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Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone's Kids[†]

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There is a widespread perception that externalities from troubled children are significant, though measuring them is difficult due to data and methodological limitations. We estimate the negative spillovers caused by children from troubled families by exploiting a unique dataset in which children's school records are matched to domestic violence cases. We find that children from troubled families significantly decrease the reading and math test scores of their peers and increase misbehavior in the classroom. The achievement spillovers are robust to within-family differences and when controlling for school-by-year effects, providing strong evidence that neither selection nor common shocks are driving the results. (*JEL* D62, I21, J12, J13, K42)

It is estimated that between 10 and 20 percent of children in the United States are exposed to domestic violence annually (Bonnie E. Carlson 2000). Research indicates that these children suffer from a number of social and emotional problems including aggressive behavior, depression, anxiety, decreased social competence, and diminished academic performance (Jeffrey L. Edleson 1999; David A. Wolfe et al. 2003; John W. Fantuzzo and Wanda K. Mohr 1999; Karestan C. Koenen et al. 2003). There is also widespread belief among parents and school officials that troubled children negatively affect learning in the classroom. For example, a nationally representative survey found that 85 percent of teachers and 73 percent of parents said that the "school experience of most students suffers at the expense of a few chronic offenders" (Public Agenda 2004).[1]

While little is known about the extent of spillovers caused by children from troubled homes, understanding them is important for two reasons. First, because many

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[1] In addition, parents cited undisciplined and disruptive students (71 percent) and lack of parental involvement (68 percent) as the top two problems facing our nation's school system in the National Public Radio/Kaiser Family Foundation/Kennedy School of Government Education Survey (National Public Radio 1999).

education policies change the composition of students across schools and classrooms, it is important to understand how these changes may impact student achievement. For example, a common concern regarding school choice and tracking is that disadvantaged children may have greater exposure to the most disruptive peers in the cohort. The importance of this concern depends on how exposure to troubled peers affects student achievement and behavior. Second, the existence of economically meaningful spillovers caused by family problems would provide a compelling justification for all citizens and policymakers to be concerned about how best to help troubled families.

Credibly estimating peer effects caused by troubled children has been difficult due to data and methodological limitations, however. As a practical matter, most datasets do not allow researchers to identify exogenously troubled children. For example, it is difficult to determine if a disruptive child causes his classmates to misbehave or if his classmates cause him to be disruptive. In addition, troubled children are likely to self-select into the same schools as other disadvantaged children. Consequently, one must rule out the possibility that the disruptive student and his classmates misbehave due to common unobserved attributes.

We overcome these identification problems by utilizing a unique dataset in which student outcomes are linked to domestic violence cases. This allows us to identify the group of troubled children in a more precise way than by using demographic measures such as peer, gender, or race. Carlson (2000) indicates that children from violent homes commonly exhibit anger, aggression, and difficulty in relating to peers. Consequently, this study provides a particularly good test of whether some "bad apples" harm the learning of all other students. An additional advantage is that we can identify children who are troubled for family reasons exogenous to their peers (i.e., a child's peers do not cause domestic violence in the household). The panel nature of our dataset allows us to include school-by-grade fixed effects to control for the nonrandom selection of individuals into schools. Thus, our identification strategy relies on idiosyncratic shocks in the proportion of peers from families linked to domestic violence within a particular school and grade over time.

We find that increased exposure to children linked to domestic violence causes a statistically significant reduction in math and reading test scores and significant increases in misbehavior at school. Troubled boys and children from low-income families primarily drive the negative spillovers. For example, we estimate that adding one more troubled boy peer to a classroom of 20 students reduces boys' test scores by nearly 2 percentile points (one-fifteenth of a standard deviation), and increases the number of disciplinary infractions boys commit by 40 percent.

To ensure that the results are not driven by selection, we perform several falsification exercises and robustness checks. We find that the within-school variation in peer domestic violence is uncorrelated with own domestic violence, cohort size, race, gender, and household income. In addition, there is no evidence that children exit the school after being exposed to above-average levels of troubled peers. Furthermore, we show that the effects on academic achievement are robust to within family comparisons, which provides further evidence that selection is not driving our results. Specifically, we find that a child exposed to troubled peers at school performs significantly worse than her sibling who was not exposed to such peers. Finally, we show that the negative spillovers on achievement are unchanged when we control for school-by-year-specific effects, which suggests common shocks to a given school and year are not driving the results.

Our findings have important implications for education and social policy. First, they provide strong empirical evidence of the existence of the "bad apple" peer effects model, which hypothesizes that a single disruptive student can negatively affect the outcomes of all other students in the classroom. Second, our results suggest that policies that change a child's exposure to classmates from troubled families will have important consequences for his educational outcome. Finally, our results provide a compelling reason for policymakers, and society in general, to be concerned about family problems such as domestic violence. Indeed, the results here indicate that any policies or interventions that help improve the family environment of the most troubled students may have benefits that are larger than previously anticipated.[2]

I. Identification Strategy and Methodology

Our approach to measuring negative externalities in the classroom is to examine the impact of children from troubled families on their peers. However, measuring such effects has proven difficult for reasons that are well documented in the peer effects literature. First, because child and peer outcomes are determined simultaneously, it is difficult to distinguish the effect that the group has on the individual from the effect the individual has on the group. This is commonly called the reflection problem (Charles F. Manski 1993). Second, when individuals self-select into peer groups, it is impossible to determine whether the achievement is a causal *effect* of the peers or simply the *reason* the individuals joined the peer group (Caroline M. Hoxby 2002). Finally, common shocks or correlated effects confound peer effects estimates because it is often difficult to separate the peer effect from other shared treatment effects (David S. Lyle 2007).

