The Long-Run Effects of Disruptive Peers

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Abstract

A large and growing literature has documented the importance of peer effects in education. However, there is relatively little evidence on the long-run educational and labor market consequences of childhood peers. We examine this question by linking administrative data on elementary school students to subsequent test scores, college attendance and completion, and earnings. To distinguish the effect of peers from confounding factors, we exploit the population variation in the proportion of children from families linked to domestic violence, who were shown by Carrell and Hoekstra (2010, 2012) to disrupt contemporaneous learning. Results show that exposure to a disruptive peer in classes of 25 during elementary school reduces earnings at age 26 by 3 to 4 percent. We estimate that differential exposure to children linked to domestic violence explains 5 to 6 percent of the rich-poor earnings gap in our data, and that removing one disruptive peer from a classroom for one year would raise the present discounted value of the classmates' future earnings by $100,000.

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1 Introduction

A large and growing literature has documented the importance of peer effects in the education production function. This line of research has focused primarily on how peers affect contemporaneous outcomes such as test scores or disciplinary infractions in school. In contrast, relatively little is known about the long-run impact of childhood peers, particularly with respect to labor market outcomes in adulthood. This lack of evidence has important implications for the evaluation of education policies that affect peer composition. For example, if peer effects diminish over time and do not affect adult outcomes, then concerns over how educational policies such as tracking or school vouchers affect peer composition may be overstated. On the other hand, if peers in early childhood do impact outcomes into adulthood, then it raises the importance of concerns regarding changes in student composition. In addition, the presence of long-run peer effects also has important implications for understanding the role of sorting into schools and peer composition as determinants of income inequality. To the extent that disadvantaged groups attend schools with more disruptive peers, this differential exposure may contribute to income inequality later in life.

This paper documents the existence of long-term peer effects by estimating the effects of disruptive elementary school peers on high school test scores, college attendance and degree attainment, and earnings at age 24 to 28. It does so by linking administrative and public records data on elementary school students from a Florida county to long-term educational and earnings records. An important feature of these data is that they enable us to proxy for disruptive peers by measuring whether the peer's family is characterized by domestic violence. The advantage of this measure is twofold. First, it is exogenous to the disruptive student's classmates, which is critical for overcoming the reflection problem (Manski, 1993). In addition, exposure to domestic violence is a particularly good predictor of a disruptive peer. Research on domestic violence shows that children exposed to domestic violence are associated with a number of emotional and behavioral problems including, aggressive be-

havior, bullying, depression, animal cruelty, diminished academic performance, and violence in adulthood (Edleson, 1999; Wolfe et al., 2003; Fantuzzo et al., 1997; Koenen et al., 2003; Holt, Buckley and Whelan, 2008; Baldry, 2003; Carlson, 2000; Currie, 2006; Black, Sussman and Unger, 2010). Carrell and Hoekstra (2010, 2012) show that exposure to peers linked to domestic violence significantly worsens contemporaneous achievement and behavior. Importantly, these effects appear to be driven by both troubled boy peers and peers from families that have not yet reported the domestic violence.

To distinguish the long-run effects of disruptive peers from confounding factors, we follow Hoxby (2000*b*) in exploiting the idiosyncratic variation in the population by including school-by-grade fixed effects.[1] Intuitively, we ask whether students in cohorts with an idiosyncratically high number of disruptive peers have worse long-run educational and labor market outcomes than students in the same school whose cohort had fewer disruptive peers. The identifying assumption is that all other determinants of long-run educational and labor market outcomes are orthogonal to this within-school-grade variation in peer domestic violence. Empirical evidence in this study and in previous work by Carrell and Hoekstra (2010, 2012) has shown that the within-school variation in disruptive peers is uncorrelated with cohort size and exogenous student characteristics such as own domestic violence, gender, race, and subsidized lunch status. We also show our estimates are unchanged when including these individual-level and cohort-level controls, which is consistent with the identifying assumption.

Results indicate that exposure to disruptive peers in childhood has important long-run consequences for both educational attainment as well as subsequent earnings in adulthood. Estimates indicate that exposure to one additional disruptive student in a class of 25 during elementary school reduces math and reading test scores in grades 9 and 10 by 0.02 standard

[1]While Hoxby (2000*b*) used population variation to address the question of the impact of class size, that approach has been widely used subsequently in studying peer effects in K-12 education (Hoxby, 2000*a*; Lefgren, 2004; Lavy and Schlosser, 2011). In contrast, researchers examining peer effects in college have been able to identify effects using random assignment of roommates or squadrons (Sacerdote, 2001; Kremer and Levy, 2008; Carrell, Malmstrom and West, 2008; Carrell, Fullerton and West, 2009).

deviations. More targeted measures of disruptive peers - such as male peers exposed to domestic violence, or peers exposed to as-yet-unreported domestic violence, result in larger effects on high school test scores, as well as significant declines in college degree attainment. Most importantly, exposure to an additional disruptive peer throughout elementary school leads to a 3 to 4 percent reduction in earnings at age 24 to 28. Collectively, these findings demonstrate that exposure to disruptive peers in elementary school has important implications for outcomes in adulthood. We estimate that one year of exposure to a disruptive peer in elementary school reduces the present discounted value of classmate future earnings by around $100,000, suggesting large efficiency losses due to disruptive students. In addition, the uneven distribution of disruptive peers across schools has important consequences for income inequality. We estimate that the increased exposure to (our measure of) disruptive peers by children from lower- relative to higher-income households explains around 5 or 6 percent of the rich-poor earnings gap in adulthood observed in our data.

This study's findings contribute to two different literatures. The first is a small literature that documents the persistence of peer effects on outcomes measured after the peer interactions. For example, Gould, Lavy and Paserman (2009) examine whether idiosyncratic cohort-to-cohort variation in exposure to immigrants during elementary school affects the passing rate on a high school matriculation exam that is necessary to attend college. They show that a 10 percentage point increase in the concentration of immigrants leads to a 2.8 percentage point decline in the passing rate. Bifulco, Fletcher and Ross (2011) report that a higher percentage of high school classmates with college-educated mothers decreases the likelihood of dropping out and increases college attendance. Finally, Black, Devereux and Salvanes (2013) show that a higher proportion of females in ninth grade reduces mean educational attainment and the likelihood of selecting the academic (as opposed to vocational) track, but helps women by leading to lower teenage birth rates and higher earnings. They also find that higher peer father earnings leads to better outcomes, especially for men.

