

# **MORE CHOICE, LESS CRIME**

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## **Abstract**

Previous research debates whether public school (Tiebout) choice improves students' academic outcomes, but there is little examination of its effects on non-academic outcomes. We use data from a nationally representative sample of high school students, a previously developed public school choice measure, and metropolitan-level data on teenage arrest rates to examine how public school choice affects students' propensity to be arrested, to join a gang, and to commit crimes. Adolescents in metropolitan areas with more public school choice are less likely to be associated with criminal activity, suggesting that the benefits of public school choice extend outside of the classroom.

**JEL codes:** K4, H52, I22

**Keywords:** juvenile crime, school choice

## I Introduction

The current debate on public education in the United States often centers on improving schools through increased competition: public school choice, charter schools, private schools, and vouchers. Economic theory posits that increased public school choice should improve both allocative and productive efficiency as the market would penalize poorly performing school districts. Empirical tests of these predictions seem settled on, at best, a mild improvement in academic outcomes from a variety of types of school choice (Belfield and Levin, 2002; Greene and Winters, 2006).

Increased access to private schools appears to provide non-academic benefits. This may be because private schools increase retention, improve security, and provide a greater variety of activities outside the classroom (Figlio and Stone, 1999). Catholic school students exhibit lower rates of some delinquent behaviors (Figlio and Ludwig, 2000), but these results are mixed (Mocan et al, 2002). Bettinger and Slonin (2006) find that students who win a school voucher in a randomized lottery in Toledo are more altruistic toward charitable organizations, although their parents are unchanged. In Chicago, students are allocated to public high schools via randomized lotteries. Cullen, Jacob, and Levitt (2005) find that winners of these lotteries—students assigned to schools with better peers—have statistically similar academic outcomes to the losers, but significantly lower self-reported measures of discipline and arrests.

The largest source of choice for parents in educational markets does not come through private schools or vouchers, but through the choice of a public school district made jointly with a residential choice (NCES, 2003). We examine whether higher levels of traditional public school choice improve the non-academic outcomes of students.

Specifically, we test whether the bad behavior and arrest rates of high school students are lower in metropolitan areas with a greater degree of Tiebout school choice.

The degree of Tiebout choice in an area may be affected by the success of its schools, both academically and non-academically. We use instrumental variables techniques to estimate the effect of public school choice on adolescent criminal behavior, following Hoxby (2000). Hoxby proposes that the streams and rivers in a metropolitan area predict how much school choice is available, but are unrelated to the student's academic outcomes. The waterways should also be unrelated to juvenile behavior and crime, making them appropriate instruments for our study.

We find lower propensity to join a gang and lower adolescent arrest rates in metropolitan areas with a greater degree of public school choice. Whether or not public school choice improves the academic outcomes of students, there are clear non-academic gains to public school choice. Although we are unable to isolate a clear mechanism, we propose some possibilities. Estimated effects are robust to differing instrument definitions and included controls.

## II School Choice and Student Crime

In most parts of the United States, when parents choose where to live, they also choose their children's public school. Survey data suggest that among parents sending their children to the school assigned by their residence, 54 percent consider school quality when making their location decision (Hoxby, 2001). However, Rothstein (2006) argues that academic effectiveness is unable to explain parents' choice of school district. This may be because parents care about more dimensions of school quality than just

academics; parents may want schools that improve student behavior and citizenship.<sup>1</sup>

That is, schools may provide added value in non-academic ways, including in improving student behavior and controlling student crime. The voucher literature suggests that parents perceive safer school environments when their children enroll in private schools or receive scholarships (Peterson et al, 2001) Parents may also perceive these benefits of higher school safety in a public school environment, possibly more easily than they would academic benefits.

Hoxby (2000) suggests that increased Tiebout choice may increase educational attainment; this effect may also serve to reduce adolescent crime. Increased educational attainment lowers crime (Lochner and Moretti, 2004). Keeping students in schools incapacitates potential criminals, significantly reducing crime (Luallen, 2006). To the extent that increased Tiebout choice reduces crime through its effect on educational attainment, our estimates below capture both of these effects.