The reflection problem is best resolved by finding a suitable instrument for peer behavior or ability. One strategy in the primary and secondary education peer effects literature has been to use lagged peer achievement as an instrument for current achievement.[3] While this strategy is presumably the consequence of data constraints, lagged peer achievement may not be exogenous to contemporaneous achievement.[4] Another strategy has been to proxy for peer ability/behavior using preexisting measures such as race and gender (Hoxby and Gretchen Weingarth 2006; Hoxby 2000b; Victor Lavy and Analia Schlosser 2007), student relocations (Joshua D. Angrist and Kevin Lang 2004; Scott Imberman, Adriana Kugler, and Bruce Sacerdote 2009), the presence of boys with feminine names (David N. Figlio 2007), or the presence

[2] We recognize the possibility remains that solving family problems may not eliminate the negative externalities if they are caused by factors correlated with domestic violence such as low cognitive ability.

[3] Papers that do so include Julian R. Betts and Andrew Zau (2004), Mary A. Burke and Tim R. Sass (2004), Hoxby and Weingarth (2006), Eric A. Hanushek et al. (2003), and Jacob Vigdor and Thomas Nechyba (2007).

[4] This is because many of the peers in an individual's current peer group were also likely to be peers in the previous period(s). Hence, previous peer achievement is not exogenous to individual current achievement due to the cumulative nature of the education production function.

of children who had previously been retained (Lavy, M. Daniele Paserman, and Schlosser 2007).

Our approach is similar in that we use the presence of family problems, as signaled by a request to the court for protection from domestic violence, as an exogenous source of variation in peer quality. Doing so overcomes the reflection problem so long as there is no feedback loop where a student's peers *cause* the domestic violence in the household. This assumption appears reasonable. None of the primary determinants of domestic violence analyzed by Rachel Jewkes (2002) can plausibly be linked to one's own elementary school child or her peers.[5] We also test directly for this and find no evidence that own domestic violence is affected by peer domestic violence.[6] In addition, using family violence as an exogenous proxy for peer quality is advantageous because it provides a much finer measure of peers who are likely to be disruptive than other measures such as race or gender.

Resolving the self-selection problem has been handled in the peer effects literature in two ways. The first strategy, primarily used in the higher education literature, is to exploit the random assignment of individuals to peer groups (Gigi Foster 2006; Sacerdote 2001; David J. Zimmerman 2003; Lyle 2007; Ralph Stinebrickner and Todd R. Stinebrickner 2006; Michael Kremer and Dan Levy 2008; Carrell, Richard L. Fullerton, and James E. West 2009). As this rarely occurs in primary and secondary education,[7] a second approach has been to exploit the natural variation in cohort composition across time within a given school.[8] This is accomplished by using large administrative panel datasets while employing a series of fixed effects models.

To overcome self-selection, we follow this latter approach by exploiting the variation in peer domestic violence that occurs at the school-grade-year (cohort) level while controlling for school-grade specific fixed effects. Thus, our identification strategy relies on idiosyncratic shocks in the proportion of peers from families linked to domestic violence, across grade cohorts, within a school, over time.[9] Formally, we estimate the following equation using ordinary least squares:

$$y_{isgt} = \varphi_0 + \varphi_1 \frac{\sum_{k \neq i} DV_{ksgt}}{n_{sgt} - 1} + \varphi_2 X_{isgt} + \lambda_{sg} + \sigma_{gt} + \varphi_{sg}\, t + \varepsilon_{isgt}, \tag{1}$$

where y_{isgt} is the outcome variable for individual i, in school s, grade g, and in year t. $\sum_{k \neq i} DV_{ksgt}/(n_{sgt} - 1)$ is the proportion of peers in the school grade cohort from families linked to domestic violence, except individual i. We measure peer domestic violence at the cohort level as opposed to the classroom level due to potential

[5] Jewkes (2002) notes that the causes of domestic violence are complex, but cites alcohol, power, financial distress, and sexual identity as the primary determinants.

[6] Furthermore, as our results will show, the negative peer effects we find operate primarily through boys and on boys. Therefore, if a feedback loop were present, one would expect more boys than girls to come from troubled families. The fact that boys and girls in our dataset are equally likely to come from domestic violence households provides further evidence that reflection is not biasing our results.

[7] The one exception is Project STAR.

[8] See Hoxby 2000b; Hoxby and Weingarth 2006; Vigdor and Nechyba 2007; Betts and Zau 2004; Burke and Sass 2004; Hanushek et al. 2003; Lars Lefgren 2004; and Carrell, Fredrick V. Malmstrom, and West 2008).

