Juvenile Incarceration & Adult Outcomes:
Evidence from Randomly-Assigned Judges

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February 2011

Abstract:

Approximately 100,000 youths are currently incarcerated in the US, yet little is known whether such a penalty deters future crime or interrupts human capital formation in a way that increases the likelihood of later criminal behavior. This paper uses the incarceration tendency of randomly-assigned judges as an instrumental variable to estimate causal effects of juvenile incarceration on adult recidivism. Using nearly 100,000 juvenile offenders over a ten-year period from an urban county in the U.S., the estimates suggest large increases in the likelihood of adult incarceration for those who were incarcerated as a juvenile. Marginal treatment effect estimates show that these results are found across a relatively wide range of judges, which adds confidence that our estimates are uncontaminated by selection into juvenile detention.

This paper is preliminary. Comments welcome.

I. Introduction

Crime is a social problem with enormous costs. At the end of 2007, nearly 2.3 million

people were incarcerated in the U.S.—an incarceration rate that is roughly ten times those of

other countries with similar crime rates (Mauer, 1999). The costs associated with this

incarceration, as well as costs to victims of crime, are estimated to be over $450 billion each year

(NIJ, 1996). Meanwhile, private expenditures that aim to prevent the externalities associated

with crime are of a similar magnitude.

An economic model of crime suggests that a source of these costs associated with criminal

activity is a low reservation wage in the legal sector among offenders (Becker, 1968).[1] A

growing body of empirical research has sought to test the implications of this model by

estimating the impact of incarceration on future employment, earnings and criminal activity. In

general, researchers have found that incarceration has minimal impact on future labor market

employment and earnings and mixed results with respect to recidivism. However, the focus of

most of the existing work is adult offenders. We argue that these null effects may not apply to

juveniles, whose incarceration rates have increased at rates even higher than those of adults over

the last 20 years.[2] In a life-cycle context, incarceration during adolescence may interrupt human

and social capital accumulation at a critical moment. More generally, interventions during

childhood are thought to have greater impacts than interventions for young adults due to

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[1] Criminal activity has received considerable attention from economists following Becker (1968). Recent papers
and reviews include Levitt (1997); Freeman (1999); Glaeser and Sacerdote (1999); Jacob and Lefgren (2003); Di
Tella and Schargrodsky (2004); Lee and McCrary (2005); Lochner and Moretti (2004), among others.
[2] In 2003, 96,655 juvenile offenders were incarcerated in the US (Snyder and Sickmund, 2006), a rate of 2.3 per
1,000 aged 10-19, or 1.2 per 1,000 aged 0-19.

propagation effects (see, for example, Cuhna et al., 2006), and criminal activity is a particularly important context to consider such effects.[3]

To consider whether juvenile incarceration deters future crime or interrupts human capital formation in a way that makes adult crime more likely, this paper estimates causal effects of juvenile incarceration on the likelihood of adult incarceration. This is part of a larger project that will include estimates of the effects of juvenile incarceration on high school graduation and employment and earnings, as well. Estimation of a causal relationship between juvenile incarceration and adult outcomes is complicated by the fact that those who are incarcerated are more likely to be disadvantaged and thus more likely to suffer worse adult outcomes independent of their juvenile incarceration status. This would bias OLS estimates of the relationship between juvenile incarceration and adult incarceration upwards.

The identification strategy exploits plausibly exogenous variation in juvenile detention stemming from the random assignment of cases to judges who vary in their sentencing severity. This strategy is similar to that used by Kling (2006) to estimate the impact of length of sentence on adult labor market outcomes but in a context of juvenile offending where long term effects may well be greater. We find that judge's incarceration propensity in other cases is highly predictive of whether a juvenile is incarcerated despite similar observable case characteristics across judges. This suggests that a judge's propensity to incarcerate can be used as an instrument for an individual juvenile's incarceration status to estimate causal effects of juvenile incarceration on adult outcomes.

