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 have been shown to disrupt contemporaneous behavior and 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 percent of the rich-poor earnings gap in our data, and that each year of exposure to a disruptive peer reduces the present discounted value of classmates' future earnings by $80,000.

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1 Introduction

A large and growing literature has documented the importance of peer effects in education. This line of research has focused primarily on how peers affect contemporaneous outcomes such as test scores and 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 is important because it is not clear that one's peers will necessarily affect outcomes years after those peers are gone. For example, peers could primarily affect contemporaneous performance on standardized exams, rather than learning, in which case the effects could be short-lived. Similarly, while certain peers may induce some students to misbehave during school, those behavioral issues may go away when the student integrates into new and different peer groups in the future.

This lack of evidence on long-run impact of childhood peers 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 underscores 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 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 identify children whose families are characterized by domestic violence. This is critical for our study for two reasons. First, exposure to domestic violence is exogenous to the student's classmates, which is critical for overcoming the reflection problem (Manski, 1993). In addition, exposure to domestic violence has been shown to be a particularly good proxy for a disruptive peer. Previous research by Carrell and Hoekstra (2010, 2012) has shown that exposure to these peers significantly disrupts contemporaneous achievement and behavior, and that these effects are driven by boys and children whose families have not yet reported the domestic violence. These contemporaneous effects are large; Carrell and Hoekstra (2010) report that having one additional classmate exposed to domestic violence reduces achievement by one-fortieth of a standard deviation, and increases disciplinary infractions by 17 percent. These findings are also consistent with a much larger literature documenting that children exposed to domestic violence are associated with a number of emotional and behavioral problems including aggressive behavior, 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). The purpose of this paper is to document whether exposure to these elementary school students, hereafter referred to as "disruptive" peers, affects long run educational and labor market outcomes.[1]

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.[2] Intuitively, we ask whether students never linked to domestic violence who are in cohorts with an idiosyncratically high number of disruptive peers have worse long-

[1]In referring to these students as "disruptive", we do not mean to assume that the only mechanism through which any long-run effects arise is through classroom disruption. Rather, while we would expect much of any long-run effect to be due to classroom interaction, it could also be due to interactions separate from classroom disruptions.

[2]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).

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 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 that this within-school variation in exposure to disruptive peers is uncorrelated with predicted earnings using a full set of fixed effects and covariates, which is consistent with the identifying assumption.

Results show 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 throughout elementary school reduces math and reading test scores in grades 9 and 10 by 0.02 standard 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 and 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 adult outcomes. We estimate that one year of exposure to a disruptive peer in elementary school reduces the present discounted value of classmates' future earnings by around $80,000, 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 percent of the rich-poor earnings gap in adulthood.

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, though Bifulco et al. (2014) show that this effect diminishes over time and that there is no evidence of an effect on labor market outcomes. Anelli and Peri (2015) analyze the long-term effects of high school gender composition and find that a higher proportion of female peers reduces the likelihood males choose a "prevalently male" major, but has no effect on graduation and labor market outcomes. 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, because we observe test scores through the 10th grade, we are able to examine whether test score effects "fade out" over time, as has been shown to be the case in the teacher quality literature. Third, to our knowledge, we are first to identify the long-term effects of elementary school peers on adult earnings.

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 and the Perry Preschool programs (Garces, Thomas and Currie, 2002; Ludwig and Miller, 2007; Heckman, Pinto and Savelyev, 2013), 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 link administrative school records to several other administrative data sets. The school records contain 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. These cases were filed when one member of the family petitioned the court for a temporary injunction for protection against another member of the family. The data include 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.[3]

To link long-run education and earnings outcomes to the administrative school records from Alachua County, the data from Alachua County were sent to the Florida Department of Education (FLDOE), who then linked the data to longer-term outcomes as of the end of 2010. We obtain (raw) test scores for grades 6 through 10.[4] 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.[5] Finally, the FLDOE also provided quarterly earnings for the students working in the state of Florida for the years 2000–2013. These earnings are transformed to 2013 real values, and are averaged for each individual across all quarters between ages 24 and 28.[6]

Table (1) presents summary statistics for our main independent variables. These statistics show that around 37 percent of the sample is black and just over 50 percent are on subsidized lunch. Just under five percent of peers are linked to domestic violence, though for reasons outlined in the following section, we exclude from the sample all children who themselves

[3] For cases in which the same petitioner filed multiple requests, we used the first request.

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

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

[6] We note that, as we describe in more detail in the results section, we get similar results when we perform a more complicated first-within-then-between analysis that exploits the Frisch-Waugh-Lovell Theorem.

were linked to domestic violence. Of the peers linked to domestic violence, roughly half are male and half are female. In addition, of the peers 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.[7] Around 75 percent of the students in our sample have ever enrolled in college, 29 percent have received some type of college degree, and around 24 percent have received a bachelor's degree. Average quarterly earnings calculated across all quarters including those with zero earnings (even for individuals sometimes observed with positive earnings) is $1,460. Average quarterly earnings for those observed with positive earnings between age 24 and 28 is $5,006 dollars.

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 proportion 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.[8]

[7] The panel nature of our elementary school 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.

[8] 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 also perform an empirical test of whether this year-to-year variation at the school-grade level is consistent with a random process. Following the resampling technique used in Carrell and West (2010), for each cohort in each school and grade combination, we first randomly draw 10,000 cohorts of equal size, drawn from the relevant school/grade. Secondly, for each of the random cohorts we compute the average proportion of peers exposed to domestic violence. Thirdly, we compute empirical p-values for each of these random draws, where the p-value represents the proportion of simulated cohorts with average exposure to disruptive peers smaller than the average actually observed in that cohort. If the year-to-year variation at the school-grade level is random, we expect the distribution of the p-value to be uniform. Hence, we use a Kolmogorov-Smirnov one sample equality of distribution test to test whether the distribution of p-values is uniform, and we reject uniformity only 2 times out of 65.

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:

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

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.[9] 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. We note that because we exclude children who are themselves linked to domestic violence from the sample, there is no need to control for own family violence. 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-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 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

[9]Note that these outcomes are grade invariant.