[9] Our identification strategy is similar to that used by Hoxby (2000a, 2000b) in identifying class size and peer effects using idiosyncratic variation in the population.

nonrandom selection of students into classrooms within a school and grade.[10] $\mathbf{X}_{isgt}$ is a vector of individual *i*'s specific (pre-treatment) characteristics, including own family violence, race, gender, subsidized lunch, and median zip code income. λ_{sg}, σ_{gt}, and φ_{sg} are school-grade fixed effects, grade-year fixed effects, and school-grade specific linear time trends. The linear time trends are included to account for any changes in the neighborhood or school that are specific to that school-grade. ε_{isgt} is the error term. Given the potential for error correlation across individuals who attended school with the same classmates in the third through fifth grades, we correct all standard errors to reflect clustering by the set of students who attended third through fifth grade in the same school.

We take several steps to ensure that the coefficient of interest φ_1 is not confounded by common shocks, which can cause problems for identification when individuals and peers share common treatments. As demonstrated by Lyle (2007), common shocks are most likely to be a problem when using contemporaneous measures of peer achievement, since own and peer contemporaneous achievement are influenced by common factors such as teachers or classroom lighting. Since our measure of peer quality is whether the child was *ever* exposed to domestic violence, common shock biases are less likely to be a problem.

Nevertheless, one may still be worried about common shocks specific to a school-grade-year. To bias our estimates, common shocks would have to be correlated with the level of domestic violence in a school-grade-year. While most of the common shocks we can think of would bias our results toward zero (e.g., the school counselor allocating more time toward cohorts with more children from troubled homes), we, nonetheless, take several steps to help alleviate this concern. First, we include school-grade specific linear time trends to account for the fact that some schools or neighborhoods may be worsening over time, affecting both domestic violence and achievement. Second, we control for school-by-year specific fixed effects, which suggests that any shock must differentially affect the cohort with the highest number of children exposed to domestic violence within a given school and year. Third, we demonstrate that our results are robust to the inclusion of student and cohort-level controls for race, gender, subsidized lunch status, and cohort size. Finally, we include family fixed effects and identify the effects using only within-family comparisons. Collectively, these tests imply that for a common shock to explain our results, it must affect the cohort with the most children from troubled homes without affecting the other grades in that school and year; it must affect that grade without affecting the family income, race, gender, or own domestic violence status of the children in that grade; and it must affect one child without affecting his brother. While one example would be the worst teachers systematically looped year-over-year with the worst cohorts of students within a particular school, we find such scenarios unlikely.

Finally, of critical importance to our identification strategy is that students are not systematically placed into or pulled out of a particular grade cohort within a school depending on the domestic violence status of the student or their peers. For example,

[10] This strategy is essentially a reduced-form instrumental variables approach in which peer domestic violence at the cohort level instruments for peer domestic violence at the classroom level. Our data do not contain classroom identifiers, so we are unable to estimate the structural IV estimate.

if parents with a high value of education were to pull their children out of a cohort with a particularly high proportion of peers from troubled families, such nonrandom selection would cause us to erroneously attribute lower student performance to the presence of the troubled peers.

We formally test for this and other types of self-selection by examining whether cohort size or other exogenous family characteristics, such as own domestic violence, race, gender, and household income are correlated with the proportion of peers exposed to domestic violence after conditioning on school-grade fixed effects. We find that the within-school variation in peer domestic violence is orthogonal to other determinants of student achievement, suggesting that our estimates are not biased by self-selection of students into or out of particular cohorts within a school. In addition, our within family estimates provide a particularly strong test of whether the peer effects are driven by the selection of certain families toward or away from cohorts with idiosyncratically high proportions of troubled peers.

II. Data and Results

A. *Data*

To implement our identification strategy, we use a confidential student-level panel dataset provided by the School Board of Alachua County in Florida. These data consist of observations of students in the third through fifth grades from 22 public elementary schools for the academic years 1995–1996 through 2002–2003. The Alachua County School District is a large school district. In the 1999–2000 school year, it was the 192nd largest school district in the country. Table 1 shows summary statistics for our data. The student population in our sample is approximately 55 percent white, 38 percent black, 3.5 percent Hispanic, 2.5 percent Asian, and 1 percent mixed. Fifty-three percent of students were eligible for subsidized lunches. The test score data consist of a panel of norm-referenced reading and mathematics exam scores from the Iowa Test of Basic Skills and the Stanford 9 exams. Reported scores reflect the percentile ranking on the national test relative to all test-takers nationwide.[11] For all academic outcome specifications, we report results using a composite score, which is calculated by taking the average of the math and reading scores.[12]

In addition, we observe the number of disciplinary infractions committed in school, each year, for every student in the sample, which represent "incidents that are very serious or require intervention from the principal or other designated administrator"

[11] In the 1999–2000 school year, the district switched from the Iowa Test of Basic Skills to the Stanford 9. Both exams test reading and math skills and report percentile rankings that show how the student ranks relative to students taking the same exam nationwide.

[12] Using a composite score has the advantage of increasing precision by reducing measurement error in the dependent variable (Martin R. West and Paul E. Peterson 2006). When we estimate our effects separately for reading and math scores, the peer coefficients are not statistically distinguishable from each other, and are generally within one half of a standard error of one another. For example, the coefficient corresponding to the result for the peer variable in Table 2, Specification 8, is 12.76 for reading scores and 17.32 for math scores. Separate results for math and reading scores for all of the specifications reported in the paper are available upon request from the authors.