If schools can increase the price of crime, we expect students to respond. Mocan and Rees (2005) find that when the probability of arrest increases, students are less likely to commit certain kinds of crime. Ludwig, Duncan, and Hirschfield (2001) find that relocating juveniles to neighborhoods with lower rates of poverty decreases the likelihood that they will commit a crime. Levitt (1998) shows that juvenile crime responds to the relative price of committing a crime as a minor and as an adult: as the penalty for minors becomes relatively smaller, juveniles are more likely to commit these

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<sup>1</sup> In South Carolina, for example, schools are charged with addressing character education. “Any character education program implemented by a district as a result of an adopted policy must, to the extent possible, incorporate character traits including, but not limited to, the following: respect for authority and respect for others, honesty, self-control, cleanliness, courtesy, good manners, cooperation, citizenship, patriotism, courage, fairness, kindness, self-respect, compassion, diligence, good work ethics, sound educational habits, generosity, punctuality, cheerfulness, patience, sportsmanship, loyalty, and virtue.” (SC Code of Laws § 59-17-135)

crimes. Administrators may recognize the degree to which students respond to penalties and adopt policies that lead to better behavior.

Since parents care about the safety of schools and students respond to the price of crime, addressing non-academic outcomes may have a high payoff to administrators. If modifying behavior is easier than educating students, then focusing on these behaviors might be a more efficient way to improve school quality. One potential mechanism for improving behavior may be attendance. Schools in high choice areas may more quickly notice missing students and counsel potential offenders or more strongly enforce rules. Previous research shows that higher levels of this Tiebout choice result in higher high school graduation rates (Greene and Winters, 2006) as well as increased attendance rates (Toma, et al, forthcoming).

### III. Estimation Methods

Hoxby (2000) considers the effect of public school choice on student test scores. Rothstein (2005) disputes Hoxby's methods and her findings; his modifications produce weak to no evidence of academic benefits to public school choice. Hoxby (2005), in turn, defends her original results. We employ the methods described in Hoxby (2000) and Rothstein (2005) to estimate the effect of public school choice on students' non-market outcomes such as criminal behavior and gang membership.

Using individual level data, we estimate the following for student  $i$  in school district  $j$  in metropolitan area  $m$ :

$$\text{Bad Behavior}_{ijm} = \alpha_1 \text{school choice}_m + X_{ijm}'\beta + W_{jm}'\delta + T_m'\gamma + e_{ijm}$$

Students' bad behaviors are a function of school choice, student characteristics, school characteristics, and metropolitan area characteristics. The vector,  $X$ , includes logged household income, sex, race, ethnicity, whether the parents' highest grade is some college, and whether the parents' highest grade is a college degree or more. The school district vector,  $W$ , includes the shares of the district that are Asian, black, and Hispanic, an index of district racial homogeneity, an index of ethnic homogeneity in district, the average logged household income in the district, the Gini coefficient to measure income inequality in the district, the share of the district with some college, the share of the district with at least a college degree, and an index of educational homogeneity for the district. The metropolitan-area level vector,  $T$ , includes population, land area of metropolitan area, the average logged income, the Gini coefficient for the metropolitan area, the shares of the city that are Asian, black, and Hispanic, an index of racial homogeneity, an index of ethnic homogeneity, the share of adults with some college, the share of adults with at least a college degree, and an index of educational homogeneity. We also include nine Census division dummies.

The choice of controls is derived from Hoxby's regressions explaining student test scores and school expenditures. Rothstein and Hoxby differ in their measurement of some control variables. When employing Hoxby's method we use her controls; when Rothstein's, his. These measures include income, education, and race: good predictors of overall crime rates.<sup>2</sup>

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<sup>2</sup> In specifications not reported, we include police officers per capita or police expenditures per capita for the more limited sample for which these data are available. This produces qualitatively similar results; we do not present these as police is likely endogenous to crime. We also experiment with adding measures of police competition per capita—the number of police jurisdictions in the metropolitan area; these results are also qualitatively similar.