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 multiple 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 are identified using average peer exposure across the 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.

One potential concern with this type of research design was raised by Angrist (2014), who argues that estimates using this approach can be biased due to a mechanical relationship between own domestic violence status and peer domestic violence. To address this concern, we do two things. First, we exclude children linked to domestic violence from the data. As a result, the thought experiment underlying our research design is whether *other* students happen to land in a cohort with an idiosyncratically high number of peers linked to domestic violence, or an idiosyncratically low number of peers linked to domestic violence. Second, in the spirit of Feld and Zolitz (Forthcoming), we empirically examine the impact of adding increasing amounts of measurement error to our data on point estimates.[10,11] As shown in

[10]For each error rate (e.g., 10%), we perform the following: i) randomly create a 10% sample to which to assign error; ii) among those in the sample assigned to have error, randomly assign 4% of them (the average rate of domestic violence in our sample) to have DV=1 and the others to have DV=0; iii) create new peer variables and exclude from the sample those linked to domestic violence (a combination of actual and those mis-assigned); and iv) estimate equation (1).

[11]A previous version of the paper, NBER Working Paper 22042, included these students in the sample. Results are qualitatively similar, though estimates when excluding these students are slightly smaller. This

Appendix Figure A1, adding measurement error to the disruptive peer measure results in attenuated estimates, with larger amounts of error leading to more attenuated estimates.[12] Consequently, we conclude that our findings are unlikely to be confounded by this issue.

However, the validity of our research design could still potentially be threatened to the extent 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 note that this would be a relatively extreme response given it likely involves moving one's residence. Instead, we believe it is much more likely that certain types of parents may lobby school principals to ensure their child is not put in the same classroom as certain other children perhaps known to be disruptive, rather than moving to a new residence and school altogether. Importantly, this type of avoidance behavior within schools does not invalidate our design or bias our estimates. This is because our estimates capture the reduced-form (average) effect of treatment at the cohort level, rather than the classroom level.

Nevertheless, we perform three exercises to address the possibility of selection into and out of cohorts across schools. 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 2, which shows the correlation between our three measures of disruptive peers and gender, race, subsidized lunch status, and neighborhood income level. We emphasize that the coefficients are interpreted as the effect of going from 0 to 100 percent disruptive peers, and thus need to be rescaled. For example, one of the largest coefficients—the marginally significant coefficient of -0.35 on unreported peer domestic violence—indicates that adding one student linked

is likely due to the fact that earnings effects are largest for those students in the left tail of the earnings distribution, as shown and discussed later. We note that when including children linked to domestic violence in the sample, estimates were insensitive to whether or not we controlled for the own domestic violence effect.

[12] We are grateful to Ulf Zoelitz for suggesting this exercise.

to as-yet-unreported domestic violence to a class of 25 is associated with a 1.4 (0.35/25) percentage point reduction in the likelihood of being black.[13] Overall, Table 2 shows that of the 30 estimates, only one is significant at the 5 percent level, which is approximately what one would expect due to chance. None is significant at the 1 percent level. As a result, we conclude based on Table 2 that there is 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. Similarly, in a related approach we estimate effects both without and with individual and other peer controls.

Finally, we also combine all of our covariates into a predicted log earnings measure for each individual, and show that predicted log earnings is uncorrelated with whether the individual was exposed to an idiosyncratically high or low concentration of disruptive peers during elementary school. Specifically, we regress log earnings on the full set of fixed effects and controls, excluding peer domestic violence, and use the estimated coefficients to predict earnings for each individual who is subsequently observed with positive earnings. We do so only for those who are observed with positive adult earnings. We then graph predicted log earnings against the percent change in residual exposure to disruptive peers (relative to the average peer exposure for that school and grade) after controlling for school-grade and grade-year fixed effects. Results are shown in the red dashed lines and corresponding local averages in Figure 1. Importantly, across all three measures of treatment, the relationship between predicted earnings and treatment is flat. That indicates there is no reason to believe that students across these different cohorts should have had different earnings levels, absent the effect of exposure to disruptive peers. In addition, because Figure 1 shows predicted earnings *only for those observed with positive adult earnings*, it demonstrates that the income-earning potential for those observed with earnings is not systematically correlated with treatment. This suggests that attrition out of the state is unlikely to bias our estimates.

[13] We also note that this particular correlation is the wrong sign for those concerned with selection into or out of cohorts.

Figure 1 also highlights our main findings on the long-run impact of disruptive peers on earnings. In contrast to predicted earnings, which do not vary with intensity of treatment as graphed on the x-axis, *actual* earnings (shown in solid black) do vary significantly with whether one was exposed to an idiosyncratically high or low proportion of peers linked to domestic violence. Consistent with expectations, the raw data shown in Figure 1 indicate that children who were exposed to an above-average concentration of disruptive peers in elementary school have much lower-than-predicted earnings. Thus, while we will document the magnitude of these effects empirically in the next section, Figure 1 provides an illustration of both the validity of the research design as well as the qualitative long run impact of peers on earnings.

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 examine 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. 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 3, where the first two columns of Panel A assess 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 and school-grade fixed effects, while column (2) additionally controls for other individual and cohort-level controls. The estimate in column (1) of -0.32 suggests that adding one disruptive student to a class of 25 reduces achievement by 0.01 of a standard deviation (1/25 * -0.32). Estimates in columns (3) and (4) indicate a more modest impact during grades 6 through 8, though the effect of that same disruptive peer during elementary school is again a reduction of around 0.01 to 0.02 standard deviations in grades 9 and 10. Only the estimates in grades 9 and 10 are statistically distinguishable from zero, and only at the 10 percent level. Across grades, none of the estimates are statistically distinguishable from each other.

Panel B of Table 3 shows estimates of the impact of male and female peers from families linked to domestic violence. The estimate in column (2) indicates that adding one disruptive male peer to a class of 25 reduces grade 3 – 5 test scores by 0.02 standard deviations (1/25 * -0.58), while female peers from families linked to domestic violence do not appear to reduce their peers' academic performance. In short, results indicate that it is the boys from these troubled families that most negatively disrupt contemporaneous academic performance, with some evidence that these effects persist afterward into high school.