Table 1—Descriptive Statistics

Panel A. Student demographics

Variable	Mean
Black	0.378 (0.485)
Male	0.493 (0.500)
Free/reduced lunch	0.532 (0.499)
Exposed to domestic violence	0.046 (0.210)
Boys exposed to domestic violence	0.023 (0.150)
Girls exposed to domestic violence	0.023 (0.150)
Peer domestic violence	0.046 (0.032)
Cohort size	87.3 (32.7)

Panel B. Academic outcomes by student type

Sample	Reading and math composite score	Number of disciplinary incidents
All students	52.91 (29.02)	0.56 (1.92)
Subsidized lunch	39.74 (26.08)	0.92 (2.46)
Unsubsidized lunch	68.00 (24.51)	0.16 (0.83)
All boys	50.98 (29.40)	0.84 (2.39)
All girls	54.80 (28.51)	0.29 (1.26)
Boys exposed to domestic violence	36.56 (25.00)	1.77 (3.68)
Girls exposed to domestic violence	40.79 (26.49)	0.53 (1.63)

Notes: Each cell contains the mean with the standard deviation in parentheses. Demographic and disciplinary variables are based on 44,882 observations. There were 42,478 observations containing test scores. Cohort refers to a group of children in the same grade, in the same school, in the same year. Average cohort size was computed at the cohort level ($n = 514$).

(School Board of Alachua County 1997). Finally, we observe information on each student's race, gender, school lunch status, and median zip code income.

The domestic violence data used in this study were gathered from public records information at the Alachua County Courthouse and include the date filed and the names and addresses of individuals involved in domestic violence cases filed in civil court in Alachua County between January 1, 1993 and March 12, 2003. These domestic violence cases are initiated when one family member (e.g., the mother) petitions the court for a temporary injunction for protection against another member of the family (e.g., the father or boyfriend).[13] Students were linked to cases in which the petitioner's first and last name and first three digits of her residential address matched the parent name and student's residential address in the annual school record. In that way, we were able to identify the set of students within a school-grade-year cohort who were ever matched to a domestic violence case from 1993 to 2003. In total, 4.6 percent of the children in the sample were linked to a domestic violence case filed by a parent, equally split between boys and girls. Sixty-one percent of these children were black and 85 percent were eligible for subsidized school lunches.

[13] The judge then decides whether to issue a 15-day injunction against the alleged offending party and sets a date for a hearing to decide on further action. If the request for a temporary injunction is denied, the petitioner is typically given the opportunity to provide more evidence that an injunction is necessary.

We examine how peers affect student performance and behavior using two different outcome variables from our school dataset. The academic performance outcome is a composite (average) score on the annual mathematics and reading scores on the Iowa Test of Basic Skills or Stanford 9 examinations. We also examine the total number of disciplinary incidents per student per year.

B. *Mean Effects*

Results from various specifications of equation (1) are shown in Table 2. Panels A and B, respectively, show results for academic achievement and disciplinary incidents. Specifications 1–8 start with a simple regression and progressively add controls.

Our estimated effects indicate that even in the most highly specified models, exposure to domestic violence in one's own home is associated with substantially lower achievement and higher levels of misbehavior. For example, results shown in Specification 8 indicate that children from troubled homes score 3.87 percentile points lower on math and reading exams and commit 0.31 (55 percent) more disciplinary infractions.

Next, we turn to whether peer exposure to children from troubled homes affects the academic achievement and behavioral problems of other children in the school, with special emphasis on addressing the validity of our identification strategy. We posit that if the within-school variation in peer domestic violence over time is exogenous to own achievement, then the magnitude of the estimated peer effects should remain relatively unchanged as we progressively add more covariates that are known to impact own achievement. In contrast, if adding controls such as individual and cohort-level controls or grade-year fixed effects affects the peer coefficient, then one might be concerned that our identification strategy does not fully overcome the problems of selection and/or common shocks.

Specification 1 begins by simply regressing math and reading test scores on the own and peer domestic violence variables. Specification 2 additionally controls for year fixed effects. Results indicate that peer domestic violence is associated with a very large decrease in student test scores; adding one more troubled student to a class of 20 is associated with a decline of 10.4 percentile points (0.05×207.30) for each of his classmates. However, as shown in Specification 3, controlling for school fixed effects causes the coefficient to drop substantially to -13.23 from -207.30. This demonstrates the extent of the selection problem. On average, lower-achieving students select into schools with higher proportions of peers exposed to domestic violence.