The variable of interest is the choice index based on enrollment. We follow Hoxby (2000) and measure school choice using a Herfindahl index constructed from the enrollment shares of school districts in the metropolitan area. The included measure is one minus the Herfindahl so that increases in the variable reflect increases in school choice in a metropolitan area.<sup>3</sup> Rothstein differs in his choice measure by using eighth grade enrollment shares instead of Hoxby's use of enrollment in all grades.

We also estimate regressions using metropolitan-level data on juvenile arrest rates. Data on arrests are not available at the student or school district level, so we estimate for metropolitan area  $m$ :

$$Arrest\ Rate_m = \alpha_2\ school\ choice_m + T_m'\theta + u_{ijm}$$

For the metropolitan-level regressions we include  $T_m$ , metropolitan-level control variables and the regional indicator variables.

### *Endogeneity and Potential Biases*

A concern with estimating causal effects using the above regressions arises if school choice is affected by students' bad behavior. School districts may have been less willing to consolidate with their neighboring district when the neighbor was of lower quality. This could result in increased school choice in areas with higher crime rates. This endogeneity is likely to upward bias the effects of school choice on crime, making it harder to find that school choice reduces crime rates.

To mitigate the endogeneity problem, we employ the instruments suggested by Hoxby as well as the ones modified by Rothstein. Hoxby cleverly proposes using the

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<sup>3</sup> Hoxby also proposes using a Herfindahl index based on the land area of each school district. Our results are robust to the use of this alternate measure.

number of rivers and streams in a metropolitan area as instruments for school choice. Historically, these bodies of water limited student transportation to school and frequently delineated school district boundaries. Even today, these geographic characteristics correlate with the degree of school choice in a metropolitan area. We use Hoxby's (2000) and Rothstein's (2005) instruments of river and streams to determine whether their data differences affect the estimation results in this application.

Rothstein prepares a set of instruments that, though highly correlated with Hoxby's, are not identical. The main difference between the two instruments rests in how they count the rivers and streams in an MSA. Hoxby studied maps provided by the US Geologic Survey to generate a count of the number of large streams in an area; she then uses the USGS count of total streams to separate her total streams into large and small ones. Rothstein argues that Hoxby's measures are subjective and proposes two additional classifications of streams that are derivable from numerical data. The first separates the streams into intra-county streams and inter-county streams. Presumably, the streams that cross county lines will tend to be larger than those which do not. The second classification separates streams based on whether they exceed 3.5 miles. We present results using Hoxby's instrument and these two of Rothstein's proposed alternatives.<sup>4</sup>

To be valid instruments, crime must not be affected directly by rivers and streams. If streams are not exogenous to juvenile crime, they could affect it positively or negatively. Students in areas with many waterways may not be able to wander as far from home, which might help them stay out of trouble. On the other hand, when students misbehave, they may be easier to catch if there are more natural obstacles that keep them

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<sup>4</sup> Hoxby (2005) argues that Rothstein's streams measures ignore stream width and, thus, the navigability of streams.

from escaping. Rivers and streams might actually aid an escape, though, if they're able to mask scents or eliminate tracks. These opposing effects may cancel each other out.

Areas with greater Tiebout choice in public schools may also maintain more police departments. If police jurisdictions are correlated with school districts, then we may be capturing that the police competition (or cooperation) is lowering crime, not school competition. Adult criminals respond to difference in police jurisdictions' enforcement levels. When a local jurisdiction increases its enforcement of a particular crime, the rate of that crime falls in that jurisdiction; criminals substitute towards other jurisdictions, raising crime rates elsewhere (Rasmussen and Benson, 1993). To the extent that neighboring jurisdictions are unable to coordinate, negative spillovers may lead to higher than optimal levels of spending on criminal enforcement. Areas with many police jurisdictions may have crime levels that are too low.