Estimates of the impact of peers exposed to as-yet-unreported and reported domestic violence are shown in Panel C of Table 3. 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 between 0.02 and 0.03 standard deviations in grades 9 – 10.

Importantly, estimates across all grade levels in Table 3 change little when including individual-level and cohort-level controls. This is consistent with the identifying assumption, and provides additional evidence 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 4, which takes the same form as Table 3. 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 4 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 (1.4 percent) reduction in college enrollment (1/25 *-0.26), which is significant at the 10 percent level. Similarly, the estimate of -0.53 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 1.8 percentage point (2.4 percent) reduction in college enrollment and a 2.7 percentage point (9.4 percent) 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 5. 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 for all quarters in which individuals were not observed with earnings; columns (5) through (8) show estimates for the level and log of quarterly earnings conditional on being observed with positive earnings. The estimate of -0.95 in column (7) indicates that adding one child linked to domestic violence to a classroom of 25 reduces earnings by 3.8 percent (-0.95*1/25). To put this in perspective, we note that while our main analysis excludes children linked to domestic violence from the sample, if we were instead to include those children in this specification we estimate that they earn 13 percent less than their peers not linked to domestic violence. Overall, estimates across columns (5) through (8) in Panel A indicate that elementary school exposure to one additional disruptive student in a class of 25 reduces earnings by between 3 and 4 percent. All four 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 slightly reduced labor force participation, while exposure to girls is associated with increased labor force participation.[14]

While we do not have a good interpretation of exactly why some measures of peers have 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 10 percent level, with estimates conditional on positive earnings indicating that exposure to one of these disruptive boys reduces earnings by 4 to 5 percent. For example, the estimate in column (6) of -6,163 indicates that exposure to one more disruptive male in a class of 25 throughout elementary school reduces earnings by $247. That drop in earnings represents a reduction of 4.9 percent, given average quarterly earnings of $5,064 as shown in the bottom of Table 4.

Results in Panel C of Table 5 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 elementary school to one more peer from a family with unreported domestic violence in a class of 25 is associated with a 5 to 6 percent reduction in earnings.

[14] We suspect that the marginally significant 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. In addition, we note that predicted earnings for those subsequently observed with positive earnings (i.e., excluding those not observed with earnings such as those who left the state), as shown in Figure 1, are uncorrelated with the level of exposure to disruptive peers.

Importantly, we note that while the outcome of interest in Table 5 is earnings averaged across all quarters—either including or excluding quarters with zeros, depending on the column—results are similar when we use a more complicated estimator that adjusts explicitly for calendar year and age effects.[15]

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 and Quantile Analyses

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. In addition, we also perform a quantile analysis of the test score and earnings results.

Subgroup results are shown in Table 6. 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 there are few meaningful differences between the men and women with respect to the long-run impacts of disruptive peer exposure. Estimates for

[15]Specifically, we perform a first-within-then-between analysis that exploits the Frisch-Waugh-Lovell Theorem. We first partial out age and year effects from both log earnings and the peer domestic violence treatment measure, then average residual earnings by individual, and then regress average residual earnings on residual treatment. Estimates are very similar. For example, the first-within-then-between estimates corresponding to the estimates of -0.81, -0.81, and -1.16 shown in Column 8 of Panels A, B, and C in Table 5 are -0.83, -0.89, and -1.24, respectively. Because the results are so similar—which is in part because grade-by-year effects included in our analyses capture many of the differences in labor market conditions across individuals—we focus on the simpler analysis that uses average earnings.

men and women are similar for all outcomes including grade 9 and 10 test scores, degree attainment, and earnings. In only 2 of the 15 cases are the estimates for men and women statistically different from each other (earnings levels including zeros and log earnings 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 substantive difference by gender (which is not shown in Table 6 for brevity purposes) is that while disruptive boys reduce the adult earnings of both peer boys and peer girls, disruptive girls also reduce girls' adult earnings.

In the third and fourth columns of Table 6, 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 point estimates indicate that students with higher socioeconomic status experience larger declines in their high school test scores and degree attainment, though 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 6, 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 addition, we also perform a quantile analysis to examine how the long run effects differ across the test score and earnings distributions. Results for high school test scores and log earnings are shown in Appendix Figures A.1 and A.2, respectively. Results for test scores suggest that the biggest effects of disruptive peers are on those in the top half of the achievement distribution, while results for earnings suggest the largest effects are at the very bottom of the earnings distribution, though effects are present and roughly similar across the rest of the earnings distribution.

In summary, results from Table 6 yield three patterns with respect to the heterogeneous impacts of disruptive peers. First, students seem to experience similar long-run effects across gender and socioeconomic status. Second, white students seem to experience larger declines in earnings due to disruptive peers relative to black students. Third, while the test score impacts seem to affect the top of the distribution the most, the earnings effects are largest for students at the bottom of the earnings distribution.

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 2.7 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 0.81 percent decrease in earnings through this one educational channel (-0.026*30). Similarly, a back-of-the-envelope calculation indicates that the 9th and 10th grade test score reductions we observe can explain only around 15 percent of the total reduction in earnings.[16] In addition, the results of the subgroup and quantile analyses are difficult to reconcile with

[16] We estimate that adding one disruptive student to a classroom of 25 reduces earnings by 3.24 percent. By comparison, the same disruptive student reduces test scores by 0.014 standard deviations. Using a simple hedonic regression of log earnings on grade-by-year fixed effects, school-by-grade fixed effects, and test scores, we estimate that a one standard deviation increase in test scores is associated with a 20 percent increase in earnings. That implies a 0.025 standard deviation reduction in test scores would result in a 0.28 percent reduction in earnings, which is 8.7 percent of our overall effect of 3.24 percent.

the hypothesis that the effects work largely through educational achievement or attainment. For example, disruptive peers have the largest effects on the test scores of high-achievers, while the earnings effects are largest at the bottom of the earnings distribution. Similarly, while the effects on earnings are largest amongst whites, both whites and blacks experience similar effects on educational achievement.

