MORE CHOICE, LESS CRIME*

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Abstract

Previous research debates whether public school choice improves students' academic outcomes, but there is little examination of its effects on their non-academic outcomes. We use data from a nationally representative sample of high school students, a previously developed Tiebout choice measure, and metropolitan-level data on teenage arrest rates to examine how public school choice affects students' propensity to be arrested or to join a gang. 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.

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I Introduction

The policy discussion on public education in the United States often centers on improving school quality by increasing competition. Several mechanisms used to achieve that goal are public school choice, charter schools, private schools, and vouchers. Economic theory posits that increased competition should improve both allocative and productive efficiency as the market penalizes poorly performing schools (Hoxby, 2000). 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).

School choice also appears to provide non-academic benefits. For example, in Chicago, students are allocated to public high schools via randomized lotteries. Cullen, Jacob, and Levitt (2006) 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. Catholic school students exhibit lower rates of some delinquent behaviors (Figlio and Ludwig, 2000), although these results are mixed (Mocan and Tekin, 2006). Private schools seem to improve non-academic outcomes for their students through their increased retention, better security, and a greater variety of activities outside the classroom (Figlio and Stone, 1999).

The largest source of choice for parents in educational markets comes, not through private schools, vouchers, or lotteries, but through the choice of a public school district made jointly with the choice of a residence (NCES, 2003). Unlike previous studies, 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 adolescents are lower in metropolitan areas with a greater degree of Tiebout school choice. Crime is not the only

outcome that could be affected by school choice: the possibilities range from academic outcomes to property values to teacher salaries. However, this study focuses on the adolescent crime rate.

The degree of Tiebout choice in an area may be affected by the success of its schools, both academically and non-academically. For example, school districts neighboring crime-ridden school districts may be less willing to consolidate, leading to more Tiebout choice. To correct for this potential endogeneity, we use instrumental variables techniques to estimate the effect of public school choice on adolescent criminal behavior. Hoxby (2000) 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 a lower propensity of gang membership, lower rates of cocaine use, and lower adolescent arrest rates in metropolitan areas with a greater degree of public school choice. Estimated effects are robust to differing instrument definitions and included controls. We propose some possible mechanisms for these non-academic gains to public school choice.

II School Choice and Adolescent Crime

In much of the United States, parents simultaneously choose their child's public school and their residence. Survey data suggest that, among parents sending their children to the school assigned by their residence, 54 percent considered school quality when making their location decision (Hoxby, 2001). Rothstein (2006) demonstrates that academic effectiveness alone does not explain parents' choice of school district. One explanation reconciling these two points is that parents care about dimensions of school quality other than academics: parents may also care

about schools' non-academic effectiveness, such as safer schools that improve behavior and citizenship. Parents may more easily observe non-academic measures, such as safety.

Schools teach non-cognitive skills as well as cognitive ones. Historically, public schools in the U.S. taught patriotism and the rights and responsibilities of citizenship (Ravitch, 2006) and aimed to produce compliant workers (Bowles and Gintis, 1976). Current school reforms often focus on implementing a paternalistic school culture that emphasizes middle-class values in behavior, comportment, and dress (Whitman, 2008). There is good reason for schools and parents to focus on non-academic factors: skills such as motivation and effort play important roles in success as an adult (Heckman, 2008; Babcock and Betts, 2009). An extreme, yet important, manifestation of these non-cognitive skills is understanding and respecting the legal standards of the community.

Schools facing greater competition through school choice enhance their students' cognitive *and* non-cognitive skills. Peterson et al. (2001) find that parents perceive safer school environments when their children enroll in private schools or receive scholarships. Figlio and Ludwig (2000) and Cullen, Jacob, and Levitt (2006) show better behavior resulting from Catholic school attendance and public school lotteries. Public schools also likely add value in non-cognitive ways, such as improving behavior and controlling crime. If, as argued by Hoxby (2000), increased Tiebout choice improves schools' academic performance then it may also improve their fostering of non-academic skills.

