Family Business or Social Problem? The Cost of Unreported Domestic Violence

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Abstract

Social interest in problems such as domestic violence is typically motivated by concerns regarding equity, rather than efficiency. However, we document that taking steps to reduce domestic violence by reporting it yields substantial benefits to external parties. Specifically, we find that while children exposed to as-yet-unreported domestic violence reduce the achievement of their classroom peers, these costs disappear completely once the parent reports the violence to the court. This suggests the public has an interest in helping families overcome their problems in general, and to report domestic violence in particular. It also suggests that social and judicial interventions may help combat negative peer effects in schools.

JEL Codes: J12 (Domestic Abuse), D62 (Externalities), I21 (Analysis of Education)

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1. Introduction

Domestic violence is a significant problem facing families in the United States. A study by the US Department of Justice found that 1.3 million women are physically assaulted by an intimate partner annually in the United States, and 22 percent of women have been physically assaulted by an intimate partner in the past (Tjaden and Thoennes, 2000). Exposure to domestic violence imposes significant costs on both the victimized adult and her children. Adult women abused by their partners incur $4.1 billion per year in direct costs for medical and mental health services, in addition to lost productivity and psychological costs (CDC, 2003). Children exposed to domestic violence experience a number of social and emotional problems such as aggressive behavior, depression, anxiety, decreased social competence, and diminished academic performance (Edleson, 1999; Wolfe, Crooks, Lee, McIntyre-Smith, and Jaffe, 2003; Fantuzzo & Mohr, 1999; Koenen, Moffitt, Caspi, Taylor, and Purcell, 2003).

Perhaps the most obvious policy intervention to help the victimized families is to encourage victims to report the violence and enlist the help of the judicial and/or social systems. This is potentially very important, as it enables the victim to get protection enforced by the police, as well as access other support such as women's shelters, etc. In addition, children exposed to domestic violence may also benefit from reporting: school counselors with whom we have spoken say that they often first learn that a child was exposed to domestic violence once it was reported, which helps them better address the student's needs.

In fact, encouraging reporting is a major goal of current policy; the Federal Government encourages reporting domestic violence as a step toward resolving the problems in the home (USDA, 2009), and established a website and telephone hotline that offers counseling and

referrals to shelters, among other things.[1] However, underreporting is still believed to be widespread, with fear of retribution being the primary reason (Rodriguez, Bauer, McLoughlin, and Grumbach, 1999). Consequently, there is considerable debate over other policy options such as mandatory reporting by education and health professionals (e.g., Rodriguez, McLoughlin, Nah, and Campbell, 2001).

Importantly, the motivation behind nearly all policy responses to domestic violence is to provide justice for the victims of the violence. However, a broader policy question is whether outside parties have an interest beyond altruism in helping families overcome domestic problems in general, and violence in particular. In this study, we examine whether taking steps to reduce domestic violence by reporting it to the court reduces the negative peer effects caused by children exposed to family violence. Thus, we seek to provide for a more complete accounting of the social costs of unreported domestic violence.

To do so, we exploit a unique dataset in which a panel of administrative school records containing performance on annual math and reading standardized exams is linked to public records on domestic violence. This enables us to examine how the parental reporting of domestic violence affects classmates' performance. These panel data also allow us to identify peer effects from reported versus unreported domestic violence by exploiting the within-school variation in exposure to children from violent homes. Following the methodology of Carrell and Hoekstra (2009), we perform several robustness tests and falsification exercises to demonstrate that the within-school variation in peer domestic violence is exogenous to own achievement and uncorrelated with other individual and cohort characteristics.

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[1] See http://www.ndvh.org/.

Results indicate that the social costs of unreported domestic violence extend well beyond the immediate family. Specifically, we show that while children exposed to as-yet-unreported domestic violence significantly reduce the reading and math achievement of their peers in school, these negative spillovers are virtually eliminated once the parent reports the domestic violence to the court. Thus, our results suggest that reporting domestic violence is likely a critical link to undoing the negative spillovers caused by children from troubled families.

Our findings have two important policy implications. While an increasing amount of evidence has focused on credibly estimating peer effects in education (e.g., Lavy, Paserman, and Schlosser, 2007; Carrell and Hoekstra, 2010), little is known regarding how to combat negative peer effects caused by disruptive students.[2] This is important for several reasons. First, research suggests that the magnitude of negative classroom spillovers is large: estimates presented in this paper indicate that adding two disruptive children to a classroom of twenty students causes the same negative effect on classmates as being assigned to a teacher who is one standard deviation below average quality. Second, survey evidence shows that classroom disruptions are correlated with low teacher morale and turnover,[3] which is an especially critical issue for urban schools found to have greater difficulty in attracting and retaining quality teachers (Hanushek, Kain, and Rivkin, 2004; Lankford, Loeb, and Wyckoff, 2002). Finally, a major criticism of education policies such as tracking and school choice is that such policies may leave some disadvantaged students even more exposed to disruptive students. While recent work on tracking has raised doubt regarding this concern (e.g., Duflo, Dupas, and Kremer, 2008; Figlio and Page, 2002), the

[2] An important exception is Aizer (2008), who shows that treating Attention Deficit Disorder improves peer test scores.

[3] For example, teachers in the 1999-2000 Schools and Staffing Survey who reported disruption as a problem in their school are more than three times as likely to also say they "definitely plan to leave teaching as soon as I can."

crux of our study is that solutions to these concerns need not be limited to the scope of education policy. Instead, our results suggest that social and judicial policy may be effective at reducing the negative spillovers caused by disruptive students.