For all of these reasons, we expect that much of the earnings effects documented above likely comes from non-cognitive skills. Unfortunately, the nature of non-cognitive skills makes it difficult to test this directly. In Appendix Table A.2, we provide some evidence by showing the impacts on suspensions during high school. While the results are not perfectly consistent with the impacts on earnings shown in Table 5, they do suggest that exposure to disruptive peers during elementary school can have long-run impacts on the type of behavior that may have significant implications labor market success. In addition, it is important to note that the likelihood the earnings effects work through non-cognitive channels is broadly consistent with the existing literature on the long-run impacts of other childhood interventions, much of which finds large long-run effects that are difficult to explain through achievement. 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). Similarly, Chetty, Friedman and Rockoff (2014) document large effects of teacher quality on earnings despite evidence that test score gains due to better teachers fade out in subsequent years. Finally, the likelihood that the long-run effect 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. We note that the estimates shown in this paper should be interpreted as cumulative effects of peer exposure during elementary school. As a result, we divide those estimates by five years in order to obtain an approximate per-year estimate, though we note that the qualitative conclusions of our study hold even if one were to scale by slightly more years to account for possibility that the mixing of cohorts in middle and high school may not wash out all of the variation from elementary school. Our findings indicate that one year of exposure to a disruptive boy peer reduces college enrollment by 0.2 percentage points.[17] 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,[18] it implies that adding one disruptive peer is equivalent to reducing overall class

[17] Given a coefficient of -0.26 in Column 2 of Panel B in Table 4, 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.

[18] Coefficients in columns 5 through 8 of Panel A in Table 5 indicate that exposure to a disruptive peer *throughout elementary school* in a class of 25 reduces earnings by 2.8 to 3.8 percent. Scaling these estimates

quality by around 7 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,[19] we estimate that one year of exposure to a disruptive student reduces the present discounted value of lifetime earnings by around $80,000.[20,21] Similarly, using estimates from columns 5 - 8 of Panel B in Table 5, we estimate that removing a male

by one-fifth, we estimate that each year of exposure reduces earnings by 0.6 to 0.8 percent.

[19]First, we assume that the impact of disruptive children is constant over the life cycle using estimates from columns 3 - 8 in Table 5. 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.

[20]This figure is based on estimates presented in Columns 5 through 8 of Panel A in Table 5; the full range of estimates corresponding to estimates in columns 5 - 8 of Panel A in Table 5 is $71,000 - $95,000, with an average of $79,993. For example, a coefficient of -0.81 shown in Column 8 of Table 5 suggests that one year of exposure to a disruptive peer in a class of 25 reduces earnings by 0.65 percent (1/25 *-0.81/5). 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 $3,393, or $81,432 across all students for that year.

[21]We note that this estimate is somewhat smaller than the $100,000 figure cited in a previous version of this paper. This is because for reasons discussed earlier, we now exclude children linked to domestic violence from the data set. This reduces point estimates somewhat, as one would expect given Appendix Figure A3, which shows that disruptive peers have the largest effects on those in the left tail of the earnings distribution.

peer linked to domestic violence would increase the present discounted value of classmate earnings by $81,000 to $122,000, and removing a peer linked to unreported domestic violence would increase the present discounted value of classmate earnings by $116,000 to $153,000. Thus, our findings imply that having two to three peers from families linked to domestic 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.[22] 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 5, back-of-the-envelope calculations indicate that the differential exposure to disruptive peers during elementary school explains around 4 to 6 percent of the rich-poor earnings gap in adulthood.[23] We view this as a meaningful part of the earnings gap, particularly since we have only one particular measure of disruptive peers.

[22] 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).

[23] 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. Estimates for the more targeted measures of disruptive peers are 3 to 5 percent. If we instead use log earnings estimates from the subgroup analyses presented in Table 6, we estimate that the increased exposure explains 21 percent, 10 percent, or 0 percent of the rich-poor earnings gap when defining a disruptive peer as any peer linked to domestic violence, a male peer linked to domestic violence, or a peer linked to as-yet-unreported domestic violence, respectively.

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 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 and behavior 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 $80,000. We also show that due to sorting into schools, differential exposure to disruptive children explains around 5 percent of the earnings gap between those who grew up in lower-income versus higher-income families. Given that we only have one particular proxy for disruptive peers, we view this as a lower bound of the impact of disruptive elementary school peers on income inequality.