Our study contributes to this literature in several ways. The first is that our measure of peer quality - children from families with domestic violence - is a measure that is both exogenous to peers and also identifies students who are particularly disruptive to contemporaneous peer learning. This enables us to better measure the impact of the type of disruptive peer in the Lazear (2001) model of education. Second, to our knowledge, we are only the second paper to identify the long-term effects of peers on subsequent earnings in adulthood after Black, Devereux and Salvanes (2013), and the first to do so for peer exposure during elementary school.

Finally, in assessing the long-term effects of elementary school peers on earnings, we join an emerging literature that has analyzed the long-run effects of early childhood educational inputs more generally. For example, previous studies have analyzed the long-run effects of the Head Start program (Garces, Thomas and Currie, 2002; Ludwig and Miller, 2007), kindergarten classroom assignment (Krueger and Whitmore, 2001; Chetty et al., 2011; Dynarski, Hyman and Schanzenbach, 2013), and teacher value added (Chetty, Friedman and Rockoff, 2014). Our paper complements this broader literature by documenting that exposure to disruptive peers during childhood leads to lower subsequent academic achievement in high school, a diminished likelihood of graduating with a college degree, and reduced earnings.

2 Data

To conduct our empirical analysis we utilize and extend the original dataset in Carrell and Hoekstra (2010, 2012). This original dataset contains information on (national percentile) math and reading test scores, as well as demographic characteristics for children attending grades 3 to 5 in the Alachua County (Florida) primary schools between the academic years 1995–1996 and 2002–2003. The dataset contains approximately 41,500 observations of 20,000

unique individuals, with around 14,000 observations per grade.

These student-level data were linked to domestic violence data that were gathered from public records information containing information on all domestic violence cases filed in civil court in Alachua County between January 1, 1993 and March 12, 2003. This information includes the names and addresses of the individuals involved and the date on which the case was filed. The names and addresses are used to link the student level information to the domestic violence data, while the date of filing is used to compute whether the domestic violence is already or yet-to-be reported at the time that the child was observed in elementary school.

We then linked these data to long-run education and earnings outcomes. Specifically, we worked with the Alachua County School District and the Florida Department of Education (FLDOE) to link longer-term outcomes, as of the end of 2010, for the students in the original dataset. We obtain (raw) test scores for grades 6 through 10.[2] While this does not allow us to observe test scores for students who switched to private schools or moved out of state, we do observe test scores for students outside of Alachua County so long as they attended public schools within the state of Florida.

Moreover, the FLDOE provided us with information on each student's college enrollment, courses completed, and degrees attained as of the end of 2012. However, the FLDOE collects such data only for students enrolled in public post-secondary Florida institutions. To supplement these data, we collect additional college enrollment and completion data from the National Student Clearinghouse (NSC), which has data from the majority of colleges and universities in the U.S.[3] Finally, the FLDOE also provided quarterly wages for the students working in the state of Florida for the years 2000–2013. These wages are transformed to 2013 real values.

[2]In order to have consistent test scores across grades and cohorts, we transform all the (national percentile or raw) scores into z-scores.

[3]See `http://www.studentclearinghouse.org/colleges/enrollment_reporting/participating_schools.php` for the full list of reporting colleges and universities.

Table (1) presents summary statistics for our main independent variables. These statistics show that around 38 percent of the sample is black and just over 50 percent are on subsidized lunch. Just under five percent of the students have been exposed to domestic violence at home, which is evenly split among boys and girls. In addition, of those students linked to domestic violence, around half are from homes that reported the domestic violence prior to the year and grade in which we observed them. The other half are from homes with as-yet-unreported domestic violence that was reported sometime after the year and grade in which we observed them.[4] Around 75 percent of the students in our sample have ever enrolled in college, 27 to 30 percent have received some type of college degree, and around 20 percent have received a bachelor's degree. Average quarterly earnings is around $5,000 dollars for those observed with earnings between ages 24 and 28.

3 Empirical Strategy

The two main threats to identification in the peer effects literature are the reflection and the selection problems. The reflection problem arises since it is hard to disentangle whether disruptive peers affect a student's outcomes or whether the student negatively affects her peers (Manski, 1993). To overcome this problem, we define peer quality as the percentage of one's peers whose families have been linked to domestic violence. Thus, we assume that a child's peers do not cause that child's family to be characterized by domestic violence. While we would argue that this assumption is reasonable *ex ante*, we also note that Carrell and Hoekstra (2010) explicitly test for whether own domestic violence is affected by peer domestic violence, and find no evidence of such a correlation.[5]

[4] As discussed in Carrell and Hoekstra (2012), the panel nature of our data allow us to exploit the timing of the reporting of the violence. Kaci (1994) finds that on average violence had occurred in the family for over four years prior to the reporting of the incident.

[5] We also note that to the extent one believes that domestic violence is affected by one's child's classmates, one would then expect boys to be over-represented amongst families linked to domestic violence since boys have more behavioral problems. However, as noted in Table (1), boys and girls are equally likely to be linked to a family with domestic violence.

The selection problem arises because students self-select into schools and peer groups that are similar to them (Hoxby, 2000*a*). In the absence of being able to randomize students into peer groups, the main approach to overcome selection has been to exploit the natural variation in cohort composition across time within a given school (Hoxby and Weingarth, 2006; Vigdor and Nechyba, 2006; Hanushek et al., 2003; Lefgren, 2004; Bifulco, Fletcher and Ross, 2011). We also follow this approach and argue that while there is selection into schools, there is natural year-to-year population variation in the proportion of peers linked to domestic violence across cohorts within the same school. This is precisely the variation that we exploit in order to identify the impact of disruptive peers.

We begin our analysis by focusing on a baseline model in which we control for school-by-grade fixed effects, grade-by-year fixed effects, and the proportion of peers in one's school-grade-year cohort linked to domestic violence. Specifically, we estimate the following model:

$$y_{igst} = \theta_0 + \theta_1 \frac{\sum_{k \neq i} DV_{kgst}}{n_{gst} - 1} + \theta_2 DV_{igst} + \theta_3 X_{igst} + \lambda_{gs} + \sigma_{gt} + \epsilon_{isgt}, \tag{1}$$

where i, g, s and t respectively represent the individual, grade, school and academic year. y represents the outcome variables of interest - test scores for grades three through ten, college enrollment, college graduation, labor force participation, and earnings.[6] Test scores are calculated by taking the average of the reading and the math score for each student in each grade. λ and σ are grade-school and grade-year fixed effects. The coefficient of interest is θ_1, which is the coefficient on the proportion of peers from families linked to domestic violence. DV is an indicator variable that controls for own family violence, and X is a vector of additional controls that are included in some specifications. Individual-level controls include gender, race, neighborhood median family income (measured by zip code of home address), and subsidized lunch status, while cohort-level controls measure these same variables as well as both cohort size and median zip code family income at the school-grade-

[6]Note that these outcomes are grade invariant.

year level. Lastly, all standard errors are clustered by the set of students who attended third through fifth grade in the same school.