Schools may directly improve behavior through increased discipline or somewhat indirectly through better monitoring or enforcement of attendance. Geographically smaller school districts experience higher high school graduation rates (Greene and Winters, 2006); districts with fewer schools and schools with smaller enrollments experience higher average

daily attendance rates (Jones *et al.*, 2008). These findings are consistent with more Tiebout choice raising attendance. School attendance may have a direct effect on crime: keeping students in schools incapacitates potential criminals, significantly reducing crime (Luallen, 2006). Competition may encourage schools to better note students' absences and enforce rules more strongly.

Increased Tiebout choice may indirectly affect student behavior through increased educational attainment. Hoxby (2000) suggests that increased Tiebout choice raises educational attainment; this higher educational attainment may reduce adolescent crime (Lochner and Moretti, 2004). To the extent that increased Tiebout choice reduces crime through its effect on educational attainment, our estimates below also capture this indirect effect.

Tiebout choice and the resulting Tiebout sorting may affect adolescent behavior through peer effects. Students with a high propensity to commit crimes may locate near each other or in the same schools. Isolating criminogenic teenagers may increase crime rates in their school district through peer education as occurs in juvenile correctional facilities (Bayer, Hjalmarsson, and Pozen, 2009). However, it potentially reduces their negative influence on teenagers in other school districts, lowering crime rates. Separating out the criminogenic adolescents reduces their 'educational' impact on more law-abiding teenagers while allowing districts to foster other, more law-abiding identities a la Akerlof and Kranton (2000). On net, the result may be lower crime rates in the metropolitan area.

Tiebout (1956) choice refers to residential choice among jurisdictions that tax residents and provide public goods. The relevant measure of school choice is across districts, not across school attendance zones within a school district. School policy on such important issues as spending and discipline are typically set at the district, not at the school level. In addition, prior

research examining the effects of Tiebout choice relies on inter-district choice (Borland and Howsen, 1992 and 1996; Hoxby, 2000; Marlow, 1999; Zanzig, 1997, Hanushek and Rivkin, 2003; Blair and Staley, 1995).[1]

III. Estimation Methods

We estimate the effect of Tiebout choice on adolescent behavior using both individual-level data and metropolitan-level data. Using individual-level data, we estimate the following for student *i* in school district *j* in metropolitan area *m*:

$$Bad\ Behavior_{ijm} = \alpha_1\ school\ choice_m + X_{ijm}'\beta + W_{jm}'\delta + T_m'\gamma + e_{ijm}$$

We consider a variety of bad behaviors: whether the teenager was arrested; got in a fight; is in a gang; has a friend in a gang; and has consumed alcohol, binge drank, or used marijuana or cocaine in the previous year. We choose these behaviors as they reflect criminal activity on the part of teenagers and parallel the metropolitan-level measures discussed below.

Teenagers' bad behaviors are a function of Tiebout choice, teenager characteristics, school district characteristics, and metropolitan area characteristics.[2] 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 district ethnic homogeneity, 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,

[1] Metropolitan areas typically have similar levels of choice among schools while having significantly different amounts of choice among districts (Hoxby, 2000). Further, Rothstein (2006) notes that district boundaries are more fixed than school attendance boundaries.

[2] For the samples that include high-school dropouts, we include the characteristics of the last school they attended.

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. These measures of income, education, race, and their degrees of homogeneity in the district and MSA capture potential sorting across school districts along these measures. Including these measures reduces the degree to which the estimated effect of Tiebout choice arises from peer effects based on changes in sorting on observables. We also include the percent of the metropolitan-area population residing in each of the nine Census division dummies.[3]

We follow Hoxby (2000) in her choice of controls in regressions explaining student test scores. Rothstein (2007) criticizes the measurement of some of Hoxby's control variables. We estimate some regressions that follow Hoxby's work and others following Rothstein's. In the regressions where we follow Hoxby's work, we include her controls; in the ones where we use Rothstein's, his. [4] In particular, Rothstein prefers to use the logged average income while Hoxby argues that the averaged logged income behaves better econometrically. The differences due to the choice in control variables are relatively minor in their effect on the estimated results.

[3] For most metropolitan areas, these variables take on a value of 0 or 1. Only four of the 160 metropolitan areas in the sample overlap Census regions. For the Hoxby specifications, we assign each to the division in which most of the students appear: Cincinnati: 85% are in Division 3; Louisville, 97% are in division 3; Minneapolis/St. Paul 98% are in division 4; and Wilmington, 61% are in Division 5. For the Rothstein specifications, we include the percent of the population residing in each of the nine divisions.