These findings illustrate the importance of peer composition in determining long-run educational attainment and labor market outcomes. This is significant, 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 effect 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 quality 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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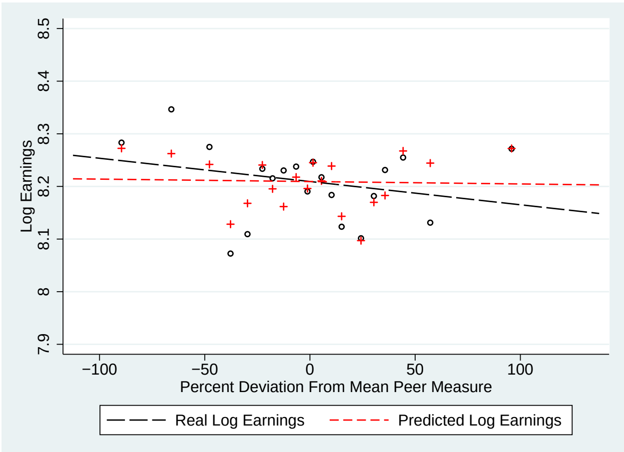
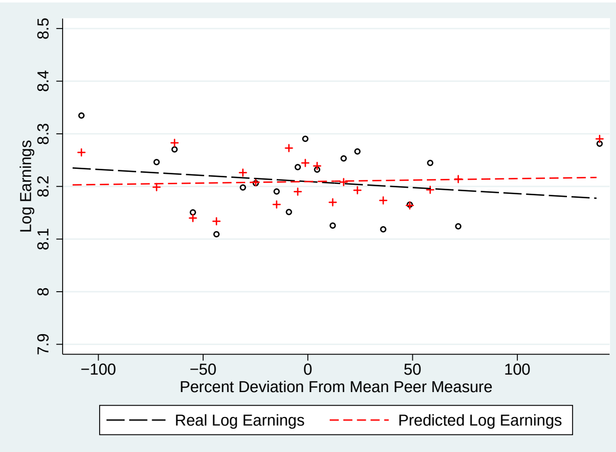
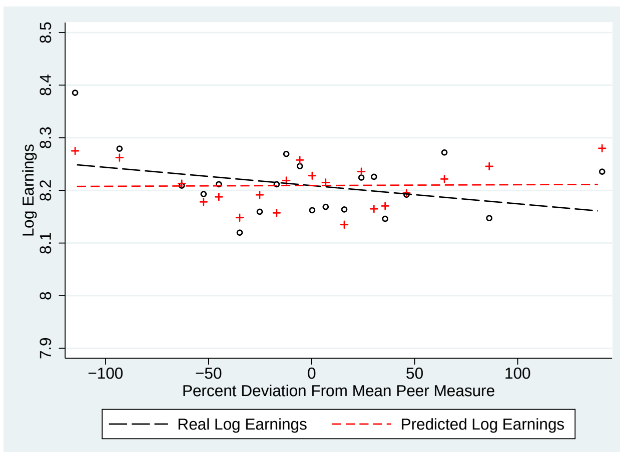
Figure 1: Relationship Between Disruptive Peers and Real or Predicted Log Quarterly Earnings



(a) Fraction Peers w/ DV



(b) Fraction Male Peers w/ DV



(c) Fraction Peers w/ Unreported DV

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data) and whose family did not report domestic violence. We create the predicted log earnings outcome by first running a regression that includes controls for grade-year and school-grade fixed effects for grades third to fifth, as well as additional individual and cohort level controls. Individual controls include 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. The regression is weighted by the inverse of the number of times a student is observed in the sample. Second, we predict log earnings using the estimated coefficients. Lastly, we collapse the data to 20 groups defined according to the percent change in residual exposure to disruptive peers (relative to the average peer exposure for that school and grade) after controlling for school-grade and grade-year fixed effects.

Table 1: Descriptive Statistics

	Mean	Std. Dev.
Panel A: Demographic Characteristics		
Black	0.368	(0.482)
Male	0.494	(0.500)
Free/reduced lunch	0.518	(0.500)
Fraction peers with domestic violence	0.046	(0.033)
Fraction peers with yet-to-be reported domestic violence	0.020	(0.020)
Fraction peers with already reported domestic violence	0.025	(0.023)
Fraction male peers with domestic violence	0.023	(0.021)
Fraction female peers with domestic violence	0.023	(0.021)
Panel B: Educational Attainment		
College Enrollment	0.749	(0.433)
Any Degree	0.288	(0.453)
Bacc. Degree	0.237	(0.425)
Panel C: Labor Force Outcomes - Quarterly Earnings Ages 24-28		
Positive	0.676	(0.468)
Average (Include Zeros) ($2013)	1,460	(2,502)
Average (Exclude Zeros) ($2013)	5,006	(4,892)
Observations	39,535	

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). We restrict the sample to individuals whose family did not report domestic violence.

Table 2: 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	0.005	-0.102	-0.072	0.010	-0.086	0.017
	(0.112)	(0.112)	(0.125)	(0.109)	(0.060)	(0.023)
Fraction Male Peers w/ DV	0.056	-0.189	-0.053	0.144	-0.053	0.044
	(0.158)	(0.147)	(0.164)	(0.149)	(0.080)	(0.040)
Fraction Female Peers w/ DV	-0.046	-0.014	-0.091	-0.125	-0.120	-0.009
	(0.169)	(0.163)	(0.192)	(0.147)	(0.084)	(0.034)
B: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	0.028	0.090	-0.350*	-0.085	0.026	0.072*
	(0.164)	(0.169)	(0.186)	(0.170)	(0.085)	(0.043)
Fraction Peers w/ Reported DV	-0.014	-0.267*	0.167	0.092	-0.182**	-0.030
	(0.151)	(0.155)	(0.175)	(0.164)	(0.078)	(0.036)
Mean Y	0.50	0.56	0.36	0.52	10.67	0.01
Observations	39535	39535	39535	39535	39151	39535
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. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include controls for 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.

Table 3: 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	-0.32	-0.30	-0.16	-0.16	-0.44*	-0.36*
	(0.24)	(0.20)	(0.22)	(0.18)	(0.24)	(0.19)
B: Exposure to Male and Female Peers with DV						
Fraction Male Peers w/ DV	-0.67**	-0.58**	-0.34	-0.22	-0.69**	-0.52**
	(0.33)	(0.29)	(0.34)	(0.29)	(0.33)	(0.26)
Fraction Female Peers w/ DV	0.03	-0.03	0.02	-0.10	-0.19	-0.20
	(0.32)	(0.27)	(0.35)	(0.30)	(0.34)	(0.26)
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-0.79**	-1.02***	-0.46	-0.68**	-0.60*	-0.73***
	(0.36)	(0.29)	(0.34)	(0.27)	(0.36)	(0.28)
Fraction Peers w/ Reported DV	0.08	0.32	0.10	0.29	-0.31	-0.04
	(0.33)	(0.27)	(0.31)	(0.24)	(0.33)	(0.24)
Observations	37994	37994	36781	36781	35242	35242
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. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include controls for 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 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 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	-0.14	-0.13	-0.19	-0.17	-0.13	-0.09
	(0.11)	(0.11)	(0.13)	(0.13)	(0.11)	(0.11)
B: Exposure to Male and Female Peers with DV						
Fraction Male Peers w/ DV	-0.29*	-0.26*	-0.57***	-0.53***	-0.19	-0.05
	(0.15)	(0.15)	(0.18)	(0.17)	(0.15)	(0.15)
Fraction Female Peers w/ DV	0.01	0.00	0.20	0.19	-0.06	-0.14
	(0.16)	(0.16)	(0.19)	(0.19)	(0.17)	(0.16)
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-0.44***	-0.45***	-0.68***	-0.68***	-0.22	-0.18
	(0.16)	(0.15)	(0.19)	(0.18)	(0.16)	(0.14)
Fraction Peers w/ Reported DV	0.13	0.15	0.28	0.32*	-0.03	0.01
	(0.14)	(0.14)	(0.18)	(0.18)	(0.15)	(0.15)
Mean Y	0.74	0.74	0.29	0.29	0.23	0.23
Observations	37726	37726	34548	34548	25041	25041
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). We also restrict the sample to individuals whose family did not report domestic violence. All regressions include controls for 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 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: Effects of Disruptive Peers on Labor Force Outcomes - Students Aged 24-28

