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Essays in Empirical Microeconomics

A Dissertation submitted in partial satisfaction of the
Requirements for the degree Doctor of Philosophy

in

Economics

by

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The Dissertation of Yuan Emily Tang is approved, and it is acceptable in quality and form for publication on microfilm:

Chair

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ABSTRACT OF THE DISSERTATION

Essays in Empirical Microeconomics

by

Yuan Emily Tang

Doctor of Philosophy in Economics

University of California, San Diego, 2007

Professor Julian Betts, Chair

This dissertation is comprised of three papers using empirical methods to study issues in public and social economics.

My first paper, co-authored with Julian Betts, analyzes the performance of San Diego's charter schools using fixed-effect methods on panel student data. We find that charter school performance in San Diego varies by subject matter, grades served, school type and years of operation. In many cases, we find that charter school performance is indistinguishable from that of traditional public schools. Startup elementary charter schools perform poorly in math in early years, but catch up after year three, while conversion charter schools persistently underperform in both elementary math and reading, as well as in middle school reading. Checks for dynamic selection indicate that transitory performance dips preceding switches between school types do not strongly bias our estimates. Differences in performance do not seem to be due to school characteristics such as average class size and teacher experience. Analyses of differential impacts by student race and ethnicity suggest that charters may benefit some students more than others. Finally, an alternative test score measure indicates that charter schools at the

middle school level may focus less on state-developed content standards than traditional public schools.

My second paper investigates the relationships between measures of conflict and group composition and economic and social variables in US primary and secondary schools. Racial tension occurs most often when there is no majority group. More of it occurs when Asians or whites are the largest group than when blacks or Hispanics are the largest group. It is most prevalent in middle schools, and occurs more frequently in larger schools than smaller schools. When the race of the largest group is controlled for, racial tension increases with poverty, indicating there may be an economic component to racial tension. I find no strong evidence for any relationship between racial tension and between-group income disparities. I also find no evidence that recent changes in school racial composition are related to racial tension. Racial diversity in schools is associated with more racial tension, but not more violent activity or more gang activity.

My third paper analyzes a panel of United States areas to investigate the contention that rising income inequality may increase crime rates. I first replicate findings from previous research that a strong positive correlation between local crime rates and local household income inequality appears across specifications in cross-section ordinary least squares regressions. I then demonstrate that the positive relationship between inequality and crime does not survive, and in fact reverses in some cases once local fixed effects are controlled for. I discuss and examine the possible reasons for this statistical reversal. While rising income inequality may have some negative social consequences, I find no strong evidence that it causes increased crime, at least in the short-term.

Chapter 1

Student Achievement in Charter Schools in San Diego

Abstract

We analyze the performance of San Diego's charter schools using fixed-effect methods on panel student data. We find that charter school performance in San Diego varies by subject matter, grades served, school type and years of operation. In many cases, we find that charter school performance is indistinguishable from that of traditional public schools. Startup elementary charter schools perform poorly in math in early years, but catch up after year three, while conversion charter schools persistently underperform in both elementary math and reading, as well as in middle school reading. Checks for dynamic selection indicate that transitory performance dips preceding switches between school types do not strongly bias our estimates. Differences in performance do not seem to be due to school characteristics such as average class size and teacher experience. Analyses of differential impacts by student race and ethnicity suggest that charters may benefit some students more than others. Finally, an alternative test score measure indicates that charter schools at the middle school level may focus less on state-developed content standards than traditional public schools.

Introduction

Charter schools are an important component of a recent movement in education reform emphasizing choice and competition, in which the underlying principle is that if parents and students are given opportunities to leave failing schools, all schools will be forced to improve in order to attract and retain students. Charter schools are publicly funded but free from many of the regulations concerning curriculum and staffing governing traditional public schools. They must abide by their charter agreements with their chartering authority, usually a school district or state education agency. Given their recent entry and growing roles in public school systems, their performance is of natural interest.

Much of previous research analyzing charter school performance is limited by the lack of longitudinally-linked student-level data. As discussed in Betts and Hill (2006) comparisons of average test scores in charter and non-charter schools do not provide useful information on how well charter schools are serving students because students attending charter schools may be very different from those attending regular public schools. Research accounting for the unobserved differences between charter students and non-charter students is critical for learning whether charter schools are successfully serving students. Given the localized nature of public education in the US, it is also useful to study the performance of charter schools in different settings. This paper documents the recent experience of charter schools in the San Diego Unified School District (SDUSD).

The focus of this paper is on quantifying the benefits of charter schools, measured by gains in standardized achievement test scores for students attending charter schools.

To control for unobserved differences in student ability or motivation between charter and non-charter students, we use a student-fixed effect methodology exploiting student switches into and out of charter schools to identify the charter school effect. While we briefly discuss the typical funding of charter schools and the resulting financial challenges facing charter school administrators, we do not have the data necessary to perform a comprehensive cost-benefit analysis.

In line with previous research, we find charter school performance to be mixed. Startup charter schools, which are entirely new, are indistinguishable from regular public schools except in the case of elementary school math and reading where they underperform. This difference appears to be transitory, disappearing in schools that have operated for four or more years. This result follows closely research in Florida (Sass 2006) and Texas (Hanushek, Rivkin, and Kain 2006) which finds using similar methodologies that charter school performance is not significantly different from that in regular schools after an initial startup period. Bifulco and Ladd (2006) however find that charter schools in North Carolina underperform even after the initial startup period. We find persistent charter underperformance in San Diego as well, but only for conversion charter schools which are regular public schools that have converted to charter school status. In our sample, conversion charter schools underperform in elementary school math and reading, as well as in middle school reading. In addition to these baseline findings that charter school performance varies according to subject, gradespan, charter school type and age, we show that charter school performance varies according to the type of test as well as by student race.

The paper is organized as follows. We first offer background on charter schools in San Diego, describing the characteristics of the students at charter schools compared to those at regular public schools. We also summarize what we learned from administrative data on staffing at charter schools compared to traditional schools, and a survey of charter school principals we distributed in June 2004. Next, we describe the data and methodology for our analysis of charter school performance. We present the baseline results and extensions to these results. Finally, we discuss the robustness of our findings and suggest directions for future research.

Background on Charter Schools in San Diego

As shown in Figure 1, charter schools have increased their share of overall SDUSD enrollment steadily since 1997. An important distinction is between conversion and startup charter schools. Conversion charter schools are former regular public schools that change their relationship to the district, often retaining teachers and serving the same student population on the same school site as before their conversion, but no longer bound by district regulations concerning curriculum, staffing, budget, and other operating policies. Startup charter schools on the other hand are entirely new. These schools secure facilities often unaffiliated with the district and independently recruit new classes of students and teachers when they open.

Federal No Child Left Behind legislation allows schools to convert to charter status as one way to comply with its mandate that schools restructure after six consecutive years of not making “Adequate Yearly Progress” according to state standards. Many schools have in fact begun to do this. In San Diego, autumn 2005 witnessed the re-opening of three large district schools as conversion charter schools, after they had been identified as "failing" for six years. Because No Child Left Behind creates a new pressure on districts nationwide to create conversion charters out of existing public schools, and because these schools may well be very different from startup charter schools, we emphasize this distinction in our analysis.

Most of the recent growth in San Diego's charter enrollment has been in startup schools. Figure 1.1 shows that while conversion school enrollment has been relatively constant over time, the percentage of charter school students who attend startups has risen markedly. This trend changes with the recent conversion of several schools discussed above, and may change further with the installment of a new district superintendent and changes in school board membership in 2005.

We begin with a comparison of students enrolled in charters and regular public schools in San Diego. A concern often voiced by opponents is that charter schools may "cherry-pick" students, choosing to admit only students predicted to do well. Opponents worry that this not only diverts financial resources from traditional public schools, but also removes from these schools their strongest students who may offer valuable peer effects. A survey we distributed to charter school administrators in San Diego in June of 2004 revealed that many charter schools use a random lottery to determine admission whenever a school has more applicants than available slots.[1] Over half of the schools target certain student populations. In most of these cases the targeted groups are residents of traditionally underserved neighborhoods, and disadvantaged, at-risk, or limited English-proficient students.

[1] Preference may be given for siblings of currently enrolled students, and in the case of conversion charter schools students living in the former attendance area of the school before the conversion.

Table 1.1 illustrates the differences between San Diego's charter and non-charter students in terms of race and eligibility for meal assistance. Hispanic and black students constitute disproportionately more of the charter sector than they do of the district as a whole, while white and Asian students constitute disproportionately less. The over-representation of Hispanic students in the charter sector is due to the conversion schools. Hispanic students actually make up a slightly smaller share of all startup charter schools than regular public schools as a whole. In contrast, black students appear to be disproportionately attending startup charter schools. Black students' share of conversion schools is similar to their share in regular public schools. The table also shows that students in charter schools tend to be economically disadvantaged relative to their regular public school counterparts according to measures of free or reduced-price meal eligibility. This result is also driven by conversion schools, where nearly three-quarters of students qualify for free meals. At startup schools, a slightly smaller percentage of students qualify for subsidized meals than in the district as a whole.

Another way to look at charters is through test scores. The state Department of Education annually calculates the Academic Performance Index (API) for every school in the state. The API is an index of test scores weighted towards low-scoring racial/ethnic and socioeconomic groups. Table 1.2 shows average Academic Performance Index (API) scores for regular schools and charter schools by year. Charters have tended to lag behind but do catch up significantly over time. This convergence may be related to the large growth in startup charter schools, which tend to have higher API scores than conversion charters, and by 2005 even higher API scores on average than regular public schools. The bottom lines in the table show the number of schools of each type in each year contributing to the averages displayed in the upper part of the table.[2] Average test scores tell us little about the relative quality of charter and regular schools, because students' initial academic achievement before coming to charter schools may be higher or lower than the district-wide average due to family inputs or previous school quality. The API scores only provide cursory snapshots of average student performance that may be unrelated to school influence.

These averages also tell us little about whether charter schools are selecting students, or differentially attracting students of higher or lower ability or motivation. Though a definitive answer to this question is beyond the scope of this paper, we present suggestive evidence that charter schools in San Diego do not appear to be consistently "skimming" or differentially attracting students of higher ability. Table 1.3 shows the average percentile rankings of students among all of the grade level peers at his or her school, in the previous and current years. A number below 50 indicates the average student in that category scored below the grade-year median at his former or current school. These numbers are presented separately for the sample of students who switched into conversion and startup charter schools, those who switched into a traditional public school from another public school, and students who did not switch schools.

None of these numbers is significantly different from 50, suggesting that students switching to charter schools are not likely to have either significantly higher or lower test scores than their peers at the schools they leave. However we do note a few patterns. Students switching to conversion schools tend to come from a lower part of their grade-

[2] This is not the entire sample of district schools. A small number of schools do not have APIs in some years due to insufficient data or testing irregularities.

school-year distribution than students switching to startup schools. In addition, in most of these cases both of these groups tend to be lower scoring than their peers who choose not to switch schools. Only in the case of middle school startups does it seem that students switching in are higher scoring than their classmates at the schools they leave, and even here the average percentile ranking of switchers is not significantly different from 50.

We also look at schools individually to see whether particular schools seem to consistently draw students from either end of the test score distribution. We do find in several cases that schools appear to be drawing students from high ends of the test score distribution of the schools that they switched out of, that is, the students entering the school are the high scoring among their peers at the schools they left. However, in none of these cases can we conclude that the average percentile rankings are significantly different from 50, though they were as high as 75 in a few cases.

Overall in San Diego, it appears that conversion charter schools serve economically disadvantaged students. More of the students at these schools are eligible for meal assistance, and the average test scores are lower than in traditional public schools. In terms of both racial mix and test scores, startup charter schools appear to be more similar than conversion schools to average traditional public schools, though they serve somewhat disproportionately more black students and fewer Asian students than the traditional public sector, and have somewhat fewer students eligible for meal assistance. Charter schools as a whole do not appear to be consistently attracting above- or below-average students, though individual charter schools may be.

Data and Methodology

Our analysis relies on administrative panel data covering the universe of students in SDUSD beginning in the 1997-98 school year. Students in grades 2 through 11 were required to take the Stanford 9 Achievement Test (SAT9) between the 1997-98 and 2001-02 school years.[3] The SAT9 test is vertically-scaled, meaning scores across years can be compared and differences between years represent gains in student achievement or learning. In contrast, the California Standards Test (CST), which was required of students in grades 2 through 11 beginning in the 2001-02 school year, is criterion-referenced. This means that the underlying content of the exam is determined by state standards according to grade level. Changes in score across years therefore do not necessarily capture student learning from one year to the next, because the score in each year instead reflects student grasp of the particular content covered on that test, different from one grade to the next. We perform some analysis using this test by standardizing scores within a grade-year to a mean of zero and a standard deviation of one so that test score differences from one grade-year to the next measure a student's change in relative standing within a student cohort.

The data allow for the tracking of students over time, and our analysis compares the gains in achievement of individual students in years they attend charters to their gains in years they do not attend charters. Our baseline results come from estimating:

(1) $$\Delta y_{itg} = bCharter_{itg} + m_i + t_t + g_g + e_{itg}$$

[3] The test was replaced after 2001-02 by a similar exam, the California Assessment Test (CAT6). The scores on these two exams cannot be compared to each other in a straightforward manner so we focus our analysis on the SAT9 scores for which we have more years of data.

where *i* indexes student, *g* grade, and *t* school year. Δy_{itg} , or y_{itg}-$y_{it-1g-1}$, represents the test score gain over the last year, $Charter_{itg}$ is a binary variable indicating whether the student is enrolled in a charter school, m_i is a student fixed effect, t_t is a year fixed effect, and g_g is a grade fixed effect. b, the coefficient of interest, is identified by student switches into and out of charter schools. This specification assumes that there is an individual specific component in year-to-year achievement gains. We use the mathematics and reading scores, and estimate the equation separately according to elementary, middle, and high school grade levels to allow for different charter school effects according to gradespan.[4]

By including student fixed effects we are able to control for unobserved characteristics of students that do not vary over the course of our data collection. These controls are necessary because there are likely some unobserved differences between students that are correlated with the decision to attend a charter school, biasing OLS estimates of the effect of attending a charter school. While this methodology accounts for selection into charter school across students, it does not account for the possibility that students switch to charter schools after an unusually bad year academically, or, conversely, that students switch out of charters after an unusually bad year. We discuss this in more length in the robustness section.

We also consider an alternative specification modeling the level of a student's test score as a function of previous year test score and the same set of additional regressors.

$$y_{itg} = ay_{it-1g-1} + bCharter_{itg} + m_i + t_t + g_g + e_{itg} \tag{2}$$

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[4] Elementary level testing grades are 2-5, middle are 6-8, and high school are 9-11.

This model is more appropriate under the assumption that the achievement in a given year is also influenced by the level of achievement in the prior year, which may be the case if there is regression to the mean or a test score ceiling.[5] Because adding a lagged dependent variable (namely, lagged test scores) to a student fixed effect model can lead to bias and inconsistency, we use the method of Anderson and Hsiao (1982) to estimate these latter models. The Anderson-Hsiao approach involves first-differencing the model and then using a twice lagged test score as an instrumental variable for the first-differenced lagged test score. We report results for both specifications and typically the results are similar.

Baseline Results

Table 1.4 reports the baseline results. Switching to a charter school results in lower elementary math and reading gains, and lower middle school reading gains. The sizes of these effects are moderate, on the order of 6-18% of a standard deviation in score for that gradespan and year. Charters may also boost high school reading test score gains and cause smaller math gains, but these results are not robust to the specification chosen. The coefficient on the charter school indicator is significant in the student fixed effects estimation but insignificant in the Anderson-Hsiao model. Similarly, charters appear to detrimentally affect high school math gains in the Anderson-Hsiao specification, but not in the student fixed effect model. Appendix Table 1.1 shows summary statistics for test scores and test score gains in the sample. Table 1.4 also reports the regression estimates from the second model, which adds the lagged score as

[5] Equation (2) can be written as $\Delta y_{itg} = (a-1)y_{it-1g-1} + bCharter_{itg} + m_i + t_t + g_g + e_{itg}$, which illustrates that this is a generalization of the simple gains model in (1).

an explanatory variable. Next to each estimate is the number of students included in each regression and the number of these who attend a charter. We count only the number of students with at least two gains since only these students will contribute to our estimates. In our main specification, the number of students entering our regressions ranges from about 26,000 students in the high school model to about 34,000 students in the elementary school model. Of these samples, the number of students who ever attended a charter school ranged from about 1,500 for the elementary school models up to about 5,800 for the middle school models. This suggests that although we have reasonably large numbers of charter school students, the elementary school sample will be the one least likely to reveal small (positive or negative) effects of charter schools on achievement, because of the relatively small sample size.[6]

There is a second reason we need to be cautious about sample size. To contribute to our estimate of the effect of attending a charter school, a student must attend a charter for at least one year and a regular public school for at least one year within a gradespan, so that we can compare his or her test-score gains in the two types of schools. Most of our charter enrollees attend charter schools and stay in charter schools throughout the gradespan. This was most prevalent in our middle school sample, where 90 percent of charter attendees are in charters throughout the sample period. In the elementary and high school samples, the percentages are 85 percent, and 73 percent, respectively. These students did not contribute to our estimated effect of attending a charter school. This may

[6] The sample size in the specification including lagged test score decreases because the Anderson-Hsiao method first differences the equation and then instruments for the lagged difference using the double-lagged test score, requiring one additional year of test score data from each student.

be of special concern at the elementary school level, because there are fewer charter students overall than at the middle and high school levels.

Put differently, the fixed-effect method should give a quite accurate estimate of the effect of attending a charter for switchers, but we cannot say for certain whether the same effect applies to students who, for instance, enroll in a charter in kindergarten and stay in charter schools throughout the sample period. A second implication for interpretation of our results is that when we find a “zero” effect of attending a charter school, we are more confident that the effect is truly zero in the middle and high school samples than in the smaller elementary samples.

In Table 1.5 we separately present the effects of switching to startup charters and conversion charters to allow for performance differences between the types of schools. The table reveals that at the elementary level, both startups and conversions produce significantly lower test score gains in math relative to regular public schools. Both startups and conversion elementary schools also produce smaller gains in reading, but only the negative effect of conversions is statistically significant. At the middle school level, it is the conversion schools driving the overall result that charters are significantly worse at teaching reading.[7] Startup schools at the middle school level are statistically indistinguishable from traditional public middle schools in both math and reading. We do not break down high school results by charter type, because all of the charter high schools in San Diego are startup schools.

Given that we did find a few cases in which charter schools appeared to differ significantly from regular public schools in terms of gains in reading or math, it is natural to ask whether we can explain these differences in terms of observable characteristics of the classrooms. Accordingly, for the subsamples for which we had class size and detailed teacher qualifications (credential status, highest degree and years of teaching experience), we re-ran our basic models and obtained very similar results on these subsamples. Next we re-estimated these models after adding class size and controls for teacher qualifications at the grade-school level.[8] The intuition here is that if the simpler model showed that charter schools were more effective, after we control for class size and teacher traits, the size of the charter school coefficient should fall toward zero, as we have (perhaps) explained why the charter school was more effective. Conversely, in cases of a negative charter school effect, after controlling for class size and teacher qualifications we should expect to see the charter school coefficient rise towards zero. In fact, adding these controls did not change the size of the charter coefficient markedly and in almost all cases the controls moved the charter dummy away from zero. In other words, the class size and teacher controls do not appear to explain any of the gaps in effectiveness between charters and regular public schools. Whatever explains the differences, it has to do with unobservable factors that are not related to class size or teacher qualifications.

Extensions

We next turn towards examining whether San Diego's charter schools face startup problems and under-perform in their first few years of operation as found in some studies of charter schools in other areas. To do this, we re-run the basic student fixed-effect

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[7] Two large conversion schools dominate the middle school charter school sample.

analysis, this time adding additional control variables indicating whether a school is in its first, second, or third year of operation.[9] Conversion schools are only observed four years or later after their conversion, so we are not able to test if conversions have difficulties in their first three years. Thus, our tests for startup problems quite literally apply only to charter schools that have started from "scratch."

Our central finding changes the flavor of our earlier results on the relative performance of startups and regular public schools. Recall that in our main specification in column 1 of Table 1.5, startup schools performed the same as regular public schools except that they under-performed in math at the elementary level. As shown in Table 1.6, by year four and higher, startup charters show gains in reading and math that are statistically indistinguishable from regular public schools. It is only in their initial years that elementary startup charters produce much lower test score gains than regular public schools in both reading and math. Table 1.6 shows that the largest effects are found in math scores, where attending an elementary charter school in its first year of operation results in a test score gain that is almost an entire standard deviation below the average test score gain at a non-charter school. In reading, the first year startup effect is still significant but smaller, around forty percent of a standard deviation below the reading gain at a traditional school. Reading gains in elementary charters are statistically indistinguishable from reading gains in traditional schools after the first year of a startup, while math gains suffer until a school has been operating for four or more years.

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[8] We match students to the within-grade average teacher and classroom characteristics at their schools because data limitations do not allow us to match individual students to classrooms and teachers in some charter schools.

At the middle school level, there is also a negative startup effect in math gains. Students in new middle schools make math gains just under half of a standard deviation below their gain at traditional schools. There do not appear to be any startup effects on reading gains at the middle school level. At the high school level, the startup effects are less straightforward, with math gains in startups not significantly different from regular schools until year three in which they are significantly lower. The gains rebound and are indistinguishable from those in regular schools after year three. The reading picture at the high school level is even more mixed, with startup effects not appearing until year two, and completely turning around in year three. Startup high school charters in their third year significantly outperform regular schools, and then settle down in year four or later with gains indistinguishable from regular schools.

We conclude that startup charter schools in the elementary gradespan have often experienced teething pains in their first one to three years of operation, but after this point perform at the same level as their regular school counterparts. Startup charters at the middle and high school levels also experience some startup effects, but to a less pronounced extent than elementary schools. In contrast, the earlier results we presented in Table 1.5 for conversion charters apply to schools that had converted from regular into charter schools more than three years earlier, so in three cases - elementary math and reading, and middle school reading - they under-perform regular public schools well into their histories. Conversion charter schools outperform in middle school math but this difference is not statistically significant.

Previous researchers have noted that students often appear to need time to fully adjust to a new school environment, having lower test score gains in their first year at a new school. (See e.g. Hanushek, Rivkin, and Kain 2005.) We check whether these effects are apparent in our data, and if so, whether they are more or less pronounced at charter schools than at traditional schools. Table 1.7 summarizes the results of these analyses, which show that students switching to new schools do sometimes have lower test score gains. (The comparison group here is students who have been in the given type of school for four years.)

In our sample, students switching to traditional schools face difficulties in the first year at a new school only in middle school math and middle school reading. The negative effect for reading only exists in the first year, while in math middle school students continue to face difficulties in the second year at a new school. Students switching to charters face difficulties in the first year after a switch in middle school reading as well, but in contrast do not have adjustment problems in middle school math. Charter school switchers do however have especially poor math gains in their first year after a switch in elementary school. At the high school level, students do not appear to experience adjustment problems expressed in test score gains switching to either charter or traditional schools.