Perhaps even more importantly, our results also suggest that the public has a compelling interest in helping troubled families overcome domestic problems in general, and to report domestic violence in particular. Our findings also help bolster the case of those who favor laws mandating that health care or other professionals report evidence of domestic abuse.

2. Data

We use a confidential student-level dataset containing a panel of annual test scores provided by the School Board of Alachua County (SBAC) in Florida. The data cover every 3rd through 5th grader in the twenty-two elementary schools in the county from the 1995-1996 academic year through 2002-2003. Alachua County is a large school district containing nearly 30,000 students, making it the 192nd largest school district in the county in 1999-2000 among the nearly 15,000 districts nationwide. The school system is also demographically diverse: 38 percent of students are black and 53 percent are eligible for free or reduced lunches.

The test score measure reflects the percentile ranking on the Iowa Test of Basic Skills and Stanford 9 exams.[4] Specifically, we use the average percentile ranking on the math and reading sections of the test, though we note that separate results for reading and math are qualitatively similar and are available in the web appendix Table A2. Over ninety percent of students took the

[4] In the 1999-2000 school year, the district changed from the Iowa Test of Basic Skills to the Stanford 9 exam. Both exams test reading and math skills and both report how the student ranks relative to students taking the same exam nationwide.

test in a given year. Additional variables include student race, gender, subsidized lunch status, neighborhood family income (from the Census using zip codes), school attended, and the number of counselors at the school.[5]

These school data were then linked to public records on domestic violence. To do so, the petitioner's name and first three digits of the student's residential address were matched to the student address and parent name from the administrative school data. These civil cases are filed when a parent (typically the mother) requests a temporary injunction for protection against another member of the family (most often the father or boyfriend). The judge then decides whether to issue a temporary 15-day injunction against the alleged perpetrator and schedules a date at which time both parties appear in court. While there is significant judicial discretion in how the case proceeds, it is worth noting that DeJong and Burgess-Proctor (2006) rate Florida in the top 18 percent of states in terms of having victim-friendly personal protection order laws.

Table 1 shows summary statistics of the data. Just less than 5 percent of the children in our sample had a parent who filed a domestic violence case at some point; 60 percent of those students are observed after the domestic violence was reported while 40 percent lived in a household that had not yet reported domestic violence when the student-level observation was recorded.

[5] While we also observe the number of disciplinary infractions committed by each student, for brevity these results can be found in web appendix Table A2. Additionally, reporting of disciplinary infractions may be endogenous to knowledge of domestic violence. That is, we are concerned that the threshold for reporting an infraction may be affected by knowledge (i.e., reporting) of peer domestic violence in the classroom.

3. Identification Strategy and Methodology

In assessing whether parental reporting of domestic violence yields external benefits, we extend the analysis of Carrell and Hoekstra (2010), who show significant negative peer effects associated with children *ever* exposed to domestic violence. To do so, we must overcome the difficulties with credibly estimating peer effects in general. The first identification challenge is that because child and peer outcomes are determined simultaneously, a "reflection" problem arises because it is difficult to distinguish the effect the child's classmates have on her from the effect she has on her classmates (Manski, 1993). We overcome reflection by using a measure of peer quality—exposure to reported or as-yet-unreported domestic violence—that is unlikely to be caused by a child's peers.[6]

The second identification problem is self-selection, which is caused by students and their families sorting themselves into neighborhoods and schools based on observed and unobserved characteristics. Thus, it can be difficult to distinguish whether a child's performance is due to her peers or to the reasons that caused the individuals to join that group. While researchers studying peer effects in higher education have exploited the randomization of peer groups,[7] like other papers using elementary school data, we utilize the natural population variation in cohort composition within a given school by utilizing school and time fixed effects.[8] Importantly, this

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[6] Other researchers have used different preexisting measures such as peer race and gender (Hoxby & Weingarth, 2006; Hoxby, 2000b; Lavy & Schlosser, 2007), peers who relocated from low socioeconomic regions (Angrist and Lang, 2004; Imberman, Kugler, and Sacerdote, 2009), boy peers with feminine names (Figlio, 2007), and peers who had previously been retained (Lavy, Paserman, and Schlosser, 2007).

[7] For example, see Foster, 2006; Sacerdote, 2001; Zimmerman, 2003; Lyle, 2007; Stinebrickner & Stinebrickner, 2006; Kremer & Levy, 2008; Carrell, Fullerton, & West, 2009; Carrell, Malmstrom, & West (2008).

[8] This approach was first used by Hoxby (2000a) to examine the impact of class size on achievement. It has subsequently been used by several papers to examine peer effects, including Aizer (2008); Hoxby (2000b); Hoxby

approach overcomes bias that may arise due to nonrandom sorting into classrooms. This is critical, as it implies any difference in the effect of peer reported versus unreported domestic violence is not confounded by a systematic change in classroom assignments once the violence is reported.