In addition, because our primary goal is to assess the long-run consequences of exposure to disruptive students, we also use more targeted measures of disruptive students by focusing on certain subsets of children from families linked to domestic violence shown in previous research to have especially large effects on contemporaneous outcomes. Specifically, in some specifications we focus on the impact of boys from families linked to domestic violence, since Carrell and Hoekstra (2010) show that it is the boys from these families that are most disruptive to contemporaneous peer achievement. This is also consistent with Evans, Davies and DiLillo (2008), who find that boys exposed to domestic violence are significantly more likely to exhibit externalizing behaviors. In addition, we also present specifications in which we allow children from families with as-yet-unreported domestic violence to affect their peers differently than children from families who had already reported the domestic violence. Carrell and Hoekstra (2012) show that the negative contemporaneous impact these children have on their peers abruptly disappears once the family reports the domestic violence to the court, and survey evidence suggests that reporting domestic violence helps stop the physical abuse (Kaci, 1994). As a result, we would expect that children exposed to an idiosyncratically high number of peers with as-yet-unreported domestic violence will exhibit worse outcomes than children in other cohorts in that same school.

Finally, we note that because our data are composed of a panel of students who attended grades three through five in Alachua County, some students are observed only once while others are observed up to three times. Consequently, all of our results are estimated using probability weights, where the weight is the inverse of the number of times a student is observed in the sample. In addition, we note that while we do not observe students while they are in the first or second grade, we expect a high level of correlation between one's peers in those grades and one's peers in grades three through five. Thus, while our estimates

represent the average peer effect across third through fifth grades, we believe our estimates are properly interpreted as the cumulative impact of disruptive peers throughout the five grades of elementary school.

The main threat to identification for our research design is the possibility that students and families select into or out of schools on the basis of peer domestic violence. For example, our estimates could be biased if motivated parents, with higher achieving children, move their children across schools when they notice an idiosyncratically high proportion of disruptive peers in their child's grade. We perform two exercises to address this possibility. First, we formally test for selection by analyzing whether cohort size or other family characteristics are correlated with the proportion of peers with domestic violence. We find no evidence of such relationship. Results are shown in Table (6), which shows the correlation between our three measures of disruptive peers and gender, race, subsidized lunch status, and neighborhood income level. Among the 30 estimates, only one is significant at either the 5 percent or 10 percent levels, which is approximately what one would expect due to chance. None is significant at the 1 percent level. Thus, we find little evidence to suggest that students are entering or leaving schools in a way that is systematically correlated with our three different proxies for disruptive peers.[7]

Along similar lines, we estimate effects both without and with individual and other peer controls, and show that the inclusion of controls does not affect our estimates. If our estimates were sensitive to these controls, then we would worry that even conditional on school-by-grade fixed effects, students more exposed to disruptive students may be otherwise different from those who are less exposed.

[7]One might also worry that exposure to disruptive students is correlated with attrition. In Appendix Table (A.2), we test whether this is true in the test score data. We define an individual as dropping in grade g if we observed a test score in grade $g-1$ and do not observe any future scores for grades g and higher. Results shown there indicate that peer composition does not affect the likelihood of not being observed with a test score. In addition, when we report earnings effects, we explicitly test whether exposure to disruptive peers is correlated with being observed with positive earnings in Florida.

4 Results

To examine the long-run consequences of exposure to disruptive peers during elementary school, we focus on three sets of outcomes. First, we replicate the findings of Carrell and Hoekstra (2010, 2012) by examining the impact of disruptive peers on test scores during elementary school. We then ask whether the impacts of those disruptive peers are evident in middle and high school test scores, college attendance and degree attainment, and labor market earnings as adults aged 24 to 28. Importantly, for each outcome we restrict our data to the sample of students old enough to have been observed with that outcome.

In addition, we focus on three different measures of disruptive peers. The first is the proportion of peers exposed to domestic violence, who were shown by Carrell and Hoekstra (2010) to have marginally significant impacts on contemporaneous achievement. We then focus on two other measures of disruptive peers previously shown to have larger impacts on contemporaneous learning: male peers from families exposed to domestic violence, and peers from families with as-yet-unreported domestic violence.

4.1 Test Scores

We begin by showing the impact of disruptive peers on contemporaneous and subsequent standardized test scores. Results are shown in Table 2, where the first two columns of Panel A replicate Carrell and Hoekstra (2010) by assessing how children linked to domestic violence affect the third- through fifth-grade test scores of their peers. The specification in column (1) includes only grade-year fixed effects, school-grade fixed effects, and own domestic violence status as controls, while column (2) additionally controls for other individual and cohort-level controls. The estimate in column (1) of -0.48, which is significant at the 5 percent level, suggests that adding one disruptive student to a class of 25 reduces achievement by 0.02 of a standard deviation (1/25 * -0.48). Estimates in columns (3) and (4) indicate a more modest

and statistically insignificant impact during grades 6 through 8, though the effect of that same disruptive peer during elementary school is again a reduction of around 0.02 standard deviations in grades 9 and 10.

Panel B of Table 2 shows estimates of the impact of male and female peers from families linked to domestic violence. As shown by Carrell and Hoekstra (2010), it is the boys from these troubled families that most negatively affect peer academic performance. The estimate in column (1) indicates that adding one disruptive male peers to a class of 25 reduces grade 3 - 5 test scores by 0.03 standard deviations (1/25 * -0.81), while female peers from families linked to domestic violence do not appear to reduce their peers' academic performance.

Estimates of the impact of peers exposed to as-yet-unreported and reported domestic violence are shown in Panel C of Table 2. Consistent with Carrell and Hoekstra (2012), results indicate it is the children from families who have not yet reported the domestic violence that negatively impact their peers' contemporaneous achievement. Estimates in columns (1) and (2) indicate that adding one peer with as-yet-unreported domestic violence significantly reduces test scores by between 0.03 and 0.04 standard deviations. As with the results in Panels A and B, this peer effect appears to diminish in grades 6 – 8, though it is again statistically significant and of similar magnitude in grades 9 – 10.