The result that charter students face difficulties in their first year at a new school in elementary math and middle school reading echoes the overall results presented in Table 1.4. With these two exceptions, students who switch into charter schools do not seem to experience transitory declines in achievement. When school type is controlled for with an additional conversion variable, the middle school reading effect switching actually reverses so that a student in the second year after switching to a charter school actually has reading gains larger than they would have in a traditional school.

Notably, after controlling for student switching behavior the overall charter indicator is not significant in any case. The negative overall effects in elementary school math and elementary school presented in Table 1.5 disappear, suggesting that charter school performance is comparable to traditional school performance for students enrolled in the same school for three continuous years. These results together imply that it may be the move across schools itself that drives the overall negative results for charter schools in elementary school math, and for conversion schools in elementary school math and reading. Students in their second year and beyond in elementary charter schools do as well as students in traditional schools. However, the overall negative result for conversion schools in middle school reading persists even after the controls for switching behavior, indicating that middle school conversation schools are indeed truly underperforming in reading, even after accounting for school switching adjustment costs.

We have shown that there appear to be different charter school effects according to subject, grade level, charter school type, and charter year of operation. It may also be the case that granting charter schools more flexibility results in the implementation of vastly different policies between schools that generate large variations in performance among charter schools. In other words, quite heterogeneous effects of charter schools may underlie the overall average effect of entering a charter school. We check whether some charter schools are systematically improving student scores at greater or lesser rates than others by testing for the significance of separate charter school fixed effects for each school, after dropping the single dummy variable for charter schools. In most cases, we do not find these individual charter school effects to be significantly different from zero. In part this is because these components can only be identified by the numbers of students switching into and out of each school within each gradespan and in some cases these are quite small. The point estimates of these effects do vary, indicating that perhaps more observations would deliver smaller standard errors and higher statistical significance.

Although F-tests reject the equivalence of the individual charter school effects in elementary math, middle school math, and middle school reading, we cannot reject the hypothesis that individual charter schools are performing about the same as each other in elementary reading, high school math, and high school reading.

Figure 1.2 displays the point estimates and 95% confidence intervals for each gradespan and subject. We showed earlier that charter schools underperform public schools in elementary math and middle school reading, and outperform public schools in middle school math. Figure 1.2 illustrates that these overall results occasionally mask differences among schools. Although the negative overall elementary math and positive overall middle school math results appear to be roughly mirrored by the sign on many of the individual school effects, the case of middle school reading is quite different. In looking at the results by school, it appears that the overall negative result is driven by two large schools. In eight cases out of the thirteen, the point estimate of the effect of attending a charter school on middle school reading is actually positive, though in no case is it significantly different from zero. It appears that in this case important heterogeneity in charter school performance underlies the average effect.

Just as there may be no such thing as the completely “typical” charter school, there may be no such thing as a “typical” charter school student. Which types of students gain the most and least from the experience offered by San Diego’s charter schools? As shown above, several schools target at-risk or disadvantaged students, and several more offer other special programs tailored to their students. Given this ability to tailor curricula for their student populations, it is natural to ask whether some students benefit disproportionately more or less from attending charter schools. To answer this question, we investigate differential effects by student race. By re-running our basic specifications separately for each major racial subgroup, we can then see whether the estimated effects of charter schools differ by race. Again, because we rely on students who switch back and forth between charters and regular public schools, our sample size dictates some caution.

Table 1.8 and 9 show these charter school effects by race and ethnicity. In Table 1.8 we test for overall differences between charters and traditional public schools, without distinguishing between startup and conversion charters. There we find six combinations of race and gradespan for which charters produce different gains in math, and four cases of significant charter effects for reading. The largest of these effects is in elementary school math where it appears that Hispanic students have over one-third of a standard deviation smaller test score gains when switching to charter schools. Asian students in elementary schools also have significantly smaller test score gains in both math and reading in charter schools. Table 1.9 distinguishes between charter types and shows that the overall negative math results for Hispanic students are driven by lower gains in startup schools, while for Asian students the results are found primarily in conversion schools. There are no groups that benefit significantly in math learning overall from attending an elementary charter school; these results may drive the overall finding that elementary charter schools are not doing well in math.

At the middle school level, Hispanic and white students have smaller reading gains, reflecting the overall results. Asian students have lower math gains. When looking at the effects according to school type, we see that the smaller reading gains for both Hispanic and white students are driven by the conversion schools; in startups schools these students also do somewhat worse but the coefficients are small and not statistically significant. The overall negative effect for Asians in middle school math is apparent in both startup and conversion schools.

Black middle school students, like white and Hispanic students, do worse in reading in conversion schools, but have higher reading gains in startups, so that their overall performance in middle school charters is statistically indistinguishable from their performance in traditional public schools. The overall math middle school result that Hispanic students in charter schools are performing about the same also masks a difference between these students' performance in startup schools, where they do not do well, and conversion schools, where they do do well. These effects are all smaller than at the elementary school level and are about on the order of the overall results.

At the high school level, the results are also mixed. White and Asian students do relatively poorly in math at the high school level, mirroring the overall results, while Hispanic students actually do somewhat better. In contrast to how they do at the middle school level, white charter students do better in reading at the high school level. Again, since there are no conversion high schools we do not break the high school results down further.

The conclusion from breaking down the overall results by student race and ethnicity is that no students of any race/ethnicity do better in elementary charter schools.

The negative coefficients however are not significant for all subject/gradespan/race or ethnic combinations. Hispanic and Asian students are the two groups that are most negatively affected in elementary charter school math, and Asian students also suffer in reading in elementary schools. Asian students in fact appear to do worse in charter schools in all gradespans/subject combinations (though the negative effect is small in magnitude and not significant in middle and high school reading). Hispanic and white students appear to be most negatively affected by charters in middle school reading. However, Hispanic students do relatively well in math at conversion middle schools and high school charters, while black students do well in reading in startup middle schools. White students also benefit from charters in reading in high school. It is important to note that the identifying samples in these regressions are sometimes small in size because only students switching school types within a gradespan contribute to identification of the charter school effect in our fixed effects specification.

Robustness

Our estimation strategy controls for the time-unvarying unobserved differences between students that may affect the decision to attend a charter school, but there could also be other time-specific unobserved factors that cause particular students to switch to a charter school in a given year. In one possible scenario, parents of a student suffering from unusually poor performance in a given year may feel compelled to act by transferring their student to a new charter school in the next year. When the negative performance shock is transitory an unusually low score in the previous year results in a larger than usual gain in the next year. In this case, our estimate of the charter school effect would be biased upward by the correlation between the current year's unusually high gain due to recovery and the charter school entry decision in the current year, a problem equivalent to Ashenfelter's Dip in the job training program evaluation literature (Ashenfelter 1978).

To check whether this behavior is a problem in our data, we regress the current year change in score on the decision to switch from a regular public school into a charter school in the next period, a variable indicating current attendance in a charter school, the student fixed effect, and grade and year fixed effects:

$$\Delta y_{itg} = h\ IntoCharter_{it+1g+1} + bCharter_{itg} + m_i + t_t + g_g + e_{itg}$$

A negative coefficient on the switch into charter school in the next period variable would suggest that an unusually low change in score in the current year is correlated with the decision to switch to a charter school in the next year, after controlling for differences in performance between charter and non-charter schools and fixed unobserved student characteristics. Appendix Table 1.2 shows these coefficients and illustrates that transitory dips in performance precede switches into charter schools in two cases, elementary math and high school reading. Conversely, the coefficient is actually positive, though not significant in elementary reading, middle school reading, and high school math.

Breaking the results down by charter school type shows that in the case of startups, in all cases but middle school math students have unusually low score growth in the years directly preceding a switch out of a regular public school into a charter school. Students have unusually high middle school math score growth in the years preceding switches into startup charter schools. Selection into conversion schools appears to be different from startup schools and to vary according to subject area. For conversion schools, the case of math is similar to the majority of the cases for startups - unusually low scores appear to precede entry into the charter school. However, for reading it is the opposite. Reading scores tend to be unusually high in the year immediately before a student switches to a conversion charter school. It seems that students do not typically have unusually strong or weak years simultaneously in both subjects - an unusually low math performance may be accompanied by an unusually high reading performance. Since the dips preceding school switches are not consistent across subjects and gradespans, we conclude that transitory performance dips preceding switches between school types do not strongly bias our estimates.[10]

Another concern about our methodology is that while we obtain quite reliable estimates of the charter school effect for the group of students observed in both charter schools and non-charter schools, we do not know whether the effect would be the same for the group of students never or always enrolled in charters throughout a gradespan. To address this concern, we test for observable differences between students only sometimes enrolled in charters and students always enrolled in charters within a gradespan, as well as between students sometimes enrolled and students never enrolled in charters within a gradespan. While this does not tell us exactly how the estimated effect for the identifying sample would be different to the effect for other students if we were able to estimate the latter, it offers a picture of some ways in which the groups of students are different.

Appendix Table 1.3a and 3b present the results of differences between always charter and sometimes charter students, and never charter and sometimes charter students

[10] We also check in the same manner for performance dips preceding charter school exits. If transient negative shocks induce charter school students to switch out of a charter school into a regular school the

respectively. Overall, we find only few significant differences among students who switch between school types and students who do not. At the elementary and middle school levels, students always in charters in comparison to those only sometimes in charters are more likely to be black. Students always enrolled in middle school charters are also more likely to be Asian and classified special education compared to students only sometimes enrolled in charters. At the high school level, students always in charters are more likely to be English learners compared to those only sometimes enrolled in charters. The largest differences between students always in charters and students sometimes in charters is that at all levels, students with parents with less than a high school education are more likely to be always enrolled in charters than sometimes enrolled in charters, though this is not statistically significant at the middle school level. The differences here are quite large.

We also test for differences between students sometimes enrolled in charters and students never enrolled in charters. Students never in charters at the elementary and middle school levels are less likely to be black, more likely to be Asian, less likely to be English learners, and more likely to be classified special education than students sometimes enrolled in charters. There are again differences in parent education. Students with parents without high school diplomas are more likely to be never enrolled in charter schools than to be sometimes enrolled in charters but the differences are quite small. Overall there are fewer observable differences between the sample of students always enrolled in charters and the sample of students sometimes enrolled than

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following year, our baseline charter school estimates would be biased downwards. We do not find a significant correlation between performance and charter school exit in any subject/gradespan.

differences between students never enrolled and sometimes enrolled. This fact is somewhat reassuring because it suggests that our measured charter effects on the sometimes enrolled students may not be too different for the effects for the students always enrolled in charter schools that we are unable to estimate. The biggest exception is that students whose parents did not graduate from high school are much more likely to be always enrolled in charters than only sometimes enrolled in charters relative to other students, so our estimates may not be representative for children of high school dropouts. It also suggests some caution in extrapolating our estimates to the students never enrolled in charters whom are more observably different; charters may affect this group of students differently since they are more different on observable characteristics.

Finally, we examine performance according to an alternative achievement measure. All of the previous analyses focused on student performance as measured by the norm-referenced standardized Stanford 9 test, which California used as a state test from spring 1998 through spring 2002. However, we also have available a criterion-referenced test, designed to measure whether schools are meeting content standards developed by the California Department of Education, known as the California Standards Test (CST). We have CST data from spring 2002 through spring 2004. Because the test is not vertically-scaled and therefore scores between years are not easily used to construct measures of student achievement gains, we normalize the test scores so that in each grade and in each year the average score is 0 and the standard deviation is 1. Changes between years in these measures therefore capture students' change in *relative* standing.

The results in this section differ qualitatively somewhat from the results using Stanford 9 test scores. In part this might reflect a different time period (Stanford 9 data are available from 1998 through 2002). In the latter time period, the number of charter schools was 23 while in the earlier time period, the total number of charters in the sample was 17. Also, in part it might reflect the fact that with a smaller number of years of CST availability, we lack enough observations to detect meaningful effects of charter schools. In the middle and high school gradespans, we have significantly fewer charter school-student observations in the CST models than we do in the Stanford 9 models.

With these qualifications in mind, we see in Table 1.10 that according to the criterion-based measure, charter schools appear to perform better than regular public schools in math at the elementary level. This is contrary to the SAT9 results in which elementary schools underperform in math, and could be due to the fact that the CST data, which were gathered later in time, reflect the improvements we have already documented with startups as they gain experience. As can be seen in Table 1.11, the positive math result is driven by startup schools.

As measured by the CST, charters under-perform in both middle school math and middle school reading. While the negative reading result echoes that of the SAT9, the math underperformance is contrary to the SAT9 results which demonstrate a positive effect of charter schools. Table 1.11 shows that the negative overall middle school math effect is due to the large negative effects in the startup schools. Conversion middle schools actually generate math test score gains that are indistinguishable from traditional public schools. Both startup and conversion charter schools appear to face challenges in teaching middle school reading. The coefficients can be read directly as changes in proportions of a standard deviation, and they do not appear to be very large. The exception is the case of startups for middle school math, where charters produce slightly more than one-quarter of a standard deviation smaller gains. The negative charter school effects at the middle school level on this criterion-based test may suggest that on average charter schools may be focusing somewhat less on state-developed content standards than are regular public charter schools.

Conclusion

Does attending a charter school affect test scores in reading and math? We find that charter schools in San Diego appear on the whole to be performing about as well as regular public schools, with some important exceptions. Startups appear to perform equally as well as regular schools in both math and reading, at all gradespans, by year four of operation and higher, but in some cases under-perform considerably in their first few years. Conversion charters, all of which were in their fourth or higher year of operation, under-performed regular schools in two cases - elementary math and middle school reading.

Combining the startups with the conversion schools to obtain overall average effects of charters, we find that students at elementary charter schools have lower math and reading test score gains than in regular elementary schools. The math results are driven by both startup schools in their first three years of operation, and conversion schools. The difference in elementary reading gains also reflects conversions as well as startups, but startups only in the first year of operation. Reading gains in middle school suffer when students attend conversion charters. At the high school level, our results differ based on model specification. Charters outperform in reading, and are statistically indistinguishable from traditional schools in math in the main fixed effects specification, while in the Anderson-Hsiao specification charters underperform in math, and are statistically indistinguishable from traditional schools in reading. From those results, we conclude that there are not significant performance differences between charter and traditional schools at the high school level.[11]

Just as our estimates of the average effects of attending a charter school appear to mask considerably heterogeneity among charter schools, students may also vary in their response to switching to a charter school. In this study we have taken a first step towards testing for variations in the effect of charter schools among students, based on students' race. We found some evidence that charter and regular schools differ in their effect on gains in achievement by race and ethnicity. The overall results that charters experience problems, at least in early years, in teaching elementary math and reading and middle school reading holds for all racial and ethnic groups though the magnitudes vary and are not significant in every case. The overall result that high school charters are improving reading gains applies mainly to white students.

If charter schools are not faring dramatically worse, or dramatically better, than regular public schools in terms of boosting student achievement, it is natural to ask whether one type of school is more cost effective than the other. We do not address the intricacies of charter school finance in this paper. Yet we note that charter schools appear to be less well funded than traditional schools. One of the main reasons for this is that charters often have to pay a portion of their building costs (often in the form of rent) from

[11] A quasi-experimental study of the Preuss School, a charter school at UCSD, compares lottery winners and losers. (See McClure et al., 2005) This approach has been quite rare to date. Notably, the study finds fairly similar results to our own analysis of all charter schools in San Diego, with zero or small differences in test scores between Preuss attendees and students who had applied in the same year and grade but who lost the school's admissions lottery. In what may be a unique finding nationally, the authors report that graduates of the Preuss School are attending colleges in greater numbers than the comparison group. The

their general funds.[12] We show in Betts et. al. (2006) that teachers in charter schools are typically younger and have much less experience and education than teachers in traditional public schools. Given these stark differences it is indeed somewhat surprising that charter schools on the whole seem to boost student achievement at about the same rate as in regular public schools. We do note that any existing gaps in performance between the two types of schools could not be explained by variations in teacher qualifications or class size. This meshes with earlier results based on SDUSD data by Betts, Zau and Rice (2003) that suggest that teacher qualifications play a limited role in explaining rates of student gain in achievement, especially in elementary schools.

While it would have been much more dramatic to have found huge and consistent performance differences between charter and regular public schools, what we have in fact discovered may be equally important: With some notable exceptions, we have found that charter schools are faring about as well as regular public schools, and are doing so with relatively less experienced teachers. This finding raises important questions about whether charter schools in San Diego may prove somewhat more cost effective than regular public schools.

A natural question arises: even if it is true today that charter schools tend to hire less experienced teachers than do regular public schools, does this represent a long-term pattern or merely teething pains?[13] Nothing in our data can answer this question decisively. However, conversations that we have had with several charter school leaders in San Diego and other California cities suggest that this pattern will persist for the

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sample sizes in this initial study are very small, but the report suggests that future research that extends beyond test scores toward longer term outcomes could prove quite illuminating.

[12] For a discussion of this point see Betts, Goldhaber and Rosenstock (2005).

foreseeable future. The reason is simple: the funding pressures that charter schools face relative to regular public schools are endemic and apparently long-term: charter schools often receive less in funding per capita than conventional public schools, because they often must use funding to pay to rent buildings, and because they must sometimes bear the costs of busing their students from farflung neighborhoods. Given that salaries comprise the main cost of running a school, charter schools will have no alternative but to economize by hiring a relatively young and less experienced mix of teachers.

This study suggests promising avenues for future research. First, policymakers stand to gain a lot from a detailed comparative analysis of revenue streams and costs between charter schools and regular public schools. What are the exact mechanisms that drive charters to focus on hiring teachers who are relatively new to the profession? Is this apparent under-funding a matter of policy concern? Second, our finding that in some cases conversion and startup charter schools perform differently begs questions about other aspects of charter schools that matter for student performance. Currently available data cannot be relied upon to explain variations in charter school outcomes. Over time, as more charters enter the district, and individual charters fine-tune their academic approaches, it may become possible to distinguish between superior and inferior policies. Third, we need to learn more about the types of students who benefit the most from attending a charter school. Our analysis by race and ethnicity represents only a first step in this direction.

Professor Julian Betts is a co-author of this chapter.

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[13] We thank Mark Schneider for raising this point.

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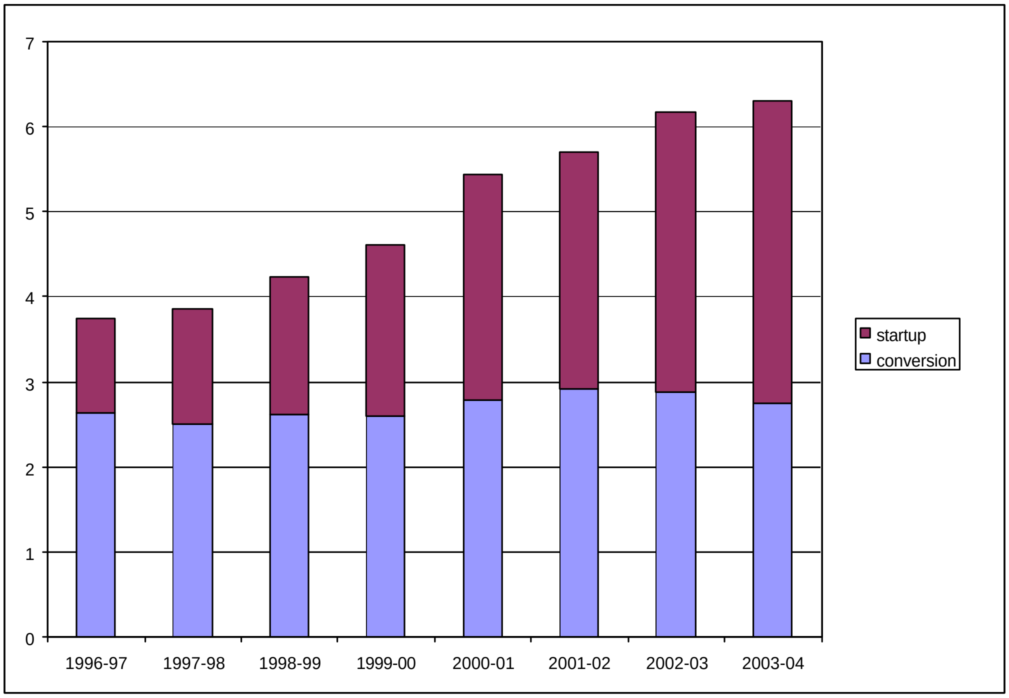

Figure 1.1
Charter School Enrollment Growth in San Diego City Schools
(% of district students enrolled in charter schools)

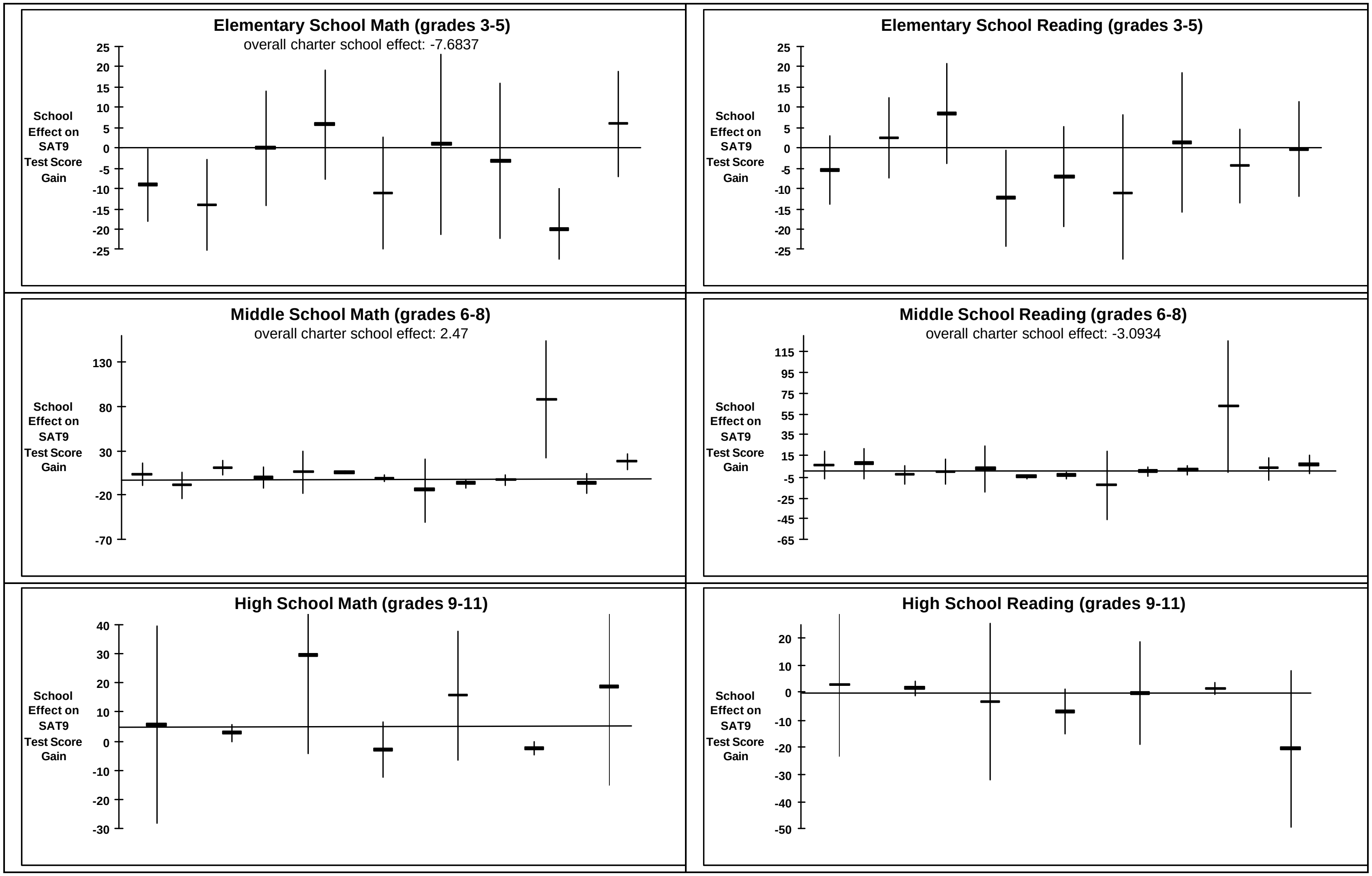

Figure 1.2: Individual Charter School Fixed Effects with 95% Confidence Intervals