Importantly, the effect of troubled peers on test scores remains very stable at around -13 as school-grade fixed effects, school-grade specific linear time trends, grade-year fixed effects, individual controls, and cohort controls are progressively added to the model in Specifications 4–8. This provides strong evidence that the within-school variation in exposure to peers from troubled families is exogenous to own achievement, and implies that the resulting estimates are causal rather than being driven by selection or common shocks. Results for our preferred Specification 8 imply that adding 1 troubled child to a classroom of 20 students (roughly a 1

Table 2—Family Violence Linear-in-Mean Peer Effects

Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Reading and math composite score								
Own family violence	−12.63***	−12.64***	−10.11***	−10.15***	−10.09***	−10.09***	−3.85***	−3.87***
	(0.93)	(0.93)	(0.95)	(0.95)	(0.95)	(0.95)	(0.76)	(0.75)
Proportion peers with family violence	−211.45***	−207.30***	−13.23	−15.82*	−12.89	−13.09	−13.74*	−13.79*
	(15.31)	(14.50)	(8.92)	(9.23)	(9.62)	(9.58)	(7.76)	(7.70)
Observations	42,478	42,478	42,478	42,478	42,478	42,478	42,478	42,478
Panel B. Number of disciplinary incidents								
Own family violence	0.56***	0.56***	0.51***	0.51***	0.52***	0.52***	0.31***	0.31***
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.08)	(0.08)
Proportion peers with family violence	5.00***	5.04***	1.16	0.98	1.83**	1.78**	1.76**	1.86***
	(1.00)	(1.01)	(0.86)	(0.87)	(0.74)	(0.72)	(0.68)	(0.67)
Observations	44,882	44,882	44,882	44,882	44,882	44,882	44,882	44,882
Year fixed effects	No	Yes	Yes	Yes	Yes	—	—	—
School fixed effects	No	No	Yes	—	—	—	—	—
School-grade fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes
School-grade-specific linear time trends	No	No	No	No	Yes	Yes	Yes	Yes
Grade-year fixed effects	No	No	No	No	No	Yes	Yes	Yes
Individual controls	No	No	No	No	No	No	Yes	Yes
Cohort controls	No	No	No	No	No	No	No	Yes

Notes: Each column represents a different regression. Robust standard errors in parentheses are clustered at the level of the groups attending grades 3–5 together in the same school. Individual controls include gender, race, median family income, subsidized lunch status, and counselors. Cohort controls include race, subsidized lunch, gender, size, and number of counselors.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

standard deviation increase) causes the achievement of the other students to fall by 0.69 percentile points (0.05×13.79), which is statistically significant at the 10 percent level. The effect is approximately 1/40 of a standard deviation, or 18 percent of the effect of being directly exposed to domestic violence in one's own home.

Results for the number of disciplinary infractions are shown in panel B. As with test scores, the effect of peer domestic violence falls dramatically once we condition on school fixed effects. Progressively adding more controls changes the estimates very little with the exception of adding school-grade specific linear time trends, which increases the estimate from 0.98 to 1.83. While there are multiple explanations for why accounting for trends could impact the estimates, one relates to the potentially subjective nature of the disciplinary infractions variable. For example, if the neighborhood surrounding a school is worsening over time, that school will also experience relative increases in the proportion of children exposed to domestic violence. If school teachers and administrators respond to the trend by increasing the threshold above which a disciplinary infraction is reported, the peer effects estimate will be biased toward zero. Once controlling for school-specific linear time trends, the estimates, again, remain stable as grade-year fixed effects, individual controls, and cohort controls are added to the model. The preferred estimate in Specification 8 implies that adding one more troubled child to a class of 20 causes each child to commit 0.093 more infractions, an increase of 17 percent that is statistically significant at the 1 percent level.

TABLE 3—DIFFERENTIAL EFFECTS BY THE FAMILY INCOME OF THE TROUBLED CHILDREN AND THEIR PEERS

Outcome variable	Reading and math composite score	Number of disciplinary incidents
Specification	(1)	(2)
Own subsidized lunch family violence	−3.19***	0.32***
	(0.76)	(0.09)
Own unsubsidized lunch family violence	−7.39***	0.26**
	(1.92)	(0.12)
Proportion of subsidized lunch peers with family violence × subsidized lunch	−12.13	2.22**
	(8.86)	(1.00)
Proportion of subsidized lunch peers with family violence × unsubsidized lunch	−29.92**	0.48
	(12.76)	(0.84)
Proportion of unsubsidized lunch peers with family violence × subsidized lunch	5.65	6.96**
	(24.29)	(3.36)
Proportion of unsubsidized lunch peers with family violence × unsubsidized lunch	23.35	−1.39
	(26.24)	(2.17)
Observations	42,478	44,882

Notes: Each column represents a different regression. Robust standard errors in parentheses are clustered at the level of the groups attending grades 3–5 together in the same school. All specifications include school-grade and grade-year fixed effects, as well as school-grade-specific linear time trends. In addition, all specifications control for individual gender, race, median family income, and subsidized lunch status, as well as a full set of cohort-level controls including mean gender, race, subsidized lunch, and size by school/grade/year.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

C. *Differential Effects by Family Income*

Having found that troubled families impose statistically significant externalities on classroom peers, on average, we attempt to learn which subgroups of children exposed to domestic violence cause the spillovers and which groups of classmates are most affected. Doing so may provide insight into the potential mechanisms driving the results or provide potential policy implications for combating these negative peer effects, such as sorting students into classrooms or schools. In Tables 3 and 4, we examine the heterogeneity of these effects across the family income and gender of both the children exposed to the domestic violence and their classmates.