[4] Rothstein also has concerns with Hoxby's instruments. We discuss these later in the paper.

They construct their specifications to explain students' academic achievement, while we seek to explain adolescents' bad behavior. The control variables from the Hoxby and Rothstein regressions include good predictors of an area's overall crime rates: income, education, and race. The regressions in this study add a Herfindahl index of police departments in the metropolitan area.[5] This variable is discussed in detail below.

The variable of interest is the choice index based on enrollment. We measure school choice in two ways. For the Hoxby-like specification, we 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.[6] For the Rothstein-like specifications we use eighth grade enrollment shares instead of enrollment in all grades to measure choice.[7]

We also estimate regressions using data on juvenile arrest rates for metropolitan area *m*, since these are not available at the student or school district level:

$$Arrest\ Rate_m = \alpha_2\, school\ choice_m + T_m'\theta + u_m$$

We consider juvenile arrest rates for property crime, violent crime, any major (Index I) crime, and any minor (Index II) crime. These are measured at the metropolitan level. For these regressions we include T_m, metropolitan-level control variables, including a police Herfindahl index that captures the choice between police departments in the metropolitan area, and the regional indicator variables.

[5] 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.

[6] 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.

[7] Including school size in these regressions does not affect our findings.

Endogeneity and Instrumental Variables

If adolescent misbehavior affects the degree of school choice, the above regressions may not estimate causal effects. 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 likely biases upward the estimated effect 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 (2000). Hoxby cleverly uses the 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. A count of large rivers and the count of small streams serve as Hoxby's instrumental variables for Tiebout choice. Rothstein (2007) proposes two additional classifications of streams derived from numerical data. The first separates streams into intra-county streams and inter-county streams; the second separates streams based on whether they exceed 3.5 miles. We use Hoxby's (2000) and these two of Rothstein's (2007) alternative instruments of river and streams to determine whether their choice of stream measurement affects the estimation results in this application.

To be valid instruments, rivers and streams must not directly affect juvenile crime and arrests. Many of these rivers and streams have ceased to be obstacles in everyday transportation; this mitigates the possibility that they affect juvenile crime. In some regressions we consider adolescent gang membership; it is unlikely that more watery locales correlate with gang membership. Clever stories can be told however: rivers and streams provide natural obstacles

that prevent adolescents from escaping or, alternatively, aid in their escape by masking scents or eliminating tracks. To the extent these stories significantly explain behavior, rivers and streams should also be correlated with adult crime rates. To test this, we estimate a regression of adult arrest rates on each of these three sets of rivers and streams variables. The coefficients on the rivers and streams, holding constant the other measures of metropolitan-level characteristics, are not significant in two out of the three measures of rivers and streams. The estimate is significant only in Rothstein's count of rivers as those longer than five miles and streams as those shorter than five miles; these are also the set of instruments that prove weakest in the first stage in the analysis below. Hoxby (2000) provides further evidence of exogeneity: the number of streams is not correlated with industrial composition or commuting time in a metropolitan area and the number of streams does not directly affect schools' academic productivity.

Police departments and Adolescent Misbehavior

Although we follow Hoxby's and Rothstein's specifications, we recognize that additional factors may influence adolescent crime. To bias our instrumental variables estimates on school choice, an omitted variable would have to be correlated with the instrumental variables and affect adolescent misbehavior. A concern is that police departments experience a similar degree of Tiebout choice as schools and that the Tiebout choice in police agencies influences the effectiveness of their enforcement. We include the degree of Tiebout choice over police departments in the metropolitan area. The included measure is one minus the Herfindahl index of concentration based on population covered by police departments in the metropolitan area. Tiebout choice in police departments is highly correlated ($\rho = 0.68$) with the degree of school choice in a metropolitan area.