To formally estimate the negative spillovers caused by children from troubled families and to determine whether social policy can combat them, we estimate the following equation using ordinary least squares:

$$y_{isgt} = \phi_0 + \phi_1 PeerUnreportedDV_{sgt} + \phi_2 PeerReportedDV_{sgt} + \beta_1 X_{isgt} + \beta_2 \overline{X}_{sgt} + \lambda_{sg} + \sigma_{gt} + \varphi_{sg} t + \varepsilon_{isgt}$$

where y_{isgt} is the outcome variable for individual *i* in school *s* grade *g*, and in year *t*. λ_{sg} is a set of school-by-grade fixed effects, σ_{gt} is a set of grade-year fixed effects, and $\varphi_{sg} t$ is a set of school-by-grade specific linear time trends included to account for any changes in the neighborhood or school over time. X is a vector of individual characteristics including own family violence (reported and unreported), race, gender, subsidized lunch, and median zip code income and $\overline{X}$ measure average cohort-level race, gender, subsidized lunch and size. Given the potential for error correlation across classmates who attended grades three through five in the same school, we cluster at the level of students who progressed through elementary school together. In additional specifications we also make both within-family and within-individual comparisons (i.e., including sibling and individual fixed effects) and control for school-by-year specific fixed effects.

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and Weingarth (2006); Carrell and Hoekstra (2009); Lavy, Paserman, and Schlosser (2007); and Lavy and Schlosser (2007).

The primary variables of interest are *PeerReportedDV* and *PeerUnreportedDV,* which measure the proportion of a student's peers in the same grade, school, and year whose parents had and had not yet reported domestic violence, respectively. Our primary identification assumption is that the within-school variation in peer domestic violence, and in particular the timing of the reporting, is exogenous to own achievement. Results in Table A1 in the web appendix provide evidence to support this assumption; peer domestic violence is uncorrelated with own domestic violence, cohort size, race, gender, household income, attrition,[9] and missing test scores once we condition on school-grade fixed effects. These tests also indicate that there is no evidence of selection into or out of cohorts with idiosyncratically high or low proportions of children from troubled families. In addition, if reflection were present, one would expect to observe a positive correlation between own domestic violence and peer domestic violence. Results show this is not the case.[10]

In addition, while there is a general concern in the literature that peer effect estimates can be biased by "common shocks" that arise when students and their peers are subjected to the same influences (Lyle, 2007), there are several reasons to believe this is not a problem in our study. First, since our study compares cohorts within the same school and grade, it is not obvious how peer exposure to reported or unreported domestic violence could be correlated with, say, below-

[9] In addition to testing for whether students subject to peer reported or unreported domestic violence are differentially likely to leave the sample or not take the exam, we also test whether children from troubled families are themselves any more or less likely to leave the sample once the violence is reported. We find there is only a small and insignificant difference (0.6 percentage points) in the likelihood of leaving the sample after 3rd and 4th grade for children with reported versus unreported domestic violence. The attrition rates of both groups are significantly lower (2.3 and 2.9 percentage points) than those of students from more stable homes. Thus, our results do not appear to be driven by systematic attrition of children exposed to domestic violence or by their peers.

[10] To overcome the mechanical negative bias that arises in performing this test, we apply the technique proposed by Guryan, Kroft, and Notowidigdo (2009) in which we additionally control for the proportion of peers exposed to domestic violence at the school-grade level.

average teacher quality. In addition, we show the results are robust to school-by-year specific fixed effects, which suggests that any confounding common shock must differentially affect the cohort with the highest proportion of troubled children but not the children in the other grades in that school in that year. Furthermore, in some specifications we also include school-by-grade specific linear time trends to account for the fact that some schools and neighborhoods may be worsening over time, affecting both domestic violence and achievement. Finally, we show that the results are robust to within-family comparisons, which rules out the possibility that family-specific common shocks correlated with peer domestic violence are biasing our results.

Finally, we also need to know that any differences in the peer effects of children exposed to unreported and reported domestic violence are not due to compositional changes. For example, one might worry that families who report domestic violence when the child is older are different in unobserved ways than families who report the violence when the child is younger. To test for this, we note that this type of selection story should cause smooth changes over time in the size of the negative peer effect. In contrast, if the act of filing itself matters, one would expect a sharp break in the estimated peer effect at the time of reporting. Consequently, we also estimate a more flexible specification where we estimate separate peer variables for the years leading up to, during, and after the reporting of the violence. By observing whether the peer effect changes abruptly at the time of the reporting, we can distinguish the effect of reporting from the effect of confounding factors such as compositional changes.

However, one might also be concerned that peer composition changes in a less gradual way. For example, perhaps when a child's parent reports domestic violence, the child is placed in a different peer group. Thus, we test whether own exogenous characteristics are correlated with either peer reported or peer unreported domestic violence. We also test whether selection

into or out of schools is correlated with peer reported and unreported domestic violence. Results are in Table A1 in the Appendix. In short, we find no evidence that exposure to children with reported or unreported domestic violence is correlated with own student and family characteristics such as race, sex, family income, or own domestic violence status.

4. Results

Estimates of the effects of peers exposed to reported and as-yet-unreported domestic violence are shown in Table 2. Specifications 1 through 8 start with a simple regression and progressively add controls as shown in equation (1). 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 unchanged as we progressively add more covariates that are known to impact own achievement. In contrast, if adding individual and cohort-level controls or grade-year fixed effects affects the peer coefficient after including school fixed effects, then one might be concerned that our identification strategy does not fully overcome the problems of self-selection and/or common shocks.[11]

Specification 1 shows results from regressing math and reading test score on only the own and peer domestic violence variables, while Specification 2 additionally controls for year fixed effects. While both specifications indicate that unreported and reported peer domestic violence are associated with a very large decrease in student test scores, the coefficients are reduced substantially once we control for school fixed effects in Specification 3. This demonstrates the extent of the selection problem; on average, lower-achieving students select

[11] See Carrell & Hoekstra (2009) for an in-depth discussion of selection and common shocks when estimating peer effects.

into schools with higher proportions of peers exposed to reported and unreported domestic violence.