These spillovers arise from the ability of criminals to move away from strict jurisdictions. This is less likely for students as children are unable to move away from their parents' home to another police jurisdiction because they find the police to be too strict. Selection may, in fact, move in the opposite direction: parents may move towards, rather than away from, strict police jurisdictions. If police jurisdictions also follow streams and rivers, then our school choice estimates will capture this effect of stricter police enforcement. We find the correlation between the number of police jurisdictions and public school choice to be modest, about 0.2411.

One possibility is that increased Tiebout choice increases the sorting of students by their propensity to commit crimes. Students likely to commit crimes may all end up in the same school district or districts. This isolation may make it easier for the police to

find and catch those juvenile offenders, raising arrest rates for these locally isolated criminals.

Schools may compete on their ability to curb student crime. On the other hand, schools may cooperate: if one school district becomes particularly good at addressing student crime issues, neighboring districts might recommend students to that district. Increased school choice could allow for more specialization by one district in dealing with crime, leading to lower levels of crime. Alternatively, schools may free-ride on the strict enforcement of neighboring school districts and not be strict enough. Any free-riding would reduce any negative effect of school choice on juvenile crime.

### III Data

We use student-level data from the National Educational Longitudinal Survey (NELS) and juvenile arrest data from the Uniform Crime Reports (UCR). The outcomes we consider are self-reported arrests, whether the student spent money on drugs or alcohol, whether the student got into a fight, and whether the student or the student's friends belong to a gang.

The UCR provide arrests for juveniles by county for 1990. The National Center for Health Statistics compiles juvenile population by age and by county from Census data for the same year (NCHS, 2004). We calculate arrest rates for PMSA's from county-level data. About 80 percent of juvenile arrests are of 14 to 17 year olds; we divide the total juvenile arrests in the covered counties by the population of 14 to 17 year olds in the covered counties.<sup>5</sup> A crosswalk of counties and PMSAs is derived from the Census

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<sup>5</sup> Dividing by the population of 10 to 17 years olds does not significantly affect the results. Only 2.35 percent of juvenile arrests are of children 10 or younger.

classification (US Census Bureau, 2001). We examine arrest rates for Part I and Part II offenses. Part I offenses are major violent and property crimes, such as murder and auto theft; Part II offenses are minor crimes, such as drug possession and driving under the influence.

Table 1 provides averages of juvenile outcomes for two representative cities: a city with a lot of Tiebout choice, San José, and one with no Tiebout choice, Miami. Students are asked whether in the first semester of the twelfth grade they have ever been arrested or gotten into a physical fight either at school or on the way to or from school. They are also asked how much of the money they make is spent on alcohol and on drugs; responses include none, some, or most of the money they earn. For alcohol and drugs, we generate an indicator variable equal to one if any of the student's money is spent on these illegal substances. Students are asked whether they belong to a gang as well as the number of their friends that belong to a gang (none, some, or all). The indicator variable for friends in gang equals one if any of the student's friends are in a gang.

Those crimes most committed by juveniles are assault, car theft, burglary, disorderly conduct, drug abuse (in particular, drug possession), larceny, liquor law violations, possession of stolen property, vandalism, and a residual all others category. Juveniles are also frequently picked up although not arrested for curfew violations and loitering, running away, or on suspicion.

#### IV Results

The first-stage regressions consist of estimating school choice in a PMSA as a function of its rivers and streams. We estimate the first-stage regressions for Hoxby's

measures of rivers and streams and for two of Rothstein's proposed alternatives. Table 2 presents the results of the first-stage regressions. The estimates are not identical to Hoxby (2000) or Rothstein (2005) because the number of students answering the arrest question in the NELS, for example, is smaller than the test score sample they use. In most cases, the instruments are not weak. The UCR regressions using Rothstein's inter- and intra-district streams have an F-statistic just less than 10; estimates using these instruments may suffer from a weak instruments problem and should be interpreted cautiously.