	Positive Earnings		Mean Earnings (Include Zeros)		Mean Earnings (Exclude Zeros)		Log (Earnings)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Exposure to Peers with DV								
Fraction Peers w/ DV	0.08	0.10	-1261.04	-1155.05	-3676.22**	-3577.83*	-0.95***	-0.81**
	(0.14)	(0.14)	(801.55)	(874.31)	(1684.16)	(1939.87)	(0.33)	(0.34)
B: Exposure to Male or Female Peers with DV								
Fraction Male Peers w/ DV	-0.25	-0.34*	-2380.21*	-2595.65*	-6114.71**	-6163.64**	-0.95*	-0.81*
	(0.21)	(0.20)	(1361.30)	(1396.50)	(2601.38)	(2912.99)	(0.50)	(0.48)
Fraction Female Peers w/ DV	0.44**	0.58***	-21.96	423.51	-895.91	-650.48	-0.96**	-0.81
	(0.20)	(0.19)	(924.85)	(917.78)	(2112.75)	(2092.51)	(0.48)	(0.49)
C: Exposure to Peers with Unreported or Reported DV								
Fraction Peers w/ Unreported DV	-0.11	-0.13	-2747.75**	-2998.49**	-7109.04**	-7713.34**	-1.25***	-1.16***
	(0.19)	(0.18)	(1355.79)	(1491.98)	(3286.45)	(3737.30)	(0.42)	(0.44)
Fraction Peers w/ Reported DV	0.29	0.36*	374.94	868.57	73.02	899.02	-0.63	-0.43
	(0.19)	(0.18)	(1058.44)	(993.95)	(2312.19)	(2238.35)	(0.50)	(0.50)
Mean Y	0.67	0.67	1585.52	1585.52	5063.69	5063.69	8.21	8.21
Observations	20185	20185	20185	20185	13650	13650	13650	13650
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 earnings data) and whose family did not report domestic violence. All regressions include controls for 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 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.

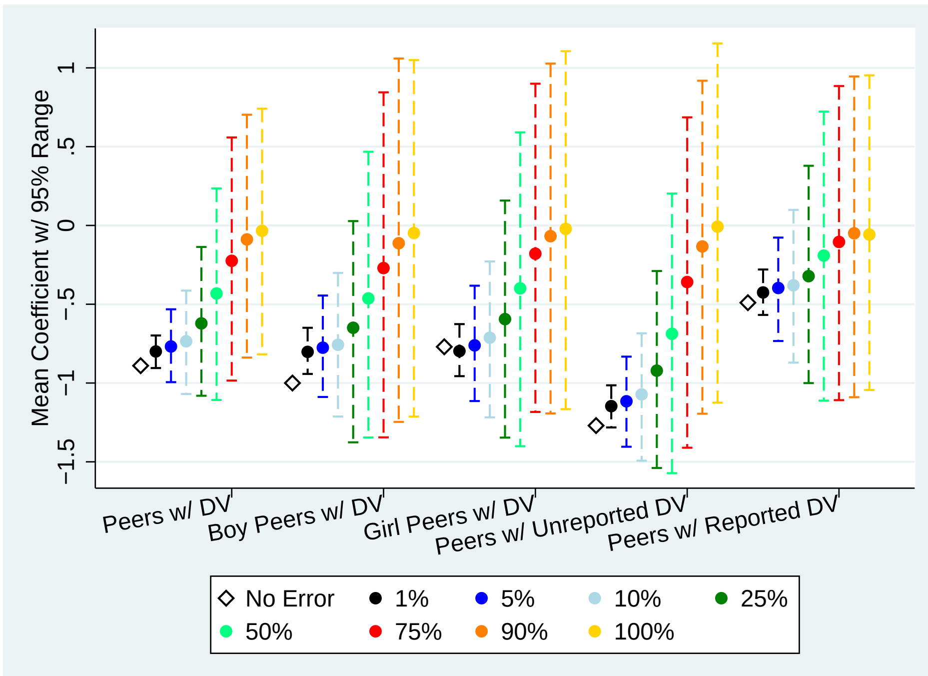
Table 6: Heterogeneity in the Long Term Effects of Disruptive Peers