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[3] When considering the determinants of criminal activity dominated by young adults, large effects of juvenile interventions are plausible. See, for example, Currie and Tekin (2006) and Doyle (2008).

The analysis uses administrative data for nearly 100,000 juveniles over 15 years who came before a juvenile court in a large, urban county in the U.S. These data were linked to adult incarceration data in the same state to investigate whether the juvenile offenders are later found in adult prisons. We find that in OLS regressions, juvenile incarceration is positively correlated with recidivism: those incarcerated as a juvenile are 3 times more likely to enter adult prison later in life compared to those who were not incarcerated. When we instrument for juvenile incarceration, the estimate is even larger, although not statistically significantly different from the OLS estimate. The main IV estimate and subgroup analyses suggest that marginal cases are at particularly high risk of adult incarceration as a result of juvenile custody. The results are also consistent with the idea that the timing of incarceration matters: the strongest results are for juveniles whose initial cases are between the ages of 15 and 17 years old—a critical period of adolescence when incarceration may lead to an end to high school education.

The rest of the paper is organized as follows: in section II we summarize the existing theoretical and empirical literature on the relationship between incarceration and future outcomes and provide background on judge assignment in our context, in section III we describe the empirical framework, including the use of a continuous instrument to estimate marginal treatment effects; in section IV we describe the data and how the instrument is constructed; section V presents the results; and section VI offers interpretation and conclusions.

II. Background

A.   A Model of Crime

In an economic model of crime, criminal activity and participation in the legitimate market are substitutes (Becker, 1968). In deciding whether to commit a crime, individuals weigh the net

gains of criminal versus legal labor market activity on the basis of the expected utility to be gained from each. The net gains of criminal activity are a function of the monetary rewards, the probability of being caught, and the severity of sentence. Net gains of participation in the legal sector are largely a function of wages. In this paper we take the individual's decision to commit a crime as a juvenile as given and instead focus on how incarceration as a juvenile might influence adult criminal behavior and legitimate labor market participation.[4]

We argue that juvenile incarceration can affect future criminal activity and labor market participation by reducing expected market wages via two potential channels. First, incarceration can reduce human capital accumulation, thereby reducing productivity and market wages. This mechanism is particularly relevant for juveniles as incarceration during adolescence may interrupt high school completion (Samson and Laub, 1993, 1997), and previous work has already linked failure to complete high school with lower future earnings and increased criminal activity (Lochner and Moretti, 2004). Second, incarceration can encourage the accumulation of "criminal capital" (see Bayer, Hjalmarsson, and Pozen, forthcoming) and hinder the accumulation of social capital that can aid in job search, lowering the probability of employment (Granovetter, 1995).[5] If juvenile detention deters future criminal activity, then we would expect a reduction in the likelihood that an incarcerated juvenile is found in adult prison later in life. If such a detention provides greater incentives to continue criminal behavior, then we expect to find an increase the likelihood of adult incarceration.

B. Previous Empirical Work

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[4] This is consistent with myopic behavior on the part of juveniles as suggested by a nearly perfect inelasticity to penalty severity at age 18 (Lee and McCrary, 2006). Further, if we apply the economic model of crime to the decision to commit a crime as a juvenile, it can explain the negative, but not necessarily causal, relationship observed between criminal activity and wages: individuals choose to engage in crime because their market wages will be relatively low.

[5] In the criminology literature this is often referred to as deviant labeling (see Bernberg and Krohn, 2003).

Existing empirical research on this topic focuses mostly on adults and falls into two general categories: 1) the relationship between incarceration and recidivism and 2) the relationship between incarceration and labor market outcomes. According to the economic model of crime, however, these two questions are related given the substitutability of labor market participation and criminal activity.