Table 3 presents the coefficient estimates of the effect of school choice on crime using the dependent variables from NELS. Each dependent variable is coded so that a negative coefficient implies more choice, less crime. Panel A contains OLS and IV estimates using Hoxby's instruments, sample, and covariates; panel B contains OLS and IV estimates using two sets of Rothstein's instruments, his sample, and covariates. The first set of IV estimates use the inter/intra-county separation of streams and rivers; the second row separates the streams based on their length.

The IV estimates are more negative than the OLS estimates confirming an upward bias arising from the endogeneity of Tiebout choice. The IV estimates are all negative and occasionally significant. An increase in Tiebout choice reduces the probability of gang membership by either the respondent or the respondent's friends. In one specification, Tiebout choice is associated with lower probability of arrest; in another with lower probability of fights.<sup>6</sup>

Table 4 shows the regression results for juvenile arrest rates for Index I crimes. Most estimates are negative. We find lower juvenile arrests rates for robbery, assault, burglary, and larceny in metropolitan areas with more Tiebout choice. These estimates

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<sup>6</sup> Probit estimates produce qualitatively similar results.

are large and statistically significant using Hoxby's specifications. The estimates are less significant using Rothstein's specifications although the point estimates are still large. For example, the IV estimate using Hoxby's specification implies that a one standard deviation increase in Tiebout choice decreases crime by 50 percent.

Tables 5a and 5b presents the estimates for Index II crime juvenile arrest rates. Tiebout choice has negative effects on most Index II crime arrest rates, although many of these estimates are not significant. Choice has positive and significant effects on stolen property, drug possession, drunkenness, and vagrancy. The Rothstein specification finds fewer measures of crime to be significant than the Hoxby specification.

Increased Tiebout choice appears to reduce serious crimes and increase crimes typically associated with bored teenagers like substance abuse, loitering, and shoplifting. These results seem surprising. However, we present several explanations for these effects. The first is that drug possession, drunkenness, and vagrancy are crimes that are likely to be committed on weekends. Gottfredson and Soul (2005) find that drug use and property crime are more prevalent on weekends, while crimes against persons are most common during and after school on weekdays. Dahl and DellaVigna (2006) also show that assaults are most common during the evenings and on Fridays and Saturdays. High-schoolers may be more closely supervised by schools and parents during the weekdays, and when they have more unsupervised time in the weekends, they will be more likely to commit these crimes. Possibly students subjected to stricter schools may be more likely to act irresponsibly in the weekend. The major crimes are less likely to be driven by lack of supervision or to respond as strongly to weekends. Luallen (2006) finds that days when teachers go on strikes increase mischievous and property crime, but decrease

violent crime. This suggests that violent crime might be more likely to be correlated with attendance, making it easier for schools to address it. Since students commit these crimes less when they are not at school, it may be easier to get them to commit them less when they are at school. We find suggestive evidence, presented in Table 6, consistent with Toma et al. (forthcoming), that students in metropolitan areas with more Tiebout choice are more likely to be in class.

The crimes that are most diminished by school choice—robbery, assault, burglary, and larceny—are crimes that are as feasible in school as they are outside it, unlike drunkenness. Since schools are more able to supervise the students while they are in school, one would expect these crimes to decrease. This explanation is supported by the findings of Jacob and Lefgren (2003) that property crimes committed by juveniles decrease by 14 percent when school is in session, while violent crimes increase by 28 percent.

An additional mechanism through which choice might decrease crime is by informing students of the real cost of crime. The message may get across for big crimes more clearly than for small ones, increasing the relative cost of big crimes and decreasing the relative cost of small ones. As a result, we find students substituting away from serious crime to misbehavior.

Overall, we find that the results using the two estimates suggest that increasing school choice has negative effect on most crimes, negative significant effects on some, and a positive significant effect on drunkenness.

## V Conclusion

The current arguments favoring increased school choice receive support from our study: competition leads to slightly lower rates of juvenile crime. The high rates of juvenile crime and bad schooling of inner cities may both be improved simultaneously by giving more school choice.