Importantly, estimates across all grade levels in Table 2 change little when including individual-level and cohort-level controls. This is consistent with the identifying assumption, and provides additional evidence beyond that documented by Carrell and Hoekstra (2010) that there is little evidence that high-ability students selected out of schools when they were subjected to an idiosyncratically high proportion of disruptive peers.

4.2 College Attendance and Degree Attainment

We now turn to the question of whether having disruptive peers in elementary school also leads to worsened college attendance and degree attainment. Results are shown in Table 3, which takes the same form as Table 2. Columns 1 and 2 show results for college enrollment without and with additional individual and cohort-level controls; columns (3) and (4) show results for the likelihood of receiving any college degree; and columns (5) and (6) show results for four-year degree.

Results in Table 3 indicate that elementary school exposure to boys from troubled families and to children from families with as-yet-unreported domestic violence has significant impacts on college enrollment and degree attainment. For example, estimates in column 2 suggest that adding one disruptive boy to a class of 25 throughout elementary school leads to just over a 1 percentage point (1.6 percent) reduction in college enrollment (1/25 *-0.29), which is significant at the 10 percent level. Similarly, the estimate of -0.47 in column (4) of Panel B indicates that exposure to that disruptive boy reduces the probability of receiving any degree by a statistically significant 2.0 percentage points, or 7 percent.

Estimates in Panel C suggest similarly large negative impacts of elementary school exposure to peers from families linked to as-yet-unreported domestic violence. For example, estimates in columns (2) and (4) indicate that exposure to one peer in a class of 25 leads to a 1.6 percentage point (2.2 percent) reduction in college enrollment and a 2.6 percentage point reduction in the likelihood of receiving any college degree. Both estimates are statistically significant at the one percent level. In short, there is strong evidence that exposure to disruptive peers during elementary school leads to significantly worse outcomes with respect to both college attendance and degree attainment years later.

4.3 Labor Market Outcomes

Finally, we turn to labor market outcomes. Results for the baseline specification are shown in Panel A of Table 4. Columns (1) and (2) show evidence that the proportion of peers during elementary school linked to domestic violence has little effect on labor force participation. However, there is strong evidence that these peers reduce earnings. Columns (3) and (4) show estimates for average quarterly earnings, including zeros; columns (5) through (8) show estimates for the level and log of quarterly earnings conditional on being observed with positive earnings. Estimates across columns (3) through (8) in Panel A indicate that elementary school exposure to one additional disruptive student in a class of 25 throughout reduces earnings by between 3 and 4 percent. All estimates are significant at the 10 percent level, and all but one is significant at the 5 percent level.

Somewhat surprisingly, when we define our peer domestic violence variable by gender of the student as in Panel B, we do find some evidence that peers impact labor force participation. Specifically, exposure to boys from domestic violence families is associated with reduced labor force participation, while exposure to girls is associated with somewhat increased labor force participation.[8]

While we do not have a good interpretation of exactly why the different measures of peers have different effects on labor force participation, it is important to note that the estimated peer effect of disruptive male students does not depend on whether we include individuals not observed with earnings as in columns (3) and (4), or condition on positive earnings as in columns (5) through (8). All of those estimates are statistically significant at the 5 percent level, with estimates conditional on positive earnings indicating that exposure to one of these

[8]We suspect that the reduction in labor force participation associated with disruptive males is due to a combination of increased unemployment and perhaps incarceration among those exposed to them. A more worrisome explanation is that high-ability peers who are exposed to an idiosyncratically high number of disruptive boys in elementary school systematically leave the state. However, we find comfort in the fact that for our other measures of disruptive peers in Panels A and C we find no evidence of any impact on the likelihood of being observed with positive earnings, and still find statistically significant and economically meaningful impacts on all three measures of earnings.

disruptive boys reduces earnings by 4 to 5 percent. For example, the estimate in column (5) of -6,529 indicates that exposure to one more disruptive male in a class of 25 throughout elementary school reduces earnings by $261. That drop in earnings represents a reduction of 5.2 percent, given average quarterly earnings of $5,018 as shown in the bottom of Table 4.

Results in Panel C of Table 4 also show strong evidence that disruptive peers, as defined as those exposed to as-yet-unreported domestic violence, reduce adult earnings. While there is no effect of peers with unreported domestic violence on labor force participation (columns (1) and (2)), all estimates on earnings in columns (3) through (8) are negative and statistically significant at the 5 percent level. Estimates in columns (5) through (8) that condition on being observed with positive earnings imply that exposure during elementaryschool to one more peer from a family with unreported domestic violence in a class of 25 is associated with a 5.4 to 6.7 percent reduction in earnings.

Importantly, estimates across all specifications are unaffected by the inclusion of other individual and cohort-level controls. This suggests that other observable determinants appear to be uncorrelated with the idiosyncratic year-to-year variation in disruptive peers we are exploiting, which is consistent with the identifying assumption.

In summary, we find strong evidence that exposure to disruptive peers during elementary school leads to significantly lower earnings in adulthood. These effects are consistent across several different measures of disruptive peers and are robust to different ways of modeling the relationship between earnings and disruptive peers.

4.4 Subgroup Analysis

We now turn to the question of which students are most affected in the long-run by exposure to disruptive peers during elementary school. Specifically, we test for differences by gender,

by parental socioeconomic status (as proxied by subsidized lunch status), and by race.

Results are shown in Table 5. Panel A shows results for Grade 9 and 10 test scores; Panel B shows estimates for graduating from college with any degree; Panel C shows results for the likelihood of being observed with positive earnings; Panel D shows results using earnings (including zeros), and Panel E shows results using log earnings (which exclude zeros).

Results regarding gender show that, in contrast to what Carrell and Hoekstra (2010) found when examining contemporaneous outcomes, there are few meaningful differences between the men and women with respect to the long-run impacts of disruptive peer exposure. Estimates for men and women are similar for all outcomes including grade 9 and 10 test scores, degree attainment, and earnings. In only 1 of the 15 cases are the estimates for men and women statistically different from each other (earnings levels including zeros for the peer domestic violence measure). But even there, we note that the estimates for the other two measures of disruptive peers are neither statistically nor economically different between men and women. In fact, the only difference (which is not shown in Table 5 for brevity purposes) is that while disruptive boys and girls both lead to reduced peer boys' adult earnings, disruptive girls also reduce girls' adult earnings.