Prior research on whether police agency choice affects adolescent misbehavior is limited. Wheaton (2006) finds that metropolitan areas with more police departments experience lower crime rates. To focus on *adolescent* misbehavior, we are mostly constrained to measuring arrests not crimes, and arrest rates reflect both adolescent criminality and police effectiveness. An increase in choice among police agencies may positively or negatively affect adolescent arrest rates. When a local jurisdiction increases its enforcement of a particular crime, criminals substitute towards other jurisdictions, raising crime rates elsewhere (Rasmussen and Benson, 1993). If an agency raises its level of enforcement, it pushes neighboring police agencies to increase their enforcement. An increase in police choice could lead to more enforcement and less crime. On the other hand, Cordner (1989) suggests that, within metropolitan areas, larger police agencies solve more of their cases than smaller police agencies. Since, *ceteris paribus*, larger police departments in a city imply less Tiebout choice, Cordner's evidence suggests that MSAs with more Tiebout choice in police agencies may provide less effective services.

If police agencies are less willing to consolidate with more criminogenic neighboring agencies, the degree of police concentration is endogenous to adolescent arrest rates. The instrumental variables estimates are overidentified; we could use the additional instrumental variable to instrument for the police Herfindahl index. However, the instruments are significantly weakened in this specification.[8] For this reason, we do not report these estimates although they are qualitatively similar to the ones reported. Including an endogeneous variable potentially biases the estimates on all of the included variables; we trade off this endogeneity bias for an omitted variables bias. The estimates are qualitatively similar if we exclude police choice.

[8] The F-test for the significance of the excluded instruments range from 1.33 to 10.49.

III Data

To examine how Tiebout choice affects adolescent misbehavior we require data on juvenile non-academic outcomes and the degree of public school choice. We use student-level data from the National Educational Longitudinal Survey (NELS) and juvenile arrest data from the Uniform Crime Reports (UCR). The NELS is a nationally representative sample of eighth graders in 1988; we use the second follow-up survey in 1992. The NELS surveys both students still in school and ones who dropped out of school.

From the NELS, we use teenager self-reports on various criminal behaviors. Students were asked whether, in the first semester of the school year, they were ever arrested or got in a fight. These two questions exclude students who dropped out before 12^{th} grade. The remaining questions are asked to both current students and dropouts. Respondents are asked whether they belong to a gang as well as how many of their friends belong to a gang (none, some, or all). The indicator variable for "friends in gang" equals one if any of the adolescent's friends are in a gang. They are also asked if they have consumed alcohol in the past year and, if so, whether they binge drank. Finally, they are asked whether they have consumed marijuana or cocaine in the past year.

The upper part of Table 1 provides averages of juvenile outcomes for metropolitan areas above and below the median amount of choice. For most behaviors, we observe no statistically significant difference in the means in the outcomes between the higher choice and lower choice metropolitan areas. The exceptions are that students in higher choice areas are more likely to get arrested and consume drugs and alcohol. However, metropolitan areas with less choice have higher rates of juvenile arrests for violent and Index II crimes.

The UCR provide arrest counts 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 using the population of 14 to 17 year olds in the covered counties and a crosswalk derived from the Census classification (US Census Bureau, 2001).[9] 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.[10]

The lower part of Table 1 lists the average arrest rates for above and below median choice areas. The mean arrest rate is higher for low choice areas in three of the five crime categories considered, though these differences are not significant. The aggregate picture suggests that there are *not* significant adolescent crime differences among jurisdictions with varying degrees of Tiebout choice. We explore this further in the section below with multivariate regression analysis.

IV Results

The regression analysis employs Two-Stage Least Squares. 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

[9]About 80 percent of juvenile arrests are of 14 to 17 year olds. 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.

[10] We are unable to calculate crime rates for PMSA's in New England, as they do not match up to county borders. To keep a comparable sample in the NELS regressions, we also eliminate the New England observations from that data set, which removes 38 observations from the Hoxby regressions and 43 from the Rothstein ones.

estimates are not identical to Hoxby (2000) or Rothstein (2007) because the number of students answering the arrest question in the NELS, for example, is smaller than the test score sample they use. In the Hoxby cases, the instruments are not weak. However, in the Rothstein cases, all but the first UCR regression have an F-statistic substantially less than 10. Estimates using these instruments may suffer from a weak instruments problem and should be interpreted cautiously.[11] Overidentification tests show that we fail to reject the null hypothesis that the instruments satisfy the orthogonality conditions, supporting the argued exogeneity of the instrumental variables.

Table 3 presents the coefficient estimates of the effect of school choice on crime using the student self-reports 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 Panel B IV estimates use the inter- and intra-county separation of streams and rivers; the second separates the streams based on their length.