In Table 3, results show that the peers from low-income families exposed to domestic violence primarily drive the negative effects on reading and math achievement, and these spillovers are primarily incurred by children from higher-income families. The estimated effect is statistically significant at the 5 percent level and implies that adding one additional low-income troubled child to a classroom of 20 decreases the test scores of higher-income students by 1.5 percentage points, an effect more than twice the size of the average effect. Conversely, we find troubled children from both high- and low-income families cause statistically significant increases in misbehavior, but the increase in misbehavior occurs primarily among children from low-income families.

TABLE 4—DIFFERENTIAL EFFECTS BY THE GENDER OF THE TROUBLED CHILDREN AND THEIR PEERS

Outcome variable	Reading and math composite score	Number of disciplinary incidents
Specification	(1)	(2)
Own boy family violence	−3.51***	0.64***
	(1.01)	(0.15)
Own girl family violence	−4.17***	−0.02
	(1.09)	(0.05)
Proportion of boy peers with family violence × boy	−36.84***	6.65***
	(13.94)	(1.32)
Proportion of boy peers with family violence × girl	5.47	0.94
	(11.54)	(1.03)
Proportion of girl peers with family violence × boy	−13.19	0.29
	(13.05)	(1.32)
Proportion of girl peers with family violence × girl	−11.49	−0.63
	(12.94)	(1.01)
Observations	42,478	44,882

Notes: Each column represents a different regression. Robust standard errors in parentheses are clustered at the level of the groups attending grades 3–5 together in the same school. All specifications include school-grade and grade-year fixed effects, as well as school-grade-specific linear time trends. In addition, all specifications control for individual gender, race, median family income, and subsidized lunch status, as well as a full set of cohort-level controls including mean gender, race, subsidized lunch, and size by school/grade/year.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

D. *Differential Effects by Gender*

Results examining the extent to which the classroom spillovers vary by gender are shown in Table 4. We find that boys who come from troubled families primarily cause the negative effects on achievement and behavior and that these effects manifest themselves primarily in boys. The coefficient for boys on boy-peer family violence (−36.84) implies that adding one additional troubled boy peer to a classroom of 20 students decreases boys' test scores by nearly 2 percentile points. Estimates from Specification 2 predict that adding one additional troubled boy peer to a classroom of 20 students increases the number of infractions each boy will commit by 0.33, or 40 percent.

In summary, results from Tables 3 and 4 provide two interesting findings. First, low-income children and boys from troubled families primarily impact the behavior and academic performance of their classmates. Second, troubled children appear to primarily impact the math and reading achievement of boys and children from high-income families.[14]

[14] In results not shown, we find that the proportion of boys from troubled families has a statistically significant effect on the misbehavior of black girls. We also find that exposure to black girls from troubled families within

E. *Robustness Checks*

We provide two robustness tests of our results. First, we include school-by-year fixed effects, which control for any common shocks to schools in that particular year. Second, we include family fixed effects. This allows us to test whether our effects are driven by common shocks that affect an entire family, or by the inability of certain families to move their children out of cohorts with above-average exposure to peers from troubled homes. Such nonrandom selection would cause us to erroneously attribute lower performance to the presence of troubled peers.

We note that both of these tests represent high bars that substantially limit the useable variation in peer domestic violence[15] and could potentially bias the estimates toward zero. For example, controlling for school-year specific fixed effects helps overcome potential biases due to a common shock to a particular school in a particular year. However, doing so may bias the estimates toward zero, since children in one grade almost certainly interact with children in the other grades during recess and after school. Similarly, while the inclusion of family fixed effects will help rule out the possibility that a certain family trait is correlated with peer domestic violence, interactions between the siblings at home likely bias the estimates toward zero.

Results in Specification 3 of Table 5 show that the effects on academic achievement are quite robust to the inclusion of school-year and family fixed effects. The magnitudes of both the mean effect and the effect of troubled boys on boys are virtually unchanged and are statistically significant at the 5 percent and 1 percent levels, respectively. The estimated effect of troubled children from low-income households on the achievement of their high-income classmates is reduced by about one-third from −29.92 to −19.31, and is significant at the 10 percent level.

Collectively, these results provide strong evidence that the effects we find are not driven by family selection or by shocks that are common to either families or to a school in a given year. For example, in order for selection to be driving the results, it would have to be that parents systematically place their high-ability children in "good" cohorts, and their low-ability children in "bad" cohorts, within a given public elementary school. Similarly, for a region-time specific negative common shock to be responsible for the effects found, it must systematically affect boys with greater exposure to troubled peers more than it affects their brothers, and systematically affect those in a school-grade-year with greater exposure to peers from troubled families more than those in a different grade in the same school and year.

The robustness results for the disciplinary outcome shown in Specifications 4–6 are more mixed. While including school year and family fixed effects causes the overall impact of troubled peers on disciplinary infractions to become insignificant, the effect of troubled boys is reduced by half, but is still statistically significant at the

a cohort has a statistically significant negative effect on the achievement of black girls. We find no effect of any group of troubled children on the achievement of nonblack girls.

[15] Web Appendix Table A1 shows the usable variation in the peer domestic violence variable after conditioning on our various sets of controls. Adding school-year fixed effects and sibling fixed effects reduces the variation in the peer domestic violence variable by nearly 60 percent compared to our preferred specification.

