

One implication of interval-coding is that, conditional on the regressors, the disturbance term in the model can take on only a limited number of values. Thus the error term is likely to be heteroskedastic much for the same reason that the error term in the linear probability model is heteroskedastic. For this reason, I report standard errors that are robust to arbitrary forms of heteroskedasticity in addition to groupwise dependence.

In the context of the teacher salary regressions, X_{ij} includes characteristics of the teacher that affect his or her pay. Within a school district, teacher salaries typically depend strongly on the teacher's education and experience. Moreover, experience in other districts often counts for less than experience in the district of employment. I include both types of experience and their squares, and several dummies indicating the teacher's educational attainment. I also include dummies reflecting whether the teacher teaches vocational classes, whether she is certified, and whether she is tenured.

The vector C_j contains the same school characteristics that were included in the student performance regressions, plus the average score on a standardized math test taken by students. I include this variable on the grounds that lower-achieving students may represent a workplace disamenity for which teachers might be compensated. I also include dummy variables indicating whether teachers are represented by the National Education Association, the American Federation of Teachers, or some other union. Table 4 provides summary statistics for many of the variables used in the teacher salary regressions.

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8 Only 0.37 percent of the teachers fell in the top category.

IV. Results

A. School Violence and Student Performance

Estimates of the effect of local violence on high school graduation and college attendance are presented in Table 5. Because the dependent variables are binary, I use the probit method to estimate the regressions. The reported standard errors are robust to any groupwise dependence that may arise due to the presence of multiple observations per school. The numbers in square brackets are marginal effects, that is, the average effect on the outcome probability of changing the explanatory variable from zero to one.

The top panel of Table 5 presents estimates of the effects of local violence on high school graduation, and the bottom panel presents estimates of the effects of local violence on college attendance. Local violence is represented by a set of three dummy variables that correspond to minor, moderate, and serious levels of violence as measured by the student index. Schools with minimal violence form the base group.

In column (1), the regression includes only race dummies in addition to the violence measures. The specification in column (2) adds the proxies for the students' own violence-proneness, the family background measures, and the other school characteristics. Finally, the model in column (3) adds the set of state/urbanicity dummies.

The estimates in column (1) show a strong negative correlation between local violence and high school graduation. In column (2), the log-likelihood statistic, lnL, shows that adding the measures of violence-proneness, family background, and school characteristics greatly increases the explanatory power of the model. These control variables also reduce the estimated effects of local violence. Results from the model with

the state/urbanicity indicators, in column (3), are slightly stronger, though the coefficients are still much smaller than those from the most restrictive model.

Nevertheless, the estimates provide significant evidence that local violence reduces the student's likelihood of graduating high school. Although students in schools reporting minor levels of violence are only one percentage point less likely to complete high school than their counterparts in the least violence schools, students in schools with moderate levels of violence are 5.1 percentage points less likely to graduate. For students in the most violent schools, the likelihood of graduating falls by 5.7 percentage points. These are sizable effects relative to the sample dropout rate of 21 percent. Minor violence raises the dropout rate by 5 percent, moderate levels of violence raise the risk of dropping out by 24 percent, and more substantial violence raises the risk by 27 percent.

Panel B of Table 5 presents estimates of the effect of local violence on the likelihood that students attended a four-year college. Once again, the estimated effects of local violence fall sharply as one moves from the more restrictive models to the less restrictive ones. However the estimates remain sizable, even when the full set of control variables is added to the model. Serious violence lowers the likelihood of attending college by 15.9 percentage points. Even minor violence, a problem faced by nearly two-thirds of all public school students, has a significant effect, reducing college attendance rates by 3.9 percentage points.

As a proportion of the college attendance rate of 31 percent, the effect of violence on college attendance is somewhat larger than its proportionate effect on dropping out of high school. The effect of minor violence amounts to a 13 percent reduction in the college

attendance rate; the effect of moderate violence amounts to a 22 percent reduction; and the effect of substantial violence amounts to a 51 percent reduction. The larger effects of violence on college attendance may have to do with the effects of violence on achievement, since lower achievement could make it harder for students who graduate to gain admission to college. Table 6 presents the results of achievement regressions in which the dependent variables are constructed from standardized math tests which were administered to HSB students in 1980 and 1982.[9] Violence has a significant effect on students' sophomore test scores, as shown by the results in column (1). Substantial violence reduces the math score by 1.92 points (equivalent to two-tenths of a standard deviation); moderate violence reduces the test score by nearly one point; and minor violence reduces it by 0.65 points.

Violence has little effect on the change in students' math scores between their sophomore and senior years, as shown in column (2), but this may have little bearing on college attendance if students' attendance plans are largely formulated already by their sophomore year. Table 7 provides some evidence on this point in the form of a cross-tabulation of students' intentions to attend college, as expressed in their sophomore year, and their actual attendance after finishing high school. Of the 3,325 students who stated that they intended to attend a four-year college after high school, 64 percent actually did. Of the 6,823 who stated other plans, only 16 percent attended a four-year college.