In the third and fourth columns of Table 5, we examine the impact of disruptive peers on the outcomes of children who come from lower- and higher-income households, measured by subsidized lunch status during elementary school. The results show that while the point estimates indicate that students with higher socioeconomic status experience larger declines in their high school test scores and degree attainment, the results on earnings are more mixed and depend on specification.

The most interesting subgroup effects are shown in the last two columns of Table 5, which show that while there are relatively few differences between whites and blacks with respect to high school test scores and degree attainment, there are significant differences with respect to earnings. White students experience significant declines in earnings due to disruptive peer

exposure; the estimate from the log specification implies that exposure to one disruptive student in a class of 25 reduces earnings by 5 percent. This is more than twice the estimated effect for blacks, which is not statistically different from zero.

In summary, results from Table 5 yield several findings with respect to the heterogeneous impacts of disruptive peers. First, students seem to experience similar effects across gender and socioeconomic status. However, white students seem to experience much larger declines in earnings due to disruptive peers relative to black students.

5 Discussion and Interpretation

Given the large long-run peer effects documented in the previous section, a natural question is the exact mechanism through which those effects arise. One such potential mechanism is the impact of disruptive peers on educational attainment. Our findings above indicate that exposure to an additional disruptive peer reduces the likelihood of receiving any type of college degree by 0.7 to 2.6 percentage points, depending on the measure of disruptive peer used. In a review of the literature on the economic returns to community college degrees, Belfield and Bailey (2011) report that the return to those degrees is between 10 and 30 percent. If these returns hold in our sample, an additional disruptive peer would lead to as much as a 0.78 percent decrease in earnings through this one educational channel (-0.026*30). Thus, we expect that a significant proportion of the earnings effects documented above likely come from non-cognitive skills. For example, recent studies on the Perry Preschool Program and Project Star have shown that the impact of these programs on non-cognitive skills can explain a larger share of actual earnings gains compared to their impact on cognitive performance (Almlund et al., 2011; Chetty et al., 2011; Heckman, Pinto and Savelyev, 2013). The likelihood that the long-run effects of peers linked to domestic violence works through a non-cognitive channel is also consistent with recent research on peer effects in crime;

Stevenson (2015) finds that the juvenile correctional center peers that increase future crime the most are those who come from difficult or dangerous homes.

In addition, it is also helpful to place the magnitudes of these effects in a larger context by comparing them to other educational inputs. With respect to college attendance, our findings indicate that one year of exposure to a disruptive boy peer reduces college enrollment by 0.2 percentage points.[9] These effects are relatively small compared to the impact of other inputs. For example, Dynarski, Hyman and Schanzenbach (2013) and Chetty et al. (2011) report that being randomly assigned to a small class rather than a regular class with 50 percent more students in Project STAR for roughly two years increased college enrollment by 2.7 and 1.8 percentage points, respectively. Garces, Thomas and Currie (2002) estimate that Head Start increased college enrollment by 9.2 percentage points, while Chetty, Friedman and Rockoff (2014) estimate that a one standard deviation increase in in teacher quality in one grade increases college attendance by 0.82 percentage points. Thus, our estimates imply that with respect to college enrollment, a year of exposure to a disruptive male peer is equivalent to a 7 to 11 percent increase in class size for one year, a 2 percent reduction in Head Start participation, or a one-fourth standard deviation reduction in teacher quality.

We can also put the magnitude of our earnings estimates in the context of existing papers on the effects of long-run educational interventions. Chetty et al. (2011) estimate that a one-standard deviation increase in overall "class quality" (which includes class size, teacher quality, peer quality, etc.) for one year results in a 9.6 percent increase in earnings. Given our estimate that one year of exposure to a disruptive peer reduces earnings by 0.6 to 0.8 percent,[10] it implies that adding one disruptive peer is equivalent to reducing overall class quality by around 7 percent.

[9] Given a coefficient of -0.28 in Column 2 of Panel B in Table 3, we scale first by 1/25 to obtain the effect of cumulative elementary school exposure in a class of 25, and then divide by 5 to obtain the effect of each year of exposure.

[10] Coefficients in columns 5 through 8 of Panel A in Table 4 indicate that exposure to a disruptive peer *throughout elementary school* in a class of 25 reduces earnings by 3.2 to 4.2 percent. Scaling these estimates by one-fifth, we estimate that each year of exposure reduces earnings by 0.6 to 0.8 percent.

Similarly, Chetty, Friedman and Rockoff (2014) estimate that a one standard deviation increase in teacher quality in one grade increases earnings by 1.3 percent. Thus, our estimates of the impact of one disruptive peer for one year imply an effect that is equivalent to approximately a one-half standard deviation reduction in teacher quality. Estimates for more targeted measures of disruptive peers are larger; a year of exposure to a boy from a family linked to domestic violence and to a child linked to as-yet-unreported violence has the same effect on earnings as a 0.7 and 0.9 standard deviation reductions in teacher quality, respectively.

Along similar lines, we can compare our estimates to potential policy experiments. Chetty, Friedman and Rockoff (2014) estimate that replacing a teacher estimated to be in the bottom 5 percent of the distribution with an average teacher for one year would increase the present discounted value of earnings of the students in that classroom by $250,000. Under similar assumptions,[11] we estimate that one year of exposure to a disruptive student reduces the present discounted value of lifetime earnings by $81,000 to $105,000.[12] Similarly, using estimates from columns 5 - 8 of Panel B in Table 4, we estimate that removing a male peer linked to domestic violence would increase the present discounted value of classmate earnings by $98,000 to $135,000, and removing a peer linked to unreported domestic violence would increase the present discounted value of classmate earnings by $134,000 to $169,000. Thus, our findings imply that having two to three peers from families linked to domestic

[11]First, we assume that the impact of disruptive children is constant over the life cycle using estimates from columns 3 - 8 in Table 4. Second, we assume the absence of general equilibrium effects. Third, to facilitate comparison, we assume that the present discounted value of earnings from children at age 12 in our sample are the same as those in Chetty, Friedman and Rockoff (2014) at $522,000. These estimates follow Krueger (1999) in discounting earnings gains at a 3 percent real annual rate. Finally, since the earnings losses estimated here represent the impact of cumulative exposure to disruptive peers throughout elementary school, we assume that each of these effects comes from five years of exposure. To the extent that students continue to have significant exposure to disruptive peers from their elementary school years, this may overstate the per-year impact of those peers.