Table 1.1: Enrollment by Race and Meal Assistance Eligibility
San Diego Unified School District
2002-2003

	Overall District	Regular Public	Charter	Charter Startup	Charter Conversion
% Hispanic	40.88	40.39	48.23	35.62	61.85
% White	26.16	26.77	17.19	28.14	5.38
% Asian	17.44	17.79	12.25	9.06	15.70
% Black	15.00	14.53	21.89	26.64	16.77
% American Indian/Other	0.52	0.52	0.43	0.54	0.30
Total Students	**140,753**	**131,865**	**8,888**	**4,613**	**4,275**
Number of Schools	185	165	20	16	4
% Eligible for Free Meals	45.96	45.22	56.87	40.26	74.81
% Eligible Free/Reduced Price Meals	56.63	56.02	65.68	49.90	82.71

Source: Common Core of Data 2002-2003

Table 1.2: Academic Performance Index (API) Averages, 1999-2005

	1999	2000	2001	2002	2003	2004	2005
Regular Public Schools	646	694	685	693	717	735	745
Charter Schools	558	630	646	648	690	718	743
Startups	531	646	685	672	707	734	757
Conversions	571	599	568	527	629	659	681
Difference							
Regular - Charter	89	64	39	45	27	17	2
# Regular School Scores	157	142	157	158	162	157	171
# Charter School Scores	6	9	12	12	18	19	22
# Startup Scores	2	6	8	10	14	15	18
# Conversion Scores	4	3	4	2	4	4	4

Source: http://api.cde.ca.gov/datafiles.asp

Table 1.3: Average Student Stanford 9 Achievement Test Percentile Ranking within School/Grade/Year

	Switched into a conversion charter school	Switched into a startup charter school	Switched into another regular public school	Did not switch schools
MATH				
Elementary				
before switch	40.25	46.61	46.00	50.17
after switch	43.95	49.91	47.11	50.89
Middle				
before switch	48.08	57.68	49.57	50.53
after switch	47.77	48.89	48.01	50.89
High				
before switch	No	42.16	49.19	50.98
after switch	Conversions	47.82	47.86	49.69
READING				
Elementary				
before switch	41.56	48.40	47.28	50.15
after switch	45.72	50.20	47.57	51.14
Middle				
before switch	47.58	59.92	49.72	50.19
after switch	47.76	48.47	48.17	50.87
High				
before switch	No	48.05	49.31	51.23
after switch	Conversions	47.90	48.43	50.43

Table 1.4: Regression Coefficients on Charter School Indicator

dependent variable = **SAT 9 test score gain**		**Coefficient on Charter School Indicator**					
		Student Fixed Effects **OLS**	Sample Sizes: # of students [# of observations]	Sample Sizes: # of charter students	Anderson-Hsiao **IV**	Sample Sizes: # of students [# of observations]	Sample Sizes: # of charter students
Elementary	Math	**-7.6837**	34,055	1,524	**-6.4625**	34,023	768
		(1.9457)**	[82,573]		**(2.4196)****	[48,484]	
	Reading	**-3.567**	32,732	1,489	**-3.4854**	32,702	737
		(1.3776)*	[78,812]		**(1.6258)***	[46,048]	
Middle	Math	2.47	30,925	5,898	1.4758	41,050	4,329
		(1.6629)	[74,964]		(1.5464)	[67,680]	
	Reading	**-3.0934**	30,378	5,810	**-2.5034**	40,181	4,181
		(0.7226)**	[73,467]		**(0.7248)****	[65,706]	
High	Math	-0.3107	25,992	3,176	**-3.3482**	34,459	1,947
		(1.1476)	[62,419]		**(1.3626)***	[56,134]	
	Reading	**1.1219**	25,607	3,135	0.4856	34,112	1,896
		(0.4072)**	[61,472]		(0.3041)	[55,459]	

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level

Note: All estimates include year and grade fixed effects. The Anderson-Hsiao IV estimate additionally controls for the lagged test score.

Table 1.5: Regression Coefficients on Charter School Type Indicators

Dependent variable = **Stanford 9 test score gain**			Student Fixed Effects **OLS**	Anderson-Hsiao **IV**
Elementary	Math	Charter	**-6.8158**	**-7.0437**
			(2.8942)*	**(3.1286)***
		Conversion Charter	-2.6216	1.7796
			(3.5167)	(4.2578)
		p-value (charter+conversion)	**0.0000**	0.0804
	Reading	Charter	-2.4397	-3.1629
			(1.7263)	(2.0392)
		Conversion Charter	-3.5305	-1.0292
			(2.4990)	(3.7567)
		p-value (charter+conversion)	**0.0014**	0.1663
Middle	Math	Charter	-0.6462	0.4117
			(2.0875)	(1.8373)
		Conversion Charter	4.0915	1.4220
			(2.6772)	(2.4807)
		p-value (charter+conversion)	0.0619	0.3253
	Reading	Charter	1.2482	0.0013
			(0.9088)	(1.1953)
		Conversion Charter	**-5.7536**	-3.3758
			(1.0563)**	(1.4299)*
		p-value (charter+conversion)	**0.0000**	**0.0001**

School clustered robust standard errors in parentheses.

* significant at 5 percent level; ** significant at 1 percent level

Note: All estimates include year and grade fixed effects.
Because there are no conversion high schools the results in Table 1.4 fully capture results for startup charter high schools.
The p-value provides the level of significance for a test that the charter and conversion variables both equal zero.

This provides a test of whether conversions are identical to regular public schools.

Table 1.6: Year in Operation Coefficients

			Calculated with Student Fixed Effects **OLS** Specification	**p-value** (charter+X)
Elementary	Math	charter	3.4104 (2.7027)	
		conversion	-12.9473 (3.2351)**	**0.0000**
		1st year	-25.7978 (5.5972)**	**0.0000**
		2nd year	-6.8796 (7.3932)	0.5691
		3rd year	-15.0550 (2.4326)**	**0.0078**
	Reading	charter	4.0684 (2.3479)	
		conversion	-10.1279 (2.8892)**	**0.0011**
		1st year	-12.6611 (4.6780)**	**0.0155**
		2nd year	-7.5121 (7.3641)	0.6173
		3rd year	-9.6153 (3.6390)**	0.1250
Middle	Math	charter	-2.4711 (2.0566)	
		conversion	5.9541 (2.5525)*	0.0558
		1st year	-7.0906 (4.3623)	**0.0178**
		2nd year	9.9954 (3.1855)**	0.0602
		3rd year	10.6459 (7.3411)	0.1935
	Reading	charter	1.9183 (1.7178)	
		conversion	-6.4402 (1.8048)**	**0.0000**
		1st year	-1.6537 (4.5473)	0.9516
		2nd year	-1.3295 (1.9278)	0.7221
		3rd year	-4.4910 (8.5981)	0.7246

Continued

Table 1.6: Year in Operation Coefficients, continued

			Calculated with Student Fixed Effects OLS Specification	p-value (charter+X)
High	Math	charter	-0.3475	
			(1.1493)	
		1st year	6.5924	0.2392
			(4.2858)	
		2nd year	1.1995	0.9719
			(8.1284)	
		3rd year	-13.3198	**0.0173**
			(5.6440)*	
	Reading	charter	1.1336	
			(0.4138)**	
		1st year	0.4747	0.8439
			(7.7895)	
		2nd year	-12.9407	**0.0095**
			(4.3373)**	
		3rd year	13.8253	**0.0000**
			(1.5630)**	

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level.
Note: All estimates include year and grade fixed effects. There are no conversion high schools.

Table 1.7: Switcher Coefficients Calculated with Student Fixed Effects

switchercharter1/switchernoncharter1: student is at a different school than last year and the new school is a charter/noncharter
switchercharter2/switchernoncharter2: student is at a different school than two years ago and the new school is a charter/non-charter

		OLS Specification (no conversion control)	**p-value** (charter+X)	**OLS Specification** (conversion control)	**p-value** (charter+X)
Elementary Math	charter	-3.0976		-4.6281	
		(4.4039)		(5.1457)	
	conversion			3.9633	0.8878
				(3.4918)	
	switchercharter1	-6.8698	**0.0197**	-6.5181	**0.0236**
		(3.4118)*		(3.3482)	
	switchercharter2	2.1893	0.6648	2.3367	0.3967
		(3.9918)		(4.0583)	
	switchernoncharter1	-1.2182		-1.2163	
		(0.8409)		(0.8404)	
	switchernoncharter2	-0.2829		-0.2709	
		(0.9696)		(0.9720)	
Elementary Reading	charter	0.2661		0.9093	
		(5.4042)		(5.7563)	
	conversion			-1.6458	0.8906
				(3.5196)	
	switchercharter1	-0.3019	0.9964	-0.4496	0.9549
		(3.6778)		(3.7176)	
	switchercharter2	-3.8636	0.3556	-3.9277	0.4876
		(7.6715)		(7.6621)	
	switchernoncharter1	-0.8753		-0.8761	
		(0.5337)		(0.5338)	
	switchernoncharter2	0.6669		0.6619	
		(0.6027)		(0.6033)	
Middle Math	charter	0.4388		-3.1165	
		(1.6521)		(2.6334)	
	conversion			4.6840	0.3594
				(2.6925)	
	switchercharter1	-0.3290	0.9680	-0.3842	0.1666
		(2.2871)		(2.2765)	
	switchercharter2	-0.6863	0.8771	-0.8009	0.1841
		(1.1344)		(1.2347)	
	switchernoncharter1	-5.2124		-5.2334	
		(1.0287)**		(1.0291)**	
	switchernoncharter2	-2.1316		-2.1193	
		(0.8389)*		(0.8394)*	

continued

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level.
Note: All estimates include year and grade fixed effects.

Table 1.7 continued: Switcher Coefficients Calculated with Student Fixed Effects

switchercharter1/switchernoncharter1: student is at a different school than last year and the new school is a charter/noncharter
switchercharter2/switchernoncharter2: student is at a different school than two years ago and the new school is a charter/non-charter

		OLS Specification (no conversion control)	**p-value** (charter+X)	**OLS Specification** (conversion control)	**p-value** (charter+X)
Middle Reading	charter	-1.8495 (1.2247)		2.3570 (1.8525)	
	conversion			-5.5714 (1.4391)**	**0.0289**
	switchercharter1	-4.6756 (0.7021)**	**0.0000**	-4.6053 (0.6929)**	0.1954
	switchercharter2	0.4460 (0.9491)	0.0667	0.5813 (0.9824)	**0.0367**
	switchernoncharter1	-3.5393 (0.6764)**		-3.5144 (0.6755)**	
	switchernoncharter2	-0.3409 (0.6588)		-0.3536 (0.6578)	
High Math	charter	-1.5699 (1.1129)		No conversions at this level.	
	conversion				
	switchercharter1	-0.9854 (1.6303)	0.1316		
	switchercharter2	-0.6127 (1.1271)	0.0701		
	switchernoncharter1	0.1904 (0.8384)			
	switchernoncharter2	-1.2305 (0.8203)			
High Reading	charter	-0.6257 (1.0710)		No conversions at this level.	
	conversion				
	switchercharter1	1.5517 (0.9663)	0.3903		
	switchercharter2	0.7060 (0.9049)	0.9497		
	switchernoncharter1	-0.0664 (0.4177)			
	switchernoncharter2	0.0845 (0.6281)			

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level.
Note: All estimates include year and grade fixed effects.

Table 1.8: Stanford 9 Charter School Effects by Race

		Elementary	Middle	High
Hispanic	Math	**-14.3254**	2.6719	**1.8892**
		(4.4776)**	(1.6435)	**(0.3971)****
	Reading	-3.2561	**-2.7209**	0.1102
		(2.0880)	**(0.7979)****	(0.4335)
White	Math	-4.8953	-1.4672	**-4.1776**
		(3.2673)	(1.9612)	**(1.4163)****
	Reading	-4.0034	**-4.6703**	**3.2503**
		(3.3003)	**(1.7019)****	**(1.2154)***
Black	Math	-1.9595	2.7508	0.2522
		(2.4285)	(1.9507)	(1.1333)
	Reading	-3.1010	-0.0377	2.2221
		(2.2647)	(1.3267)	(1.1610)
Asian	Math	**-12.6229**	**-6.3300**	**-4.6054**
		(4.8914)*	**(3.1763)***	**(1.6711)****
	Reading	**-8.4089**	-0.9627	-0.8230
		(3.1072)**	(2.1776)	(1.3714)

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level.
Note: All estimates include year and grade fixed effects.

Table 1.9: Stanford 9 Charter School Effects by Race and Type of Charter School

			Elementary	Middle
Hispanic	Math	startup	**-20.5379**	**-5.0228**
			(6.4014)**	**(1.9197)***
		conversion	**-5.3047**	**3.9401**
			(2.3805)*	**(1.5103)***
	Reading	startup	0.3183	-0.4823
			(2.6589)	(1.4036)
		conversion	**-8.8470**	**-3.0973**
			(2.8688)**	**(0.8721)****
White	Math	startup	-0.8960	**-5.1487**
			(3.7048)	**(2.1505)***
		conversion	**-12.8034**	4.5220
			(5.2711)*	(3.0941)
	Reading	startup	-1.2095	-2.5841
			(3.9277)	(2.3618)
		conversion	-10.9494	**-8.1934**
			(5.7427)	**(2.4644)****
Black	Math	startup	-0.5546	6.1923
			(2.5749)	(3.1490)
		conversion	-7.2503	0.8488
			(3.7209)	(1.5734)
	Reading	startup	**-5.2806**	**5.1774**
			(2.3449)*	**(1.8708)****
		conversion	5.3200	**-3.0458**
			(5.0545)	**(1.0509)****
Asian	Math	startup	4.7842	-8.3050
			(7.8617)	(5.4681)
		conversion	**-21.3299**	-5.4680
			(5.5274)**	(3.0911)
	Reading	startup	1.5140	2.1498
			(2.2592)	(4.1273)
		conversion	**-12.3469**	-2.3358
			(2.9483)**	(2.1523)

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level.
Note: All estimates include year and grade fixed effects. There are no conversion high schools.

Table 1.10: Effect of Attending a Charter School on California Standards Test Score Score Normalized to Mean 0, Standard Deviation 1 in Each Grade-Year

		Student Fixed Effects **OLS**	# of students [# of observations]	# of charter students	Anderson-Hsiao **IV**	# of students [# of observations]	# of charter students
Elementary	Math	**0.0990**	41,433	1,456	0.1025	17,960	599
		(0.0422)*	[59,393]		(0.0656)	[17,960]	
	Reading	-0.0364	40,375	1,384	-0.0228	16,863	546
		(0.0409)	[57,238]		(0.0640)	[16,863]	
Middle	Math	**-0.1053**	38,785	5,115	-0.0680	25,540	3,266
		(0.0457)*	[55,626]		(0.0704)	[25,540]	
	Reading	**-0.0413**	38,130	4,962	**-0.0588**	24,714	3,125
		(0.0162)*	[54,440]		**(0.0265)***	[24,714]	
High	Math	-0.0348	30,739	2,137	-0.0297	19,679	1,137
		(0.0415)	[43,285]		(0.0579)	[19,679]	
	Reading	-0.0045	32,483	2,636	0.0132	20,008	1,264
		(0.0180)	[45,571]		(0.0141)	[20,008]	

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level

Note: All estimates include year and grade fixed effects. The Anderson-Hsiao IV estimate additionally controls for the lagged test score.

Table 1.11: Effect of Attending a Startup or Conversion Charter School on California Standards Test Score Score Normalized to Mean 0, Standard Deviation 1 in Each Grade-Year

			Student Fixed Effects **OLS**	Anderson Hsiao **IV**
Elementary	Math	Startup	**0.1127**	0.1154
			(0.0476)*	(0.0746)
		Conversion	0.0421	0.0485
			(0.0480)	(0.0712)
	Reading	Startup	-0.0581	-0.0489
			(0.0450)	(0.0672)
		Conversion	0.0653	0.1001
			(0.0527)	(0.0948)
Middle	Math	Startup	**-0.2898**	**-0.2653**
			(0.0670)**	**(0.1128)***
		Conversion	-0.0239	0.0206
			(0.0236)	(0.0215)
	Reading	Startup	-0.0343	-0.0428
			(0.0198)	(0.0670)
		Conversion	**-0.0446**	**-0.0660**
			(0.0192)*	**(0.0193)****

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level
Note: All estimates include year and grade fixed effects. The Anderson-Hsiao IV estimate additionally controls for the lagged test score. There are no conversion schools at the high school level.

Appendix Table 1.1: Means and Standard Deviations of Test Scores and Test Score Gains, By Gradespan and Subject

			Math	Reading
Elementary	Test Score Gains	Mean	26.54	26.75
		SD	26.22	23.39
	Test Scores	Mean	626.90	632.05
		SD	40.77	43.01
Middle	Test Score Gains	Mean	12.37	15.14
		SD	20.10	19.57
	Test Scores	Mean	669.93	673.77
		SD	39.48	38.29
High	Test Score Gains	Mean	8.78	3.58
		SD	21.59	18.61
	Test Scores	Mean	698.42	693.47
		SD	36.12	37.05

Appendix Table 1.2: OLS Relationship between Current Test Score Gain and Switch into Charter School in the Next Period

	Overall		Startup		Conversion	
	Reading	Math	Reading	Math	Reading	Math
Elementary	0.9541	**-5.0728**	**-5.7815**	-4.16	**5.2797**	-5.5175
	(2.3327)	**(2.0745)***	**(1.5966)****	(2.1111)	**(2.5861)***	(2.7987)
Middle	0.0702	-0.8272	**-2.4341**	**3.8003**	**2.6155**	**-4.1599**
	(0.9760)	(1.2621)	**(0.9160)****	**(1.5172)***	**(1.1318)***	**(1.8473)***
High	**-3.0602**	0.0344				
	(1.0340)**	(1.5782)	no conversion schools at the high school level			

School clustered robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level
Note: All estimates include student, year and grade fixed effects, as well as a control for current enrollment in a charter school.

Appendix Table 1.3a: Linear Probability Regression Comparisons of Always Charter and Sometimes Charter Students
Dependent Variable=1 if student is "always charter", 0 if "sometimes charter", "never charter" students dropped

	Elementary	Middle	High
Black	**0.1264**	**0.0736**	0.0175
	(0.0368)**	**(0.0324)***	(0.0358)
Hispanic	0.0384	0.0109	0.0166
	(0.0540)	(0.0299)	(0.0296)
Asian	0.0436	**0.2064**	-0.0070
	(0.0798)	**(0.0546)****	(0.0487)
Female	0.0003	0.0037	0.0154
	(0.0259)	(0.0169)	(0.0199)
English learner	-0.0106	0.0016	**0.1268**
	(0.0446)	(0.0231)	**(0.0281)****
Special education student	0.0765	**0.2289**	-0.0586
	(0.0751)	**(0.0581)****	(0.1123)
Parent Education (high school dropout omitted)			
High school grad	**-0.4634**	-0.1533	**-0.5307**
	(0.0578)**	(0.1816)	**(0.0728)****
Some college	**-0.2786**	-0.1774	**-0.5264**
	(0.0411)**	(0.1813)	**(0.0734)****
College graduate	**-0.3187**	-0.1633	**-0.4666**
	(0.0397)**	(0.1811)	**(0.0730)****
Graduate degree	**-0.3066**	-0.0319	**-0.5520**
	(0.0445)**	(0.1809)	**(0.0746)****
Unknown	**-0.3896**	0.0406	**-0.5275**
	(0.0708)**	(0.1857)	**(0.0863)****
Multiple marked	**-0.3703**	-0.2854	**-0.5395**
	(0.0380)**	(0.1810)	**(0.0739)****
Constant	0.9998	0.5918	0.9619
	(0.3113)**	(0.2009)**	(0.2335)**
Observations	1,171	3,360	2,535
Mean of dep. var.	0.7869	0.6915	0.5954
R-squared	0.04	0.05	0.01

Robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level

Appendix Table 1.3b: Linear Probability Regression Comparisons of Never Charter and Sometimes Charter Students
Dependent Variable=1 if student is "never charter", 0 if "sometimes charter", "always charter" students dropped

	Elementary	Middle	High
Black	**-0.0063**	**-0.0082**	**0.0102**
	(0.0013)**	**(0.0023)****	**(0.0023)****
Hispanic	**0.0051**	**-0.0110**	**-0.0051**
	(0.0008)**	**(0.0022)****	**(0.0025)***
Asian	**0.0055**	**0.0268**	**0.0291**
	(0.0007)**	**(0.0015)****	**(0.0017)****
Female	-0.0001	0.0001	0.0029
	(0.0006)	(0.0016)	(0.0016)
English learner	**-0.0026**	**-0.0188**	**0.0109**
	(0.0009)**	**(0.0027)****	**(0.0026)****
Special education student	**0.0041**	**0.0394**	**0.0323**
	(0.0012)**	**(0.0023)****	**(0.0019)****
Parent Education (high school dropout omitted)			
High school grad	**-0.0083**	0.0092	**-0.0333**
	(0.0013)**	(0.0397)	**(0.0117)****
Some college	**-0.0060**	0.0129	**-0.0296**
	(0.0012)**	(0.0396)	**(0.0116)***
College graduate	**-0.0074**	0.0198	-0.0185
	(0.0013)**	(0.0396)	(0.0116)
Graduate degree	**-0.0061**	0.0270	-0.0180
	(0.0013)**	(0.0396)	(0.0115)
Unknown	**-0.0049**	0.0426	0.0010
	(0.0014)**	(0.0396)	(0.0115)
Multiple marked	**-0.0098**	-0.0055	**-0.0343**
	(0.0013)**	(0.0396)	**(0.0117)****
Constant	1.0008	0.9508	0.9814
	(0.0012)**	(0.0396)**	(0.0115)**
Observations	59,235	50,776	44,328
Mean of dep. var.	0.9947	0.9648	0.9717
R-squared	0.00	0.02	0.01

Robust standard errors in parentheses.
* significant at 5 percent level; ** significant at 1 percent level

Chapter 2

Racial Tension in U.S. Primary and Secondary Schools

Abstract

I investigate the relationships between measures of conflict and group composition and economic and social variables in US primary and secondary schools. Racial tension occurs most often when there is no majority group. More of it occurs when Asians or whites are the largest group than when blacks or Hispanics are the largest group. It is most prevalent in middle schools, and occurs more frequently in larger schools than smaller schools. When the race of the largest group is controlled for, racial tension increases with poverty, indicating there may be an economic component to racial tension. I find no strong evidence for any relationship between racial tension and between-group income disparities. I also find no evidence that recent changes in school racial composition are related to racial tension. Racial diversity in schools is associated with more racial tension, but not more violent activity or more gang activity.