Finally, I offer a calculation and a comparison that may aid further in interpreting the magnitude of the effect of violence on college attendance. In table 1 we saw that 9.9 percent of all students attend schools with moderate to serious levels of violence, and 62.8

9 These regressions include all of the variables included in the specification of column (3), Table 5. Sample sizes are less than 10,787 due to missing data on the test scores.

percent attend schools with minor levels of violence. The coefficients in column (3) of Table 5 indicate that, if local violence were reduced by half--that is, if 4.95 percent of students attended moderately to seriously violent schools, and 31.4 percent attended schools with minor levels of violence--then the college attendance rate would rise by 1.8 percentage points, from 31.1 to 32.9 percent. This amounts to a 5.8 percent increase.

Some further perspective can be gained by comparing the effects of a reduction in school violence to the effects of an increase in college tuition. Quigley and Rubinfeld (1993) estimate that the price elasticity of college attendance is -0.04, meaning that a 100 percent increase in tuition would reduce attendance by four percent. Thus local violence would have to be reduced nearly by half in order to have roughly the same effect on college attendance (albeit of opposite sign) as a doubling of college tuition.[10]

B. Local Violence and Teacher Salaries

Table 8 presents estimates of the effects of local violence on teacher salaries based on regression models that control for other characteristics of the school and personal characteristics of the teacher. As above, I present the results of several different specifications so as to demonstrate the role played by the conditioning variables. In column (1), the regression includes the various teacher characteristics in addition to the school violence dummies. The next regression, in column (2), adds other attributes of the school and its student body. The final specification, in column (3), adds the state/urbanicity dummies as well.

10 Of course, it could be quite costly to reduce violence by half.

In contrast to the results for students, the estimates of the effects of local violence vary little across specifications, even though the additional variables are generally quite significant. The estimates in column (3) indicate that serious violence raises teacher salaries on average by about 7.4 percent. Lesser levels of violence, prevalent in roughly one-third of all schools nationwide, raise salaries by 2.4 percent.

To assess the importance of these effects, consider a thought experiment similar to the one above. If local violence were cut in half, then teacher salaries nationwide could be reduced by eight-tenths of one percent. In other words, even if local violence fell by as much as 50 percent, the savings available to school districts through reductions in violence-related wage premia would amount to only $144 million out of $18 billion spent annually on secondary teachers' salaries.

The small magnitude of these violence-related wage premia may stem from institutional features of wage determination within public school districts. Generally, all secondary teachers within a district are covered by the same salary scale, under which salaries are set as a rigid function of experience and education. This setting provides little leeway to provide compensating differentials among teachers at different schools within a district; violence premiums in the teacher salary regressions above result primarily from salary scale variation between districts, which presumably reflects only the variation in the average level of violence across districts. With data on multiple schools per district, one could test this notion by looking for within-district correlations between violence and either non-price compensation or, in its absence, teacher turnover. The HSB does not identify

school districts, however, so a further analysis of these issues will have to await the availability of more suitable data.

V. Discussion

The results presented above provide support for the hypotheses that school and neighborhood violence affect the educational attainment of students and result in compensating differentials for teachers. Even in models with controls for factors likely to be correlated with both the outcomes and the level of local violence, local violence has significant and substantial effects. Nevertheless, as discussed in the Introduction, there still may be unobserved characteristics of schools and students that are correlated with the level of local violence and also with student performance and teacher salaries. In this section, I address various aspects of this identification issue by comparing results across subsamples and across the various measures of local violence. The goal is to look for patterns in the results that may reveal whether local violence is responsible for the results above, or whether the results might be more readily attributable to other characteristics of students or schools that are correlated with local violence.

Consider first the question of distinguishing the peer group effects of local violence from the likely correlation between violence-proneness and low educational attainment on the part of individual students. I have attempted to deal with this problem by conditioning explicitly on student behavior problems that are likely to be correlated with the student's proneness to violence. Nevertheless, even though these proxy variables have considerable explanatory power, they may fail to capture the student's violence-proneness in its entirety. If so, then the disturbance term in the regression model includes the portion of each

individual's violence-proneness not captured by the proxy variables, which may be correlated with the level of violence in the school.

I test this notion by dividing the sample in two ways. First, I compare the results from the subsample of students with behavior problems to those without behavior problems. The rationale for this exercise is that the quality of my proxies for violence-proneness is likely to be asymmetric. Students without observable behavior problems may be unlikely to engage in other, unobservable, types of violence. Students who report behavior problems, on the other hand, may engage in other types of violence not captured by the observable measures. In other words, a lack of observable problems may be a good indicator of non-violence-proneness, but observable behavior problems may reflect violence-proneness of varying degrees of severity. If so, then my observable measures of violence-proneness would be better proxies for non-violent students than for violent students. It follows that omitted variable bias would be greater in the subsample of students with observable behavior problems than in the subsample of well-behaved students. In this case, the estimated effects of local violence would be larger in the subsample of observably violence-prone students than in the subsample of non-violence-prone students.