Table 5—Robustness Checks

Outcome variable	Reading and math composite score			Number of disciplinary incidents		
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Table 2 results						
Proportion peers with family violence	−13.79*	−8.94	−13.81**	1.86***	0.35	−0.38
	(7.70)	(6.86)	(6.31)	(0.67)	(0.60)	(0.61)
Panel B. Table 3 results						
Proportion of subsidized lunch peers with family violence × unsubsidized lunch	−29.92**	−25.47*	−19.31*	0.48	−1.19	−1.86**
	(12.76)	(12.98)	(10.01)	(0.84)	(0.92)	(0.82)
Panel C. Table 4 results						
Proportion of boy peers with family violence × boy	−36.84***	−34.60***	−36.36***	6.65***	4.39***	3.23***
	(13.94)	(12.69)	(12.15)	(1.32)	(1.18)	(1.25)
Robustness check	Baseline specification from Tables 2, 3, and 4	School-year fixed effects	Sibling and school-year fixed effects	Baseline specification from Tables 2, 3, and 4	School-year fixed effects	Sibling and school-year fixed effects

Notes: Each column represents a different regression. Robust standard errors in parentheses are clustered at the level of the groups attending grades 3–5 together in the same school. All specifications include school-grade and grade-year fixed effects, as well as school-grade-specific linear time trends. In addition, all specifications control for individual gender, race, median family income, and subsidized lunch status, as well as a full set of cohort-level controls including mean gender, race, subsidized lunch, and size by school/grade/year.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

1 percent level.[16] These results are not so surprising as the potential for downward bias may be particularly acute for disciplinary infractions because across-cohort and within-family interactions occur more frequently in nonacademic settings (i.e., recess, lunch, and in neighborhoods).

F. *Falsification Tests*

To further test for nonrandom selection of students into or out of particular school-grade-year cohorts, we perform a series of falsification tests in which we regress exogenous student characteristics on the peer family violence variables while conditioning on school-grade fixed effects. So long as the within-school variation in peer domestic violence is uncorrelated with selection into or out of the cohort, we would expect to observe zero correlation.

These results are presented in Table 6. Specification 1 is a randomization test in which we examine whether the within-school-grade variation in the proportion of peers exposed to domestic violence is uncorrelated with one's own exposure to domestic violence. To overcome the mechanical negative bias in performing this test,

[16] Although unreported, including school year and sibling fixed effects causes the coefficient measuring the impact of high-income troubled children on their low-income peers to be reduced by one-third from 6.96 to 4.54 ($p = 0.104$). Doing so causes the impact of low-income troubled children on the misbehavior of their low-income classmates to be small and statistically insignificant.

TABLE 6—FALSIFICATION TESTS: THE EFFECT OF PEER FAMILY VIOLENCE ON EXOGENOUS STUDENT CHARACTERISTICS

Outcome variable	Own DV	Cohort size	Subsidized lunch	Black	Boy	Log median zip code income	Dropout of sample after third or fourth grade	Missing test score
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proportion of boy peers with family violence	−0.003	3.79	0.21	0.05	0.11	−0.11	0.15	−0.23
	(0.03)	(33.85)	(0.17)	(0.22)	(0.19)	(0.08)	(0.12)	(0.25)
Proportion of girl peers with family violence	0.01	−20.17	−0.26	−0.26	−0.09	−0.05	0.14	0.18
	(0.03)	(40.05)	(0.17)	(0.26)	(0.22)	(0.10)	(0.14)	(0.26)
Observations	44,882	514	44,882	44,882	44,882	44,454	27,412	44,882

Notes: Each column represents a different regression. Robust standard errors in parentheses are clustered at the level of the groups attending grades 3–5 together in the same school. All specifications include school-grade fixed effects and control for own family violence by gender. Specification 1 additionally controls for the set of possible peers exposed to domestic violence in order to overcome the negative mechanical bias of that randomization test, as proposed by Guryan et al. (2009).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

we apply the method proposed by Guryan, Kroft, and Notowidigdo (2009) in which we control for the set of possible peers exposed to domestic violence.[17] Results show there is no systematic correlation between peer domestic violence and own domestic violence. This provides strong evidence that within-school variation in peer domestic violence is effectively random. The result also provides further evidence that a common shock is not impacting both peer and own exposure to domestic violence, and that reflection is unlikely biasing our results.

Results from Specifications 2–6 indicate that there is little relationship between peer domestic violence and cohort size, family income, race, or gender. The lack of a correlation indicates that the results are unlikely to be due to parents selectively removing their children from cohorts with idiosyncratically high exposure to peers from troubled homes.

In Specification 7, we examine whether students with high idiosyncratic exposure to troubled peers in the third or fourth grade are less likely to remain in the same school the following year. Results show that exposure to troubled children is unrelated to the exit rate. Finally, in Specification 8, we find that exposure to peer domestic violence is also uncorrelated with missing test scores.

In summary, we find no evidence that the cohort composition, exit rates, or test-taking of students in our sample is correlated with exposure to children from troubled homes once we condition on school-grade fixed effects. These falsification tests provide further evidence that the results presented earlier are not due to nonrandom selection into or out of school-grade-year cohorts.