The main challenge inherent in estimating the causal impact of incarceration on recidivism or labor market outcomes is to control or otherwise account for the influence of individual characteristics that may jointly influence incarceration and future criminal activity and labor market outcomes (e.g., greater disadvantage including lower levels of education and less self control). The existing research on recidivism has been conducted almost exclusively by criminologists, with mixed results. Some work finds that incarceration increases recidivism (Spohn and Holleran, 2002; Berbury, Krohn and Rivera, 2006), others that it has no effect (Gottfredson, 1999; Smith and Akers, 1993), and still other work finds that it reduces recidivism (Murray and Cox, 1979 and Brenna and Mednick, 1994). However, this work attempts to deal with the potential endogeneity of incarceration by controlling for a limited set of observable characteristics.

The existing literature on the impact of male adult incarceration on labor market outcomes, summarized by Western, Kling and Weiman (2001), makes greater attempts to deal with the potential endogeneity of incarceration. This literature often relies on either comparisons between those who have and have not been to jail and including extensive background controls or panel datasets that enable one to compare earnings before and after a spell of incarceration. Examples of the former include Freeman (1992) and Western and Beckett (1999). Both find that men who have been incarcerated have lower levels of employment compared with those who have not

been incarcerated controlling for an extensive set of observable characteristics. This "selection on observables" strategy is subject to the criticism that individuals who have been incarcerated may differ on unobservable characteristics that might bias the estimates.

Examples of research on male adult incarceration following the latter strategy include Lott (1992a, 1992b), Waldfogel (1994), Grogger (1995). Using employment data before and after release from prison, Lott (1992a and 1992b) finds that prison sentence length does not affect income for those convicted of drug offenses, but does for those convicted of fraud/embezzlement. His sample is limited to those who spent at least some time in prison. Waldfogel (1994) compares outcomes for those who were convicted of fraud and larceny but spent no time in prison with those who were convicted and did spend time in prison and finds that the latter had lower rates of employment and income after the period of incarceration. Grogger (1995) examines the impact of arrests on labor market participation and earnings using a unique panel dataset of men arrested in California sometime after their eighteenth birthday. When he limits his comparison to the earnings and employment of individuals before and after arrest, he finds only a modest effect of being arrested on earnings and employment. He finds a slightly larger effect for incarceration, but still relatively small. Together these results suggest that incarceration has a small causal impact on the labor market earnings and employment of adult men. One limitation of this approach is the assumption that the timing of incarceration is exogenous, and that it is not correlated with changing life circumstances that might also affect labor market outcomes. A shock to labor market productivity, for example, could lead to criminal behavior rather than the opposite.

A third approach proposed by Kling (2006) is to instrument for sentence length using an index of each judge's sentencing severity. This approach implicitly controls for all unobservables

(fixed and changing) that might bias estimates because judges are randomly assigned to cases. Using this IV approach, Kling (2006) shows that incarceration length has negligible effects on income and employment up to 9 years after sentencing.

Despite the extensive research on the economic effects of incarcerating adult men, little is known about the consequences of incarcerating juveniles on future outcomes. The handful of fairly recent studies that examine the effect of juvenile criminal activity on education and labor market outcomes generally find a negative correlation. However, much like the adult incarceration literature, it is difficult to isolate the effect of juvenile criminal activity from the many confounding factors.

Most of the existing studies of the impact of juvenile crime on recidivism, education and employment attempt to identify the causal link by controlling for observed individual characteristics (De Li, 1999; Tanner et al., 1999; Kerley et al., 2004) and unobserved household fixed characteristics or changes in state policy (Hjalmarsson, 2006). Although controlling for household fixed effects may account for differences in family background or neighborhood characteristics among juvenile offenders, the small number of siblings in the sample limits identification and generalizability of the results (Hjalmarsson, 2006).[6]

Our study attempts to avoid these limitations by applying instrumental variable techniques to a large dataset, albeit from one urban area in the U.S. Our strategy aims to provide causal effects of juvenile incarceration on the likelihood of incarceration as an adult. The data and strategy are described in greater detail in the next section.

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[6] Hjalmarsson (2006) identifies the effect of juvenile incarceration on high school completion using only 9 households. This is because the sample only contains 9 households that have at least one family member who is convicted while the other is incarcerated.