Results are presented in Table 9.[11] In panel A, the coefficient on the serious violence dummy is negative in the violence-prone subsample and positive in the non-violence-prone subsample. Neither of these coefficients is significant, however. Otherwise, the violence coefficients are more negative in the non-violence-prone subsample than in the violence-prone subsample.

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11 The samples in Table 9 exclude 1820 observations which had missing data on one or more of the violence-proneness variables. Other deletions occurred because there was no variation in the dependent variables within a number of state/urbanicity cells once the sample was stratified by violence-proneness.

The pattern is similar in the college attendance models, shown in panel B. In two cases out of three, the violence coefficients from the non-violence-prone sample are larger than the coefficients from the violence-prone subsample, and the difference between the moderate violence coefficients is significant. These results are less consistent with the hypothesis that the asymmetric quality of the proxies for violence-proneness exaggerates the estimates of the effect of local violence on educational attainment, than with the hypothesis that non-violent students are adversely affected by attending violent schools.

In Table 10 I present results from regressions disaggregated by sex. If the behavior problem dummies provide poor proxies for students' violence-proneness, the resulting proxy variable problem presumably would be worse for males than for females, because females in general are less violent than males. Thus, the bias in the regression for males should be greater than the bias in the regression for females, which would tend to make the school violence coefficients larger for males than for females. In Table 10, however, the coefficients for women are all more negative than the coefficients for men. This may not refute the proxy variable problem entirely, since the negative consequences of school violence may be greater for females than males. Nevertheless, the male/female contrasts, like the violence-proneness contrasts above, seem to support the notion that measured violence-proneness adequately captures students' actual proneness to violence.

Consider next the problem of subjectivity in the principal's responses to the questions about violence in his school. One possibility is that the standards for assessing the severity of school violence depend on characteristics of the student body, so that two schools with the same level of actual violence would receive different rankings from two

different principals. To the extent that items such as parental education, income, and family structure influence principals' ratings of the level of violence in their schools, then the subjectivity problem is handled by the regression model. If ratings depend on unobservable characteristics of the school, however, then the violence indicators suffer from measurement error. At the extreme, one might be concerned that principals answer the questionnaire items strategically, using claims of a violent environment to rationalize the poor performance of their students. The results seem to dispel this notion, however, since it seems doubtful that strategically spurious responses designed to rationalize poor student performance would coincidentally explain teacher salaries as well.

The next issue is whether the effects of school violence can be distinguished from the effects of school disorder. My approach here was to control for a number of characteristics that might define a bad school: vandalism, large proportions of students from socially or economically disadvantaged environments, low achievement among the student body, large class sizes. These may not be the only factors that define a bad school, however, and those that remain unaccounted for may be correlated with the level of school violence.

Presumably, any measure of school violence would provide a proxy for school disorder more generally. As I mentioned above, however, preliminary analyses indicated that the two outcomes--student performance and teacher pay--were best explained by different types of school violence. Student attainment is best explained by an index that weights weapons and fights among students most heavily, whereas teacher pay is best explained by an index weighted toward conflicts involving teachers. In other words,

student attainment is best explained by violence measures that reflect risks more specific to students, and teacher pay is best explained by measures that reflect risks more specific to teachers.

Table 11 provides some evidence. In panel A, I present two sets of estimates of the effects of local violence on student attainment and teacher salaries. In the top part, violence is represented by the student index; in the bottom part, it is represented by the teacher index. The important feature of the table is that, as measured by a joint significance test, the student index better explains student attainment, and the teacher index better explains teacher pay.

A second, somewhat more direct comparison is possible as well. In panel B I report estimates in which the principal's reports of conflicts in the school, rather than the violence indexes, are used as the measure of school violence. In the top part, I include dummies reflecting whether the principal reported that fights among students posed a substantial or moderate problem in the school (the category "no problem" is the base). In the bottom part, I include two dummies reflecting the extent of conflicts between students and teachers.

In the high school graduation and college attendance equations, the measures of fighting among students are more significant than the measures of conflicts between students and teachers. In the teacher pay equation, we see just the opposite. Thus the variables that better reflect risks faced by students do a better job of explaining student performance, and the variables that better reflect risks faced by teachers do a better job of explaining teacher pay. If school violence were merely standing in for unmeasured school disorder, then there would be no reason to expect this sorting pattern in the results.

VII. Conclusions

Violence in schools, in one form or another, is widespread. Moreover, such violence, combined with violence in the neighborhood, has significant effects on both educational attainment and teacher salaries. My estimates indicate that, if school violence were cut in half, then college attendance rates would rise by 5 percent. Compensating salary differentials paid to teachers would fall as well, but by less than one percent of the total salary bill.