	Gender		Income		Race	
	Male	Female	Low	High	White	Non-White
A: Test Scores in Grades 9-10						
Fraction Peers w/ DV	-0.28	-0.45*	-0.04	-0.86***	-0.31	-0.37
	(0.29)	(0.23)	(0.24)	(0.29)	(0.25)	(0.24)
Fraction Male Peers w/ DV	-0.66*	-0.42	-0.09	-1.23***	-0.33	-0.48
	(0.39)	(0.32)	(0.31)	(0.43)	(0.37)	(0.34)
Fraction Peers w/ Unreported DV	-0.49	-0.98***	-0.15	-1.65***	-0.55	-0.59
	(0.41)	(0.35)	(0.34)	(0.41)	(0.41)	(0.36)
B: Attainment of Any Degree						
Fraction Peers w/ DV	-0.19	-0.15	-0.06	-0.25	-0.02	-0.23*
	(0.15)	(0.17)	(0.10)	(0.23)	(0.20)	(0.12)
Fraction Male Peers w/ DV	-0.64***	-0.46*	-0.20	-0.72**	-0.33	-0.54***
	(0.22)	(0.24)	(0.14)	(0.32)	(0.26)	(0.17)
Fraction Peers w/ Unreported DV	-0.84***	-0.52**	-0.27*	-0.81***	-0.74***	-0.40**
	(0.20)	(0.26)	(0.15)	(0.29)	(0.25)	(0.18)
C: Likelihood of Positive Earnings						
Fraction Peers w/ DV	0.17	0.03	0.50***	-0.50**	0.07	0.12
	(0.23)	(0.20)	(0.18)	(0.23)	(0.22)	(0.20)
Fraction Male Peers w/ DV	-0.44	-0.18	0.09	-1.05***	-0.49	-0.20
	(0.29)	(0.29)	(0.24)	(0.39)	(0.30)	(0.28)
Fraction Peers w/ Unreported DV	-0.06	-0.21	0.35	-0.90***	0.09	-0.38
	(0.29)	(0.25)	(0.25)	(0.31)	(0.29)	(0.28)
D: Mean Earnings (Including Zeros)						
Fraction Peers w/ DV	550	-3003***	-368	-2286	-2952**	313
	(1573)	(892)	(633)	(2106)	(1472)	(799)
Fraction Male Peers w/ DV	-2969	-2125	-1233	-6780*	-5416**	-371
	(2354)	(1413)	(910)	(4035)	(2457)	(1113)
Fraction Peers w/ Unreported DV	-3121	-2797***	-579	-7383*	-5639**	-65
	(2733)	(1054)	(854)	(4145)	(2506)	(1189)
E: Log (Earnings)						
Fraction Peers w/ DV	-0.10	-1.49***	-1.31***	-0.09	-1.30***	-0.46
	(0.48)	(0.48)	(0.42)	(0.55)	(0.49)	(0.41)
Fraction Male Peers w/ DV	-0.58	-0.88	-1.23**	-0.15	-0.95	-0.91*
	(0.80)	(0.63)	(0.50)	(0.89)	(0.72)	(0.53)
Fraction Peers w/ Unreported DV	-0.83	-1.40**	-0.97*	-1.53**	-2.16***	-0.34
	(0.66)	(0.62)	(0.50)	(0.72)	(0.66)	(0.59)

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). We also restrict the sample to individuals whose family did not report domestic violence. All regressions include controls for individual and cohort level controls, as well as grade-year and school-grade fixed effects for grades third to fifth. Individual controls include 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

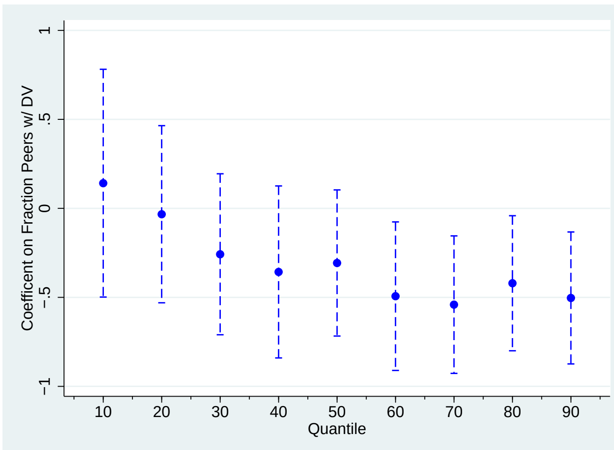
A Appendix

Figure A.1: Effects of Disruptive Peers on Log Wages – Sensitivity to Measurement Error in the Domestic Violence Variable

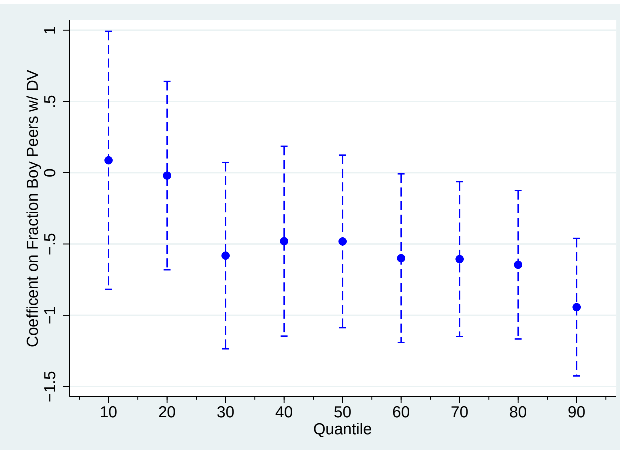


Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. Each scatter point represents the average estimated coefficient (and 95% range) obtained when introducing measurement error in the domestic violence variable in 1, 5, 10, 25, 50, 75, 90 and 100 percent of the sample. All regressions include controls for grade-year and school-grade fixed effects for grades third to fifth, as well as additional individual and cohort level controls. Individual controls include 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. In all regressions we restrict the sample to individuals whose family did not report domestic violence, and we weight by the inverse of the number of times a student is observed in the sample.

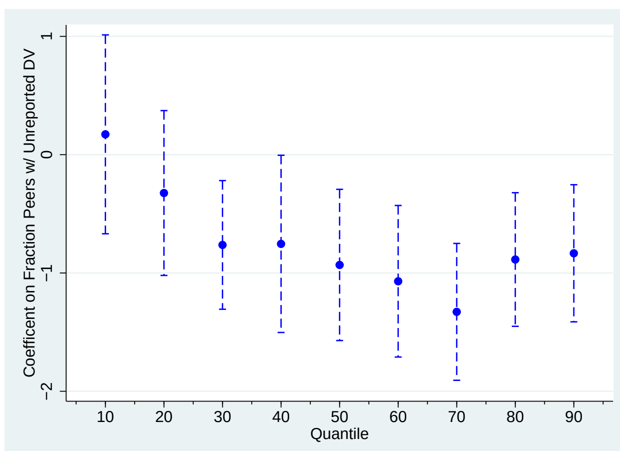
Figure A.2: Quantile Effects of Disruptive Peers on Test Scores – Grades 9, 10