Importantly, the effects remain quite stable as school-grade fixed effects, grade-year fixed effects, individual controls, cohort controls, and school-grade specific linear time trends are added to the model in Specifications 4 through 8. This provides 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.

Across Specifications 3 through 8 that utilize within-school population variation, the pattern is striking: while exposure to children from families with as-yet-unreported domestic violence reduces achievement significantly, the negative peer effect disappears once the parent reports the domestic violence. Our estimated effects from our preferred Specification 8 imply that adding one child whose parent has not yet reported the domestic violence to a class of twenty causes peer achievement to fall by a statistically significant 1.4 percentile points (-27.56*0.05).[12,13] In contrast, we find virtually no evidence of negative spillovers from children from families who previously reported the domestic violence and can reject the null hypothesis that the effects of peers with reported and unreported domestic violence are equal at the 5 percent level.

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[12] This is approximately one-twentieth of a standard deviation. This effect is relatively large; estimates from the teacher quality literature imply that a one-standard deviation increase in teacher quality causes one-tenth of a standard deviation increase in achievement (Kane, Rockoff, and Staiger 2008; Kane and Staiger, 2009; Rivkin, Hanushek, and Kain, 2005).

[13] This effect is twice as large as the 0.7 percentile point decline caused by adding a peer who was *ever* exposed to domestic violence, as reported in Carrell and Hoekstra (2009).

In Specifications 9 through 11, we examine the robustness of this result by progressively including school-year fixed effects, sibling fixed effects, and individual fixed effects. While these specifications substantially reduce the within-school variation in peer domestic violence over time, they also provide rigorous tests of whether the spillovers from domestic violence end once the violence is reported.

Results shown in Specification 9 that control for school-year fixed effects indicate that our results are not driven by school-by-year specific common shocks, such as changes in the administration. In Specification 10, including a family fixed effect reduces the estimated effect of peer as-yet-reported violence only slightly[14], while there remains no effect of exposure to children from families that reported domestic violence in the past. The robustness of the result to the inclusion of family fixed effects indicates that the results are not being driven by either the selection of certain families into cohorts with idiosyncratically high proportions of domestic violence or by shocks common to a family.

We next examine whether the change in the peer effect is driven by differences in the types of students exposed to children from families with unreported versus reported violence rather than by the judicial intervention. To do so, in Specification 11 we control for individual fixed effects. Consequently, this estimate is identified primarily by comparing how children perform before and after their peers' parent report the domestic violence. Results show that while children from families with unreported domestic violence cause a statistically significant 1.2 percentile point decline in peer achievement, children from families where the domestic

[14] This result is consistent with the likelihood that within-family spillovers may be biased toward zero. That is, one child's exposure to disruptive peers at school may well spill over to the child's siblings through their interactions at home, biasing the within-family estimates toward zero.

violence was previously reported cause no such decline. Since these effects are identified using only within-student exposure to troubled peers, it rules out the possibility that our results are driven by different types of children being exposed to peers from families with reported violence.

Finally, in Specification 12, we show results from a value-added model, in which we control for lagged test score. Results are consistent with the other results: adding one student with unreported domestic violence reduces scores by 1.2 percentile points, while adding a student with reported domestic violence has no effect on peer achievement.

5. The Timing of Reporting and Further Sensitivity Analysis

While the results of Table 2 are robust to individual and family fixed effects, there may be additional concerns regarding identification of the effect of reporting on peer achievement. For example, one might be concerned that parental reporting of domestic violence may encourage other parents to also report domestic violence within the home. While this type of scenario seems unlikely ex ante, we also note that it is inconsistent with our empirical findings. First, in results available in the web appendix (Table A1, Specification 3) we find that own domestic violence is uncorrelated with peer domestic violence. That is, there is no evidence of spillovers in reporting. Second, in results available in Table A3, we find that the largest negative peer effects are incurred by children from families who are not eligible for subsidized lunches.[15] However, of the children linked to domestic violence in our sample, only 15 percent are from

[15] Specifically, in the result corresponding to specification 8 from Table 2, we find that the effect of adding one child with unreported domestic violence to a classroom of 20 reduces test scores by 1.89 points for higher-income families and 1.61 points for lower-income families

these higher-income families. Thus, it is unlikely that spillovers in reporting of domestic violence is the mechanism through which reporting domestic violence affects a child's peers.

One might also worry that students in later grades—who are more likely to have had a parent report domestic violence—may be less affected by disruptive peers than peers in earlier grades. In that case, we could be confounding a heterogeneous treatment effect with a reporting effect. Again, however, in results available in Table A3 of the web appendix, we find that if anything, 5th-graders incur the *largest* negative peer effects.[16]

Perhaps the most serious remaining concern regarding identification is whether the time at which the mother reports the domestic violence is correlated with other determinants of her child's behavior. For example, if families that wait until their children are older before reporting domestic violence have the most disruptive children for other unobserved reasons, then we may mistakenly be attributing the reduction in the peer effect to reporting. To investigate this issue directly, we estimate a more flexible form of equation (1) in which we include a larger set of peer variables that allows the peer effect to vary across the years before and after the domestic violence is reported. If heterogeneous treatment effects or selection is responsible for the change, we would expect the change in the peer effect to be gradual. In contrast, if reporting itself matters for the peer effect, we would expect an abrupt change in the peer effect.