[12]These figures are based on estimates presented in Columns 5 through 8 of Panel A in Table 4. For example, a coefficient of -0.89 shown in Column 8 of Table 4 suggests that one year of exposure to a disruptive peer in a class of 25 reduces earnings by 0.7 percent (1/25 *-0.89/5). Assuming present discounted value of earnings of $522,000 as in Chetty, Friedman and Rockoff (2014), the estimate implies that a disruptive student reduces the lifetime earnings of each of his 24 peers by $3,654, or $87,696 across all students for that year.

violence has roughly the same effect on peer future earnings as replacing an average teacher with a teacher estimated to be in the bottom 5 percent.[13] We view this as plausible; 38 percent of teachers surveyed in the 2011-12 Schools and Staffing Survey report that student misbehavior interferes with their teaching.

Our findings also have significant implications for explaining disparities in the earnings of children who grew up in low- and high-socioeconomic status households. To the extent that school and neighborhood sorting causes students from low-income families (as proxied by subsidized lunch status) to be differentially exposed to disruptive peers, that by itself may explain some of the earnings gap observed in adulthood. For example, adults who grew up in low-income households in our sample earn roughly 70 percent of what adults from higher-income households earn, though they are also exposed to roughly 50 percent more disruptive peers of the type identified in this paper. Combined with the estimates shown in Table 4, back-of-the-envelope calculations indicate that the differential exposure to disruptive peers during elementary school explains around 5 or 6 percent of the rich-poor earnings gap in adulthood.[14] We view this as a meaningful part of the earnings gap, particularly since we have only one particular measure of disruptive peers.

6 Conclusion

In this paper, we document the long-run impact of disruptive peers during elementary school on subsequent standardized exam achievement, college enrollment and completion, and earnings. To distinguish peer effects from confounding factors, we include school-by-grade fixed

[13] we note that it would take roughly four boys from families linked to domestic violence to cause effects similar to that of replacing an average teacher with one who is *actually* in the bottom 5 percent. As noted in Chetty, Friedman and Rockoff (2014), because they can identify the bottom 5 percent of teachers with error, the improvement in present discounted value of earnings from replacing an estimated 5 percent teacher ($250,000) is significantly lower than the impact of replacing an actual bottom 5 percent teacher ($407,000).

[14] Source: Authors' calculations. This range comes from the estimates using log earnings and level earnings excluding zeros for the peer domestic violence measure of disruptive peers. By comparison, exposure to peer male domestic violence explains 6 to 8 percent of the gap, while exposure to unreported peer domestic violence explains roughly 4 percent.

effects to exploit the idiosyncratic year-to-year variation in disruptive peers within schools. We proxy for disruptive peers using three different measures of peers from families linked to domestic violence, who have been shown in previous work to negatively affect the contemporaneous achievement of their classmates.

Results indicate that the impact of these disruptive peers persist for years afterward and into adulthood. Estimates indicate that adding one student exposed to domestic violence to a class of 25 reduces high school test scores by 0.02 standard deviations and reduces earnings at age 24 to 28 by 3 to 4 percent. More targeted proxies for disruptive peers yield somewhat larger effects. These estimates reflect the impact of exposure to a disruptive peer throughout elementary school, which suggests that the per-year impact of exposure is roughly one-fifth the magnitude of these effects. These findings correspond to the same change in earnings as a roughly one-half reduction standard deviation in teacher quality (Chetty, Friedman and Rockoff, 2014), and imply that one year of exposure to a disruptive student reduces the present discounted value of classmates' combined total future earnings by around $100,000. We also show that due to sorting into schools, differential exposure to disruptive children explains roughly 5 or 6 percent of the earnings gap between those who grew up in lower-income versus higher-income families.

These findings illustrate the importance of peer composition in determining long-run educational attainment and labor market outcomes. This is important, because while a large existing literature has shown that peers impact contemporaneous learning, it was unclear whether the effects persisted for years afterward. In addition, by documenting the long-term impacts of disruptive peers, our results demonstrate the importance of potential policies that could attenuate the impact of disruptive peers. While the impact of such hypothetical policies is beyond the scope of this paper, our findings suggest that the social benefits of a reasonably effective policy are likely to be substantial. Thus, just as recent findings by Chetty, Friedman and Rockoff (2014) highlight the importance of addressing teacher qual-

ity as a way of improving long-run productivity and earnings, results here emphasize the importance of overcoming disruptive peers as a way of improving long-term outcomes.

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Table 1: Descriptive Statistics – By Grade

	3rd Grade	4th Grade	5th Grade
Panel A: Demographic Characteristics			
Black	0.385	0.374	0.380
	(0.487)	(0.484)	(0.485)
Male	0.498	0.491	0.493
	(0.500)	(0.500)	(0.500)
Free/reduced lunch	0.549	0.528	0.520
	(0.498)	(0.499)	(0.500)
Own domestic violence	0.044	0.047	0.048
	(0.206)	(0.211)	(0.213)
Fraction peers with domestic violence	0.044	0.047	0.048
	(0.034)	(0.033)	(0.033)
Fraction peers with yet-to-be reported domestic violence	0.021	0.020	0.019
	(0.021)	(0.019)	(0.019)
Fraction peers with already reported domestic violence	0.024	0.026	0.028
	(0.021)	(0.023)	(0.024)
Fraction male peers with domestic violence	0.023	0.023	0.023
	(0.022)	(0.021)	(0.023)
Fraction female peers with domestic violence	0.021	0.024	0.024
	(0.020)	(0.021)	(0.021)
Panel B: Educational Attainment			
College Enrollment	0.737	0.744	0.753
	(0.440)	(0.437)	(0.431)
Any Degree	0.265	0.281	0.293
	(0.441)	(0.450)	(0.455)
Bacc. Degree	0.197	0.207	0.215
	(0.398)	(0.405)	(0.411)
Panel C: Labor Force Outcomes - Quarterly Earnings Ages 24-28			
Positive	0.658	0.674	0.695
	(0.474)	(0.469)	(0.461)
Average (Include Zeros) ($2013)	1,126	1,433	1,689
	(1,633)	(2,150)	(3,120)
Average (Exclude Zeros) ($2013)	4,851	4,956	5,034
	(3,495)	(3,732)	(6,210)
Observations	14,144	14,384	12,961

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. Sample sizes for the outcomes in Panels B and C are smaller that the full sample, as we restrict the sample to individuals that by the end of 2012 or 2013 (last year of our education or earnings data) are old enough to be observed with the outcome of interest (age 18, 20, 22 and 24 for enrollment, any degree, college degree, and quarterly earnings respectively).