Introduction

Recent trends towards globalization and the concurrent increase in migration of peoples across borders heightens the importance of cooperation between members of groups of different backgrounds. While there is a significant body of research analyzing consequences of ethnic diversity as well as a somewhat smaller research strand exploring factors that may mitigate the negative effects, there is a notable gap in understanding why diversity sometimes results in conflict and sometimes does not. Several economic theories endogenizing ethnic conflict have recently been proposed, but on the whole the empirical evidence on the causes of ethnic conflict is relatively limited. The aim of this paper is to document empirical relationships at a more micro level that may lead to better understanding of group conflict based on race or ethnicity.

The idea that ethnic diversity may be an important barrier to growth has recently received quite a bit of attention in the economics literature, most prominently in Easterly and Levine's (1997) discussion of "Africa's Growth Tragedy." Following their claim that ethnic diversity at the country level is associated with inferior public policies such as low levels of schooling and poor financial infrastructure, a research agenda has sought to document other relationships between diversity and public outcomes at various levels of geographic aggregation. That body of evidence collectively suggests a detrimental effect of diversity on a number of outcomes such as trust, productive public spending, social insurance, corruption, civil war, and even carpooling.[14]

While ethnic diversity is associated empirically with less cooperative behavior and more conflict, diversity also appears in some cases to provide productive learning environments - a common justification for affirmative action at top-ranked U.S. universities. Moreover, the urban economics literature has long argued that the generation of new ideas and innovation is most concentrated in cities, which are typically diverse.[15] It is plausible that spillovers and interactions among people with different backgrounds can result in productive new ideas. While causality along this research strand is difficult to establish, it is nonetheless significant in its demonstration that diversity does not necessarily result in conflict, economic stagnation, or the host of other negative outcomes discussed above.

This paper investigates conditions related to racial tension in U.S. primary and secondary schools. The aim is to gain understanding as to why diversity sometimes results in conflict and sometimes does not. The focus on schools limits the scope of the results so that any lessons learned could not be directly applied towards understanding ethnic or religious conflict on a broader scale. But since schools provide clearly bounded environments across which comparisons can be made, they make a reasonable departure point for documenting in more detail sets of conditions under which conflict arises and sets under which it does not.

The remainder of the paper is organized as follows. Section 1 briefly summarizes the related literature. Section 2 describes the data and methodology employed in this analysis. Results are presented in Section 3. Finally, Section 4 concludes and discusses future research.

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[14] Alesina and La Ferrara (2005) provide a comprehensive survey of the literature.
[15] For example, Ottaviano and Peri (2004) document a positive effect of cultural diversity on wages for native born U.S. citizens, where cultural diversity is measured by share foreign-born in metropolitan areas. Sparber (2006) argues that racial heterogeneity is associated with increased productivity in many US industries.

Section 1: Review of the Literature

Independent of the effects of ethnic conflict on growth and potentially productive factors as described above, researchers have recently begun to explore theoretical explanations for the existence of ethnic conflict itself. Research in this vein is limited, but there are at least two theories in the economics literature. Glaeser (2004) posits that hatred is propagated when politicians aim to increase support for their policies. When policies adversely affect minority groups, the most efficient way to build support for such policies is to promote hatred against the minority group. Glaeser argues that hatred is more likely when minority groups are politically relevant, and when the costs to majority groups of acquiring information about minority groups are high. Since information may disprove the false hate-creating stories told by politicians, there is more likely to be hatred when majority groups interact infrequently with minority groups.

Caselli and Coleman (2006) argue that because competition for resources often requires building coalitions, and there is an incentive for people to switch coalitions after it is apparent which group has won after a battle, it is advantageous for coalitions to organize along observable traits that can allow the coalition to be enforced after the outcome is realized. Physical appearance is one such immutable trait. According to this theory of ethnic conflict, tensions should increase when there is a battle for resources necessitating the formation of coalitions, and in particular when members of the group *appear* more different from each other.

As mentioned above, the empirical literature on the causes of ethnic conflict are relatively limited. One reason for this is the lack of measures of conflict in large datasets. DiPasquale and Glaeser (1996) is an exception. Using data on race riots from the 1960s and in Los Angeles in 1990, they argue that while poverty does not appear to determine rioting, ethnic diversity does. Most of the other recent empirical work on ethnic or racial conflict has been done at the cross-country level or at the case study level.

At the country level using cross-country regressions, Bluedorn (2000) and Collier (1997) separately argue that democratic governments can at least partially offset the negative effects of diversity on growth. Further, Easterly (2002) argues that good institutions, such as rule of law and bureaucratic quality, can negate the adverse effects of diversity. Miguel (2003) presents a case study of Kenya versus Tanzania, two countries which both have high ethnic diversity, but have adopted dramatically different policies with respect to group identification. Notably, Tanzania has strongly encouraged citizens to identify themselves with their nation rather than their tribes. Miguel argues this sort of nationalist policy reduces the problem of under-provision of public goods in heterogeneous communities.

In another African case study Posner (2004) compares ethnic conflict among Chewas and Tumbukas in Zambia and Malawi. He suggests that in Malawi, the relative shares of Chewas and Tumbukas are large enough to form important political coalitions in the country, while in Zambia both groups are too small relative to others to be political action groups, resulting in better relations between the groups in Zambia. Miguel and Posner (2005) demonstrate that ethnic identification in Africa is particularly pronounced around the time of competitive national elections. These findings are consistent with Glaeser's (2002) theoretical model discussed above in which politicians soliciting support for policies that adversely affect minority groups are responsible for group-level hatred.

In these frameworks, variation in an individual's propensity to identify self and others with a racial or ethnic group is due in large part to the political processes.

As noted above, most previous studies of ethnic or racial conflict have investigated the phenomenon at either the highly aggregated country level or the extreme opposite level of a case study. This study offers an in-between perspective. Omitted variable bias from unobserved heterogeneity may pose less of a problem to the researcher when analysis is performed across schools within the U.S. than when it is done across countries in the world.

Schools are an appealing alternative point of departure for studying racial conflict in more detail for a number of reasons. First, juveniles are responsible for much of the total crime in society, and in particular the violent crime. For example in 2000 almost 13 percent of all violent crimes and 17 percent of simple assaults occurred inside school buildings or on school property (Bureau of Justice Statistics Criminal Victimization in the U.S. Statistical Tables 2000 Table 61). While some of this overrepresentation may be due to the underreporting of crimes in other places (one would expect that a crime occurring in a supervised environment such as a school may be more likely to be reported than one occurring somewhere else), it is well documented that juvenile crime does account for a significant portion of crime in society (see for example Donohue and Levitt 2001).

Moreover, each school is a distinct and clearly bounded entity with unique student body compositions and policies, and it is reasonable to think of them as independent environments across which comparisons can be made. Since students in schools typically do not have many assets (at least those that are available for immediate expropriation), schools may be an even more appropriate grounds for studying conflict independent of property crimes, e.g., violence for its own sake separate from violence employed for the purposes of committing a property crime such as robbery. Court-mandated desegregation programs also results in schools having more diverse racial mixes than neighborhoods and workplaces. Finally, detailed and precise demographic data capturing dynamics of changes in racial composition over time are available yearly at the school level that are not typically available elsewhere.

Section 2: Data and Methodology

The primary source of data is the School Survey on Crime and Safety (SSOCS) conducted in 2000 by the National Center for Education Statistics (NCES) at the U.S. Department of Education. This survey questioned 3,000 representative principals and chief disciplinarians on a wide variety of crime, safety, disorder, and related topics. The restricted use version of the dataset is matched on school characteristics to the NCES's Common Core of Data, a universe of U.S. public elementary and secondary schools, through which the exact location of the school can be determined. Finally, zip code level aggregates from the 2000 U.S. Census are matched to each school observation. Schools that could not be matched to either the Common Core of Data or the Census are dropped.[16]

In all of the subsequent analysis, I estimate the general model:

$$conflict_{ij} = a + b_1 school\,composition_i + b_2 schoolchars_i + b_3 zipchars_j + g_s + e_{ij}$$

[16] 17% of the schools in the initial sample could not be matched to the Census file because the zip code variable is missing from the school file.

where i indexes a school, j indexes the zip code where the school is located, and s indexes states so that $?_s$ represent state fixed effects.

Measures of Conflict

I employ two broad types of measures of conflict. The first category of measures uses the principal or chief disciplinarian's survey response to the question "How often do student racial tensions happen at your school?" where the possible responses are "daily", "weekly", "monthly", "on occasion", or "never". This measure is treated as both a categorical variable (1 through 5 in increasing frequency), as well as a binary variable. Three different binary variables are also constructed from this response. *Raciald1* is equal to 1 if the response is "daily", "weekly", "monthly", or "on occasion", and equal to 0 if "never", while *raciald2* is similar but groups "on occasion" with "never". Analysis is also performed with "on occasion" responses dropped, though this causes the sample size to decrease significantly since "on occasion" accounts for 57% of the responses. *Raciald3* is constructed for this purpose, and is equal to 1 for schools that answer "daily", "weekly", or "monthly", and to 0 for schools that answer "never". Appendix Table 2.1 provides counts of the responses. While the majority of respondents answered "on occasion" or "never", in nearly ten percent of schools the principal observes frequent racial tension among students.

There is a limitation to using this measure. While measurement error in the dependent variable does not create the attenuation bias it does in an independent variable, using a subjective response for a dependent variable is problematic if the error in response is correlated with any of the observables. It may be possible that a school principal is more likely to categorize any sort of student body tension as "racially related" if there is significant diversity in the school, even if the underlying tension is actually related to for example class differences, rather than racial differences.

For this reason, I supplement the subjective measures of conflict with a second category of outcomes: quantitative data on the rates of violent crimes, hate crimes, and incidents of gang activity. While these measures have the advantage of being more objective, they do not measure racial conflict but rather a more general category of conflict. Violence is of natural interest in a study of conflict since it is often the culmination of disagreement. Violent incidents in this dataset include rape, sexual battery, physical attacks or fights, and robberies (the taking of things by force). Hate crimes are of particular interest in that they are inherently a result of some sort of group identification on the part of both the perpetrator and the victim. In this survey, the definition of hate crime is “any offense or threat against a person, property or society that is motivated by offender’s bias against race, color, national origin, ethnicity, gender, religion, disability, or sexual orientation.” I also consider gang activity. Gang activity may be particularly relevant since gangs represent the formation of coalitions, often along racial lines, that typically generate conflict. The definition of a gang in the survey is an “ongoing loosely organized association of three or more persons, whether formal or informal, that has a common name, signs, symbols or colors, whose members engage, either individually or collectively in violent or other forms of illegal behavior.” Finally, while property crimes perhaps are not as direct a measure of conflict as the violent, hate, or gang incidents discussed above, a high rate of property crime does indicate some lower cohesiveness among a population.

Summary statistics of these variables are shown in Appendix Table 2.2. While violent activity is fairly distributed among schools, hate crimes and gang activity are extremely concentrated within a few schools.

Measures of School Composition

I investigate the importance of several different aspects of school composition. There are many dimensions of "diversity" and it is not obvious what makes one school more or less diverse than another. For example, which should be considered more diverse: a hypothetical school A that is 80% white, 10% black, and 10% Hispanic, or another school B that is 75% white and 25% black? In which might one expect to see more racial conflict? I try to capture these particularities with several measures of school composition.

The most popular measure of diversity employed in the literature is the ethno-linguistic fragmentation index assembled by Soviet researchers in 1960 and first used for studying growth in Mauro (1995). This fragmentation index, an inverse Herfindahl, is calculated as $1-\sum_i s_i^2$ where i indexes a specific group (most commonly determined by language spoken), and s_i represents the share of the total population that group i comprises. The oft-cited intuitive interpretation of this index is that it measures the probability that any two individuals drawn at random from the population will be from two different groups.

In the hypothetical case discussed above, by this measure school B (75% white, 25% black) is considered more diverse with a fragmentation index F=0.375 than school A (80% white, 10% black, 10% Hispanic) which has fragmentation F=0.340. However, if school B's composition were instead 78.5% white and 21.5% black, it would then be considered less diverse, with a fragmentation index of F=0.338. This example illustrates that the index can potentially be less informative than one might expect. Since the index summarizes information from several dimensions, schools that have very similar F's may in fact actually look quite different. While this index is useful in a cross-country context where groups are delineated based on language spoken (and countries often have non-overlapping group types), there is less heterogeneity in a study within only a country. U.S. schools classify students into five basic racial categories (white, black, Hispanic, Asian, and American Indians/Alaskan). For this reason, in a study within the U.S. more detailed composition variables can be constructed and analyzed, with potentially more intuitive interpretations than those offered by the fragmentation index. Appendix Table 2.3 shows the distribution of this index. While over 30 percent of the schools have a fragmentation index between 0 and .1 indicating that quite a few schools in the sample are homogeneous, the remaining 70 percent are fairly distributed between .1 and .8.

Recent work by Montalvo and Reynal-Querol (2005) has questioned the appropriateness of using the traditional fragmentation index that measures diversity in most recent research in the field. The authors argue that the fragmentation index may not capture the particular aspect of diversity that is most relevant for studying group conflict. In particular, they cite Horowitz (1985), in claiming that the relationship between diversity and civil war may be non-monotonic. Civil wars are most likely to arise when a single large minority faces a majority; countries with three significant groups appear to be more stable than those with only two. While the mechanics of the fragmentation index imply that fragmentation increases with the number of ethnic groups, a polarization index such as the RQ derived in Reynal-Querol (2002): $RQ = 4\sum_i s_i^2(1 - s_i)$ is at a maximum when there are two equally sized groups. Montalvo and Reynal-Querol argue that this is the situation that is at most risk for conflict, at least in terms of the scale of civil war. It may be worth investigating whether this index is a better measure of diversity even with regard to smaller scale conflict such as crime, which may not require as much coordination as a full-blown war. I also employ the RQ polarization index as a measure of racial composition.

The simplest indicator of racial composition might be the specific dominant group, defined as the group with the maximum share in the school, an alternative measure of dominant group measuring plurality defined when a single group constitutes over 50% share of the school, as well as indicators for the presence and specific combination of particular groups. Another straightforward measure of racial composition is the share of school that is of each race. When using this share, the group squared shares must also be considered since we would not expect the relationship between the share of a certain group and racial conflict to be monotonic; at low levels, increases in the share of a group increases heterogeneity, while at high levels increases in the share can decrease heterogeneity.

Another alternative measure of racial composition uses the number of groups constituting at least 1, 5, 10, 20, or 30 percent of the school. For example, hypothetical school A above (80% white, 10% Hispanic, 10% black) would be considered to have one group with at least 30% and 20% presence (white), but three groups at 10% presence (white, Hispanic, and black). Similarly, school B (75% white, 25% black) would be considered to have one group at 30%, but two groups at both 20% and 10% presence. By the measure "numgroups10", the number of groups at 10% presence, the above hypothetical school A would be considered more diverse than school B, but by the measure "numgroups20" school B would be considered more diverse than school A. This set of measures arguably captures an intuitive measure of diversity not quite as easily seen in the fragmentation index. This measure may shed light on critical mass or tipping point theories where the raw share of members of a minority group must exceed a certain threshold for conflict to occur.

I also construct measures that capture similarity in size of groups, by differencing the shares of the two largest groups. The conjecture with this measure is that the smaller the distance is (and therefore the less obviously dominant one group is over another) the more likely there is to be conflict. It should be noted that this measure is related with the "presence at X% levels" variables when the X is high, since if there are three groups with at least 20% presence, the difference in shares must necessarily be closer than if there were only one group with at least 20% presence. (In the three-group case, the maximum difference between the largest two is 40%, (20%/20%/60%) while in the one-group case, the minimum difference is around 62% (19%/81%).) While the measures are related, it can be argued that the *two_big_diff* variable conveys important additional information on how similar in size the two largest groups are. A measure of dominance closely related to *two_big_diff* is simply the size of the largest group, the *maxshare*. When *maxshare* is very high *two_big_diff* is also high because a school dominated by one large group will necessarily also have a large difference in size between the largest and the second largest.

<u>Other Variables</u>

I control for poverty among the school population using the percentage of students eligible for free or reduced price meals. I also control for English language abilities of students measured by the percentage of students classified as Limited English Proficient (LEP). Earlier years of racial composition data are also considered to study whether changes in racial composition are correlated with racial tension. Zip code level aggregate demographic characteristics including % female-headed households, % foreign-born, and ratios of white median incomes to black, Hispanic, and Asian median incomes are also considered.

Section 3: Results

The first task is to describe how the basic measures of composition described above relate to conflict. Table 2.1 shows how shares of groups and the shares of these groups squared, allowing for a quadratic relationship, relate to racial tension in an OLS regression.[17] The coefficients on all of the group shares terms are positive, while the coefficients on the squared terms are negative. These relationships support the intuition that at low levels, an increase in the group share of a particular group increases heterogeneity and potential frictions, while at high levels an increase decreases heterogeneity. Column 2 shows that the coefficients on the group shares and corresponding squares are similar in magnitude, regardless of the particular type of group. The point estimates of these terms imply inflection points between 41% and 58% of own group share.[18] Column 3 shows that the fragmentation index appears to capture

[17] Ordered probit specifications yield qualitatively similar results. The only exception is the coefficient on the Asian share square term, which is still negative but no longer significant.
[18] Card, et.al. (2006) calculate neighborhood tipping points, estimating that minority shares in excess of about 13% tend to trigger significant exodus of white residents. They find somewhat less evidence for tipping in schools than in neighborhoods and suggest that this may be due to court-ordered mandatory

the information in these group shares well, as it is a strong predictor of racial tension on its own. Vigdor (2002) argues that when motivated by a theory of differential altruism between group types, interpreting the coefficient on the fragmentation index as a measure of weighted average of within-group affinity requires controls for individual group shares. Empirically, controlling for group shares increases the estimate on the fragmentation index slightly. Including the squared shares as well potentially introduces collinearity problems, since fragmentation is a linear combination of the sum of squared shares. Results using the RQ index of polarization in place of the fragmentation index are not shown but are qualitatively similar. This table as a whole demonstrates the straightforward and obvious result that more racial diversity is associated with more racial tension.

Table 2.2 compares racial tension in schools that are majority black, Hispanic, Asian, white, American Indian, and those that have no majority. Majority is defined as one group comprising more than 50% of a school's total student population. The columns show results for various specifications using the four measures of racial tension described above and OLS or probit estimation procedures.[19] The indicator for majority white is omitted. The only consistently significant correlate robust across specifications is the dummy variable indicating that no group comprises greater than 50% of the student population. This suggests that racial tension occurs most frequently at schools in which there is no group constituting a plurality at the school, a situation occurring in around 8%

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desegregation programs. The inflection point I measure is much higher than their tipping point estimate because my point indicates that share at which an increase in group share begins to decrease racial tension, rather than the point at which tension exceeds a critical level as in the case of residential tipping.

of the schools in the sample. This result supports Donohue and Levitt's (1998) model explaining the occurrence of violence based on both the lethality of (potential costs of losing) a fight and the predictability of the outcome of the fight. When there is no clear majority group, the predictability of a conflict is more uncertain than when there is a dominant group, and this can make conflict more likely.

Majority black schools, around 10% of the sample, appear to have less racial tension than majority white schools. This relationship is apparent in each specification, though it is only significant according to the measure that categorizes "occasional" racial tension as racial tension. The relatively low levels of racial tension at majority black schools may be related to the possibility that schools that are majority black tend to be more homogeneous than schools that are majority white. Table 2.3 addresses this possibility by controlling for diversity at the school. It also replaces the measure of the dominant group with a dummy indicating the largest group in the school, regardless of whether the group comprises over half of the student population. According to this measure, at least one of the five groups must be the largest, so there is no longer an indicator for no dominant group. The omitted category is again the indicator for white-dominated schools. It appears that schools in which black students are the largest group have significantly less racial tension than schools in which white students are the largest group. Schools in which Asians are the largest group have more racial tension than schools in which whites are the largest group, though the relationship is not significant.

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[19] I also replicate the analyses after scaling the racial tension variable by coding "daily" as 180, "weekly" as 36, "monthly" as 9, "never" as 0, and dropping "on occasion". Using this scaled dependent variable produces no substantive changes in the results.

The literature on the relationships between ethnic diversity and civil war has posited that countries with three sizable groups can be more stable than countries in which a single majority group faces a large minority. It is certainly theoretically possible that identification by racial group may not occur, and therefore racial conflict can be avoided, when the number of groups becomes large. Table 2.4 and 2.5 test this contention that racial conflict can potentially be avoided in environments with more groups rather than less. Table 2.4 indicates that regardless of what size share, 1%, 5%, 10%, 15%, 20%, 25%, or 30% defines whether a group has a "significant presence" at a school, it appears that the greater the number of groups the higher the likelihood of racial tension. Table 2.5 again tests the relationship between racial tension and the number of groups, but this time additionally controls for the size of the largest group.[20] In this regression, increases in the number of groups by low-level definitions of significant presence (1% or 5%) again increase likelihood of racial tension. However, at medium levels (10% or 15%) more groups do not increase racial tension. At high levels (20%, 25%, or 30%), controlling for the size of the largest group, it appears that an increase in the number of groups are associated with lower levels of racial tension. One possible interpretation for this table is that as long as one large group maintains its share of the population, there is less likely to be racial tension when there are two smaller groups than when there is only one smaller group.

The discussion so far has focused mainly on easily conceptualized measures of diversity. The economics literature relies primarily on the fragmentation index which

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[20] The variable *maxshare* described earlier controls for the size of the largest group. Results are qualitatively similar when the variable measuring the difference in size between the two biggest groups *two_bigs_dist* controls for the relative sizes of the two largest groups.

incorporates some of the dimensions of diversity discussed above (the number of groups and the relative sizes of groups) into a single measure. Table 2.6 shows that while racial tension naturally increases with this fragmentation index, there are a number of schools exhibiting significant diversity that nonetheless do not report frequent student racial tensions. Figure 2.1 demonstrates this graphically by plotting histograms of the fragmentation index separately for schools in which no racial tension is reported and schools in which frequent racial tension is reported. The left panel shows that many schools are very highly fractionalized schools and yet do not experience racial tension, echoing the work of Fearon and Laitin (1996) among others, emphasizing that in many places diversity and cooperation coexist. Conversely, the right panel illustrates that a great number of schools reporting frequent racial tension actually have relatively low levels of diversity.