The IV estimates tend to be more negative than the OLS estimates confirming an upward bias arising from the endogeneity of Tiebout choice. Specifications following Hoxby are almost all negative, although only significant in two cases. With Hoxby's instruments and sample, an increase in Tiebout choice reduces the probability of gang membership and of cocaine use.[12] Specifications following Rothstein are more mixed. The IV estimates are more negative than the OLS estimates about two-thirds of the time. The estimates are never significant; two-thirds are

[11] We include these specifications even though the instruments are weak both for completeness and, potentially, to shed further light on the debate over specification between Caroline Hoxby and Jesse Rothstein.

[12] Probit and ivprobit estimates produce qualitatively similar results and are available upon request.

negative. As in the Hoxby/Rothstein discussion, the modifications that Rothstein suggests weaken the power on the instruments in the first stage; the result is imprecisely estimated effects in the second stage. The estimated effects are, however, large. The mean 12^{th} grade arrest probability in NELS is 0.032 with an estimated coefficient on choice of -0.055. A one standard deviation increase in choice (about 0.25) reduces the arrest probability by 43 percent for 12^{th} graders.

To avoid cluttering the tables, we do not report estimated coefficients on the control variables. However, coefficients on these variables are consistent with prior work. Girls are less likely to misbehave than boys. White and Asian children whose parents are more educated are less likely to misbehave (Mocan and Rees, 2005). Metropolitan areas with a higher rate of college-educated adults have lower crime rates among youth. Most of the coefficients on the control variables are statistically insignificant and small.

Table 4 shows the regression results for juvenile arrest rates for the aggregated crime categories. The estimates are negative and generally statistically significant. These effects are also large: a one standard deviation increase in Tiebout choice, at the mean, reduces the total arrest rate by about 40 percent, the Index 1 crime arrest rate by about 41 percent, and the violent crime arrest rate by 25 percent. The Rothstein and Hoxby specifications produce estimated effects that are comparable in magnitude. Further, the UCR results are consistent with the magnitude of the estimated effects of Tiebout choice on self-reported arrests.

Estimated coefficients on the control variables in this regression are, for the most part, again small and insignificant. However, the regression results suggest that MSAs with larger populations are associated with higher rates of juvenile crime. As in the regressions in Table 3, populations with a higher rate of college-educated adults are correlated with lower crime rates in

youth. Finally, a higher percentage of Asian population and a higher level of income are also associated with lower crime among youth.

Overall, we find that estimates using NELS consistently generate large, negative, but not significant effects of school choice on students' misbehavior. Using the overall juvenile arrest rates for an MSA, we find a large, negative, and statistically significant effect of school choice on juvenile arrest rates. These estimates suggest that a one standard deviation increase in Tiebout choice reduces arrests by 25 to 40 percent.

These estimates include both an indirect effect through increased educational attainment as well as a direct effect through improved school productivity. Hoxby (2000) finds that a one standard deviation increase in Tiebout choice raises educational attainment by about 0.4 years. Lochner and Moretti (2004) suggest that a one year increase in educational attainment reduces the probability of arrest by about 16 percent. Focusing on this indirect effect, a back-of-the envelope calculation suggests that a one standard deviation increase in Tiebout choice reduces the probability of arrest by about 6 percent.

Tiebout choice may influence adolescent misbehavior through peer effects. To gauge the magnitude of the influence of peers, we use the NELS data to estimate regressions including the proportion of students in student *i*'s school district, other than student *i*, who report that misbehavior (for example, in the 12th grade arrest regressions, those students reporting being arrested). Including the peer misbehavior variable typically reduces the magnitude of the estimated coefficient on Tiebout choice by 10 to 50 percent.[13] Changing peer effects from increased Tiebout choice may account for a significant fraction of the reduction in misbehavior.

A one standard deviation increase in Tiebout choice reduces arrests by 25 to 40 percent. The indirect effect of increased educational attainment accounts for 15 to 24 percent of the

[13] Results available upon request.

decline in arrests. Peer effects may account for another 10 to 50 percent of the decline.[14] We attribute the remaining 25 to 75 percent of the decline to schools' increasing attendance or improving productive efficiency.