The results on students' educational attainment stand in contrast to much of the educational production function literature. Recent studies of the effects of school characteristics such as class sizes and teacher education generally show that those inputs have little effect on student performance, regardless whether student performance is measured by educational achievement, educational attainment, or post-schooling earnings (Betts 1996, Hanushek 1986). This suggests that researchers interested in educational production functions would do well to focus less on traditional measures of school quality such as class sizes, and more on less traditional measures that have received less attention in the past. The results also suggest that policies to reduce local violence could have important effects on educational attainment, although caution must be used in drawing specific policy implications.

Finally, it is important to bear in mind that these estimates are based on conditions that existed in the early 1980s. Although hard data are difficult to obtain, there is evidence to suggest that school violence may have increased recently, at least in its severity. Certainly the number of congressional hearings devoted to the topic was greater in the early

1990s than in the early 1980's. If the extent of school violence has increased, then its effects may have become more important. It would be desirable to study the issue further using data on more recent cohorts of students.

Appendix A
Constructing the Violence Indexes

The three (collapsed) response categories for each of the three questions about school violence together provide 27 possible response sequences, or potentially distinct levels of violence. My goal in constructing indexes is to determine if each of these 27 levels of violence has a distinct effect, or if instead the dimensionality of the violence measures can be reduced. My approach to the data reduction exercise is influenced by concerns for both explanatory power and interpretability of the resulting index.

To construct the student index, I first defined one dummy variable for each response sequence, and then fit a preliminary regression of college attendance variable on these dummies plus a number of other control variables. Although the questionnaire items potentially provide for 27 dummies, 5 cells were empty, and 3 were nearly so. In cases where schools map uniquely into response sequences, the response sequence dummy is also a school dummy. Thus its coefficient is difficult to interpret as a measure of the effects of a particular level of violence. When a cell contains only two schools, the problem is only slightly less serious. For this reason I first dropped from the sample four schools that occupied three nearly empty cells.

Columns 4 and 5 of Table A1 give the frequency distribution of the remaining response sequences. Columns 6 and 7 give regression coefficients and standard errors from the preliminary regression of the college attendance dummy on the violence dummies. The estimates provide substantial support of the basic hypothesis that local violence affects students' college attendance decisions. All of the coefficients are negative, except one that is insignificant. Of the 19 coefficients, eight are significant at the 5 percent level. If

violence were not related to college attendance, one would expect about half the coefficients to be positive, and only one of them to be significant. The F-statistic for the joint significance test was 3.07, which is significant at any level.

The coefficients cluster in roughly four groups. There are two that are quite large, that is, greater than 0.15 in absolute value. There are several that are just a bit smaller, about -0.10. Several others cluster near -0.05; the remainder are quite small. If my interest were to construct a parsimonious index that minimized the sum of squared residuals, then I would have grouped the response sequences into four categories on the basis of their coefficients alone.

However my interest is in constructing an index whose values have a simple interpretation. For this reason I defined the first, most serious value as including all response sequences that reflect substantial weapons problems. This captures the two largest coefficients. The next value consists of all remaining sequences that reflect substantial fighting among students. This captures most of the coefficients of magnitude -0.10. The next value consists of all remaining sequences that reflect moderate fighting both among students and between students and teachers. This captures the remaining sizably negative coefficients.

I followed the same approach to construct the teacher index, using log salary as the dependent variable in the preliminary regression. Because there were fewer schools in the ATS, there were more empty and near-empty response sequences. I dropped from the sample 15 schools from 9 cells that contained at most two schools each.

Results from the preliminary regression are given in columns 5 and 6 of Table A2. Here, only one of the 10 coefficients is significant, which is roughly what one would expect by chance. Nevertheless, the estimates largely support the hypothesis that local violence affects teacher pay. Eight of the coefficients are positive, consistent with the compensating differentials model. The F-statistic for the joint significance test is 1.86, which is significant at the 5 percent level.

There are a few relatively large coefficients, greater than 0.3. All of these attach to response sequences reflecting substantial levels of fighting between students and teachers. I group these sequences together to form the first, most serious value of the teacher index. The second value includes all the remaining sequences that reflect moderate weapons problems. This captures the coefficients that cluster around 0.2. The remaining cells are grouped together to form the third value of the index; these response sequences have small or negative coefficients.