Racial diversity thus does not appear to be either sufficient or necessary for racial tension to exist. The remainder of this paper investigates factors besides racial composition that are related to racial tension. Table 2.7 shows that diverse schools, large schools, and middle schools have more racial tension whether or not state fixed effects are controlled for. When the type of the largest group is controlled for in columns 3 and 4 of Table 2.7, higher levels of student poverty as measured by the percentage of school population eligible for free or reduced price meals are associated with high levels of racial tension. This suggests the possibility that there may be an economic component to racial tension. It may also be the case however that omitted characteristics of schools are jointly correlated with both high levels of student poverty and high degrees of racial tension. More research is necessary to make statements about causality.

Table 2.8 presents column 4 of Table 2.7 re-run on sub-samples of schools separated by the type of the largest group. The poverty/eligibility for free lunch variable is positive and significant for the schools in which white students are the largest group, and positive but not significant for schools in which black students or Asian students are the largest group. However, for the sample of schools that are predominantly Hispanic, the coefficient on the poverty term is negative. It seems that in these schools income is not a major factor associated with racial tension.

I also test whether school racial composition dynamics are related to racial tension. To do this, I match school composition variables lagged one year, two years, and five years from the Common Core of Data, generate the changes between the racial composition variables over time, and run similar analyses to those above using these changes. I find no clear evidence that school level racial tension is related to recent changes in school racial composition.

Quantitative measures of conflict constructed from counts of violent crimes, hate crimes, gang activity, and property crimes are analyzed using OLS regressions in Table 2.9. The conflict rates are calculated as the number of incidents per 100 students and state fixed effects are included in all specifications. Diversity does not appear related to violence or gang activity, but it is positively and significantly related to hate crimes and property crimes. Poorer schools and middle schools have higher rates of violence and gang activity.

The last task is to consider what if any relationships exist between racial tension at the school and zip code level aggregate characteristics. Table 2.10 adds as controls the percent foreign-born in the zip code and the percent female-headed households. It also includes the ratios of group median incomes since we might expect there to be a correlation between-group income differences and racial tension. The sample size in these tables drops because not all zip codes have median incomes available for all groups. In the specifications without controls for dominant group type, the coefficient on the percent female-headed household variable is negative and significant. When the dominant group type is controlled for, the coefficient loses statistical significance, though the coefficient is still negative. This result is interesting in light of Fershtman and Gneezy (2001)'s finding in an experimental study that ethnic discrimination is a primarily male practice.

Another interesting result in this table is that the coefficient on the percent foreign-born in the zip code is negative—suggesting that controlling for levels of diversity, increasing numbers of individuals born abroad can be associated with decreasing racial tension. This is consistent with the theories of racial conflict discussed earlier in which, for example, countries with three sizable groups are more stable and less prone to civil war than countries with only two sizable groups. If individuals come from many different places, there may be less historical animosity. More evidence is certainly necessary to make stronger claims, but it is possible that increasing diversity can possibly actually be associated with less racial tension, for example if we constructed fragmentation indices over country of birth rather than over the five racial categories employed in most of the analysis of this paper.

Coefficients on the ratios of median incomes between groups are not significant. While it is certainly possible that average income differences between groups is associated with racial tension, the measure I use does not capture any correlation.

Table 2.11 adds these same zip code level characteristics to regressions using quantitative measures of conflict as the dependent variable. None of the zip code level characteristics are significantly correlated with the conflict rates. However, the directions of the correlations are interesting. The coefficients on percent foreign-born are all negative, which is consistent with the idea that non-citizen foreign-born individuals typically face much higher costs when caught committing crimes. There is also a positive correlation between female-headed households and gang activity, which might be expected as many observers suggest that adolescents join gangs seeking safety and belonging, which is sometimes lacking in households without fathers.

Section 4: Conclusion and Discussion of Future Work

The findings may be summarized as follows. Racial diversity is not a necessary nor sufficient condition for the existence of racial tension. Racial tension is most likely to be present in schools without an obvious majority group. More of it occurs when Asians or whites are the dominant group than when blacks or Hispanics are the dominant group. Racial tension is more likely to occur in larger schools than in smaller schools and is most prevalent in middle schools. When the race of the largest group is controlled for, racial tension increases with poverty, indicating there may be an economic component to racial tension. Further research is necessary to determine causality since it is possible that an unobserved characteristic jointly determines student poverty and racial tension.

I find no strong evidence for any relationship between racial tension and local between-group income disparities. I also find no evidence that recent changes in school racial composition are related to racial tension. While the cross-sectional nature of the data generally limits causal inference, the correlations documented in this paper are robust to many alternative specifications.

One step towards making stronger claims about racial conflict would be to perform analysis in the context of a panel, which can presumably control for some of the omitted variables that may result in biased estimates. A panel would allow for richer analysis, particularly if a source of exogenous variation can be identified. An example of such a natural experiment was the closure of magnet schools in the San Diego Unified School District. Between the 2002-2003 and 2003-2004 school years, seven magnet programs closed in San Diego, immediately affecting the racial composition of the schools.[21] These schools continued to operate but with fewer students bused in, which presumably decreased diversity at the school. Difference-in-difference estimates of the effect of the magnet closures on crime indicate that decreasing racial heterogeneity within schools may have resulted in less crime, but not significantly. Similar variation on a larger scale might yield clearer results.

Future work must address the fact that individuals are able to move. People living in a place with more underlying racial conflict might be more likely to sort based on ethnicity, and therefore end up in more homogeneous schools. If the underlying conflict were to persist in spite of the sorting, this would bias the estimate of the effect of diversity on conflict downwards. Similarly, places without much underlying racial conflict might be likely to have very racially mixed schools, again biasing the effect of diversity on conflict downwards.

[21] A total of 23 magnet schools have closed in the San Diego Unified School District since the 1999-2000 school year. However, most of these magnet closures affected elementary schools, at which crime is generally low. When the sample is limited to middle and high schools, these 7 remain.

Research may also aim to document how segregation of groups within a region may relate to racial conflict. By limiting interaction between groups, segregation may reduce racial conflict in the short run, but increase it in the long run if eventual interaction is unavoidable.

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Schools Reporting Racial Tension “Never”	Schools Reporting Racial Tension “Daily”, “Once a Week”, or “Once a Month”

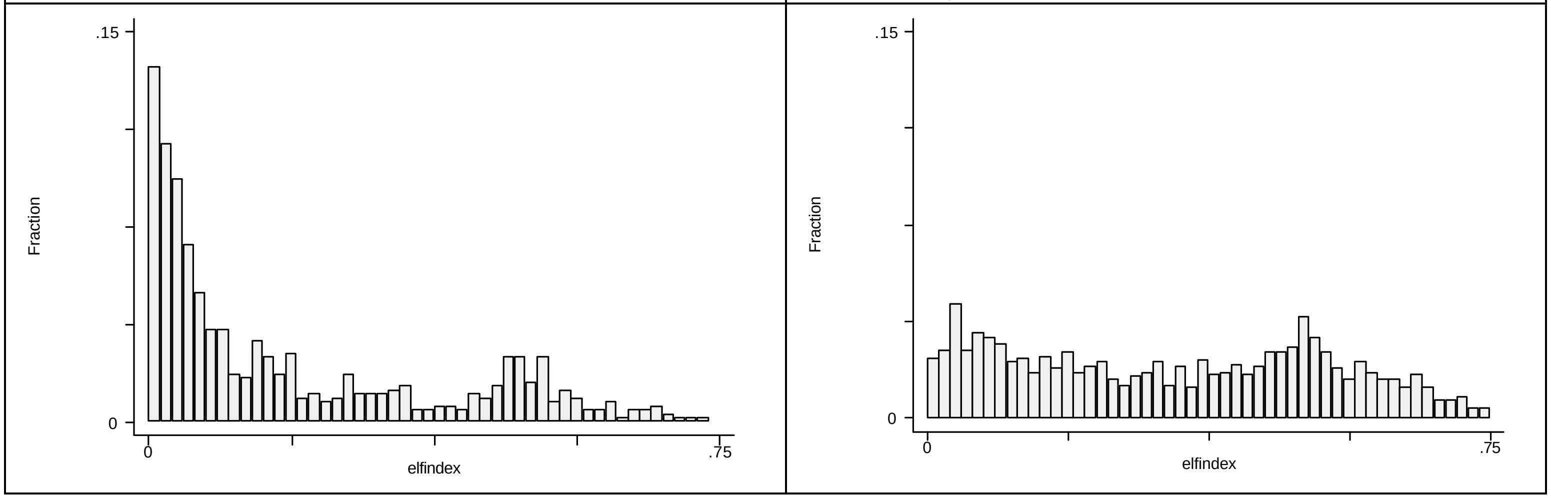

Figure 2.1: Distribution of Fragmentation Index

Table 2.1: OLS Relationships Between Racial Tension and School Racial Composition

Dependent Variable: Categorical (1-5) Measure of Racial Tension

How often do student racial tensions happen at your school?
5=’daily’
4=’once a week’
3=’once a month’
2=’on occasion’
1=’never’

	(1)	(2)	(3)	(4)	(5)	(6)
Fragmentation			1.010	1.184	1.088	1.341
			(0.072)**	(0.230)**	(0.087)**	(0.312)**
Nonwhiteshare	2.311			-0.261		
	(0.174)**			(0.520)		
Nonwhitesquare	-2.285			0.080		
	(0.187)**			(0.485)		
Blackshare		1.478			-0.251	-0.764
		(0.199)**			(0.074)**	(0.579)
Blacksquare		-1.775				0.551
		(0.246)**				(0.616)
Hispshare		1.547			-0.099	-0.375
		(0.272)**			(0.081)	(0.528)
Hispsquare		-1.746				0.273
		(0.330)**				(0.581)
Asianshare		1.637			0.278	-0.188
		(0.524)**			(0.258)	(0.645)
Asiansquare		-1.392				0.588
		(0.662)*				(0.800)
AmIndshare		1.809			-0.059	-0.525
		(0.692)**			(0.224)	(0.872)
AmIndsquare		-1.920				0.483
		(0.748)*				(0.927)
Constant	1.531	1.589	1.525	1.542	1.544	1.538
	(0.024)**	(0.023)**	(0.023)**	(0.024)**	(0.024)**	(0.025)**
Observations	2241	2241	2241	2241	2241	2241
R-squared	0.07	0.08	0.08	0.08	0.09	0.09

Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

Table 2.2: Relationships Between Dominant Groups and Racial Tension

Racial Tension: 5=’daily’
4=’once a week’
3=’once a month’
2=’on occasion’
1=’never’

Raciald1: 1=’daily, once a week, once a month, or on occasion’, 0=’never’
Raciald2: 1=’daily, once a week, once a month’, 0=’on occasion’, ’never’
Raciald3: 1=’daily, once a week, once a month”, 0=’never’

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy indicating group share is over 50%	racialtension	racialtension	raciald1	raciald1	raciald2	raciald2	raciald3	raciald3
% White>50 omitted	OLS	ordered probit	linear prob	probit	linear prob	probit	linear prob	probit
% Asian> 50	0.441	0.597	0.213	0.698	0.062	0.328	0.314	0.892
	(0.272)	(0.305)	(0.094)*	(0.416)	(0.094)	(0.417)	(0.251)	(0.629)
% Hisp> 50	0.075	0.129	0.060	0.167	0.007	0.043	0.043	0.152
	(0.062)	(0.092)	(0.038)	(0.109)	(0.023)	(0.147)	(0.056)	(0.186)
% Black> 50	-0.087	-0.158	-0.072	-0.187	-0.013	-0.093	-0.049	-0.199
	(0.054)	(0.086)	(0.035)*	(0.091)*	(0.018)	(0.137)	(0.036)	(0.159)
% AmInd >50	0.083	0.058	-0.001	-0.004	-0.010	-0.069	-0.019	-0.075
	(0.265)	(0.352)	(0.129)	(0.345)	(0.069)	(0.507)	(0.153)	(0.612)
NoGroup>50%	0.360	0.548	0.209	0.681	0.085	0.429	0.346	0.972
	(0.070)**	(0.088)**	(0.031)**	(0.130)**	(0.031)**	(0.130)**	(0.074)**	(0.191)**
Constant	1.774		0.644	0.370	0.081	-1.396	0.186	-0.892
	(0.018)**		(0.012)**	(0.031)**	(0.007)**	(0.044)**	(0.014)**	(0.054)**
Observations	2241	2241	2241	2241	2241	2241	963	963
R-squared	0.02		0.02		0.01		0.04	

Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

Table 2.3: Relationships Between Racial Tension and Largest Group Type, Controlling for School Diversity

	(1) Racialtension OLS	(2) Racialtension ordered probit	(3) Raciald1 OLS	(4) Raciald1 probit	(5) Raciald2 OLS	(6) Raciald2 probit	(7) Raciald3 OLS	(8) Raciald3 probit
Fragmentation	1.034	1.848	0.737	2.171	0.195	1.280	0.718	2.643
	(0.080)**	(0.136)**	(0.050)**	(0.169)**	(0.031)**	(0.192)**	(0.071)**	(0.250)**
Maxamind	-0.054	-0.324	-0.253	-0.716	0.014	0.055	-0.167	-0.561
	(0.446)	(0.598)	(0.193)	(0.496)	(0.115)	(0.566)	(0.174)	(0.642)
Maxasian	0.249	0.254	0.036	0.392	0.075	0.222	0.258	0.590
	(0.189)	(0.207)	(0.057)	(0.385)	(0.080)	(0.276)	(0.146)	(0.470)
Maxblack	-0.183	-0.321	-0.119	-0.355	-0.040	-0.272	-0.112	-0.467
	(0.052)**	(0.087)**	(0.033)**	(0.100)**	(0.019)*	(0.144)	(0.039)**	(0.183)*
Maxhisp	-0.081	-0.160	-0.064	-0.182	-0.021	-0.149	-0.058	-0.266
	(0.061)	(0.094)	(0.036)	(0.117)	(0.024)	(0.146)	(0.052)	(0.195)
Constant	1.538		0.474	-0.094	0.037	-1.733	0.057	-1.477
	(0.026)**		(0.018)**	(0.048)	(0.008)**	(0.074)**	(0.015)**	(0.084)**
Observations	1888	1888	1888	1888	1888	1888	806	806
R-squared	0.09		0.11		0.02		0.15	

Notes: Maxwhite omitted. Robust standard errors in parentheses.
* significant at 5%; ** significant at 1%

Table 2.4: OLS Relationships between Racial Tension and the Number of Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
numgroups30=2	0.196						
	(0.042)**						
numgroups25=2		0.235					
		(0.038)**					
numgroups25=3		0.382					
		(0.266)					
numgroups20=2			0.271				
			(0.035)**				
numgroups20=3			0.465				
			(0.113)**				
numgroups20=4			2.287				
			(0.745)**				
numgroups15=2				0.294			
				(0.034)**			
numgroups15=3				0.498			
				(0.076)**			
numgroups15=4				0.766			
				(0.247)**			
numgroups10=2					0.342		
					(0.034)**		
numgroups10=3					0.391		
					(0.057)**		
numgroups10=4					0.484		
					(0.116)**		
numgroups5=2						0.340	
						(0.035)**	
numgroups5=3						0.473	
						(0.046)**	
numgroups5=4						0.552	
						(0.066)**	
numgroups5=5						1.441	
						(0.730)*	
numgroups1=2							0.387
							(0.051)**
numgroups1=3							0.477
							(0.051)**
numgroups1=4							0.639
							(0.050)**
numgroups1=5							0.776
							(0.075)**
Constant	1.764	1.743	1.713	1.679	1.635	1.559	1.343
	(0.018)**	(0.018)**	(0.019)**	(0.020)**	(0.021)**	(0.024)**	(0.041)**
Observations	2241	2241	2241	2241	2241	2241	2241
R-squared	0.01	0.02	0.03	0.05	0.06	0.07	0.08

Notes: NumgroupsX indicates the number of groups comprising at least X percent of the school. There are no schools with more than 2 groups with at least 30% or 25% share. There are schools with up to three groups at 20% share. There are schools with up to four different groups with at least 15% and 10% share, and schools with all five group types at 5% or 1% share. Standard errors in parentheses.
* significant at 5%; ** significant at 1%

Table 2.5: OLS Relationships between Racial Tension and the Number of Groups, Controlling for Size of Largest Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
maxshare	-1.541	-1.709	-1.673	-1.246	-0.951	-0.432	-0.606
	(0.139)**	(0.149)**	(0.182)**	(0.211)**	(0.218)**	(0.166)**	(0.118)**
numg30dum2	-0.307						
	(0.064)**						
numg25dum2		-0.332					
		(0.063)**					
numg25dum3		-0.522					
		(0.175)**					
numg20dum2			-0.269				
			(0.069)**				
numg20dum3			-0.377				
			(0.151)*				
numg20dum4			1.287				
			(0.112)**				
numg15dum2				-0.074			
				(0.074)			
numg15dum3				-0.101			
				(0.125)			
numg15dum4				0.052			
				(0.424)			
numg10dum2					0.098		
					(0.069)		
numg10dum3					-0.010		
					(0.103)		
numg10dum4					-0.032		
					(0.169)		
numg5dum2						0.245	
						(0.052)**	
numg5dum3						0.348	
						(0.072)**	
numg5dum4						0.367	
						(0.102)**	
numg1dum2							0.309
							(0.050)**
numg1dum3							0.367
							(0.055)**
numg1dum4							0.463
							(0.057)**
numg1dum5							0.589
							(0.093)**
Constant	3.097	3.256	3.230	2.831	2.530	1.972	1.942
	(0.124)**	(0.135)**	(0.168)**	(0.198)**	(0.209)**	(0.164)**	(0.122)**
Observations	1888	1888	1888	1888	1888	1888	1888
R-squared	0.08	0.08	0.07	0.07	0.07	0.08	0.09

Notes: NumgX indicates the number of groups comprising at least X percent of the school. There are no schools with more than 2 groups with at least 30% or 25% share. There are schools with up to three groups at 20% share. There are schools with up to four different groups with at least 15% and 10% share, and schools with all five group types at 5% or 1% share. Robust standard errors in parentheses.
* significant at 5%; ** significant at 1%

Table 2.6: Frequencies in Each Category of Fragmentation and Racial Tension

Fragmentation Index	1 *never*	2 *on occasion*	3 *monthly*	4 *weekly*	5 *daily*	
0-.1	407	292	11	6	6	722
	56.37	**40.44**	**1.52**	**0.83**	**0.83**	100
.1-.2	114	199	12	11	2	338
	33.73	**58.88**	**3.55**	**3.25**	**0.59**	100
.2-.3	61	141	11	15	3	231
	26.41	**61.04**	**4.76**	**6.49**	**1.30**	100
.3-.4	40	152	13	8	3	216
	18.52	**70.37**	**6.02**	**3.70**	**1.39**	100
.4-.5	69	186	15	12	1	283
	24.38	**65.72**	**5.30**	**4.24**	**0.35**	100
.5-.6	56	194	22	12	5	289
	19.38	**67.13**	**7.61**	**4.15**	**1.73**	100
.6-.7	19	96	10	9	3	137
	13.87	**70.07**	**7.30**	**6.57**	**2.19**	100
.7-.8	3	18	2	1	1	25
	12.00	**72.00**	**8.00**	**4.00**	**4.00**	100
Total	769	1,278	96	74	24	2,241
	34.32	**57.03**	**4.28**	**3.30**	**1.07**	

Note: Row percents below counts.

Table 2.7: OLS Relationships Between Racial Tension and Other School Characteristics

	(1) racialtension	(2) racialtension	(3) racialtension	(4) racialtension
Fragmentation Index	0.855	0.956	0.836	0.910
	(0.088)**	(0.102)**	(0.087)**	(0.101)**
Maxgroup – AmInd			-0.049	-0.037
			(0.448)	(0.454)
Maxgroup – Asian			0.172	0.154
			(0.181)	(0.212)
Maxgroup – black			-0.263	-0.238
			(0.063)**	(0.066)**
Maxgroup – Hisp			-0.278	-0.264
			(0.089)**	(0.093)**
School Size ('000s)	0.137	0.117	0.153	0.133
	(0.039)**	(0.040)**	(0.039)**	(0.040)**
Level - Elementary	-0.052	-0.054	-0.055	-0.054
	(0.067)	(0.069)	(0.067)	(0.069)
Level - Middle	0.144	0.141	0.148	0.148
	(0.068)*	(0.070)*	(0.068)*	(0.069)*
Level - High	0.076	0.086	0.090	0.102
	(0.070)	(0.070)	(0.070)	(0.070)
% Eligible for Meal Assistance	0.005	0.088	0.253	0.312
	(0.080)	(0.083)	(0.098)**	(0.102)**
% Limited English Proficient	0.100	-0.027	0.184	0.053
	(0.148)	(0.152)	(0.169)	(0.165)
Locale – City	-0.060	-0.063	-0.029	-0.036
	(0.061)	(0.063)	(0.060)	(0.063)
Locale - Urban Fringe	-0.031	-0.023	-0.011	-0.009
	(0.054)	(0.059)	(0.053)	(0.059)
Locale – Rural	-0.075	-0.073	-0.087	-0.087
	(0.056)	(0.058)	(0.056)	(0.058)
State fixed effects?	No	Yes	No	Yes
Constant	1.428	1.198	1.357	1.173
	(0.086)**	(0.116)**	(0.087)**	(0.116)**
Observations	1888	1888	1888	1888
R-squared	0.10	0.14	0.12	0.15

Notes: Level- Combination omitted. Locale – Town omitted. Maxgroup - white omitted.
Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

Table 2.8: OLS Relationships Between Racial Tension and School Characteristics, By Largest Group Type

	(1) maxblack	(2) maxhisp	(3) maxwhite	(4) maxasian
Fragmentation Index	1.111	0.993	0.709	-1.934
	(0.308)**	(0.419)*	(0.146)**	(1.638)
School Size ('000s)	-0.134	0.189	0.173	0.268
	(0.112)	(0.096)	(0.052)**	(0.482)
Level – Elementary	0.321	1.455	-0.124	-1.308
	(0.234)	(0.290)**	(0.075)	(1.325)
Level – Middle	0.276	1.204	0.144	-1.178
	(0.198)	(0.208)**	(0.078)	(1.308)
Level - High	0.521	0.852	0.078	0.000
	(0.230)*	(0.203)**	(0.077)	(0.000)
% Eligible for Meal Assistance	0.414	-0.199	0.332	3.884
	(0.265)	(0.344)	(0.134)*	(1.846)
% Limited English Proficient	-0.259	-0.468	0.245	-4.524
	(0.349)	(0.348)	(0.300)	(3.059)
Locale – City	0.057	0.112	-0.012	-0.284
	(0.156)	(0.238)	(0.077)	(0.364)
Locale - Urban Fringe	0.090	0.031	-0.011	0.000
	(0.178)	(0.226)	(0.066)	(0.000)
Locale - Rural	0.138	0.306	-0.104	0.000
	(0.187)	(0.381)	(0.063)	(0.000)
Constant	0.556	0.642	1.253	2.052
	(0.406)	(0.622)	(0.132)**	(1.725)
Observations	216	165	1472	27
R-squared	0.26	0.24	0.16	0.79

Notes: Level- Combination omitted. Locale – Town omitted. Maxgroup - white omitted.
State fixed effects included in all regressions. Robust standard errors in parentheses.
* *significant at 5%; ** significant at 1%