(a) Fraction Peers w/ DV



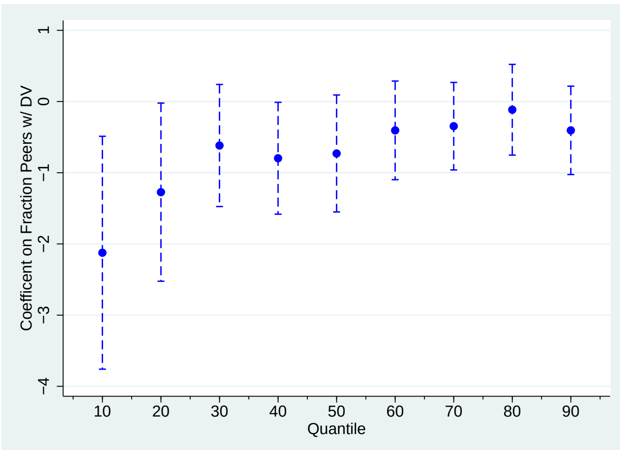
(b) Fraction Male Peers w/ DV



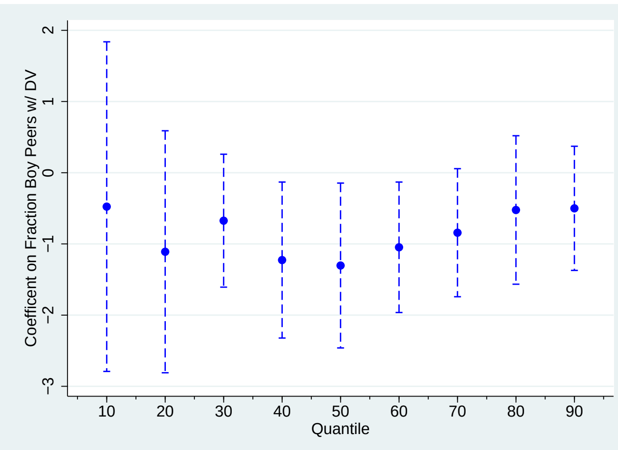
(c) Fraction Peers w/ Unreported DV

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. Each scatter point represents the estimated coefficients (and 95% confidence intervals) obtained from quantile regressions. We restrict the sample to individuals whose family did not report domestic violence. All regressions include controls for grade-year and school-grade fixed effects for grades third to fifth, as well as additional individual and cohort level controls. Individual controls include 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 are clustered at the school-cohort level.

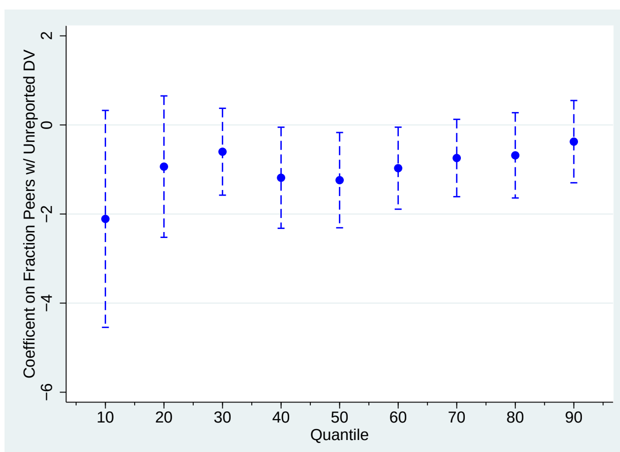
Figure A.3: Quantile Effects of Disruptive Peers on Log Quarterly Earnings - Ages 24-28



(a) Fraction Peers w/ DV



(b) Fraction Male Peers w/ DV



(c) Fraction Peers w/ Unreported DV

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data) and whose family did not report domestic violence. Each scatter point represents the estimated coefficients (and 95% confidence intervals) obtained from quantile regressions. All regressions include controls for grade-year and school-grade fixed effects for grades third to fifth, as well as additional individual and cohort level controls. Individual controls include 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 are clustered at the school-cohort level.

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	-0.11	-0.26	-0.66***	0.06	-0.31	-0.13	-0.37*	-0.45**
	(0.28)	(0.22)	(0.23)	(0.24)	(0.23)	(0.20)	(0.20)	(0.19)
Observations	28858	32471	28417	27389	31327	34613	33953	32740

Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include controls for individual controls, cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Individual controls include 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 Suspensions – Grades 9–12

	All Students				White Students			
	Total Days		Number		Total Days		Number	
A: Exposure to Peers with DV								
Fraction Peers w/ DV	4.34***	4.37***	0.41	0.35	3.77**	3.81**	0.76**	0.79**
	(1.25)	(1.27)	(0.34)	(0.33)	(1.87)	(1.81)	(0.34)	(0.32)
B: Exposure to Male and Female Peers with DV								
Fraction Male Peers w/ DV	1.67	1.70	0.65	0.46	0.43	1.13	0.67	0.75
	(1.80)	(1.78)	(0.49)	(0.47)	(2.25)	(2.23)	(0.47)	(0.46)
Fraction Female Peers w/ DV	7.07***	7.05***	0.16	0.24	6.96**	6.31**	0.85*	0.82*
	(1.99)	(1.93)	(0.43)	(0.43)	(2.90)	(2.73)	(0.47)	(0.45)
C: Exposure to Peers with Unreported or Reported DV								
Proportion Peers w/ Unreported DV	7.17***	7.44***	0.75	0.73	5.41**	5.71**	1.22***	1.28***
	(1.88)	(1.92)	(0.51)	(0.50)	(2.72)	(2.65)	(0.46)	(0.44)
Proportion Peers w/ Reported DV	1.93	1.70	0.12	0.02	2.37	2.19	0.37	0.37
	(1.77)	(1.75)	(0.44)	(0.42)	(2.49)	(2.46)	(0.47)	(0.46)
Mean Y	1.559	1.559	0.469	0.469	0.901	0.901	0.269	0.269
Observations	36334	36334	36334	36334	20012	20012	20012	20012
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

Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include controls for individual controls, cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Individual controls include 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.