Table A1

Response Sequence Frequency Distribution and Regression Results Used to Construct Student Index

Response sequences[a]							
Fights among students	Fights between students and teachers	Weapons	Number of observations	Percent of sample	Coefficients (standard errors) from linear regression of college attendance dummy on response sequence dummies[b]		Value of student index assigned
S	S	S	80	0.7	-0.155	(0.040)	1
S	S	M	343	3.2	-0.102	(0.030)	2
S	S	N	55	0.5	0.070	(0.071)	2
S	M	S	28	0.3	-0.062	(0.049)	1
S	M	M	378	3.5	-0.096	(0.029)	2
S	M	N	205	1.9	-0.071	(0.036)	2
S	N	S	0				
S	N	M	52	0.5	-0.120	(0.026)	2
S	N	N	41	0.4	-0.102	(0.068)	2
M	S	S	0				
M	S	M	246	2.3	-0.023	(0.037)	4
M	S	N	86	0.8	-0.035	(0.051)	4
M	M	S	38	0.4	-0.160	(0.078)	1
M	M	M	3080	28.6	-0.074	(0.020)	3
M	M	N	3579	33.2	-0.051	(0.019)	3
M	N	S	0				
M	N	M	265	2.5	-0.006	(0.029)	4
M	N	N	814	7.6	-0.005	(0.029)	4
N	S	S	0				
N	S	M	0				
N	S	N	0				
N	M	S	0				
N	M	M	105	1.0	-0.022	(0.043)	4
N	M	N	464	4.3	-0.018	(0.028)	4
N	N	S	0				
N	N	M	37	0.3	-0.051	(0.105)	4
N	N	N	891	8.3	(base group)		4

a - S denotes substantial, M denotes minor, and N denotes none.

b - In addition to the response sequence dummies, the regression includes dummies for black, Hispanic, other race, ever suspended, troublemaker (big and moderate), trouble with the law, discipline problems in school, low family income, mother's education (less than high school, some college, college degree), father's education (less than high school, some college, college degree), no father in household, census division, urban residence, rural residence, a sex dummy, interaction terms between the sex dummy and the parents' education dummies, and the following school characteristics: expenditures per pupil, school size (number of students), class size, percent disadvantaged, percent fatherless, percent low income, and percent high income. In addition, associated with each regressor is a missing value flag that takes on the value one for missing values of the regressor, and zero otherwise. The missing values of the regressor were recoded to zero. Standard errors are robust to arbitrary forms of heteroscedasticity and groupwise dependence.

Table A2

Response Sequence Frequency Distribution and Regression Results Used to Construct Teacher Index

Response sequences[a]							
Fights among students	Fights between students and teachers	Weapons	Number of observations	Percent of sample	Coefficients (standard errors) from linear regression of log salary on response sequence dummies[b]		Value of teacher index assigned
S	S	S	129	1.6	0.076	(0.051)	1
S	S	M	384	4.8	0.036	(0.035)	1
S	S	N	0				
S	M	S	0				
S	M	M	264	3.3	0.024	(0.041)	2
S	M	N	148	1.9	-0.026	(0.025)	3
S	N	S	0				
S	N	M	0				
S	N	N	0				
M	S	S	0				
M	S	M	232	2.9	0.079	(0.030)	1
M	S	N	0				
M	M	S	0				
M	M	M	2399	30.2	0.014	(0.022)	2
M	M	N	2738	34.5	-0.003	(0.022)	3
M	N	S	0				
M	N	M	238	3.0	0.026	(0.034)	2
M	N	N	464	5.8	0.004	(0.031)	3
N	S	S	0				
N	S	M	0				
N	S	N	0				
N	M	S	0				
N	M	M	0				
N	M	N	303	3.8	0.010	(0.037)	3
N	N	S	0				
N	N	M	0				
N	N	N	649	8.2	(base group)		3

a - S denotes substantial, M denotes minor, and N denotes none.

b - In addition to the response sequence dummies, the regression includes dummies for sex, race, urban location, rural location, education, vocational instruction, tenured, certified, census division; experience in the current school and its square, experience in others schools and its square, and the following school characteristics: percent black, percent Hispanic, percent fatherless, percent low income, percent high income, class size, school size, average math test score, and dropout rate over 20 percent. In addition, associated with each regressor is a missing value flag that takes on the value one for missing values of the regressor, and zero otherwise. The missing values of the regressor were then recoded to zero. Standard errors are robust to arbitrary forms heteroskedasticity and groupwise dependence.

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Table 1
The Distribution of School Violence

A. Principal reports of violence	Private Schools	Public Schools			
Nature and extent of school violence	Total	Total	White	Black	Hispanic
1. Students fight with each other (%)					
Serious	0.0	0.4	0.3	0.9	0.5
Moderate	0.0	9.1	7.2	15.1	12.8
Minor	49.7	75.8	75.8	77.1	75.6
None	50.3	14.7	16.8	6.9	11.1
2. Conflicts between students and teachers (%)					
Serious	0.0	0.0	0.0	0.0	0.0
Moderate	0.0	6.7	5.3	13.9	7.5
Minor	63.5	73.9	73.7	73.3	74.7
None	36.5	19.4	21.0	12.8	17.9
3. Weapons (%)					
Serious	0.0	0.0	0.0	0.1	0.2
Moderate	0.0	1.1	0.4	3.8	2.1
Minor	4.4	39.7	33.8	66.5	44.5
None	95.6	59.2	65.8	29.7	53.4
Sample sizes	2948	10,787	6379	1421	2363