Table 2.9: OLS Regressions of Quantitative Measures of Conflict (Incidents/100 students)

	(1) Violence	(2) Hate Crimes	(3) Gang Activity	(4) Property Crimes
Fragmentation Index	1.069	0.109	0.102	0.413
	(1.088)	(0.049)*	(0.080)	(0.204)*
Maxgroup – AmInd	9.080	-0.051	-0.070	-0.150
	(6.093)	(0.045)	(0.038)	(0.422)
Maxgroup – Asian	1.052	0.004	0.657	-0.202
	(1.947)	(0.067)	(0.479)	(0.256)
Maxgroup – black	0.979	-0.061	0.024	-0.069
	(0.789)	(0.021)**	(0.051)	(0.145)
Maxgroup – Hisp	-1.040	-0.026	0.162	-0.218
	(0.693)	(0.035)	(0.079)*	(0.161)
School Size ('000s)	-0.990	-0.038	0.035	-0.421
	(0.452)*	(0.025)	(0.036)	(0.152)**
Level – Elementary	-1.411	-0.043	-0.054	-0.885
	(0.837)	(0.032)	(0.023)*	(0.330)**
Level – Middle	1.299	0.010	0.058	-0.206
	(0.762)	(0.038)	(0.034)	(0.307)
Level - High	-0.401	-0.002	0.060	0.186
	(0.618)	(0.033)	(0.026)*	(0.248)
% Eligible for Meal Assistance	3.229	0.062	0.194	0.255
	(0.889)**	(0.051)	(0.083)*	(0.218)
% Limited English Proficient	-0.997	0.111	-0.182	-0.201
	(1.582)	(0.081)	(0.125)	(0.338)
Locale – City	-0.086	-0.019	0.062	0.207
	(0.720)	(0.051)	(0.059)	(0.143)
Locale - Urban Fringe	-0.505	-0.022	-0.049	0.178
	(0.707)	(0.056)	(0.060)	(0.163)
Locale - Rural	-1.072	-0.036	-0.045	-0.005
	(0.708)	(0.054)	(0.056)	(0.143)
Constant	2.155	0.017	-0.152	0.689
	(1.086)*	(0.058)	(0.069)*	(0.295)*
Observations	1864	1888	1888	1880
R-squared	0.09	0.03	0.07	0.07

Notes: Level- Combination omitted. Locale – Town omitted. Maxgroup - white omitted.
State fixed effects included in all regressions. Robust standard errors in parentheses.
* significant at 5%; ** significant at 1%

Table 2.10: OLS Relationships Between Racial Tension and Zip Code Characteristics

	(1) racialtension	(2) racialtension	(3) racialtension	(4) racialtension
Fragmentation Index	0.753	0.843	0.702	0.777
	(0.108)**	(0.117)**	(0.109)**	(0.119)**
Maxgroup – AmInd			-0.739	-0.770
			(0.295)*	(0.300)*
Maxgroup – Asian			0.257	0.240
			(0.184)	(0.215)
Maxgroup – black			-0.300	-0.260
			(0.075)**	(0.075)**
Maxgroup – Hisp			-0.222	-0.176
			(0.106)*	(0.112)
School Size ('000s)	0.153	0.115	0.159	0.124
	(0.045)**	(0.046)*	(0.045)**	(0.046)**
Level – Elementary	0.038	0.036	0.024	0.029
	(0.113)	(0.102)	(0.111)	(0.101)
Level – Middle	0.265	0.267	0.264	0.268
	(0.113)*	(0.099)**	(0.111)*	(0.098)**
Level – High	0.185	0.206	0.195	0.214
	(0.120)	(0.104)*	(0.118)	(0.104)*
% Eligible for Meal Assistance	0.271	0.347	0.470	0.514
	(0.109)*	(0.115)**	(0.125)**	(0.130)**
% Limited English Proficient	0.311	0.146	0.304	0.126
	(0.183)	(0.173)	(0.189)	(0.176)
Locale – City	-0.002	0.005	0.003	0.012
	(0.074)	(0.079)	(0.073)	(0.078)
Locale – Urban Fringe	0.018	0.037	0.032	0.053
	(0.068)	(0.076)	(0.067)	(0.075)
Locale – Rural	-0.102	-0.087	-0.095	-0.082
	(0.080)	(0.082)	(0.079)	(0.083)
% Foreign-Born	-0.732	-0.719	-0.597	-0.608
	(0.229)**	(0.253)**	(0.241)*	(0.268)*
% Female-Headed Households	-0.651	-0.500	-0.339	-0.239
	(0.224)**	(0.246)*	(0.237)	(0.249)
White/Black Income	-0.001	0.002	-0.003	-0.000
	(0.010)	(0.010)	(0.011)	(0.010)
White/Hisp Income	0.003	0.007	0.000	0.004
	(0.018)	(0.016)	(0.018)	(0.016)
White/Asian Income	0.025	0.020	0.024	0.019
	(0.017)	(0.018)	(0.017)	(0.017)
State fixed effects?	no	yes	no	yes
Constant	1.386	0.962	1.306	0.959
	(0.132)**	(0.159)**	(0.132)**	(0.159)**
Observations	1336	1336	1336	1336
R-squared	0.09	0.15	0.11	0.16

Notes: Level- Combination omitted. Locale – Town omitted. Maxgroup - white omitted.
Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

Table 2.11: OLS Regressions of Quantitative Measures of Conflict and Zip Code Characteristics

	(1) viol_per_100	(2) hate_per_100	(3) gang_per_100	(4) prop_per_100
Fragmentation Index	0.883	0.124	0.088	0.301
	(1.285)	(0.057)*	(0.102)	(0.260)
Maxgroup – AmInd	10.343	-0.001	-0.162	-0.736
	(9.798)	(0.058)	(0.069)*	(0.405)
Maxgroup – Asian	1.661	0.021	0.690	-0.106
	(2.094)	(0.071)	(0.504)	(0.285)
Maxgroup – black	1.022	-0.060	-0.016	-0.048
	(0.918)	(0.028)*	(0.058)	(0.162)
Maxgroup – Hisp	-0.441	-0.007	0.177	-0.114
	(0.788)	(0.039)	(0.099)	(0.189)
School Size ('000s)	-1.265	-0.044	0.046	-0.470
	(0.550)*	(0.032)	(0.046)	(0.185)*
Level – Elementary	-3.741	-0.010	-0.036	-1.446
	(1.747)*	(0.021)	(0.047)	(0.796)
Level – Middle	-0.123	0.060	0.108	-0.661
	(1.694)	(0.032)	(0.056)	(0.757)
Level – High	-1.914	0.042	0.097	-0.217
	(1.465)	(0.027)	(0.046)*	(0.660)
% Eligible for Meal Assistance	3.775	0.078	0.190	0.560
	(1.176)**	(0.056)	(0.112)	(0.374)
% Limited English Proficient	-1.311	0.130	-0.172	-0.265
	(1.527)	(0.088)	(0.128)	(0.351)
Locale – City	-0.559	-0.061	0.016	0.185
	(1.029)	(0.084)	(0.088)	(0.210)
Locale – Urban Fringe	-0.851	-0.070	-0.070	0.175
	(1.080)	(0.090)	(0.093)	(0.234)
Locale – Rural	-1.690	-0.077	-0.050	-0.008
	(1.112)	(0.093)	(0.095)	(0.251)
% Foreign-Born	-2.903	-0.089	-0.114	-0.630
	(2.090)	(0.077)	(0.241)	(0.582)
% Female-Headed Households	0.563	-0.083	0.339	-0.479
	(3.152)	(0.080)	(0.191)	(0.622)
White/Black Income	-0.001	-0.005	-0.011	0.004
	(0.095)	(0.004)	(0.008)	(0.023)
White/Hisp Income	-0.151	-0.003	-0.007	0.030
	(0.117)	(0.005)	(0.009)	(0.043)
White/Asian Income	0.186	0.013	0.031	0.015
	(0.237)	(0.008)	(0.029)	(0.059)
Constant	4.169	0.029	-0.242	1.290
	(2.162)	(0.101)	(0.128)	(0.754)
Observations	1316	1336	1336	1329
R-squared	0.11	0.04	0.08	0.08

Notes: Level- Combination omitted. Locale – Town omitted. Maxgroup - white omitted.
State fixed effects included in all regressions. Robust standard errors in parentheses.
* significant at 5%; ** significant at 1%

Appendix Table 2.1: Principal or Chief Disciplinarian Response to "How often do [student racial tensions] occur at your school?"

		Frequency	Percent
Racialtension			
1	Never	769	34.32
2	On occasion	1,278	57.03
3	Daily	96	4.28
4	Weekly	74	3.30
5	Monthly	24	1.07
Total		2,270	

Appendix Table 2.2: Quantitative Measures of Conflict (Incidents/100 students)

	Mean	Std. Dev.	Min	Max
Violent incidents	3.54	7.57	0	119
Hate crimes	0.047	0.456	0	15.6
Gang incidents	0.102	0.554	0	15.6
Property incidents	.720	1.73	0	31.8

Appendix Table 2.3:
Distribution of Fragmentation Index

Fragmentation Index	Number of Schools	Percent of Schools
0-.1	722	32.22
.1-.2	338	15.08
.2-.3	231	10.31
.3-.4	216	9.64
.4-.5	283	12.63
.5-.6	289	12.90
.6-.7	137	6.11
.7-.8	25	1.12
Total	2,241	

Appendix Table 2.4:
Summary Statistics of Other Measures of Composition

	Mean	Std. Dev.	Min	Max
Fragmentation Index	.270	.212	0	0.749
% Minority	.302	.313	0	1
% Black	.147	.240	0	1
% Hispanic	.109	.205	0	0.997
% Asian	.0308	.074	0	0.898
% Amer. Ind.	.0152	.0746	0	1
% White	.698	.313	0	1
maxshare	.810	.177	.309	1
two_bigs_diff	.672	.309	0.00146	1
numgroups30	1.18	.380	1	2
numgroups25	1.23	.431	1	3
numgroups20	1.32	.509	1	4
numgroups15	1.42	.599	1	4
numgroups10	1.58	.726	1	4
numgroups5	1.89	.895	1	5
numgroups1	2.89	1.16	1	5

Notes: Maxshare measures size of the largest group (as % of school). Two_bigs_diff measures difference in size between largest and 2nd largest groups. NumgroupsX measures the number of groups comprising at least X percent of the school.

Appendix Table 2.5:
Summary Statistics of Control Variables

School Level Variables	Mean	Std. Dev.	Min	Max
% Limited English Proficient	.0611	.135	0	1
% Eligible for Meal Assistance	.374	.270	0	1
School Size (000s of students)	.800	.581	.002	4.9
Level - Elementary	0.254	0.436	0	1
Middle	.328	.469	0	1
High	0.338	0.473	0	1
Combination or Other	.0797	.271	0	1
Locale – City	0.232	0.422	0	1
Urban Fringe	0.350	0.477	0	1
Rural	0.273	0.446	0	1
Town	.144	.352	0	1

Zip Code Level Variables	Mean	Std. Dev.	Min	Max
% Foreign Born	0.0785	0.108	0	0.703
% Female-Headed Households	0.199	0.120	0	1
Median White/Median Black Household Income	1.71	1.64	.0860	23.0
Median White/Median Hispanic Household Income	1.57	1.25	.104	17.6
Median White/Median Asian Household Income	1.25	1.49	.0125	24.9

Chapter 3

A Re-Examination of the Relationship between Crime and Inequality: A Panel Analysis of United States Areas

Abstract

In a panel analysis of United States areas, I investigate the contention that rising income inequality may increase crime rates. I first replicate findings from previous research that a strong positive correlation between local crime rates and local household income inequality appears across specifications in cross-section ordinary least squares regressions. I then demonstrate that the positive relationship between inequality and crime does not survive, and in fact reverses in some cases once local fixed effects are controlled for. I discuss and examine the possible reasons for this statistical reversal. While rising income inequality may have some negative social consequences, I find no strong evidence that it causes increased crime, at least in the short-term.

Introduction

Recent increases in U.S. income inequality have been well documented, and their sources extensively explored. Less work has been done on the potential consequences of such increases. This paper adds to the literature that explores one potential consequence of increasing income inequality: increased criminal activity.

At least since Becker (1968), economists have considered the relationships between economic conditions and levels of criminal activity. By placing the individual's rational decision to commit or not to commit a crime in a standard economic framework, it is clear how individuals' opportunity cost of engaging in criminal activity can potentially affect levels of crime. This intuition has led much of the previous empirical literature to focus on the effects of absolute poverty and unemployment on crime, both of which reduce the opportunity costs of committing crimes. More recently, economists have begun to explore the possibility that the distribution of income may also affect crime, perhaps by increasing the potential rewards to criminal activity or via some other mechanism. A number of recent papers investigate the empirical question of whether inequality, that is, the distribution of income within a given area, exerts an effect on crime independent of local poverty or unemployment levels. This paper adds to this literature.

I first replicate the findings from previous research that a strong positive correlation between household income inequality and crimes exists in cross-sectional ordinary least squares regressions of subnational U.S. areas in both 1990 and 2000, even after controlling for poverty, unemployment and a number of other variables. However, I then demonstrate that this positive correlation is not robust: it disappears and in fact becomes negative in some cases once unobserved area characteristics are controlled for by the inclusion of area fixed effects.

The paper is organized as follows. Section 1 discusses theoretical reasons why inequality might cause more crime and reviews previous research. Section 2 describes the data and methodology employed in this paper. Section 3 presents and interprets the results. Section 4 concludes.

Section 1: Review of the Literature

Changes in inequality can potentially alter incentives to commit crimes in a number of ways. Here I discuss three of these. First, inequality may increase criminal activity by increasing the potential benefits from committing a crime. If substantial wealth inequality exists in a society, there is a high probability that the random matching of two individuals will create an opportunity and incentive for one individual to expropriate from another.

This particular mechanism by which inequality might increase crime primarily applies to property crimes such as burglary, larceny, and motor vehicle theft. Moreover, if the assumption is made that the probability and penalty of getting caught is identical for all individuals, the mechanism primarily applies to property crime incidents in which the perpetrator holds a lower-wealth position in the distribution than the victim. This is because a rich individual has less incentive to expropriate from a poor individual than the poor individual has to expropriate from a rich individual. This story is similar to that of Glaeser, Shleifer, and Scheinkman (2002) -- however their argument is more complicated because it introduces the concept of corruption in the justice system.

Another mechanism by which inequality may increase criminal activity is by reducing some type of "psychic cost" associated with crime. This story, in the spirit of Akerlof (1997), suggests that inequality creates a "social distance" between individuals. If some type of social norm against crime exists, increasing social distance between individuals may reduce the force of these preventative pressures. Similarly, in an analysis of a drug-selling gang's finances, Levitt and Venkatesh (2000) use data on wages paid to gang members and death rates in the gang over the span of many years to calculate an extremely low implicit value of life by individuals in isolated communities. When individuals have low subjective valuation of life, they are more likely to participate in risky activities. If one accepts that inequality may increase isolation and relative distance between groups, inequality would result in increases in both violent and property crimes, both between and within groups. The relevant inequality measure of interest for this story is how poor the poor are relative the rest of the population.

Finally, public spending for security of the general public may be lower when there is inequality than when there is not. The basic idea of this mechanism is that since there are private substitutes for general police protection from crimes, such as gated communities and private guards, increases in inequality can reduce the base of support for protection provision by the public sector. Because the median voter determines the level of public provision, if the rich become rich enough to afford the private substitute, they will exit the public sector and their preference will be for minimum public provision. The median voter thus will be one who prefers less public provision than before the spread in the distribution. While the wealthier may be sufficiently protected because they are consuming the private substitute, the rest of the population may be less-protected. In this case, with less potential for and penalty from detection, the criminal's cost of committing a crime decreases.

While the relationship between inequality and public spending in this context has not been rigorously empirically tested, Alesina, Easterly, and Rodrik (2000) find in a cross-section analysis of U.S. counties that productive public spending is lower when a county is ethnically heterogeneous. Poterba (1997) similarly finds evidence suggesting that when the school-age population in a given district is composed primarily of youths of a different race than that of the elderly population, educational spending is less than when the groups are of the same race. In this mechanism, it is inequality between the rich and middle of the distribution that matters.

Most of the previous economic literature on crime has attempted to document the effects of unemployment and *absolute* poverty. However, three notable exceptions should be mentioned. Kelly (2000) analyzes a cross-section of all U.S. counties in 1991 to examine the relationship between county-level inequality and county crime rates. To measure inequality, Kelly artificially constructs a Gini coefficient for each county based on the ratio of the mean to median income in the county and the assumption of a log-normal distribution of income. In a study of 39 countries between 1965 and 1995, Fajnzylber, Lederman, and Loayza (2002) also investigate the relationship between inequality and homicides at the national level. Both studies find a significant and robust relationship between violent crime and inequality.

Levitt and Lochner (2001) are able to match detailed police reports to census tracts to investigate the effect of tract-level inequality on juvenile homicide rates in Chicago. They measure inequality by estimating the share of the tract's total wealth that would have to be transferred among residents in order to equalize incomes. While they find a strong relationship between inequality and juvenile homicide rates in the cross-section, the correlation does not survive a first-difference between 1990 and 1980.

Section 2: Data and Methodology

In this study, I analyze the relationship between crime and inequality in subnational United States areas. Because these regressions are subject to standard concerns over omitted variable bias, I construct a panel dataset in order to control for unobserved area characteristics that are fixed over time.

Measures of Crime

The measures of crime utilized in this study are obtained from the 1990 and 2000 Uniform Crime Reports (UCR) collected by the U.S. Federal Bureau of Investigation.[22] Local law enforcement agencies report monthly the number of crimes reported to them by citizens in eight categories: arson, aggravated assault, burglary, larceny, motor vehicle theft, murder, rape, and robbery. I conduct analyses of property and violent crimes separately, based on the UCR classification system. Violent crimes include aggravated assault, murder, rape, and robbery, while property crimes encompass arson, burglary, larceny, and motor vehicle theft.

It is important to note that the UCR data only consider crimes reported to police. It thus may be a biased measure of crime if crimes are less likely to be reported in large cities than in small ones, or if some crimes such as larceny, are less likely to be reported than more serious crimes, such as murder.[23]

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[22] I also perform some analysis of a three decade panel including 1980 in the state of California.
[23] I am aware of only one other large-scale dataset focusing on crime. The Bureau of Justice Statistics National Crime Victimization Survey samples households nationwide to obtain detailed information on

Measures of Inequality

In order to explore with some precision the relationship between criminal activity and local inequality, it is necessary to obtain detailed income data by area. As mentioned above, Kelly (2000) generates an artificial Gini coefficient based on the ratio of the mean to median income in a given area. This construct depends on the standard assumption of a log normal distribution of income. The methodology, while easily implemented due to wide availability of data on county mean and median income, has several drawbacks. Most prominently, it assumes an identical shape of the income distribution for all counties, and attempts to capture all idiosyncrasies of these distributions with one parameter. Because of this, it does not allow for easily interpretable results. Further, the use of a one parameter measure of inequality somewhat restricts testing of differential effects at different parts of the income distribution, and therefore of the mechanisms outlined above.

I obtain detailed distribution of income data from the Integrated Public Use Microdata Series (IPUMS) 5% samples of the 1990 and 2000 U.S. decennial censuses. (Ruggles, et.al. 2004) Due to confidentiality regulations, the sample does not allow for identification of areas populated by fewer than 100,000 individuals. While data on crimes are available at the reporting-agency level -- often cities, towns, and even National Parks, the availability of the income inequality from the Census measures dictates that the lowest level of analysis possible is the Census-defined geographic area called a Public Use Microdata Area (PUMA). PUMA units sometimes match one-to-one with counties,

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incidents of crimes that may or may not be reported. Detailed geographic data are not available publicly in these data and it is therefore not appropriate for a study of local inequality and crime.

sometimes match many-to-one with counties, and occasionally match one-to-many counties. Moreover, the boundaries of PUMA units changed between 1990 and 2000. To allow for the comparison of geographic areas over time, the IPUMS identifies Consistent PUMA areas which consist of multiple PUMAs that can be compared over time. My analysis uses this geographic unit, the Consistent PUMA, as the level of observation. After dropping areas with missing data, and those that I cannot match over time, 277 Consistent PUMAs remain.

I employ several measures to measure income inequality at the household level. Most of my analysis uses the 90/10 ratio. I also consider the spread between the rich and the middle (the 90/50 ratio) and the spread between the middle and the poor (the 50/10 ratio). I also keep the inter-quartile range - the 75th less 25th quartile - as a measure of spread. Finally, the mean and standard deviation of the distribution are also saved. These variables allow one to construct not only the mean to median ratio used in Kelly (2000) discussed above, but also the coefficient of variation of household income in an area.

Other County Characteristics

I construct a number of other area characteristics using Decennial Census data aggregated to the consistent PUMA level. For every consistent PUMA area, I calculate percent of population in various age groups, the percent black, the percent of households that are headed by females, and the percent of housing that is owner-occupied. I also obtain estimates of economic characteristics including median income, unemployment rates, and percent of persons living in poverty. I estimate unemployment by dividing the number of people classified unemployed by the number of people in the labor force. I calculate the percent living in poverty as defined by federal poverty levels.

Summary Statistics

Table 3.1 presents summary statistics of the variables described above. The unit of observation in all of the analysis is the Consistent PUMA area described above. After dropping areas with missing data as well as areas that I could not match over time, I have 277 Consistent PUMA areas, observed once in 1990 and once in 2000, for a total of 554 observations. Around 4,400 crimes on average are reported per 100,000 residents. Property crimes are much more prevalent than violent crimes - almost 90% of total crimes are property crimes. Larceny is the most common property crime, comprising approximately two-thirds of all property crimes. Aggravated assaults are by far the most common violent crime, accounting for almost 70% of all violent crimes.

Table 3.2 replicates Table 3.1, but presents the changes in the variables between the census years 1990 and 2000. The main patterns to note in this table are the fall in crime rates in every category, and the increase in inequality throughout the decade. The only measure of income distribution falling in this decade is the ratio of the median to the 10^{th} percentile, indicating that the rise in inequality occurred due to a spreading of the upper half of the income distribution, rather than a spreading in the lower half.

Figures 3.1a and 3.1b plot the total crime rate on the vertical axis and the 90/10 measure of inequality on the horizontal axis for the areas considered in the analysis in the years 1990 and 2000. A generally upward-sloping trend is evident in both cases, but the relationship seems to shift over time. In particular, the slope appears to be generally flatter in 2000 than in 1990. Since total crime rates fell and the 90/10 ratio increased on average between 1990 and 2000 as seen in Table 3.2, we expect the mass of the graphs to shift down and to the right between 1990 and 2000, and some of this is certainly evident.

Figures 3.2a-3b again plot crime rates on the vertical axis, and the 90/10 measure of inequality on the horizontal axis, but consider violent and property crimes separately to investigate the question of whether these two categories of crime follow different patterns. Since property crimes are the majority of crimes tallied in total crimes, the pattern of these generally echoes the pattern of total crimes overall—again, a generally upward-sloping trend is evident, and the slope is somewhat flatter in 2000 than in 1990. A positive correlation between violent crime rate and the 90/10 inequality measure is also seen in Figures 3.3a and 3.3b, with the most salient difference between 1990 and 2000 again being that there are fewer violent crimes per 100,000 overall in 2000 than in 1990.