Like Rothstein (2005), we find that Hoxby's instruments are more likely to find effects of choice on our dependent variables than Rothstein's instruments. Although this does not answer the question over which set of instruments is the more appropriate one, it confirms that Rothstein's instruments tend to attenuate the effect of school choice. However, the effect of switching instruments is smaller on our dependent variables than on the ones in Hoxby (2000). We also provide some evidence that parents use factors other than academic outcomes when choosing school districts, consistent with Rothstein (2006). Further research could examine additional mechanisms that affect parents' choices of school districts.

Given the high levels of crime that affect many school districts in the US, and the fact that juvenile crime often imposes a life-long cost on the offenders, increasing school choice can have large benefits. Additional research can estimate the dollar amount of these benefits and compare them to the higher costs of creating more school districts.

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Table 1: Sample Crime Rates for San Jose and Miami

	High Tiebout Choice (San Jose)	Low Tiebout Choice (Miami)
<i>NELS</i>		
Arrested?	0.040	0.030
Fight?	0.155	0.101
Friend in gang?	0.467	0.061
In gang?	0.085	0.025
Any alcohol money?	0.131	0.163
Any drug money?	0.020	0.059
<i>UCR Rates per 100,000</i>		
Murder	2.74	39.08
Rape	10.94	63.51
Robbery	143.60	779.65
Assault	404.81	1012.17
Burglary	1255.45	1265.22
Larceny	3680.20	3556.29
Auto Theft	768.59	857.81
Arson	82.06	18.56
Other Assault	1751.89	926.20
Forgery	57.44	21.49
Fraud	38.29	52.76
Embezzlement	1.37	12.70
Stolen Property	497.81	229.60
Vandalism	1051.68	509.99
Weapons	530.63	282.35
Prostitution	0	17.59
Sex Offenses	105.30	103.56
Drug Abuse	782.27	757.18
Drug Sale	124.45	188.56
Drug Possession	657.81	565.68
Gambling	0	29.31
Family Offenses	2.74	0
DUI	139.49	32.24
Liquor Laws	542.94	54.71
Drunkenness	381.56	0
Disorderly Conduct	369.25	0
Vagrancy	34.19	0
All Other Crimes	1896.86	2934.91
Suspicion	0	0
Curfew	69.75	0
Runaway	195.57	0

Table 2: First Stage Estimates for Various Samples

	Hoxby		Rothstein			
	NELS	UCR	Inter-county		> 3.5 miles	
	NELS	UCR	NELS	UCR	NELS- 2	UCR
Larger Streams (100s)	-0.050 (2.27)	-0.063 (3.90)	0.215 (4.65)	0.132 (2.54)	0.163 (5.56)	0.125 (3.53)
Smaller Streams (100s)	0.127 (5.96)	0.116 (7.09)	0.030 (2.04)	0.021 (1.42)	0.020 (1.06)	-0.003 (0.14)
N	4356	161	3784	179	3784	179
F	17.82	25.12	27.93	9.86	35.42	12.52

The NELS samples use the sample covered by 12th grade arrest. For the NELS regressions, standard errors are clustered by PMSA.