B. Indexes constructed from principal reports	Private Schools	Public Schools			
Nature and extent of school violence	Total	Total	White	Black	Hispanic
1. Student index (%)					
Serious	0.0	1.1	0.4	3.8	2.1
Moderate	0.0	8.8	7.1	13.8	12.0
Minor	35.8	62.8	63.2	61.4	62.8
Minimal	64.2	27.3	29.3	21.1	23.0
Sample sizes	2948	10,787	6379	1421	2363
2. Teacher index (%)					
Serious	0.0	7.0	4.4	17.9	10.9
Moderate/Minor	6.8	35.9	32.8	53.7	36.3
Minimal	93.2	57.1	62.8	28.5	52.8
Sample sizes	1934	10,345	6171	1339	2235

Notes: Based on weighted data. The Total column includes a number of students categorized as "other race".

Table 2
Persistence of Weapons-Related Violence in Public Schools

	No weapons-related violence in 1982	Any weapons-related violence in 1982	Total
No weapons-related violence in 1980	332	136	458
Any weapons-related violence in 1980	95	250	345
Total	417	386	803

Note: Unit of observation is the school.

Table 3
Student Sample Means

Variable			
Dependent variables		Individual and family characteristics	
Graduated high school	0.791 (0.407)	Ever suspended	0.195 (0.393)
Attended four-year college	0.311 (0.463)	Big troublemaker	0.033 (0.179)
Score on sophomore math test	12.06 (9.76)	Moderate troublemaker	0.189 (0.392)
Change in math score between 10th and 12th grades	1.81 (5.91)	Trouble with law	0.050 (0.219)
		Discipline problems in school	0.180 (0.384)
School characteristics			
Substantial vandalism	0.339 (0.473)	Low income	0.072 (0.258)
Minor vandalism	0.631 (0.482)	Mother's education:	
Expenditure/pupil	1273.8 (872.2)	< high school	0.175 (0.380)
School size	1327.9 (865.6)	some college	0.152 (0.359)
Class size	17.6 (7.1)	college grad.	0.086 (0.281)
Percent black	0.147 (0.236)	Father's education:	
Percent Hispanic	0.99 (0.205)	< high school	0.175 (0.380)
Percent disadvantaged	0.180 (0.232)	some college	0.136 (0.343)
Percent fatherless	0.283 (0.166)	college grad.	0.119 (0.324)
Percent low income	0.096 (0.166)	Fatherless	0.259 (0.438)
Percent high income	0.373 (0.205)	Male	0.503 (0.500)

Notes: Sample size is 10,787, except for the score on the sophomore math test (n=9055) and the change in the math score between 10th and 12th grade (n=7784). Standard deviations in parentheses.

Table 4
Teacher Sample Means

Dependent variable		Teacher characteristics	
Log salary	3.106	Female	0.436
	(0.278)		(0.496
School characteristics		Black	0.089
			(0.285
Substantial vandalism	0.342	Hispanic	0.034
	(0.474)		(0.182
Minor vandalism	0.637	Education:	
	(0.481)		
Percent black students	15.2	Less than BA	0.013
	(25.1)		(0.113)
Percent Hispanic students	7.8	BA	0.121
	(17.1)		(0.326)
Class size	17.7	BA plus 16 units	0.270
	(7.3)		(0.444)
School size	1395.2	Specialist	0.064
	(882.3)		(0.244)
Dropout rate over 20 percent	0.188	EDD	0.005
	(0.390)		(0.072)
Percent fatherless students	0.280	Ph.D.	0.008
	(0.169)		(0.090)
Mean math score	9.30	Experience at school	10.23
	(3.53)		(7.75)
Percent low income	0.066	Experience at other schools	4.96
	(0.068)		(6.30)
Percent high income	0.371	Vocational classes	0.170
	(0.146)		(0.375)
NEA	0.555	Certified	0.930
	(0.497)		(0.255)
AFT	0.227	Tenured	0.804
	(0.419)		(0.397)
Other union	0.322		
	(0.467)		

Notes: Sample size: 7,948. Standard deviations in parentheses.

Table 5
Probit Estimates of the Effect of Local Violence on Educational Attainment

A. Dependent variable: Received high school diploma Level of violence, student index	(1)	(2)	(3)
Serious	-0.482	-0.133	-0.240
	(0.131)	(0.123)	(0.136)
	[-0.137]	[-0.032]	[-0.057]
Moderate	-0.282	-0.170	-0.212
	(0.068)	(0.070)	(0.073)
	[-0.080]	[-0.042]	[-0.051]
Minor	-0.104	-0.056	-0.042
	(0.042)	(0.040)	(0.039)
	[-0.030]	[-0.014]	[-0.010]
lnL	-5483.2	-4726.2	-4598.7
Sample size	10,787	10,787	10,753
B. Dependent variable: Attended four-year college Level of violence, student index	(1)	(2)	(3)
Serious	-0.516	-0.381	-0.573
	(0.198)	(0.160)	(0.167)
	[-0.178]	[-0.109]	[-0.159]
Moderate	-0.345	-0.253	-0.247
	(0.067)	(0.063)	(0.064)
	[-0.119]	[-0.072]	[-0.069]
Minor	-0.201	-0.152	-0.139
	(0.040)	(0.037)	(0.034)
	[-0.070]	[-0.043]	[-0.039]
lnL	-6551.6	-5451.3	-5311.1
Sample size	10,787	10,787	10,787