The fixed effect analysis exploits the panel nature of the dataset to control for unobserved area characteristics that are constant over time. I use within-area changes between 1990 and 2000 to identify the effect of inequality on crime. Figure 3.4a illustrates that when we plot changes in total crime rate against changes in inequality, the positive correlations in Figures 3.1a and 3.1b reverse to generate a generally downward-sloped pattern. Again, since property crimes are the bulk of total crimes, the pattern in the plot of changes in property crimes against changes in inequality in Figure 3.4b mirrors roughly that for the plot of total crimes. Figure 3.4c plots changes in violent crime rates against inequality, and while there may be a slight downward trend, there is not the strong negative correlation seen in Figures 3.3a and 3.3b. The remainder of this paper explores these correlations in more detail.

Section 3: Results and Discussion

Tables 3.3a and 3.3b present the results of OLS regressions of total crimes, violent crimes, and property crimes on inequality and the control variables described previously, for the years 1990 and 2000 respectively. These models do *not* include fixed effects for individual consistent PUMA areas. The inequality measure I employ, the ratio of income of the household at the 90^{th} percentile of the distribution to the household at the 10^{th} percentile of the distribution, is significant and positive in every regression. The magnitudes of the coefficient on inequality are larger for all categories of crime in 1990 than in 2000, and are much larger for property than for violent crimes. To interpret these coefficients it is useful to refer to the summary statistics presented in Appendix Tables 3.1 and 3.2, which show the means and standard deviations of the variables in consideration in the years 1990 and 2000 separately. In standard deviation terms, a one standard deviation increase in inequality is associated with a 28% of a standard deviation increase in crime in 1990, while a one standard deviation increase in inequality in 2000 is associated with a 17% of a standard deviation increase in crime in 2000. This is the flattening of the correlation we saw in Figures 1a and 1b.

In both 1990 and 2000, I find the percentage of population living in poverty and the median income in the area to both be negatively correlated with crime. Because both of these variables are meant to control for the economic well-being of an area, and one (median income) is high when the economy is doing well, while the other (% living in poverty) is low, the negative coefficient on both variables may appear to be contradictory. However, these measures are actually measuring very distinct aspects of the economic well-being of an area. The coefficient on median income is perhaps not surprising - the better off the typical household in an area, the lower the crime rate, compared to an area where the typical household is less well off. The coefficient on the percentage of population living in poverty being negative, however, indicates that the more people living in poverty overall in an area, the less crime there is. This may be due to the fact that while the poor typically have lower opportunity costs of committing crime, when an area has a substantial population of poor people the poor people probably interact primarily with other people who are poor, and therefore the interactions may not generate many expropriation opportunities. The degree of income segregation of an area matters quite a bit for this story, but is beyond the scope of this paper.

The coefficient on percent of households headed by females is positive and significant in both years, which is in line with much research investigating correlates of local crime rates. The two other variables that are significant in one but not both years are % young and % owner-occupied. % Young is negative but not significant in 1990, and positive and significant in 2000, while % owner-occupied is negative and significant in 1990 but negative but not significant in all cases in 2000.

My cross-section results are generally quite consistent with previous research investigating the correlates of crime, and specifically with that research investigating the correlation between crime and inequality. Tables 3.4a and 3.4b break down the broad categories of crime further. In 1990, I find inequality to be significantly correlated with robbery, aggravated assaults, burglary, and motor vehicle thefts, but not murder, rape, larceny, or arson. These results are broadly consistent with Kelly's (2000) analysis of crimes in U.S. counties in 1991, which finds inequality to be positively and significantly correlated with assault and robbery, but not murder, rape, burglary, larceny, or motor vehicle theft.

I replicate this analysis for 2000, and present the results in Table 3.4b. In 2000, the coefficients on inequality in regressions where aggravated assaults, burglary, and motor vehicle thefts are the dependent variable are smaller than their 1990 values and no longer significant, though their values are still positive. The exception to this pattern is in larceny—for this category of crime, the coefficient on inequality for increases and actually becomes significant. In 2000, inequality is significantly correlated only with robbery and larceny. Moreover, for the types of crime (robbery, aggravated assault, burglary, and motor vehicle thefts) that were significantly correlated with inequality in 1990, compared to the 1990 values the point estimate of the coefficient on inequality in 2000 typically fell. In examining these tables jointly, we see that it is only in the single category of robbery that inequality has a positive and significant relationship with crime in both 1990 and 2000. However, because the coefficients on inequality in the other categories of crime tend to be positive and not small despite not being significant, the overall correlation with inequality in the cross-section becomes significant when all of the individual categories are aggregated to the broader categories of total, property, and violent crimes as shown in Tables 3.3a and 3.3b.

I now exploit the panel nature of my dataset to control for unobserved area characteristics that are constant over time. Table 3.5 presents in odd-numbered columns the OLS regressions of total, violent, and property crimes per 100,000 residents, differing from Tables 3.3a and 3.3b in that it pools the observations in 1990 and 2000. This essentially forces the slopes on the variables to be the same in both years, but controls for the overall decline in crime rates over time by including a dummy variable for the year 2000, on which the coefficient is always negative and very large. In the even-numbered columns I repeat these regressions but include area fixed effects. Including the area fixed effects changes the results, sometimes dramatically.

Most significantly, the coefficient on inequality changes signs, and actually becomes significantly negative in the cases of total and property crimes. Since property crimes are the bulk of total crimes, the results in the total crimes columns roughly mirror the results in the property crimes columns. In the fixed-effect analysis, for violent crimes no variable is significant except for population, on which the coefficient is negative. Changes in population are in fact negatively correlated with all three categories of crime. This is consistent with a story where places with rising crimes experience net outflow of people as they flee from crime, a phenomenon discussed extensively in Cullen and Levitt (1999).

In the analysis of property crimes, the signs and significance of the % poor and the % owner-occupied variables do not change. The magnitude of the % poor coefficient is remarkably stable between the specification without fixed effects and the specification with fixed effects, while the magnitude of the % owner-occupied coefficient changes drastically. An explanation for the increasing negative correlation between % owner-occupied and crime in the case of property crimes is that this variable is also capturing the degree of flight from crime—places with increasing crime may have more owners vacating homes (leaving either a vacant unit or perhaps renting to more transient residents), while places with decreasing crime may experience greater numbers of households willing to commit to a longer-term stay.

The fixed effect analysis also results in the coefficient on % unemployed to become positive and significant whereas in the cross-section it was not significant. Places with growing unemployment appear to have growing property crime rates. It may be that it is the recently unemployed that are most tempted by crime. The result that changes in unemployment are positively and significantly related to changes in crime is consistent with previous research, e.g. Raphael and Winter-Ebmer (2001) who document this relationship at the state level. I then demonstrate that the newly discovered negative correlation between inequality and property crime is only significant for burglary and larceny when crimes are broken down for type in Table 3.6.

Finally, Tables 3.7 and 3.8 demonstrate that the previously discussed results investigating the correlations between crime rates and household income inequality roughly hold regardless of what measure of inequality I employ. Table 3.7 replicates the analysis in the odd-numbered columns of Table 3.5 using alternative measures of inequality presenting only the coefficient on inequality -- the coefficients on the control variables are not shown. Here we see that in the cross-section, inequality is positively correlated with crime rates. Table 3.8 replicates the analysis of Table 3.7, but includes area fixed effects. We see here that again, the result found in the main analysis that once area fixed effects are controlled for the coefficient on inequality is no longer significantly positive and actually becomes significantly negative. This set of tables also suggests that the portion of the income distribution that is more correlated with crime (either positively in the case of the cross-section, or negatively in the case of the analysis with fixed effects) is how rich the rich are relative to the median (the 90/50 measure) and not as much how poor the poor are relative to the median (the 50/10 measure).

Section 4: Conclusions

The preceding analysis suggest that the positive correlation between inequality and crime rates in the cross-section found in much of previous research is not a causal relationship but rather may be the consequence of omitted variable bias. Unobserved area characteristics may be positively correlated with both local inequality and local crime rates. When these unobserved characteristics are controlled for using area fixed effects, the coefficient on inequality is no longer significantly positive and in fact becomes significantly negative in the case of property crimes, and specifically the crimes of burglary and larceny. This means that places with growing inequality actually experience falling crime rates.

Determining the reasons behind the negative correlation between inequality and crime in the panel analysis are beyond the scope of this paper, but one likely scenario is that places with growing inequality happen to be places where there is economic growth corresponding with increasing economic opportunities in the legal workforce that increase the opportunity costs of crime for the segment of the population most likely to commit crimes. In this story, places that experience rapid economic growth happen to experience increasing inequality, but the inequality is felt with a “lifting of all boats” so that crime rates, at least for burglary and larceny, decrease.

It is also possible that increases in inequality do have a real effect crime, but only with a long lag. If it takes some time for people to recognize the increase in inequality, places with a history of long-standing inequality may have higher crime rates than those places with less inequality, but changes in inequality would not necessarily cause changes in crime rates within the span of a decade. My results are in line with Levitt and Lochner

(2001) who find that across Chicago tracts, inequality is positively and significantly related to juvenile homicide rates, but first-differences in inequality are not correlated with first-differences in crime rates between 1980 and 1990. While they do not find the significantly negative relationship in the first-difference that I find in some cases, it is possible that the relationship between changes in inequality and changes in crime itself was not stable between the two different decades (1980 to 1990 and 1990 and 2000). Also, tract-level inequality may be capturing something different than the larger georgraphic area I study, the Consistent PUMA area. These are issues that can be explored more extensively in future work.

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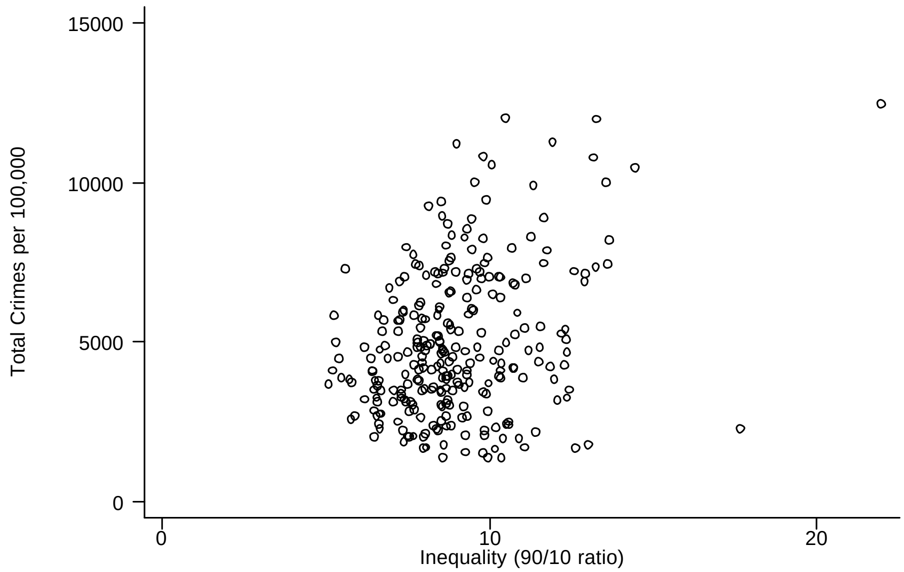

Figure 3.1a: Total Crime Rates and Income Inequality in U.S. Consistent PUMAs 1990

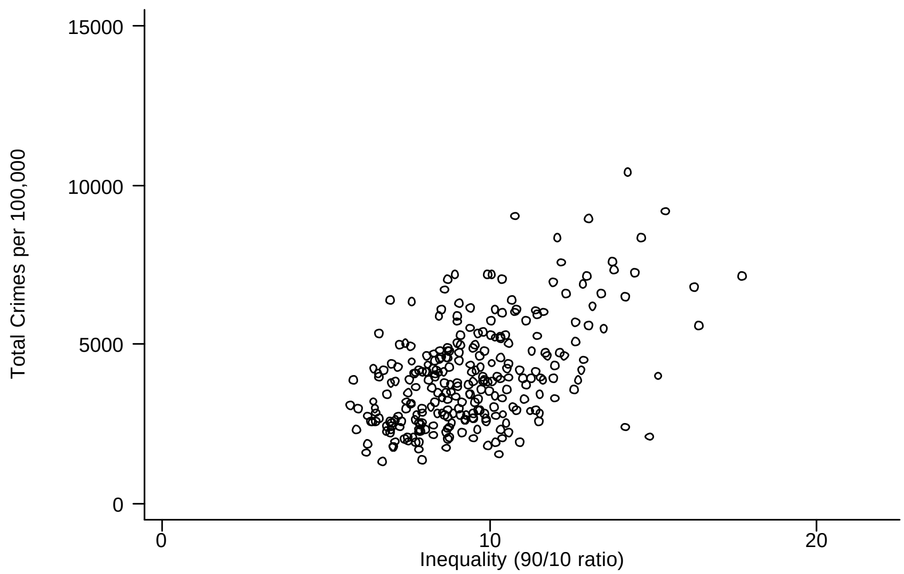

Figure 3.1b: Total Crime Rates and Income Inequality in U.S. Consistent PUMAs 2000

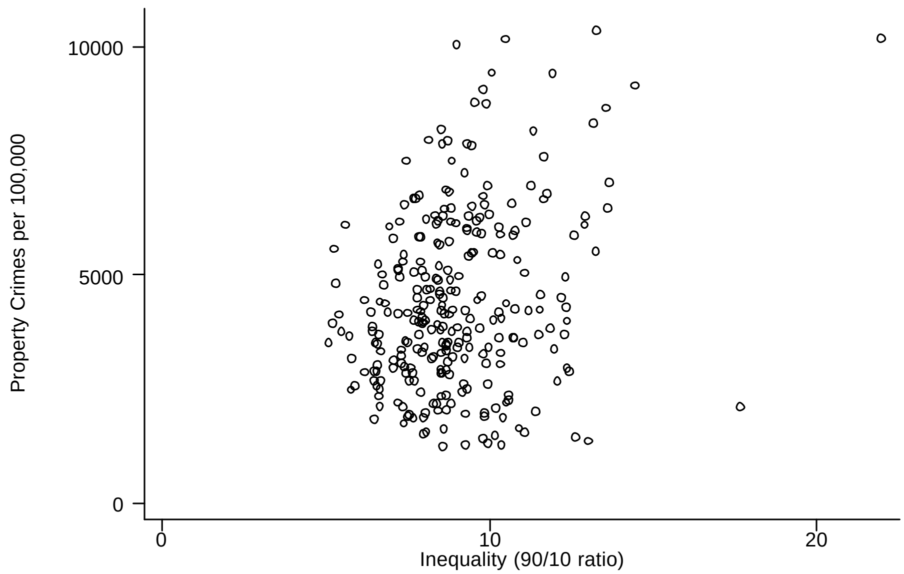

Figure 3.2a: 1990 Property Crime Rate vs Inequality

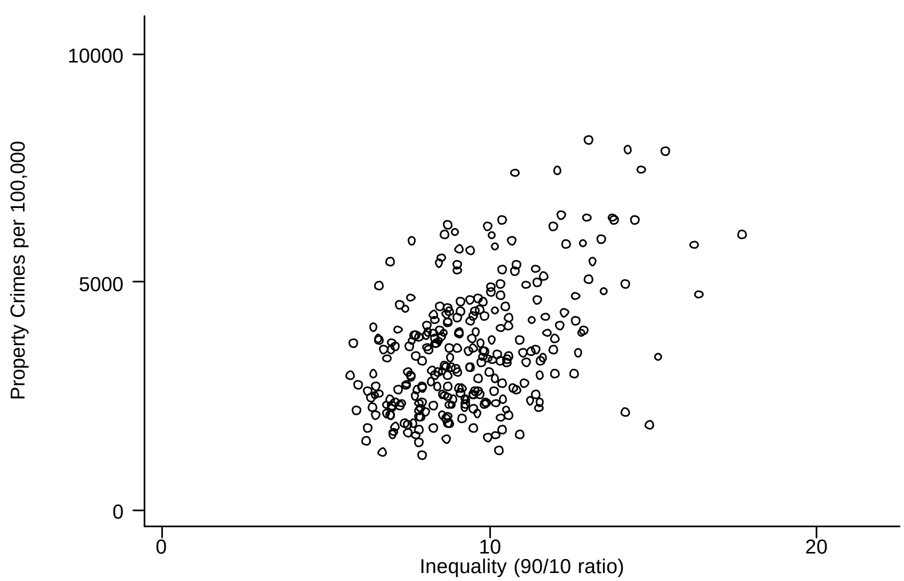

Figure 3.2b: 2000 Property Crime Rate vs Inequality

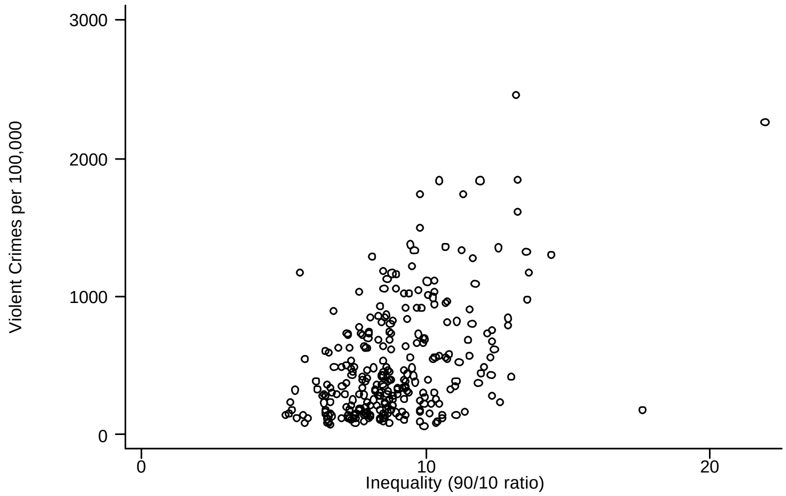

Figure 3.3a: 1990 Violent Crime Rate vs Inequality

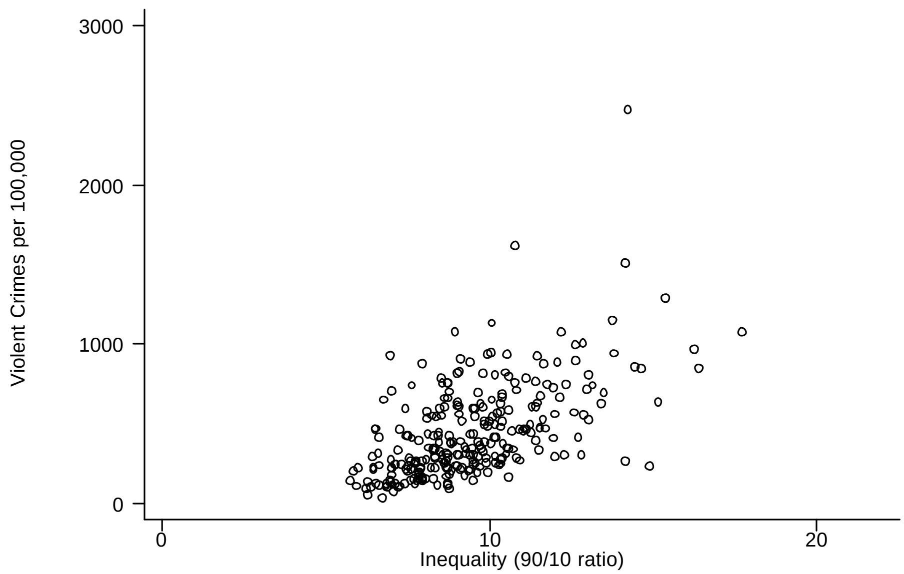

Figure 3.3b: 2000 Violent Crime Rate vs Inequality

Figure 3.4a: Changes in Total Crime and Inequality In U.S. Consistent PUMAs 1990 and 2000

Figure 3.4b: Changes in Property Crime and Inequality in U.S. Consistent PUMAs 1990 and 2000

Figure 3.4c: Changes in Violent Crime and Inequality in U.S. Consistent PUMAs 1990 and 2000

Table 3.1. Summary Statistics
Census IPUMS Consistent Public Use Microdata Areas
1990 and 2000

	Median	Mean	Standard Deviation	Min	Max
Total Crimes per 100,000	4,071	4,437	2,031	1,278	12,436
Violent	375	476	363	29	2,466
Murder	4.2	5.9	6.2	0.0	61.2
Rape	30.0	33.9	19.6	0.0	147.6
Assault	256.5	322.2	233.7	11.0	1,517.7
Robbery	69.6	113.8	146.6	0.0	1,289.2
Property	3,665	3,961	1,739	1,185	10,348
Burglary	825.0	913.0	493.2	81.6	2,891.0
Larceny	2,547.6	2,703.7	1,111.0	744.8	6,953.7
Motor Vehicle Theft	238.9	344.2	304.9	33.8	2,443.4
Arson	24.1	28.3	21.3	0.0	170.1
Control Variables					
Population	268,505	479,389	583,827	10,962	4,464,269
Percent unemployed	2.73	2.81	0.83	1.17	5.85
Percent in poverty	14.25	15.07	5.93	4.00	38.00
Mean household income	41,632	43,511	14,136	20,238	107,475
Median household income	32,699	34,665	11,017	14,057	81,150
Percent black	4.30	10.33	12.82	0.00	66.46
Percent non-white	12.80	16.75	14.30	0.34	74.16
Percent female-headed households	13.47	14.25	4.13	6.84	34.06
Percent owner-occupied housing	72.67	70.85	8.08	40.54	88.19
Percent young	12.22	12.50	2.57	6.36	29.54
Measures of Income Distribution					
90/10 ratio	8.83	9.23	2.09	5.08	21.97
Mean/Median	1.25	1.26	0.08	1.09	1.57
Interquartile Range (75%ile-25%ile)	35,505	37,007	10,657	20,798	97,860
Coefficient of Variation (sd/mean)	0.91	0.92	0.11	0.64	1.35
90/50 ratio	2.38	2.40	0.24	1.84	3.47
50/10 ratio	3.73	3.81	0.55	2.54	6.77

Source: UCR FBI Crime Reports, IPUMS 5% sample (1990, 2000)

Table 3.2. Summary Statistics
Census IPUMS Consistent Public Use Microdata Areas
1990-2000 Change