[17] Specifically, we include a control for the proportion of peers exposed to domestic violence at the school-grade level.

G. *Discussion*

One important question is the channel through which the peer effects operate. Broadly speaking, these troubled children could affect the learning and behavior of their classmates either through their own disruptive behavior or through their own (poor) academic performance. For example, students' achievement may suffer because they are distracted by the behavior of the troubled peers. Alternatively, achievement might suffer because there are fewer students from whom to learn or because students from troubled homes learn more slowly and slow down the learning of their peers.

The coefficients on the own domestic violence variables in Tables 3 and 4 show that, on average, all children from troubled homes experience substantially lower academic achievement. Additionally, both boys and girls experience large reductions in performance due to domestic violence at home, with children from high-income families experiencing the largest drops. Consequently, if the peer effects were to operate solely through the achievement channel, we would expect the negative spillovers to be caused by both troubled girls and boys, especially by children from high-income families.

In contrast, our results show that the negative peer effects are primarily driven by the subgroups most likely to be disruptive (as measured by disciplinary infractions): boys and children from low-income families. Children from low-income families commit nearly six times as many infractions as children from high-income families, while boys commit nearly three times as many infractions as girls. These results support a model that predicts student disruption is the primary channel through which the effects operate.

Less clear is why the disruptive children primarily impact the academic achievement of children from high-income families while affecting the behavior of children from poorer families. One potential interpretation offered by a school counselor with whom we spoke is that children from low-income families are more accustomed to family disruption and may be less academically sensitive to the negative behavior of their peers. Similarly, children from low-income families may be more likely to respond behaviorally to disruptive children compared to higher-income children since the latter, on average, likely face more repercussions at home for misbehavior in school.

One may also wonder why children from families linked to domestic violence are disruptive in school. This is a particularly challenging question given that researchers have consistently found, as we have, that domestic violence is correlated with other negative family characteristics such as poverty, unemployment, substance abuse, and low educational attainment (Fantuzzo et al. 1997). While we cannot conclusively attribute our results as the causal effect of domestic violence per se, in results not shown, we find that the effects are almost entirely driven by the children whose parents had not yet reported the domestic violence, but would do so at some point in the future. This finding is consistent with survey research by Judy Hails Kaci (1994) who finds that 87 percent of domestic violence respondents indicated that the reporting of the violence "helped stop the physical abuse."

Finally, we think it is important to note that while our results capture how children from troubled homes affect their classmates at school, they may understate the full extent to which children exposed to family violence impose negative spillovers on others. For example, troubled children almost certainly interact with children from other cohorts in school and in their neighborhoods. Consequently, one might reasonably interpret our estimates as a potential lower bound.

III. Conclusion

Measuring the extent to which family problems spill over to children outside the home has thus far been difficult due to data constraints and methodological problems. We estimate these externalities by examining the extent to which children from troubled families—as signaled by the presence of domestic violence within the family—negatively affect their classroom peers. To do so, we utilize a unique dataset in which children's school records are matched to domestic violence cases filed by their parent. Because these children are troubled for a reason exogenous to their peers, we can estimate these negative spillovers free from the reflection problem that has been difficult to overcome in the existing peer effects literature. In addition, the panel nature of our data allows us to control for school-by-grade fixed effects and to identify the externalities by comparing cohorts with idiosyncratically high proportions of troubled peers to cohorts in the same school and grade in a different year with idiosyncratically low proportions of troubled peers.

We find that children exposed to domestic violence significantly decrease the reading and math test scores of their peers and significantly increase misbehavior by others in the classroom. Specifically, we estimate that one more troubled peer in a classroom of 20 students reduces student test scores by 0.69 percentile points and increases the number of disciplinary infractions committed by 17 percent. This implies that given Carlson's (2000) estimate that roughly 15 percent of children are exposed to domestic violence every year, the total per student external marginal damage caused by these troubled families is a 2 point reduction in test scores and a 51 percent increase in the number of disciplinary infractions. We also find that these externalities vary across gender and family income, and appear to be caused primarily by boys from troubled families.

We conclude that our results are not biased by selection into or out of school-by-grade-by-year cohorts, since neither cohort size nor cohort composition (as measured by own domestic violence, race, gender, and household income) is correlated with the proportion of troubled peers. Similarly, our results are unaffected by the inclusion of controls for own or peer characteristics. Finally, our academic achievement results are robust to using only within-family variation in exposure to troubled children and including school-by-year fixed effects. This helps rule out the possibility that the results are being driven by the negative *unobserved* attributes of families whose children are exposed to an idiosyncratically high proportion of troubled peers or by common shocks to all children in the same school and year.

These results have significant implications for education and social policy. They provide strong empirical evidence of the existence of the "bad apple" peer effects model, which hypothesizes that a single disruptive student can negatively affect the outcomes of all other students in the classroom. They also suggest that school policies that change the composition of students across classrooms and schools may hurt the performance of groups left more exposed to children from troubled families. Finally, our results are also relevant for social policy in that they suggest that the social costs of troubled families likely extend beyond the private costs borne by the children in the home. Consequently, any intervention that reduces family conflict may well have larger positive effects than previously thought.

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