V. Comparing Aggregate Results to Individual-Level results

Although the aggregate estimates appear significantly larger, the magnitudes in the regressions that use the UCR and those that use the NELS are similar. Both suggest that a one standard deviation increase in Tiebout choice leads to a 25 to 40 percent reduction in arrest rates. The results using the aggregated city-level data, however, are much more statistically significant than those using self-reports. For the Rothstein specifications this may be due to the greater strength of the instruments in the aggregate data than in the individual-level data. This section proposes some additional explanations, focusing in particularly on arrests since this measure is more easily comparable in both data sets; the mechanisms that we discuss regarding arrests likely also apply to the other misbehaviors.

Both Tiebout choice and the instrumental variables are measured at the MSA-level. Standard errors are clustered by MSA. The clustering accounts for the aggregate nature of the control and instrumental variables as well as potential correlations in the disturbances within MSAs. As a check, we construct an MSA-level data set from the NELS. As expected, estimates using the aggregated data are similar to those using the individual-level data with clustered standard errors.

Tiebout choice may affect the frequency of misbehavior as well as the decision to misbehave. NELS asks whether a student was arrested in the past semester, but not how many times the student was arrested. The estimates in Table 3 only reflect the extensive effect of

[14] Although note that some part of the peer effect may also be reflected in the increased educational attainment

choice on misbehavior. Arrest rates in the UCR, however, reflect frequency of arrest as well as whether a juvenile was ever arrested; the estimates in Table 4 reflect effects on the extensive as well as the intensive margins. The reporting differences may be exacerbated by the skewness in the distribution of arrests.[15]

The distribution of crimes across criminals is highly skewed. Estimates of the mean number of crimes committed per year range from 60 to 180; estimates of the median number of crimes committed range from 12 to 15 (Marvell and Moody, 1994). This does not necessarily imply that arrests are similarly skewed. Frequent offenders, for example, may be especially successful at avoiding arrest. However, a study of recidivism finds that 272,111 offenders released from prison in 1994 were responsible for 4.8 million arrests in the three years following their release (Langan and Levin, 2002). 32.5 percent were not rearrested within 3 years; the remaining 67.5 percent of convicted criminals accumulated, on average, 26 arrests in 3 years.[16] These figures suggest that arrests are also highly skewed, implying that the individual-level and metropolitan-level estimates reflect average effects of choice on crime for different margins.

In addition to mis-measurement arising from not observing the frequency of arrest, individuals may not truthfully reveal their police record. Self-reported arrests contain more measurement error than police recorded arrests. For example, Huizinga and Elliott (1986) cite estimates of only 60 to 80 percent of surveyed individuals reporting their police involvement. Measurement error in a limited dependent variable presents distinct econometric difficulties. Hausman, Abrevaya, and Scott-Morton (1998) show that measurement error in a binary dependent

[15] To examine this question, we estimate the individual-level regression limiting the sample to those students most likely to be involved in misbehaviors: minorities from low-income families who live in metropolitan areas with high crime rates. In these regressions, more self-reported measures of crime are significant, and all the coefficient estimates are larger.

[16] Although the effective time available to commit a crime and be arrested is somewhat less than 3 years as about a half of these released prisoners were back in prison within 3 years. Those previously arrested for property crimes had higher recidivism rates than those previously arrested for violent crimes.

variable leads to probit coefficient estimates that are inconsistent towards zero. Further, the standard errors from a probit model tend to be too small. In Hausman *et al.*'s simulations, as the amount of measurement error increases, statistical significance tends to decrease. In our data, the individual-level data likely contain more measurement error, potentially explaining the reduced statistical significance in the NELS estimates.

VI Conclusion and Discussion

Our study supports the current arguments favoring increased school choice: competition leads to lower rates of juvenile crime. The estimated effects are large: a one standard deviation increase in Tiebout choice reduces juvenile arrest rates by 25 to 40 percent. This is consistent with evidence that other forms of school choice improve student behavior such as Cullen, Jacob, and Levitt (2006), and Figlio and Ludwig (2000). Adolescents account for about 15 percent of all arrests, so a reduction in adoloscent crime has a significant impact in the overall crime rate.[17] The high rates of juvenile crime and poor schooling in inner cities may both be improved by providing more school choice.