Table 2: Effects of Disruptive Peers on Test Scores

	Grades 3 to 5		Grades 6 to 8		Grades 9 and 10	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Exposure to Peers with DV						
Fraction Peers w/ DV	-0.48**	-0.45**	-0.24	-0.23	-0.57**	-0.50***
	(0.23)	(0.20)	(0.21)	(0.18)	(0.22)	(0.18)
B: Exposure to Male and Female Peers with DV						
Fraction Male Peers w/ DV	-0.81***	-0.72***	-0.40	-0.28	-0.84***	-0.67***
	(0.31)	(0.28)	(0.32)	(0.27)	(0.31)	(0.25)
Fraction Female Peers w/ DV	-0.14	-0.18	-0.07	-0.18	-0.30	-0.33
	(0.31)	(0.26)	(0.34)	(0.29)	(0.32)	(0.26)
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-0.93***	-1.10***	-0.37	-0.54*	-0.76**	-0.90***
	(0.36)	(0.30)	(0.33)	(0.29)	(0.36)	(0.28)
Fraction Peers w/ Reported DV	-0.03	0.14	0.03	0.25	-0.41	-0.15
	(0.33)	(0.28)	(0.29)	(0.24)	(0.31)	(0.23)
Observations	39833	39833	38656	38656	37019	37019
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls		Yes		Yes		Yes

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. Each column reports results from a separate regression. All regressions include controls for own family violence, as well as grade-year and school-grade fixed effects for grades third to fifth. Regressions in the even numbered columns include additional individual and cohort level controls. Individual controls include own 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. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: Effects of Disruptive Peers on College Enrollment and Degree Attainment

	Enrollment		Any Degree		4-Year Degree	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Exposure to Peers with DV						
Fraction Peers w/ DV	-0.15	-0.15	-0.19	-0.17	-0.17*	-0.14
	(0.11)	(0.10)	(0.13)	(0.13)	(0.10)	(0.10)
B: Exposure to Male and Female Peers with DV						
Fraction Male Peers w/ DV	-0.29**	-0.28*	-0.51***	-0.47***	-0.19	-0.07
	(0.15)	(0.15)	(0.17)	(0.17)	(0.14)	(0.13)
Fraction Female Peers w/ DV	0.01	-0.01	0.15	0.15	-0.15	-0.21
	(0.15)	(0.15)	(0.18)	(0.18)	(0.16)	(0.15)
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-0.38**	-0.41***	-0.62***	-0.66***	-0.22	-0.20
	(0.16)	(0.15)	(0.20)	(0.19)	(0.15)	(0.14)
Fraction Peers w/ Reported DV	0.02	0.04	0.34*	0.40**	0.01	0.07
	(0.15)	(0.14)	(0.18)	(0.18)	(0.16)	(0.15)
Mean Y	0.73	0.73	0.28	0.28	0.21	0.21
Observations	39570	39570	36235	36235	26255	26255
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls		Yes		Yes		Yes

Notes: Data are from the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. Each column reports results from a separate regression. We restrict the sample to individuals that by the end of 2012 (last year of our education data) are old enough to have completed the various degrees (18, 20 and 22 for enrollment, any degree and college degree, respectively). All regressions include controls for own family violence, as well as grade-year and school-grade fixed effects for grades third to fifth. Regressions in the even numbered columns include additional individual and cohort level controls. Individual controls include own 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. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Effects of Disruptive Peers on Labor Force Outcomes - Students Aged 24-28

	Positive Wages		Mean Wages (Include Zeros)		Mean Wages (Exclude Zeros)		Log (Wages)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Exposure to Peers with DV								
Fraction Peers w/ DV	0.03	0.07	-1707.97**	-1533.21*	-4174.51***	-4063.20**	-1.05***	-0.89***
	(0.14)	(0.14)	(781.26)	(853.95)	(1589.29)	(1845.40)	(0.32)	(0.32)
B: Exposure to Male or Female Peers with DV								
Fraction Male Peers w/ DV	-0.35*	-0.44**	-2868.09**	-3111.45**	-6529.19***	-6767.32**	-1.06**	-0.98**
	(0.19)	(0.18)	(1285.98)	(1334.24)	(2419.20)	(2717.30)	(0.46)	(0.43)
Fraction Female Peers w/ DV	0.47**	0.65***	-366.30	276.32	-1359.93	-849.85	-1.03**	-0.78
	(0.19)	(0.18)	(895.89)	(889.60)	(2031.27)	(2022.24)	(0.46)	(0.48)
C: Exposure to Peers with Unreported or Reported DV								
Fraction Peers w/ Unreported DV	-0.13	-0.16	-3060.98**	-3415.73**	-7725.00**	-8485.45**	-1.42***	-1.34***
	(0.20)	(0.19)	(1414.73)	(1562.87)	(3239.38)	(3767.53)	(0.42)	(0.42)
Fraction Peers w/ Reported DV	0.14	0.23	-658.18	61.02	111.41	1147.64	-0.80	-0.58
	(0.20)	(0.19)	(1230.39)	(1172.75)	(2557.70)	(2450.11)	(0.55)	(0.53)
Mean Y	0.67	0.67	1577.73	1577.73	5018.17	5018.17	8.20	8.20
Observations	21221	21221	21221	21221	14378	14378	14378	14378
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls		Yes		Yes		Yes		Yes

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. Each column reports results from a separate regression. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our wage data). All regressions include controls for own family violence, as well as grade-year and school-grade fixed effects for grades third to fifth. Regressions in the even numbered columns include additional individual and cohort level controls. Individual controls include own 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. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Heterogeneity in the Long Term Effects of Disruptive Peers