C. The Natural Experiment: Judge Assignment

When juveniles are charged with a crime in juvenile court, they are assigned to a calendar which corresponds to the youth's neighborhood of residence. Calendars usually have one or two judges that usually preside over cases on this calendar. Further, there are a large number of cases that are heard by judges that cover the calendar when the main judge(s) are not available. These judges are known as "swing judges". Given the frequency with which these judges hear cases, they are a large part of the structure in this court system. Within a calendar, the judge assignment is largely a function of the sequence with which cases happen to be heard. Conversations with administrators confirm that these assignments should be effectively random, and we will further test whether observable case characteristics are related to the judge assignment below.

III. Empirical Framework & Background

Consider a simple model that relates an indicator for adult incarceration, *AI*, and whether the individual was incarcerated as a juvenile, *JI*, along with characteristics of the juvenile, *X*:

(1) $AI_i = \gamma_0 + \gamma_1 JI_i + \gamma_2 X_i + \varepsilon_i$

Any assessment of the impact of juvenile incarceration on adult outcomes must address the problem posed by the positive correlation between juvenile incarceration and factors such as severity of the crime, criminal history and characteristics of the juvenile that are also likely to be correlated with adult outcomes. In the analysis below, we control for the most serious charge faced by the juvenile, as well as age, sex, and neighborhood. Moreover, we focus the analysis on the first juvenile offense, thereby limiting the sample to those with no criminal history. However, there may still be unobservable characteristics of either the crime or the juvenile that

are correlated with both the probability of juvenile incarceration and future adult outcomes. In particular, it appears likely that cov(JI, ε)>0, which would bias OLS estimates of γ_1 upward.

To address this endogeneity problem, we instrument for the probability of juvenile incarceration using a measure of the tendency of a randomly-assigned judge to order a juvenile be placed in custody, Z. Essentially, we compare adult incarceration rates for judges that have different propensities to incarcerate juveniles, and interpret any difference as a causal effect of the change in incarceration associated with the difference in these propensities. If there were two types of judges, this would be a local average treatment effect—the average effect of juvenile incarceration among those who were induced into custody due to the assignment to the relatively strict judge. These can be considered marginal cases where the judges may disagree about the custody decision. Our instrument is continuous, however, which allows us to explore heterogeneous effects at different values of the instrument, as described below.

In order to interpret the estimates this way as causal effects for certain marginal cases, there are three basic requirements (Angrist and Imbens, 1994; Heckman and Vytlacil, 2005):

(A1) There is a first stage.

That is, the propensity of the judge to incarcerate in other cases must be related to the likelihood that a given juvenile will be incarcerated as well. This is testable as the first stage.

(A2) Monotonicity.

Assignment to a more lenient judge does not result in an *increase* in the likelihood that a given juvenile is placed in custody. The case for monotonicity is less compelling in the current context compared to a typical treatment-control design. Judges may treat some cases in a lenient

manner and others in a strict manner. We consider these concerns below by visually inspecting the first stage and by comparing estimates across a wide range of judges.

(A3) The instrument satisfies an exclusion restriction.

The exclusion restriction is that the propensity of the judge to incarcerate does not enter the simple model (1) above. While not directly testable, we argue that this is plausible due to the random assignment of judges within court calendars at any point in time. The judge's tendency should then be unrelated to unobservable characteristics of the case. This strategy is similar to that employed by Kling (2006) in the context of adult incarceration.[7] In this context, the effective randomization of judges to cases suggests that Z should be independent of ε. In addition to appealing to the random nature of the assignment, we show that the observable characteristics of the case are unrelated to the incarceration tendency of the juvenile court judge, which provides evidence that the randomization did take place.

One concern would be that lenient judges may be particularly good at deterring future criminal activity without the need for incarceration. It would seem more plausible that the lenient judges would be less threatening to juveniles, however, which would lead to underestimates of the effect of juvenile incarceration on adult incarceration.