Results are shown in Table 3. Estimates indicate that even four years before the parent reported domestic violence, there is evidence that her child causes negative spillovers. This negative effect is largest and most precisely estimated in the year in which the restraining order

[16] Specifically, in the result corresponding to specification 8 from Table 2, we find that the effect of adding one child with unreported domestic violence to a classroom of 20 reduces test scores for 3rd, 4th, and 5th graders by at statistically significant 1.5, 1.6, and 2.3 points, respectively.

is sought. Specifically, the model estimates that adding one child to a classroom in the year his mother reports the domestic violence causes the math and reading performance of all other students to fall by 2.2 (-44.29 * 0.05) percentile points. This result likely reflects that the home situation gets worse before it finally causes the parent to report the domestic violence.[17]

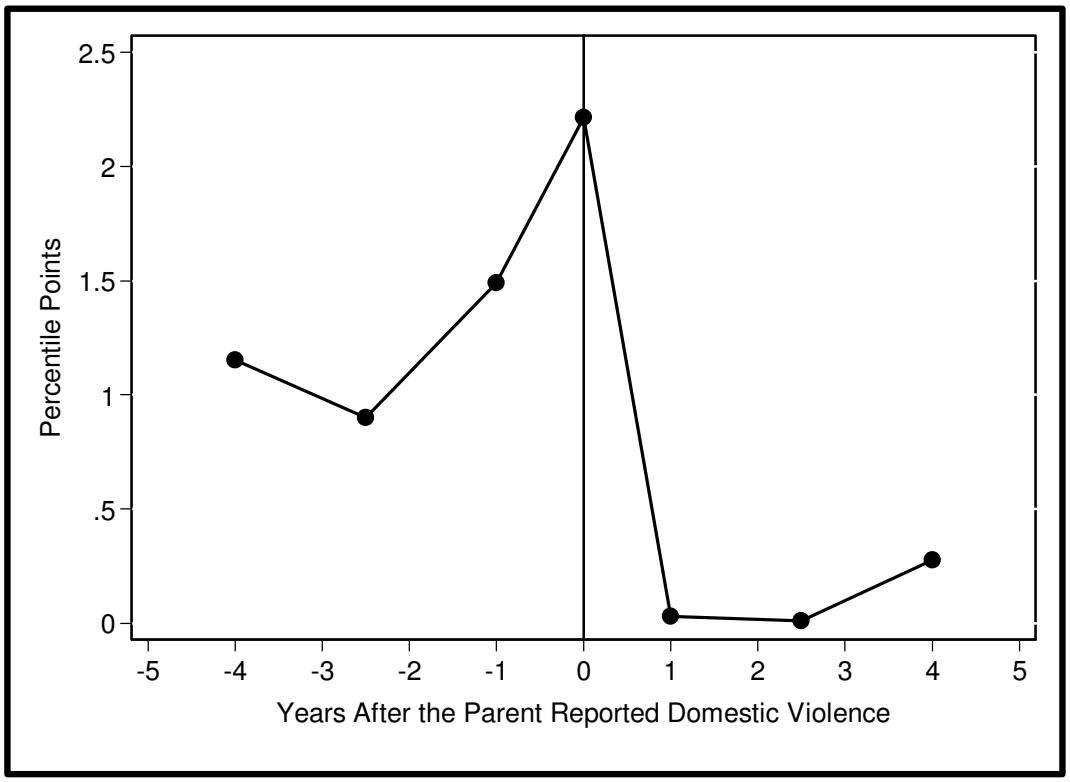

Figure 1: The Negative Effect of Adding One Troubled Child to a Classroom of 20 on Math and Reading Achievement

Strikingly, however, the negative peer effect abruptly disappears once the violence is reported. For example, the peer effect coefficient drops from -44.29 to 0.62 in the year following

[17] These results are consistent with survey research by Kaci (1994) who finds that on average violence had occurred in the family for over four years prior to the reporting of the incident.

the reporting and then remains near zero (0.21 and -5.54) in subsequent years. The full pattern is shown graphically in Figure 1 below, which illustrates how the effect of adding one child from a domestic violence family to a classroom of twenty drops off dramatically once the parent reports the domestic violence.

6. Discussion

There are multiple explanations for why filing could cause the negative peer effect to disappear so abruptly. Perhaps the simplest and most plausible explanation is that by reporting the domestic violence, the parent takes the first step in resolving the problems in the home. This interpretation is supported by Kaci (1994), who reports that 87 percent of surveyed respondents indicated that reporting domestic violence "helped stop physical abuse." Moreover, the pattern of estimates in Table 3 and Figure 1 is consistent with the problem getting worse within the family over time before improving substantially once the domestic violence is reported.

Similarly, it could also be that the parent who reports the abuse simultaneously takes other steps that could themselves be responsible for the reduction of the negative peer effect. Indeed, it is possible that other actions, such as ending the relationship or moving out of the household, would have successfully attenuated the negative peer effect even without the help of the legal system. While this seems inconsistent with the findings of Kaci (1994), we cannot rule out this possibility completely. However, we note that the policy implications are quite similar: in either case, there is a substantial social benefit to encouraging victims of domestic violence to take steps to reduce the violence.