Notes: Standard errors, adjusted for arbitrary forms of heteroskedasticity and groupwise dependence, in parentheses. Numbers in square brackets are marginal effects. In addition to variables shown, the models contain the following regressors. Column (1): dummies for black, Hispanic, and other race. Column (2): add dummies for ever suspended, troublemaker (big and moderate), trouble with the law, discipline problems in school, low family income, mother's education (less than high school, some college, college degree), father's education (less than high school, some college, college degree), no father in household, census division, urban residence, rural residence, a sex dummy, interaction terms between the sex dummy and the parents' education dummies, and the following school characteristics: expenditures per pupil, school size (number of students), class size, percent disadvantaged, percent fatherless, percent low income, and percent high income. Column (3): add state/urbanicity dummies. Several observations were dropped from this final specification (for the graduation outcome) because the dependent variable exhibited no variation within a number of state/urbanicity cells. In addition, associated with all regressors except the school violence dummies is a missing value flag that takes on the value one for missing values of the regressor, and zero otherwise. The missing values of the regressor were recoded to zero.

Table 6
Linear Regression Estimates of the Effect of Local Violence on Achievement Test Scores

Dependent variable: Level of violence, student index	Sophomore math score (1)	Change in math score between 10th and 12th grade (2)
Substantial	-1.92	1.70
	(0.95)	(0.93)
Moderate	-0.95	0.03
	(0.43)	(0.32)
Minor	-0.65	0.17
	(0.26)	(0.19)
$F_{3,n-k}$	3.06	1.32
[significance level]	[0.028]	[0.267]
R^2	0.28	0.05
Sample size	9055	7784

Notes: Standard errors, adjusted for arbitrary forms of heteroskedasticity and groupwise dependence, in parentheses. Models include all regressors from specification in column (3) of Table 5. Sample sizes are less than 10,787 due to missing test score data.

Table 7
Intended vs. Actual College Attendance

As of 10th grade, intended to attend four-year college after finishing high school	Ever attended four-year college		
	Yes	No	Total
Yes	5705	1118	6823
No	1165	2070	3235
Total	6870	3188	10,058

Note: Unit of observation is the student, of whom 729 had missing data on their post-high school intentions.

Table 8
Linear Regression Estimates of Effect of Local Violence on Teacher Salaries

Dependent variable: Logarithm of teacher salary Level of violence, teacher index	(1)	(2)	(3)
Serious	0.058	0.063	0.074
	(0.020)	(0.021)	(0.019)
Moderate/Minor	0.018	0.022	0.024
	(0.012)	(0.012)	(0.011)
R^2	0.612	0.637	0.700

Sample size: 7,948

Notes: Standard errors, adjusted for arbitrary forms of heteroskedasticity and groupwise dependence, in parentheses. In addition to variables shown the models include dummies for sex, race, urban location, rural location, education, vocational instruction, tenured, certified, census division; experience in the current school and its square, experience in others schools and its square, and the following school characteristics: percent black, percent Hispanic, percent fatherless, percent low income, percent high income, class size, school size, average math test score, and dropout rate over 20 percent. In addition, associated with all regressors except the school violence dummies is a missing value flag that takes on the value one for missing values of the regressor, and zero otherwise. The missing values of the regressor were then recoded to zero.

Table 9
Probit Estimates of the Effect of Local Violence on Educational Attainment, by Violence-Proneness of Student

A. Dependent variable: Received high school diploma

Level of violence, student index	Violence-prone students	Non-violence-prone students
Serious	-0.164	0.082
	(0.195)	(0.341)
	[-0.047]	[0.014]
Moderate	-0.119	-0.303
	(0.096)	(0.111)
	[-0.034]	[-0.051]
Minor	-0.006	-0.036
	(0.057)	(0.066)
	[-0.002]	[-0.006]
lnL	-1987.6	-1482.5
Sample size	3893	4808

B. Dependent variable: Attended four-year college

Level of violence, student index	Violence-prone students	Non-violence-prone students
Serious	-0.902	-0.453
	(0.191)	(0.256)
	[-0.207]	[-0.145]
Moderate	-0.026	-0.342
	(0.102)	(0.079)
	[-0.006]	[-0.109]
Minor	-0.072	-0.159
	(0.060)	(0.044)
	[-0.007]	[-0.051]
lnL	-1587.6	-2823.9
Sample size	3836	5007

Notes: Standard errors, adjusted for arbitrary forms of heteroskedasticity and groupwise dependence, in parentheses. Numbers in square brackets are marginal effects. Models include all regressors from specification in column (3) of Table 5. 1820 observations with missing values for one or more of the violence-proneness dummies were dropped from the sample. Other sample deletions occur due to the lack of variation in the dependent variables within a number of state/urbanicity cells once the sample is stratified by violence-proneness.