	Median	Mean	Standard Deviation	Min	Max
Total Crimes per 100,000	-744	-946	1,331	-5,704	3,041
Violent	-31	-70	227	-1,384	540
Murder	-2	-2	4	-19	10
Rape	-4	-5	17	-124	34
Assault	-16	-38	169	-1,240	540
Robbery	-1	-25	90	-717	179
Property	-747	-876	1,187	-5,432	2,639
Burglary	-281	-350	368	-1,918	821
Larceny	-377	-463	783	-3,302	1,854
Motor Vehicle Theft	-22	-63	194	-1,232	442
Arson	-4	-5	19	-82	79
Control Variables					
Population	38,104	53,033	279,560	-3,948,305	950,048
Percent unemployed	-0.15	-0.16	0.54	-1.99	1.40
Percent in poverty	-0.43	-0.71	2.12	-7.46	6.44
Mean household income	16,631	17,863	5,068	10,297	42,228
Median household income	11,500	12,209	3,363	5,288	26,313
Percent black	0.53	1.14	1.96	-4.53	14.99
Percent non-white	2.31	3.14	2.93	-3.29	17.20
Percent female-headed households	0.72	0.79	1.04	-2.47	4.09
Percent owner-occupied housing	0.29	0.49	2.07	-6.32	7.99
Percent young	-0.70	-0.57	1.21	-3.63	3.29
Measures of Income Distribution (household income)					
90/10 ratio	0.30	0.47	1.06	-4.25	5.13
Mean/Median ratio	0.07	0.07	0.04	-0.09	0.21
Interquartile Range (75%ile-25%ile)	12,869	14,131	4,746	6,671	42,545
Coefficient of Variation (sd/mean)	0.14	0.13	0.05	-0.03	0.25
90/50 ratio	0.12	0.13	0.16	-0.34	0.89
50/10 ratio	-0.03	-0.02	0.29	-1.37	1.15

Source: UCR FBI Crime Reports, IPUMS 5% sample (1990, 2000)

Table 3.3a: OLS Regressions of Total Crime and Inequality 1990

	(1)	(2)	(3)
	Total Crimes	Violent Crimes	Property Crimes
Inequality (90/10 ratio)	316.336	81.072	235.277
	(114.539)**	(21.704)**	(98.632)*
Population (00,000s)	0.614	-1.038	1.651
	(15.932)	(2.761)	(13.465)
% Poor	-186.570	-44.751	-141.836
	(45.509)**	(9.019)**	(39.346)**
% Unemployed	-332.346	-22.654	-309.726
	(290.515)	(67.689)	(234.440)
% Female-headed Households	235.214	57.692	177.536
	(89.548)**	(17.834)**	(77.192)*
Median Income ($'000s)	-70.843	-14.790	-56.060
	(25.715)**	(3.998)**	(22.565)*
% Young	-132.737	-25.425	-107.295
	(81.340)	(13.398)	(70.298)
% Owner-occupied	-175.407	-24.832	-150.578
	(18.006)**	(3.265)**	(16.507)**
% Black	-14.171	1.054	-15.228
	(29.553)	(5.976)	(24.744)
Constant	19,037.264	2,251.441	16,786.092
	(2,631.352)**	(439.783)**	(2,337.724)**
Observations	277	277	277
R-squared	0.56	0.68	0.52

Table 3.3b: OLS Regressions of Total Crime and Inequality 2000

	(1)	(2)	(3)
	Total Crimes	Violent Crimes	Property Crimes
Inequality (90/10 ratio)	192.275	26.031	166.267
	(75.300)*	(10.888)*	(68.665)*
Population (00,000s)	-8.151	-1.587	-6.563
	(15.947)	(2.267)	(14.047)
% Poor	-246.656	-27.777	-218.908
	(44.134)**	(9.042)**	(39.230)**
% Unemployed	-59.774	6.907	-66.550
	(193.540)	(33.653)	(177.715)
% Female-headed Households	283.893	51.869	232.044
	(67.310)**	(13.528)**	(60.564)**
Median Income ($'000s)	-84.669	-10.406	-74.260
	(12.884)**	(2.542)**	(11.574)**
% Young	192.524	-3.013	195.552
	(54.234)**	(8.077)	(50.778)**
% Owner-occupied	-33.192	-7.973	-25.216
	(18.134)	(3.144)*	(16.260)
% Black	1.793	1.873	-0.081
	(17.588)	(3.482)	(15.505)
Constant	5,343.741	853.855	4,488.899
	(2,462.830)*	(447.757)	(2,215.832)*
Observations	277	277	277
R-squared	0.54	0.64	0.48

Notes: Robust standard errors in parentheses. Regressions weighted by area population.
* significant at 5%; ** significant at 1%

Table 3.4a: OLS Regressions of Types of Crime and Inequality 1990

	(1)	(2)	(3)	(4)
	Violent Crimes			
	Murder	Rape	Robbery	Aggravated Assault
Inequality (90/10 ratio)	1.025	0.941	38.953	40.152
	(0.550)	(1.369)	(12.409)**	(16.467)*
Population (00,000s)	-0.026	-0.442	-0.058	-0.512
	(0.028)	(0.199)*	(1.245)	(1.677)
% Poor	-0.387	-1.289	-25.308	-17.767
	(0.166)*	(0.458)**	(4.512)**	(6.537)**
% Unemployed	0.981	0.510	14.745	-38.890
	(0.668)	(3.014)	(28.828)	(43.474)
% Female-headed Households	0.681	1.165	25.116	30.730
	(0.249)**	(0.872)	(10.880)*	(12.547)*
Median Income ($'000s)	-0.111	-0.736	-5.546	-8.398
	(0.068)	(0.214)**	(2.081)**	(2.817)**
% Young	-0.177	-1.015	-13.215	-11.018
	(0.163)	(0.604)	(7.788)	(8.344)
% Owner-occupied	-0.208	-1.482	-11.912	-11.230
	(0.057)**	(0.223)**	(2.693)**	(2.423)**
% Black	0.179	0.347	1.609	-1.081
	(0.055)**	(0.241)	(2.729)	(3.655)
Constant	9.895	169.321	932.111	1,140.114
	(7.745)	(27.098)**	(311.815)**	(337.734)**
Observations	277	277	277	277
R-squared	0.77	0.50	0.68	0.47

Robust standard errors in parentheses. Regressions weighted by area population.
* significant at 5%; ** significant at 1%

Table 3.4a continued: OLS Regressions of Types of Crime and Inequality 1990

	(5)	(6)	(7)	(8)
		Property Crimes		
	Burglary	Larceny	Motor Vehicle Theft	Arson
Inequality (90/10 ratio)	67.940	90.625	76.712	-2.035
	(33.058)*	(68.330)	(25.868)**	(1.943)
Population (00,000s)	0.335	-1.333	2.648	0.091
	(4.452)	(6.873)	(4.279)	(0.186)
% Poor	-35.555	-56.247	-50.034	-1.325
	(12.728)**	(28.600)	(8.957)**	(0.640)*
% Unemployed	-118.496	-294.920	103.690	8.286
	(70.948)	(144.467)*	(65.501)	(2.762)**
% Female-headed Households	71.111	53.752	52.672	3.692
	(25.916)**	(47.861)	(20.007)**	(1.138)**
Median Income ($'000s)	-25.422	-29.100	-1.538	-0.113
	(6.873)**	(14.172)*	(4.594)	(0.295)
% Young	-39.973	-66.203	-1.118	0.667
	(23.421)	(45.758)	(13.642)	(0.806)
% Owner-occupied	-34.293	-91.348	-24.937	-0.344
	(6.050)**	(11.249)**	(4.753)**	(0.279)
% Black	-6.727	-3.682	-4.818	-0.464
	(7.298)	(15.255)	(6.396)	(0.241)
Constant	4,145.277	11,336.477	1,304.339	18.960
	(791.001)**	(1,567.540)**	(537.954)*	(34.623)
Observations	277	277	277	277
R-squared	0.49	0.38	0.61	0.32

Robust standard errors in parentheses. Regressions weighted by area population.
* significant at 5%; ** significant at 1%

Table 3.4b: OLS Regressions of Types of Crime and Inequality 2000

	(1)	(2)	(3)	(4)
		Violent Crimes		
	Murder	Rape	Robbery	Aggravated Assault
Inequality (90/10 ratio)	0.307	1.085	12.532	12.106
	(0.184)	(0.684)	(4.780)**	(8.634)
Population (00,000s)	-0.025	-0.004	-0.616	-0.941
	(0.025)	(0.117)	(0.778)	(1.514)
% Poor	-0.199	-2.046	-18.475	-7.057
	(0.110)	(0.490)**	(2.996)**	(6.882)
% Unemployed	0.150	2.445	23.918	-19.607
	(0.504)	(2.381)	(14.115)	(25.148)
% Female-headed Households	0.497	1.607	17.555	32.210
	(0.148)**	(0.635)*	(5.029)**	(9.123)**
Median Income ($'000s)	-0.114	-0.737	-3.982	-5.573
	(0.029)**	(0.138)**	(0.837)**	(2.043)**
% Young	0.108	1.865	2.221	-7.207
	(0.102)	(0.491)**	(2.754)	(5.976)
% Owner-occupied	-0.076	-0.190	-5.042	-2.664
	(0.046)	(0.182)	(1.429)**	(2.327)
% Black	0.133	-0.024	2.495	-0.731
	(0.048)**	(0.162)	(1.177)*	(2.325)
Constant	4.693	42.839	407.477	398.846
	(5.689)	(25.938)	(200.301)*	(331.313)
Observations	277	277	277	277
R-squared	0.68	0.30	0.70	0.49

Robust standard errors in parentheses. Regressions weighted by area population.
* significant at 5%; ** significant at 1%

Table 3.4b continued: OLS Regressions of Types of Crime and Inequality 2000

	(5)	(6)	(7)	(8)
		Property Crimes		
	Burglary	Larceny	Motor Vehicle Theft	Arson
Inequality (90/10 ratio)	25.117	129.267	11.882	-1.344
	(16.886)	(49.121)**	(12.597)	(1.086)
Population (00,000s)	-1.762	-7.773	2.972	0.101
	(3.539)	(8.401)	(2.973)	(0.177)
% Poor	-28.285	-150.262	-40.361	-0.601
	(8.796)**	(29.920)**	(6.907)**	(0.738)
% Unemployed	-25.721	-72.946	32.118	18.407
	(44.545)	(124.573)	(36.741)	(4.758)**
% Female-headed Households	47.677	128.072	56.295	2.260
	(14.700)**	(41.507)**	(12.850)**	(1.166)
Median Income ($'000s)	-17.812	-47.264	-9.185	0.126
	(2.300)**	(8.399)**	(2.313)**	(0.235)
% Young	15.695	164.701	15.156	-0.164
	(9.388)	(39.620)**	(8.058)	(0.680)
% Owner-occupied	-4.310	-8.902	-12.004	0.005
	(3.921)	(12.218)	(2.355)**	(0.372)
% Black	0.840	1.633	-2.554	-0.073
	(4.056)	(10.318)	(3.494)	(0.174)
Constant	1,152.090	2,386.829	949.981	-37.494
	(576.143)*	(1,644.132)	(332.778)**	(57.288)
Observations	277	277	277	277
R-squared	0.48	0.38	0.60	0.41

Robust standard errors in parentheses. Regressions weighted by area population.
* significant at 5%; ** significant at 1%

Table 3.5: OLS/Fixed Effect Analysis of Crime by Category (Total, Violent, Property) per 100,000 and Inequality 1990 and 2000

	(1) Total	(2) Total	(3) Violent	(4) Violent	(5) Property	(6) Property
Inequality (90/10 ratio)	165.143	-418.977	41.439	-48.961	123.719	-369.997
	(70.152)*	(154.150)**	(11.255)**	(37.329)	(61.767)*	(121.942)**
Year 2000	-547.010	-737.588	-55.659	-110.227	-491.437	-627.571
	(233.440)*	(326.729)*	(40.693)	(74.901)	(203.507)*	(272.155)*
Population (00,000s)	-4.702	-123.269	-1.492	-16.579	-3.210	-106.690
	(12.144)	(25.646)**	(1.812)	(5.198)**	(10.558)	(20.724)**
% Poor	-219.277	-200.515	-35.955	-11.095	-183.340	-189.426
	(35.595)**	(51.473)**	(6.410)**	(12.937)	(31.321)**	(41.522)**
% Unemployed	-185.994	613.292	-13.518	44.701	-172.434	568.610
	(155.323)	(173.702)**	(32.411)	(39.376)	(135.094)	(145.562)**
% Female-headed Households	258.763	-115.469	58.959	-4.122	199.818	-111.323
	(53.119)**	(114.015)	(10.631)**	(23.087)	(46.707)**	(95.738)
Median Income ($'000s)	-93.731	5.741	-13.567	6.342	-80.164	-0.594
	(12.776)**	(23.131)	(2.162)**	(5.437)	(11.354)**	(19.098)
% Young	40.920	-87.636	-10.886	-2.363	51.818	-85.283
	(50.150)	(62.881)	(7.889)	(10.921)	(44.285)	(56.273)
% Owner-occupied	-108.430	-185.767	-15.803	-24.762	-92.629	-160.989
	(14.968)**	(52.084)**	(2.804)**	(13.424)	(13.008)**	(41.918)**
% Black	-1.000	-65.079	1.028	-17.442	-2.030	-47.624
	(15.412)	(66.668)	(3.049)	(11.379)	(13.344)	(57.719)
Constant	13,925.518	34,122.039	1,602.535	4,305.690	12,322.754	29,814.119
	(1,912.832)**	(4,589.312)**	(350.626)**	(1,160.112)**	(1,673.604)**	(3,941.093)**
Observations	554	554	554	554	554	554
Area Fixed Effects?	No	Yes	No	Yes	No	Yes
R-squared	0.54	0.94	0.63	0.93	0.50	0.94

Notes: Robust standard errors in parentheses. Regressions weighted by area population.
R-squared for fixed-effects regressions includes between area variation.
* significant at 5%; ** significant at 1%

Table 3.6: Fixed Effect Analysis of Crime by Type and Inequality 1990 and 2000

	(1)	(2)	(3)	(4)
		Violent Crimes		
	Murder	Rape	Robbery	Aggravated Assault
Inequality (90/10 ratio)	-0.569	-1.986	-29.824	-16.581
	(0.422)	(1.223)	(22.501)	(15.467)
Year 2000	-4.934	-3.090	-66.473	-35.731
	(0.990)**	(3.964)	(41.604)	(47.705)
Population (00,000s)	-0.143	-0.333	-6.586	-9.516
	(0.048)**	(0.194)	(2.600)*	(2.776)**
% Poor	0.055	-0.614	-6.811	-3.725
	(0.149)	(0.664)	(6.646)	(7.400)
% Unemployed	-0.629	-3.065	39.237	9.158
	(0.484)	(2.085)	(17.101)*	(27.260)
% Female-headed Households	0.350	2.610	4.742	-11.824
	(0.351)	(0.931)**	(11.146)	(18.617)
Median Income ($'000s)	0.236	-0.030	5.041	1.096
	(0.071)**	(0.294)	(3.192)	(3.159)
% Young	0.143	0.712	-3.786	0.568
	(0.155)	(0.741)	(4.904)	(8.581)
% Owner-occupied	0.004	-2.314	-10.917	-11.534
	(0.170)	(0.741)**	(7.226)	(8.657)
% Black	-0.271	-2.139	-12.868	-2.164
	(0.169)	(0.525)**	(5.760)*	(8.132)
Constant	16.116	257.292	1,723.005	2,309.277
	(12.201)	(54.278)**	(583.083)**	(858.368)**
Observations	554	554	554	554
Area Fixed Effects?	Yes	Yes	Yes	Yes
R-squared	0.94	0.86	0.92	0.90

Robust standard errors in parentheses. Regressions weighted by population.
R-squared includes between area variation.
* significant at 5%; ** significant at 1%

Table 3.6 continued: Fixed Effect Analysis of Crime by Type and Inequality 1990 and 2000

	(5)	(6)	(7)	(8)
	Property Crimes			
	Burglary	Larceny	Motor Vehicle Theft	Arson
Inequality (90/10 ratio)	-89.766	-213.267	-66.964	-0.014
	(36.991)*	(62.352)**	(36.661)	(1.480)
Year 2000	-532.757	50.842	-145.657	9.088
	(92.616)**	(155.287)	(72.759)*	(6.184)
Population (00,000s)	-31.145	-64.483	-11.062	-0.929
	(8.564)**	(8.985)**	(4.158)**	(0.282)**
% Poor	-49.861	-108.534	-31.032	-1.249
	(13.910)**	(26.051)**	(11.926)**	(0.781)
% Unemployed	142.307	286.772	139.532	5.299
	(47.083)**	(100.799)**	(35.262)**	(2.245)*
% Female-headed Households	-14.817	-92.718	-3.788	1.103
	(30.251)	(55.323)	(22.244)	(1.458)
Median Income ($'000s)	20.498	-30.273	9.181	-1.111
	(6.450)**	(10.853)**	(5.319)	(0.404)**
% Young	-19.527	-76.722	10.967	1.805
	(16.602)	(37.297)*	(14.220)	(1.095)
% Owner-occupied	-56.642	-107.691	3.345	-1.656
	(14.981)**	(25.011)**	(11.129)	(1.121)
% Black	-34.479	2.817	-15.963	1.024
	(17.221)*	(33.914)	(12.695)	(0.796)
Constant	8,779.320	19,405.621	1,629.181	75.899
	(1,356.005)**	(2,327.953)**	(960.662)	(75.367)
Observations	554	554	554	554
Area Fixed Effects?	Yes	Yes	Yes	Yes
R-squared	0.93	0.93	0.92	0.88

Robust standard errors in parentheses. Regressions weighted by population.
R-squared includes between area variation.
* significant at 5%; ** significant at 1%

Table 3.7: OLS Analysis of Crime by Type and Various Measures of Inequality 1990 and 2000

	Total	Property	Violent
90/10 ratio	165.143	123.719	41.439
	(70.152)*	(61.767)*	(11.255)**
Mean/median ratio	8,075.18	6,835.85	1,240.01
	(1,907.077)**	(1,652.948)**	(326.310)**
75th-25th quartiles	-0.005	-0.012	0.006
	(0.03)	(0.03)	(0.00)
Coefficient of variation (sd/mean)	6,887.00	5,717.65	1,169.92
	(1,683.905)**	(1,440.214)**	(299.446)**
90/50 ratio	2,104.21	1,765.42	339.046
	(550.106)**	(494.521)**	(81.092)**
50/10 ratio	167.63	71.969	95.672
	(204.72)	(181.04)	(36.205)**

Robust standard errors in parentheses. Regressions weighted by population.
All regressions include controls from previous tables.
* significant at 5%; ** significant at 1%

Table 3.8: Fixed Effect Analysis of Crime by Type and Various Measures of Inequality 1990 and 2000

	Total	Property	Violent
90/10 ratio	-418.977 (154.150)**	-369.997 (121.942)**	-48.961 (37.329)
Mean/median ratio	-14,801.42 (2,478.936)**	-12,457.24 (1,995.984)**	-2,343.88 (622.623)**
Interquartile range	-0.193 (0.039)**	-0.163 (0.033)**	-0.03 (0.008)**
Coefficient of variation (sd/mean)	-6,947.91 (2,190.762)**	-6,127.78 (1,870.311)**	-819.746 (417.344)
90/50 ratio	-4,535.69 (800.245)**	-3,865.77 (666.302)**	-669.751 (174.713)**
50/10 ratio	-394.899 (534.639)	-404.868 (420.443)	9.975 (126.202)

Robust standard errors in parentheses. Regressions weighted by population.
All regressions include controls from previous tables.
* significant at 5%; ** significant at 1%

Appendix Table 3.1. Summary Statistics
Census IPUMS Consistent Public Use Microdata Areas
1990

	Median	Mean	Standard Deviation	Min	Max
Total Crimes per 100,000	4,480	4,909	2,281	1,331	12,436
Violent	381	511	416	53	2,449
Murder	5.3	7.0	7.0	0.0	61.2
Rape	31.7	36.3	22.9	0.0	147.6
Assault	270.0	340.9	262.1	26.4	1,517.7
Robbery	66.7	126.3	171.8	3.0	1,289.2
Property	4,115	4,399	1,935	1,229	10,348
Burglary	982.3	1,088.2	550.3	81.6	2,891.0
Larceny	2,799.4	2,935.3	1,222.1	744.8	6,953.7
Motor Vehicle Theft	252.5	375.5	352.5	33.8	2,443.4
Arson	26.9	30.9	20.8	0.0	132.7
Control Variables					
Population	261,229	452,873	565,036	42,407	4,464,269
Percent unemployed	2.79	2.89	0.83	1.17	5.85
Percent in poverty	14.70	15.43	6.39	4.00	38.00
Mean household income	32,678	34,579	8,530	20,238	67,435
Median household income	26,573	28,560	7,786	14,057	59,827
Percent black	4.29	9.77	12.28	0.00	60.41
Percent non-white	10.99	15.18	13.44	0.34	67.77
Percent female-headed households	12.95	13.86	4.17	6.84	34.06
Percent owner-occupied housing	72.28	70.61	8.18	40.54	88.13
Percent young	12.54	12.78	2.48	8.28	29.54
Measures of Income Distribution					
90/10 ratio	8.68	8.99	2.04	5.08	21.97
Mean/Median ratio	1.21	1.22	0.06	1.09	1.50
Interquartile Range (75%ile-25%ile)	28,800	29,941	5,600	20,798	55,315
Coefficient of Variation (sd/mean)	0.84	0.85	0.09	0.64	1.21
90/50 ratio	2.31	2.34	0.22	1.84	3.25
50/10 ratio	3.76	3.82	0.57	2.54	6.77

Source: UCR FBI Crime Reports, IPUMS 5% sample (1990)

Appendix Table 3.2. Summary Statistics
Census IPUMS Consistent Public Use Microdata Areas
2000

	Median	Mean	Standard Deviation	Min	Max
Total Crimes per 100,000	3,798	3,964	1,617	1,278	10,359
Violent	367	441	298	29	2,466
Murder	3.6	4.8	5.1	0.0	42.1
Rape	28.8	31.4	15.4	0.0	100.8
Assault	251.0	303.4	200.1	11.0	1,350.7
Robbery	70.3	101.2	114.9	0.0	1,018.7
Property	3,340	3,523	1,389	1,185	8,110
Burglary	691.0	737.8	350.6	120.1	1,950.6
Larceny	2,307.3	2,472.0	933.7	840.3	6,571.0
Motor Vehicle Theft	232.2	312.9	245.0	44.9	1,634.9
Arson	20.9	25.7	21.4	0.0	170.1
Control Variables					
Population	286,551	505,905	601,885	10,962	3,556,797
Percent unemployed	2.68	2.73	0.82	1.27	5.79
Percent in poverty	13.96	14.72	5.42	4.56	31.30
Mean household income	49,346	52,442	12,942	31,437	107,475
Median household income	39,000	40,769	10,381	22,100	81,150
Percent black	4.50	10.90	13.34	0.21	66.46
Percent non-white	14.35	18.32	14.97	0.67	74.16
Percent female-headed households	13.94	14.64	4.05	7.95	31.64
Percent owner-occupied housing	73.00	71.10	7.98	41.85	88.19
Percent young	11.87	12.21	2.62	2.62	28.76
Measures of Income Distribution					
90/10 ratio	9.11	9.46	2.10	5.77	17.72
Mean/Median ratio	1.29	1.29	0.07	1.13	1.57
Interquartile Range (75%ile-25%ile)	41,500	44,072	9,796	29,000	97,860
Coefficient of Variation (sd/mean)	0.97	0.98	0.10	0.73	1.35
90/50 ratio	2.46	2.47	0.25	1.96	3.47
50/10 ratio	3.70	3.80	0.53	2.76	5.54

Source: UCR FBI Crime Reports, IPUMS 5% sample (2000)