On the other hand, we note that while reporting the domestic violence appears to improve own performance by one percentile point (see Table 2), this difference is not statistically

significant. Similarly, in results available in the web appendix, we find little evidence that reporting the domestic violence causes students from those households to commit fewer disciplinary infractions.[18] This is not what one might expect if reporting improved the family situation. However, this result could be due to path dependence; students may have difficulty catching up if they missed crucial lessons earlier, even if the family problems have since been resolved.

Alternatively, it could be that the judicial system triggers other interventions that help mitigate the negative spillovers caused by children from troubled homes. For example, upon seeking an injunction from the court to stop domestic violence, the school is often notified of the temporary restraining order. It may be that the knowledge of family problems at home better enables the school staff to remedy disruptions caused by children exposed to the violence. The school could also allocate existing resources toward the affected children upon learning of the family problems. While one might expect either of these things to improve the achievement of the children from the troubled family, the lack of a statistically significant change in the effect of reporting on own test scores in Table 2 need not be at odds with this interpretation. In addition to the potential for path dependence mentioned earlier, it could also be because according to the counselors with whom we spoke, the primary emphasis of counseling for these students is to improve their emotional well-being, not their performance in school. Thus, this type of intervention could well reduce classroom disruption without directly improving the child's math and reading scores.

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[18] This result appears to contrast somewhat with the findings of Currie and Tekin (2006), who find that child abuse increases crime later on. We emphasize, however, that the majority of violence in our study may well be directed at other family members, such as the mother, and thus is not necessarily inconsistent with this result.

7. Conclusions

This paper examines whether the social benefits to reporting domestic violence extend beyond the family. We do so by investigating whether reporting the violence to the court lessens negative peer effects caused by children exposed to as-yet-unreported domestic violence. Our findings show this is indeed the case: the negative peer effect drops significantly from 2.2 percentile points to zero in the year after the parent reports the domestic violence. Furthermore, this pattern is robust to school by grade specific linear time trends, grade by year specific shocks, controls for both individual and cohort characteristics, school by grade specific shocks, family fixed effects, and individual fixed effects.

Our findings have important policy implications. While the primary motivation for combating domestic violence has been the welfare of the direct victims, evidence here suggests that the social benefits are larger than that. By encouraging battered women to take steps to reduce domestic violence, benefits accrue to children outside the home through classroom interactions.

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Table 1: Summary Statistics

Variable Subgroup	Obs	Mean	Std. Dev.	Min	Max
Reading and Math Composite Score	42,478	52.9	29.0	1	99
Subsidized Lunch	22,674	39.7	26.1	1	99
Unsubsidized Lunch	19,804	68.0	24.5	1	99
All Boys	20,859	51.0	29.4	1	99
All Girls	21,619	54.8	28.5	1	99
Reported Family Violence	1,152	40.2	25.8	1	99
Unreported Family Violence	785	36.4	25.7	1	99
Ever Exposed to Family Violence	44,882	0.046	0.210	0	1
Unreported: Filing Occurred 4+ Years After Current Academic Year	44,882	0.005	0.070	0	1
Unreported: Filing Occurred 2-3 Years After Current Academic Year	44,882	0.007	0.082	0	1
Unreported: Filing Occurred 1 Year After Current Academic Year	44,882	0.004	0.066	0	1
Unreported: Filing Occurred During the Current Academic Year	44,882	0.004	0.067	0	1
Reported: Filing Occurred 1 Year Before the Current Academic Year	44,882	0.005	0.067	0	1
Reported: Filing Occurred 2-3 Years Before the Current Academic Year	44,882	0.010	0.101	0	1
Reported: Filing Occurred 4+ Years Before the Current Academic Year	44,882	0.011	0.104	0	1
Black	44,882	0.38	0.48	0	1
Male	44,882	0.49	0.50	0	1
Free/Reduced Lunch	44,882	0.53	0.50	0	1
Cohort Size	514	87.30	32.70	23	222

Notes: Cohort refers to a group of children in the same grade in the same school in the same year.

Table 2: The Effect of Exposure to Troubled Children on Reading and Mathematics Test Scores Before and After the Parent Reports Domestic Violence

Specification	1	2	3	4	5	6	7	8	9	10	11	12
Proportion of Peers with Unreported Family Violence	-274.23*** (26.80)	-227.65*** (26.18)	-27.21** (12.21)	-30.21** (12.40)	-28.85** (12.27)	-37.11*** (9.96)	-35.01*** (9.17)	-27.56*** (9.33)	-24.48** (10.02)	-22.19* (12.22)	-23.70** (11.44)	-23.53* (12.65)
Proportion of Peers with Reported Family Violence	-163.16*** (25.59)	-192.34*** (23.85)	-1.60 (11.53)	-3.62 (12.14)	-3.45 (12.14)	3.56 (9.76)	5.36 (9.78)	-2.19 (10.26)	-4.34 (11.12)	-4.11 (10.86)	1.15 (8.72)	-4.43 (9.90)
Own Unreported Family Violence	-14.15*** (1.28)	-13.58*** (1.27)	-11.09*** (1.29)	-11.13*** (1.29)	-11.11*** (1.28)	-4.48*** (1.08)	-4.47*** (1.08)	-4.39*** (1.08)	-4.35*** (1.08)	-5.81** (2.80)	0.04 (1.76)	-0.32 (0.57)
Own Reported Family Violence	-11.45*** (1.24)	-11.90*** (1.22)	-9.33*** (1.27)	-9.36*** (1.28)	-9.35*** (1.28)	-3.36*** (0.95)	-3.37*** (0.95)	-3.44*** (0.94)	-3.40*** (0.94)	-1.60 (2.33)		-0.9 (0.52)
Observations	42,478	42,478	42,478	42,478	42,478	42,478	42,478	42,478	42,478	26,922	42,478	21,148
Year Fixed Effects	No	Yes	Yes	Yes	-	-	-	-	-	-	-	-
School Fixed Effects	No	No	Yes	-	-	-	-	-	-	-	-	-
School-Grade Fixed Effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grade-Year Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	No	No	No	Yes	Yes	Yes	Yes	-	-	Yes
Cohort Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade-specific linear time trends	No	No	No	No	No	No	No	Yes	No	No	No	No
School-Year Fixed Effects	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes
Sibling Fixed Effects	No	No	No	No	No	No	No	No	No	Yes	-	No
Individual Fixed Effects	No	No	No	No	No	No	No	No	No	No	Yes	No
Lagged Test Score	No	No	No	No	No	No	No	No	No	No	No	Yes