Table 3: NELS results for 12th grade criminal behavior

	12th Grade Arrest	12th Grade Fight	In a Gang?	Friend in a Gang?	Spend any money on alcohol	Spend any money on drugs
<b>Panel A:</b>						
<i>OLS</i>						
Hoxby Choice	-0.0015 (0.06)	0.0596 (1.65)	-0.0593 (1.57)	-0.1082 (2.42)**	0.0786 (1.16)	0.0294 (1.04)
<i>IV</i>						
Hoxby Choice	-0.0699 (1.76)*	-0.0495 (0.64)	-0.1418 (1.88)*	-0.1765 (2.02)**	-0.1452 (0.92)	-0.0541 (1.00)
<b>Panel B:</b>						
<i>OLS</i>						
Rothstein Choice	-0.0790 (1.24)	-0.0754 (1.16)	-0.0983 (1.43)	-0.1634 (2.22)**	0.0301 (0.42)	-0.0134 (0.43)
<i>IV</i>						
Rothstein Choice	-0.1604 (1.54)	-0.2035 (1.73)*	-0.1652 (1.52)	-0.3180 (2.69)**	-0.0751 (0.45)	-0.0553 (0.87)
<i>IV-2</i>						
Rothstein Choice	-0.1378 (1.45)	-1.1842 (1.67)*	-0.1405 (1.36)	-0.2530 (2.28)**	-0.1143 (0.83)	-0.0487 (0.82)

Standard errors clustered by PMSA. t statistics in parentheses. About 4500-5000 observations (varies). \* significant at 10 % \*\* significant at 5%

Table 4: UCR Index I crime arrest rates

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Vehicle Theft
<i>OLS</i>							
Hoxby's Choice	0.680 (0.07)	4.967 (0.45)	-420.974 (3.48)**	-20.535 (0.16)	50.958 (0.23)	-1578.847 (1.96)*	197.1943 (1.21)
<i>IV</i>							
Hoxby's Choice	23.193 (0.71)	-16.771 (0.46)	-449.428 (2.63)**	-406.970 (2.14)**	-900.143 (2.09)**	-4413.175 (3.18)**	-556.894 (1.36)
<i>OLS</i>							
Rothstein's Choice	2.57 (0.26)	-6.73 (0.53)	63.99 (0.25)	56.79 (0.48)	-15.48 (0.07)	-1248.00 (1.86)*	234.10 (1.55)
<i>IV</i>							
Rothstein's Choice	-0.49 (0.01)	-30.57 (0.55)	-976.80 (1.10)	-510.05 (1.29)	-1363.83 (1.65)	-4498.75 (2.01)**	-632.91 (0.85)
<i>IV-2</i>							
Rothstein's Choice	29.55 (0.61)	22.22 (0.41)	-1122.05 (1.62)	-333.41 (1.10)	-471.07 (0.69)	-3036.67 (1.42)	-64.42 (0.09)

Population used is the population of 14 to 17 year olds. \* significant at 10 % \*\* significant at 5%

Table 5: UCR Index II crimes arrest rates

	Arson	Other Assault	Forgery	Fraud	Embezzle	Stolen	Vandalism	Weapons
<i>OLS</i>								
Hoxby's Choice	43.189 (2.30)**	-87.292 (0.19)	-32.378 (1.22)	-179.314 (2.18)**	-2.838 (0.32)	278.141 (2.01)**	39.957 (0.16)	13.651 (0.16)
<i>IV</i>								
Hoxby's Choice	30.827 (0.46)	-2537.752 (2.44)**	-123.241 (1.37)	99.558 (0.71)	22.511 (0.89)	564.390 (1.83)*	-1155.858 (2.34)**	-138.334 (0.89)
<i>OLS</i>								
Rothstein's Choice	38.26 (2.05)**	198.29 (0.64)	1.621 (0.07)	31.814 (0.27)	-2.069 (0.29)	314.206 (1.75)*	31.319 (0.13)	88.573 (1.10)
<i>IV</i>								
Rothstein's Choice	74.31 (0.70)	-2564.88 (1.45)	-228.90 (1.38)	96.77 (0.24)	39.71 (1.14)	-260.66 (0.41)	-2432.60 (2.40)**	-515.20 (1.53)
<i>IV-2</i>								
Rothstein's Choice	105.31 (1.16)	-2985.22 (1.67)*	-124.02 (1.44)	-139.82 (0.43)	40.42 (1.32)	26.65 (0.04)	-2085.21 (2.26)**	-285.98 (1.05)
	Prostitution	Sex Off.	Drug Abuse	Drug Sale	Drug Possess	Gambling	Family Off.	DUI
<i>OLS</i>								
Hoxby's Choice	-3.210 (0.53)	-26.974 (0.42)	100.984 (0.80)	-114.972 (1.26)	278.480 (3.52)**	-11.718 (1.13)	-0.319 (0.02)	12.949 (0.19)
<i>IV</i>								
Hoxby's Choice	-10.089 (0.57)	13.852 (0.10)	-126.962 (0.41)	-333.263 (1.55)	357.343 (1.73)*	-27.564 (0.73)	-9.698 (0.28)	171.856 (0.95)
<i>OLS</i>								
Rothstein's Choice	3.025 (0.45)	-14.168 (0.27)	379.251 (2.02)**	106.54 (0.75)	299.03 (3.62)**	4.61 (0.58)	-2.74 (0.20)	20.45 (0.33)
<i>IV</i>								
Rothstein's Choice	-13.13 (0.50)	-39.29 (0.21)	-847.76 (1.17)	-624.74 (1.20)	35.96 (0.11)	-21.55 (0.35)	-53.28 (0.99)	269.06 (0.96)
<i>IV-2</i>								
Rothstein's Choice	17.94 (0.84)	94.74 (0.52)	-532.74 (0.95)	-771.93 (1.52)	316.57 (1.26)	-38.25 (0.64)	-20.16 (0.37)	293.79 (1.21)