	Gender		Income		Race	
	Male	Female	Low	High	White	Non-White
A: Test Scores in Grades 9-10						
Fraction Peers w/ DV	-0.37	-0.62***	-0.26	-0.86***	-0.43*	-0.51**
	(0.28)	(0.22)	(0.23)	(0.28)	(0.25)	(0.23)
Fraction Male Peers w/ DV	-0.74**	-0.60**	-0.32	-1.25***	-0.43	-0.64**
	(0.37)	(0.30)	(0.29)	(0.42)	(0.38)	(0.33)
Fraction Peers w/ Unreported DV	-0.60	-1.18***	-0.40	-1.62***	-0.64	-0.74*
	(0.42)	(0.36)	(0.35)	(0.42)	(0.41)	(0.38)
B: Attainment of Any Degree						
Fraction Peers w/ DV	-0.18	-0.15	-0.05	-0.23	-0.04	-0.20*
	(0.14)	(0.17)	(0.10)	(0.23)	(0.20)	(0.11)
Fraction Male Peers w/ DV	-0.57***	-0.40*	-0.15	-0.69**	-0.33	-0.42**
	(0.20)	(0.22)	(0.13)	(0.31)	(0.25)	(0.17)
Fraction Peers w/ Unreported DV	-0.74***	-0.58**	-0.27*	-0.74**	-0.69**	-0.38*
	(0.21)	(0.27)	(0.15)	(0.31)	(0.27)	(0.19)
C: Likelihood of Positive Wages						
Fraction Peers w/ DV	0.13	0.01	0.41**	-0.49**	0.05	0.06
	(0.22)	(0.20)	(0.18)	(0.23)	(0.21)	(0.19)
Fraction Male Peers w/ DV	-0.50*	-0.39	-0.12	-1.00***	-0.65**	-0.31
	(0.27)	(0.26)	(0.22)	(0.38)	(0.28)	(0.25)
Fraction Peers w/ Unreported DV	0.01	-0.30	0.30	-0.84**	0.08	-0.36
	(0.30)	(0.27)	(0.26)	(0.33)	(0.29)	(0.29)
D: Mean Wages (Including Zeros)						
Fraction Peers w/ DV	-510	-2738***	-617	-2849	-3343**	-72
	(1554)	(843)	(607)	(2120)	(1472)	(731)
Fraction Male Peers w/ DV	-3894*	-2482**	-1784**	-7239*	-5912**	-1093
	(2345)	(1141)	(806)	(3962)	(2395)	(943)
Fraction Peers w/ Unreported DV	-3834	-2892***	-755	-8291*	-5841**	-759
	(2842)	(1066)	(829)	(4388)	(2733)	(1164)
E: Log (Wages)						
Fraction Peers w/ DV	-0.43	-1.33***	-1.20***	-0.31	-1.36***	-0.55
	(0.46)	(0.45)	(0.40)	(0.54)	(0.48)	(0.38)
Fraction Male Peers w/ DV	-1.13	-0.81	-1.22**	-0.44	-1.04	-1.19**
	(0.73)	(0.56)	(0.48)	(0.88)	(0.69)	(0.49)
Fraction Peers w/ Unreported DV	-0.83	-1.75***	-0.97*	-1.87**	-2.32***	-0.61
	(0.67)	(0.62)	(0.50)	(0.77)	(0.66)	(0.58)

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. Each column and raw reports results from a separate regression. Sample sizes vary by outcome analyzed, as we restrict the sample to individuals that by the end of 2012 or 2013 (last year of our education or earnings data) are old enough to be observed with the outcome of interest (age 18, 20, 22 and 24 for enrollment, any degree, college degree, and quarterly earnings respectively). All regressions include controls for own family violence, individual and cohort level controls, as well as grade-year and school-grade fixed effects for grades third to fifth. Individual controls include own 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. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Effects of Disruptive Peers on Exogenous Student Characteristics

					Income	
	Male	White	Black	Free Lunch	Median	Missing
A: Exposure to Peers with DV						
Fraction Peers w/ DV	0.031	-0.107	-0.079	0.003	-0.076	0.017
	(0.126)	(0.127)	(0.142)	(0.108)	(0.064)	(0.024)
B: Exposure to Male or Female Peers with DV						
Fraction Boy Peers w/ DV	0.007	-0.176	-0.049	0.155	-0.037	0.035
	(0.174)	(0.164)	(0.188)	(0.149)	(0.084)	(0.041)
Fraction Girl Peers w/ DV	0.055	-0.034	-0.112	-0.156	-0.116	-0.002
	(0.199)	(0.193)	(0.228)	(0.152)	(0.096)	(0.035)
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	0.132	0.004	-0.272	-0.148	0.033	0.066
	(0.187)	(0.208)	(0.235)	(0.200)	(0.099)	(0.048)
Fraction Peers w/ Reported DV	0.008	-0.244	0.142	0.093	-0.186**	-0.031
	(0.171)	(0.173)	(0.196)	(0.164)	(0.085)	(0.037)
Mean Y	0.50	0.56	0.37	0.54	10.66	0.01
Observations	41201	41201	41201	41201	40817	41201
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. Each column reports results from a separate regression. All regressions include controls for own family violence, as well as cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

A Appendix

Table A.1: Effects of Disruptive Peers on Test Scores for Each Grade

	Average Score in Grade:							
	3rd	4th	5th	6th	7th	8th	9th	10th
Fraction Peers w/ DV	-0.25	-0.39*	-0.79***	-0.04	-0.41*	-0.24	-0.50***	-0.58***
	(0.27)	(0.22)	(0.23)	(0.24)	(0.23)	(0.19)	(0.19)	(0.19)
Observations	30242	34049	29858	28773	32863	36354	35635	34325

Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. Each column reports results from a separate regression. All regressions include controls for own family violence, individual controls, cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Individual controls include own 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. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table A.2: Effects of Disruptive Peers on Probability of Missing Score

	Exiting Sample in Grade:						
	4th	5th	6th	7th	8th	9th	10th
A: Exposure to Peers with DV							
Fraction Peers w/ DV	-0.026	0.014	0.049	0.074*	0.008	-0.010	0.023
	(0.109)	(0.061)	(0.042)	(0.040)	(0.049)	(0.053)	(0.068)
B: Exposure to Male or Female Peers with DV							
Fraction Male Peers w/ DV	-0.198	0.064	0.061	0.076	0.021	-0.013	-0.045
	(0.140)	(0.094)	(0.069)	(0.057)	(0.068)	(0.081)	(0.093)
Fraction Female Peers w/ DV	0.165	-0.036	0.036	0.071	-0.005	-0.007	0.093
	(0.129)	(0.088)	(0.061)	(0.060)	(0.063)	(0.070)	(0.107)
C: Exposure to Peers with Unreported or Reported DV							
Fraction Peers w/ Unreported DV	0.242	0.126	-0.008	0.046	0.026	0.036	0.187*
	(0.149)	(0.095)	(0.065)	(0.055)	(0.074)	(0.080)	(0.103)
Fraction Peers w/ Reported DV	-0.387**	-0.069	0.126**	0.088	-0.039	-0.070	-0.138
	(0.161)	(0.088)	(0.063)	(0.057)	(0.070)	(0.080)	(0.090)
Mean Y	0.04	0.04	0.03	0.02	0.02	0.04	0.08
Observations	13225	25402	36990	28545	32626	36115	35421
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. Each column reports results from a separate regression. All regressions include controls for own family violence, individual controls, cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Individual controls include own 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. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.