Like Rothstein (2007), we observe that Hoxby's instruments are more likely to find effects of Tiebout choice 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 attenuate the effect of school choice. The effect of switching instruments is, however, smaller on our dependent variables than on the measures of academic outcomes in Hoxby (2000).

Choice might decrease crime by encouraging schools to inform students of the real cost of crime, improving student behavior directly or through peer effects, or increasing attendance.

[17] Authors' calculation using the UCR.

Toma et al. (2008) find that students in metropolitan areas with more Tiebout choice are more likely to be in class. When school is in session, juveniles commit 14 percent fewer property crimes and 28 percent more violent crimes (Jacob and Lefgren, 2003). When teachers go on strike, mischievous and property crime rates by students increase, but violent crime decreases (Luallen, 2006). This further suggests a role of schools in reducing adolescent property and violent crime: school choice leads to higher attendance and attending school incapacitates potential criminals.

The relationship between school choice and adolescent misbehavior also suggests that parents use factors other than academic outcomes when choosing school districts, consistent with Rothstein (2006). Our results imply that students moving from low- to high-choice MSAs would improve their behavior. Further research could examine additional mechanisms affecting parents' choice of school districts.

The number of school districts in the U.S. declined dramatically from just over 117,000 in 1940 to less than 14,000 today (NCES, 2009). Our results suggest that the resulting decline in competition likely led to more poorly behaved adolescents. Given the high levels of crime that affect many school districts in the US and that juvenile crime often imposes a lifelong cost on the offenders, increasing school choice can have large benefits. Breaking apart school districts, although politically difficult (Brasington, 1999), could benefit the community at large. Additional research can estimate the dollar amount of these benefits and compare them to the higher administrative costs of creating more school districts.

VI. Data Appendix

The data used in this paper, like the data in Hoxby (2000) and Rothstein (2005), are available under restricted use from the National Center for Education Statistics (NCES). To generate the data used in our first set of regressions, where the students' responses to survey questions are the dependent variables, we combined the restricted access version of the National Educational Longitudinal Survey (NELS 88/94), which includes the questions used from the base year through the third follow-up, with a second disc including additional data compiled by Hoxby and also available through the NCES. More information is available here: http://www.nces.ed.gov/pubsearch/pubsinfo.asp?pubid=96130. Finally, these are merged with Rothstein's additional measures according to the instructions which are available on his website (http://www.princeton.edu/~jrothst/replication/hoxbydocumentation/index.html).

The second set of regressions uses characteristics of juvenile crime in Metropolitan Statistical Areas as dependent variables. The measures of juvenile crime are calculated using Uniform Crime Reports (UCR) data, which generates the number of each crime committed by each age group in each county and year. UCR is tabulated annually and made publicly available from the FBI. We obtained the juvenile population for each metropolitan area from data posted by the Office of Juvenile Justice and Delinquency Prevention (http://www.ojjdp.ncjrs.gov/ojstatbb/ezapop/). In addition, we use data from the Law Enforcement Management and Administrative Statistics from 1990 to count the number of police jurisdictions in each MSA. These data are made available by the ICPSR, Study No. 9749.

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Table 1: Sample Crime Rates for Lower and Higher Choice Cities

	More Choice than Median	Less Choice than Median		Full Sample
NELS				
Arrested 12th grade?	0.040	0.031	**	0.035
Fight in 12th grade?	0.209	0.187		0.209
Friend in gang?	0.135	0.137		0.136
In gang?	0.033	0.042		0.039
Consumed any alcohol?	0.790	0.745	**	0.764
Binge drank?	0.360	0.322	**	0.338
Consumed marijuana?	0.229	0.169	**	0.195
Consumed cocaine?	0.044	0.021	**	0.031
UCR Juvenile Arrest Rates per 100,000				
Total Arrests	18022.08	19054.97		18033.74
Index I	6203.71	6547.53		6334.33
Violent	941.07	1260.9	**	704.49
Property	5262.64	5286.63		5629.84
Index II	11818.37	12507.44	**	12053.68

The asterisks denote whether the means in the areas with higher choice are significantly different from the means of areas with lower choice. * significant at 5% ** significant at 1%