Notes: Each column reports results from a separate regression. Individual controls include own reported and unreported domestic violence, gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. Standard errors in parentheses are two-way clustered at the school by cohort and school by year level in Specifications 1 through 8; for computational reasons standard errors in specifications including sibling or individual fixed effects are clustered at the school by cohort level.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 3: The Effect of Exposure to Troubled Children in the Years Before and After the Parent Reports Domestic Violence

Specification	1		2	
Dependent Variable: Reading and Math Score	Peer Effect Coefficients	Own DV Coefficients	Peer Effect Coefficients	Own DV Coefficients
Unreported Family Violence -- 4+ Years (filing occurred 4 or more years after the current AY)	-39.57* (23.12)	-5.10*** (1.88)	-23.04 (22.21)	-4.91*** (1.90)
Unreported Family Violence -- 2-3 Years (filing occurred more than 2-3 years before the current AY)	-31.43** (14.86)	-6.17*** (1.65)	-18.00 (15.47)	-6.04*** (1.64)
Unreported Family Violence -- 1 Year (filing occurred in the year before the current AY)	-27.24 (17.23)	-2.73 (1.75)	-29.85* (16.10)	-2.78 (1.75)
Family Violence Occurred During the Current Academic Year	-45.09** (19.53)	-3.14* (1.81)	-44.29** (18.90)	-3.11* (1.81)
Reported Family Violence -- 1 Year (filing occurred in the year before the current AY)	5.59 (20.96)	-3.83** (1.77)	0.62 (18.96)	-3.90** (1.76)
Reported Family Violence -- 2-3 Years (filing occurred 2-3 years after the current AY)	9.47 (13.18)	-1.86 (1.45)	0.21 (13.37)	-1.91 (1.44)
Reported Family Violence -- 4+ Years (filing occurred 4+ years after the current AY)	1.22 (13.28)	-4.52*** (1.10)	-5.54 (13.42)	-4.60*** (1.11)
Observations	42,478		42,478	
School-Grade Fixed Effects	Yes		Yes	
Grade-Year Fixed Effects	Yes		Yes	
Individual Controls	Yes		Yes	
Cohort Controls	Yes		Yes	
School-Grade-specific linear time trends	No		Yes	

Notes: Each numbered column represents a different regression. Standard errors clustered at the school by cohort level are in parentheses. All specifications control for individual gender, race, median family income, subsidized lunch status, and the number of counselors as well as a full set of cohort -level controls include mean gender, race, median family income, subsidized lunch, and size by school/grade/year.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Web-Only Appendix

Table A1: Falsification Tests

	Dependent Variable					
Specification	1	2	3	4	5	6
	Proportion of Peers with Unreported Family Violence	Proportion of Peers with Reported Family	Own Domestic Violence	Dropout of Sample after 3rd or 4th Grade	Missing Test Score	Cohort Size
Black	-0.0004	0.0001	-	-	-	-
	(0.0003)	(0.0003)				
Boy	0.0000	0.0001	-	-	-	-
	(0.0002)	(0.0002)				
Counselors	-0.0014	0.0035	-	-	-	-
	(0.0018)	(0.0021)				
Neighborhood Income	0.0000	0.0000	-	-	-	-
	(0.0000)	(0.0000)				
Subsidized Lunch	0.0000	0.0000	-	-	-	-
	(0.0002)	(0.0002)				
Proportion of Peers with Unreported	-	-	-0.0115	-0.0067	-0.1138	15.9900
			(0.0383)	(0.1872)	(0.2719)	(43.4604)
Proportion of Peers with Reported Family	-	-	0.0051	0.1817	-0.0815	-43.0885
			(0.0226)	(0.1628)	(0.2351)	(32.4705)
F-Test of Joint Significance (p-value)	0.85	1.30	0.09	0.65	0.17	1.08
	(0.52)	(0.26)	(0.91)	(0.53)	(0.84)	(0.34)
Observations	44,882	44,882	44,882	44,882	44,882	44,882

Notes: Each column represents a different regression. Standard errors clustered at the school by cohort level are in parentheses. All specifications control for school by grade fixed effects and year fixed effects. Specification 3 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 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table A2: The Effect of Exposure to Troubled Children on Reading and Math Scores and Disciplinary Infractions Before and After the Parent Reports Domestic Violence