Population used is the population of 14 to 17 year olds. \* significant at 10 % \*\* significant at 5%

Table 5: UCR Index II crimes arrest rates

	Liquor Law	Drunk	Disorderly	Vagrancy	All Others	Suspicion	Curfew	Runaway
<i>OLS</i>								
Hoxby's Choice	-166.408 (0.44)	225.923 (3.19)**	-514.473 (1.18)	-47.487 (0.91)	-1974.438 (2.31)**	60.709 (1.20)	-553.059 (0.88)	357.619 (0.59)
<i>IV</i>								
Hoxby's Choice	-893.314 (1.30)	479.001 (2.69)**	-2328.188 (2.04)**	75.305 (1.71)*	-4096.386 (3.10)**	-39.590 (0.49)	-1726.907 (1.77)*	-833.691 (0.81)
<i>OLS</i>								
Rothstein's Choice	-484.17 (1.14)	154.95 (2.39)**	-495.12 (1.15)	-74.11 (1.43)	-616.24 (0.70)	49.36 (1.14)	-1125.20 (2.06)**	-43.03 (0.09)
<i>IV</i>								
Rothstein's Choice	-1275.17 (0.82)	657.44 (1.86)*	-3823.55 (1.72)*	207.59 (1.10)	-3994.93 (1.31)	-169.04 (0.81)	-1949.36 (1.06)	-312.61 (0.17)
<i>IV-2</i>								
Rothstein's Choice	-581.86 (0.40)	675.53 (2.13)**	-3041.59 (1.51)	150.53 (1.17)	-4270.75 (1.71)*	-248.71 (0.92)	-396.09 (0.21)	-83.40 (0.04)

Population used is the population of 14 to 17 year olds. \* significant at 10 % \*\* significant at 5%

Table 6: Tiebout Choice and Class Attendance

	(1)	(2)	(3)
	How Many Times during the first half of 12th grade were you/did you...		
	Late for School?	Skip Classes?	Miss School?
OLS - Hoxby	-0.148 (0.27)	-0.082 (0.19)	-0.638 (1.19)
IV - Hoxby	-0.410 (0.34)	-0.890 (0.88)	-1.457 (1.23)
OLS - Rothstein	0.187 (0.38)	-1.481 (1.49)	-1.951 (2.11)**
IV - Rothstein	-2.540 (1.76)*	-2.924 (1.85)*	-1.765 (1.15)
IV2 - Rothstein	-2.441 (1.57)	-2.959 (1.90)*	-1.347 (0.90)

Standard errors clustered by PMSA. t statistics in parentheses. About 4500-5000 observations (varies). \* significant at 10 % \*\* significant at 5%