Table 2: First Stage Estimates for Various Samples

	Hoxby		Rothstein			
			Inter-county		> 3.5 miles	
	NELS	UCR	NELS	UCR	NELS	UCR
Larger Streams (100s)	-0.0006**	-0.0004**	0.0014**	0.0013**	0.0012	0.0006**
	(2.95)	(2.78)	(2.32)	(4.08)	(1.93)	(2.39)
Smaller Streams (100s)	0.0011**	0.0009**	0.0002	0.0001	0.0002	0.0001
	(5.06)	(5.62)	(1.36)	(0.56)	(0.90)	(0.69)
N	4184	160	3298	160	3298	160
F	17.82	16.75	3.92	12.37	2.77	7.42
J-statistic	1.889	2.474	0.229	1.265	0.463	0.001
P-value	0.1693	0.1157	0.6321	0.2607	0.4961	0.9789

The NELS samples use the sample covered by friend in gang. For the NELS regressions, standard errors are clustered by PMSA. We report only the estimates on the instrumental variables.

Table 3: NELS results for self-reported criminal behavior

	12th Grade Arrest	12th Grade Fight	In a Gang?	Friend in a Gang?	Consumed alcohol in last year?	Binge drank in last year?	Marijuana in last year?	Cocaine in last year?
Panel A: following Hoxby								
OLS								
Tiebout Choice	-0.007	0.06891	-0.042	-0.094+	-0.01	-0.046	0.039	-0.007
	(0.22)	(1.48)	(1.03)	(1.92)	(0.16)	(0.72)	(0.75)	(0.33)
IV								
Tiebout Choice	-0.055	-0.0993	-0.128+	-0.15	-0.049	-0.319	0.005	-0.201+
	(1.23)	(0.84)	(1.77)	(1.37)	(0.31)	(1.56)	(0.04)	(1.88)
Panel B: following Rothstein								
OLS								
Tiebout Choice	-0.012	0.06392	0.016	-0.034	-0.104	-0.017	-0.048	-0.029
	(0.41)	(1.42)	(0.63)	(0.71)	(1.20)	(0.24)	(0.93)	(1.31)
IV								
Tiebout Choice	-0.335	-0.0163	0.01	-0.151	-0.632	0.307	-0.236	-0.016
	(1.56)	(0.06)	(0.10)	(0.65)	(1.43)	(0.74)	(0.74)	(0.17)
IV-2								
Tiebout Choice	-0.257	0.11586	-0.063	-0.181	-0.756	0.078	-0.091	-0.048
	(1.32)	(0.39)	(0.58)	(0.77)	(1.58)	(0.22)	(0.35)	(0.40)

Standard errors clustered by PMSA. t statistics in parentheses. About 3900-4500 observations (varies). Individual-level, district-level, and metropolitan area-level control variables included as described in the text, although coefficients are suppressed. + significant at 10 % * significant at 5% ** significant at 1%

Table 4: UCR crime arrest rates and Tiebout Choice

	Index I	Violent	Property	Index II	Total Arrests
Panel A: following Hoxby					
OLS					
Tiebout Choice	-2700.749*	-365.272+	-2335.477+	-3183.763	-5884.511
	(1.98)	(1.80)	(1.89)	(1.08)	(1.46)
IV					
Tiebout Choice	-9735.52**	-983.636*	-8751.884**	-19179.0*	-28914.5**
	(3.22)	(2.36)	(3.19)	(2.36)	(2.82)
Panel B: following Rothstein					
OLS					
Tiebout Choice	-2380.711+	-314.857	-2065.854+	-1961.960	-4342.670
	(1.94)	(1.47)	(1.88)	(0.79)	(1.29)
IV					
Tiebout Choice	-11832.7**	-998.737	-10834.0**	-17144.8	-28977.6*
	(2.81)	(1.50)	(2.83)	(1.62)	(2.09)
IV-2					
Tiebout Choice	-11029.9*	-1372.24*	-9657.641*	-24801.5+	-35831.400*
	(2.31)	(2.11)	(2.13)	(1.87)	(2.09)

Population used is the population of 14 to 17 year olds. There are 160 observations in each regression. Regressions include metropolitan-area-level control variables as described in text although coefficients are suppressed. + significant at 10 % * significant at 5% ** significant at 1%