Specification	1	2	3	4	5	6	7	8	9	10	11	12
Panel A. Math Scores												
Proportion of Peers with Unreported Family Violence	-292.18***	-229.67***	-28.00*	-30.27**	-28.88**	-36.14***	-35.03***	-31.52***	-16.73	-35.46**	-51.27**	-18.35
	(30.49)	(28.86)	(14.91)	(14.92)	(14.41)	(12.82)	(11.89)	(12.13)	(14.80)	(16.37)	(19.72)	(21.79)
Proportion of Peers with Reported Family Violence	-147.52***	-183.45***	8.26	7.89	3.68	10.44	11.88	-5.35	4.51	-2.79	-7.95	-2.50
	(27.20)	(25.18)	(14.07)	(14.97)	(14.50)	(12.00)	(12.10)	(12.40)	(13.69)	(14.95)	(16.20)	(14.25)
Observations	38,014	38,014	38,014	38,014	38,014	38,014	38,014	38,014	38,014	24,034	38,014	17,604
Panel B. Reading Scores												
Proportion of Peers with Unreported Family Violence	-265.49***	-227.00***	-27.88**	-32.04**	-30.94**	-38.53***	-36.98***	-26.69***	-28.31***	-24.67*	-18.79	-30.26***
	(27.06)	(26.64)	(13.00)	(12.91)	(12.81)	(10.16)	(9.62)	(9.24)	(10.03)	(13.38)	(12.87)	(11.13)
Proportion of Peers with Reported Family Violence	-180.17***	-204.26***	-12.30	-12.16	-9.16	-1.73	1.47	-1.10	1.56	-1.54	13.68	0.65
	(25.87)	(24.44)	(11.69)	(11.66)	(11.94)	(9.67)	(9.75)	(9.78)	(11.37)	(11.26)	(8.48)	(8.86)
Observations	42,266	42,266	42,266	42,266	42,266	42,266	42,266	42,266	42,266	26,799	42,266	20,981
Panel C. Disciplinary Incidents												
Proportion of Peers with Unreported Family Violence	6.11***	5.35***	2.74**	3.44***	3.65**	3.94***	3.79**	3.72***	1.45	-0.49	0.54	3.95***
	(1.98)	(1.90)	(1.29)	(1.25)	(1.27)	(1.21)	(1.21)	(0.94)	(0.93)	(1.06)	(1.63)	(1.44)
Proportion of Peers with Reported Family Violence	4.15***	4.77***	-0.10	-1.07	-1.34	-1.64	-1.43	0.39	-0.91	-0.53	0.87	-0.42
	(1.36)	(1.44)	(1.15)	(1.20)	(1.17)	(1.12)	(1.08)	(0.85)	(0.93)	(0.83)	(1.31)	(1.21)
Own Unreported Family Violence	0.51***	0.50***	0.47***	0.48***	0.48***	0.26**	0.26**	0.26**	0.23**	(0.14)	0.02	0.17
	(0.13)	(0.13)	(0.12)	(0.12)	(0.12)	(0.11)	(0.12)	(0.11)	(0.12)	(0.27)	(0.22)	(0.12)
Own Reported Family Violence	0.59***	0.60***	0.54***	0.52***	0.52***	0.32***	0.33***	0.35***	0.33***	0.02		0.10
	(0.13)	(0.13)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.19)		(0.08)
Observations	44,882	44,882	44,882	44,882	44,882	44,882	44,882	44,882	44,882	28,597	44,882	23,403
Year Fixed Effects	No	Yes	Yes	Yes	-	-	-	-	-	-	-	-
School Fixed Effects	No	No	Yes	-	-	-	-	-	-	-	-	-
School-Grade Fixed Effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grade-Year Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	No	No	No	Yes	Yes	Yes	Yes	-	-	Yes
Cohort Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade-specific linear time trend	No	No	No	No	No	No	No	Yes	No	No	No	No
School-Year Fixed Effects	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes
Sibling Fixed Effects	No	No	No	No	No	No	No	No	No	Yes	-	No
Individual Fixed Effects	No	No	No	No	No	No	No	No	No	No	Yes	No
Lagged Test Score	No	No	No	No	No	No	No	No	No	No	No	Yes

Notes: Each column reports results from a separate regression. Individual controls include own reported and unreported domestic violence, gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. Standard errors in parentheses are two-way clustered at the school by cohort and school by year level in Specifications 1 through 8; for computational reasons standard errors in specifications including sibling or individual fixed effects are clustered at the school by cohort level.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table A3: The Effect of Reported and Unreported Peer Domestic Violence on Reading and Math Scores by Subgroup

Specification	1	2	3	4	5
Proportion of Peers with Unreported Family Violence	-37.83**	-32.25***	-30.61**	-32.00**	-45.13***
	(16.09)	(10.55)	(14.46)	(15.73)	(13.53)
Proportion of Peers with Reported Family Violence	-1.25	9.86	18.73	-1.79	-0.53
	(17.94)	(11.12)	(16.09)	(14.50)	(12.30)
Sample	High-Income	Low-Income	3rd Graders	4th Graders	5th Graders
Year Fixed Effects	-	-	-	-	-
School Fixed Effects	-	-	-	-	-
School-Grade Fixed Effects	Yes	Yes	Yes	Yes	Yes
Grade-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes
School-Grade-specific linear time trends	No	No	No	No	No

Notes: Each column reports results from a separate regression. High-income refers to families not eligible for subsidized lunches; low-income refers to families that are. Individual controls include own reported and unreported domestic violence, gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. Standard errors in parentheses are two-way clustered at the school by cohort and school by year level in Specifications 1 through 8.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level