Table 10
Probit Estimates of the Effect of Local Violence on Educational Attainment, by Sex

A. Dependent variable: Received high school diploma		
Level of violence, student index	Males	Females
Serious	-0.190	-0.218
	(0.166)	(0.170)
	[-0.046]	[-0.050]
Moderate	-0.139	-0.241
	(0.094)	(0.095)
	[-0.034]	[-0.055]
Minor	0.026	-0.091
	(0.051)	(0.057)
	[0.006]	[-0.021]
lnL	-2343.0	-2174.5
Sample size	5342	5317
B. Dependent variable: Attended four-year college		
Level of violence, student index	Males	Females
Serious	-0.519	-0.680
	(0.210)	(0.269)
	[-0.140]	[-0.189]
Moderate	- 0.230	-0.277
	(0.082)	(0.084)
	[-0.062]	[-0.077]
Minor	-0.115	-0.161
	(0.048)	(0.047)
	[-0.031]	[-0.045]
lnL	-2590.1	-2637.7
Sample size	5402	5333

Notes: Standard errors, adjusted for arbitrary forms of heteroskedasticity and groupwise dependence, in parentheses. Numbers in square brackets are marginal effects. Models include all regressors from specification in column (3) of Table 5. Sample deletions occur due to the lack of variation in the dependent variables within a number of state/urbanicity cells once the sample is stratified by sex.

Table 11
Estimates of the Effect of Local Violence on Educational Attainment and Teacher Pay, Using Various Violence Measures

A. Violence Indexes	Student attainment		Teacher pay
1. Level of violence, student index	Received high school diploma	Attended four-year college	Logarithm of teacher salary
Serious	-0.299	-0.846	0.090
	(0.204)	(0.185)	(0.048)
	[-0.071]	[-0.236]	
Moderate	-0.217	-0.292	0.017
	(0.079)	(0.067)	(0.015)
	[-0.051]	[-0.081]	
Minor	-0.044	-0.144	-0.011
	(0.040)	(0.035)	(0.011)
	[-0.010]	[-0.040]	
Joint χ^2_3 {significance level}	8.84 {0.032}	40.01 {0.000}	2.79 {0.039}
lnL / R^2	-4387.9	-5108.7	0.699
2. Level of violence, teacher index	Received high school diploma	Attended four-year college	Logarithm of teacher salary
Serious	-0.050	-0.106	0.074
	(0.095)	(0.082)	(0.019)
	[-0.012]	[-0.030]	
Moderate	-0.099	-0.089	0.024
	(0.042)	(0.038)	(0.011)
	[-0.024]	[-0.025]	
Joint χ^2_2 [significance level]	5.59 {0.061}	5.68 {0.058}	15.06 {0.001}
lnL / R^2	-4390.9	-5123.0	0.700

Table 11 (con't.)

B. Principal reports of violence

	Student attainment		Teacher pay
1. Students fight with each other	Received high school diploma	Attended four-year college	Logarithm of teacher salary
Substantial	-0.155	-0.321	0.035
	(0.089)	(0.075)	(0.017)
	[-0.037]	[-0.090]	
Moderate	0.038	-0.117	0.004
	(0.056)	(0.048)	(0.014)
	[0.009]	[-0.033]	
Joint χ^2_2 {significance level}	8.02 {0.018}	18.17 {0.000}	6.58 {0.037}
lnL / R^2	-4388.3	-5116.4	0.698
2. Conflicts between students and teachers	Received high school diploma	Attended four-year college	Logarithm of teacher salary
Substantial	0.008	-0.168	0.058
	(0.100)	(0.085)	(0.021)
	[0.002]	[-0.047]	
Moderate	-0.013	-0.137	0.004
	(0.046)	(0.039)	(0.012)
	[-0.003]	[-0.038]	
Joint χ^2_2 {significance level}	0.13 {0.936}	12.72 {0.002}	10.42 {0.006}
lnL / R^2	-4394.1	-5120.2	0.699

Sample sizes: Student attainment, 10,345; teacher pay, 7,948.

Notes: Standard errors, adjusted for arbitrary forms of heteroskedasticity and groupwise dependence, in parentheses. Numbers in square brackets are marginal effects. Student attainment regressions include all variables from specification in column (3) of Table 5. Teacher pay regressions include all variables from specification in column (3) of Table 8. Student regressions include only students from schools in which the teacher violence index could be assigned. See footnote 4 for discussion.