B. Marginal Treatment Effects

If we augment the above model to allow for a random coefficient on juvenile incarceration, this would allow the effects to vary by juvenile. A concern in estimating such models is a correlated random coefficient (Bjorklund and Moffitt, 1987), where the placement into custody may be related to the effect on adult incarceration. It is not clear that a juvenile justice judge

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[7] Chang and Schoar (2008) employ a similar strategy using judges assigned to Chapter 11 cases.

would aim to reduce adult criminal activity rather than simply punish juvenile offenses, although it is possible. To interpret γ_1 above as an average of these random coefficients, our instrument would need to be assumed to be independent of the random coefficient as well as the error term: a relatively strong assumption but one that seems plausible given the randomization.

Further, let P(z) represent the propensity of incarceration as a function of the instrument. We can estimate marginal treatment effects: $dAI/dP(z)$ which can vary with the instrument (Heckman and Vytlacil, 2005). Note that this slope is essentially a Wald estimator for a small change in the probability of placement into juvenile detention.

Marginal treatment effect estimates offer three main advantages. First, they allow an exploration of the source of the IV results, as effects may be concentrated among particular types of judges. This allows us to shed light on the types of juveniles who may be deterred or whose incarceration may lead to a life of crime. Second, the slope of the function provides evidence as to whether the correlated random coefficient problem is a concern. A flat MTE function would offer evidence that juveniles are not selected into custody such that those who "benefit most" from it are those who enter. A sloped MTE function can shed light on the margin at which particular types of juveniles are prone to be impacted by juvenile incarceration.

Third, the monotonicity assumption is arguably less problematic at the level of the MTE. A well known result is that if the average treatment effect of incarceration for "compliers"—those who are induced into custody due to the judge assignment—is the same for "defiers"—those who are induced to not enter custody because they are assigned to what appears to be a relatively strict judge—then the bias disappears. This is unlikely to be true when the youths differ significantly, but in a marginal treatment effect setting juveniles are compared in a manner that

their unobservables should be similar—they are induced into (or out of) custody due to an infinitesimally small change in the incarceration propensity of the judge.

IV.   Data Description

A. Sample Construction

The data used in this study come from a large, urban county. Juvenile court data are linked to adult prison data using identifiers such as name, date of birth, and social security number. The time period of the juvenile court data is 1990-2005. The time period for adult incarceration data is from 1990-2008. The initial cases of juveniles aged 10-17 are considered, and to ensure that the individuals are at risk of an adult incarceration, the sample is limited to those who are at least 18 years old in 2008. In addition, the main analysis compares judges within court calendar cells. To make our comparisons, we remove from the sample the 0.6% of juveniles who were assigned a judge who presided over fewer than 10 juvenile cases and in court calendars with fewer than 10 cases, largely to clean the data of erroneous identifiers.

The final dataset consists of nearly 100,000 juveniles. By limiting the sample to all youth who came before the juvenile court, we are implicitly comparing the outcomes of those charged with a crime and incarcerated with those who were also charged but were not incarcerated. We argue that this is the relevant comparison group for this analysis because this group represents the population at risk of incarceration.

One drawback of the data is that the adult imprisonment data includes only prisons and jails in the same state as the juvenile court. If individuals move out of the state, we do not observe their recidivism. However, data from the 2000 census suggest that this is not a major concern. Among those born in this state between the ages of 18 and 30 in the year 2000, three quarters

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remain there, and the rate of migration is lower for those with less education. We anticipate little bias to be introduced by this form of sample selection (although we are exploring this possibility with additional data).

D. Instrumental Variable

For each juvenile we assign an instrument that corresponds to the "incarceration propensity" of the initial judge. The instrument, which is defined for each juvenile i assigned to judge j is:

$$Z_{ij} = d_{ij} \left(\frac{1}{n_j - 1} \right) \left(\left(\sum_{k=1}^{n_j} I_k \right) - I_i \right)$$

Here, d_{ij} is an indicator that the judge j corresponds to the one assigned to juvenile i; n_j is the total number of cases seen by judge j; k indexes the juvenile case seen by judge j where I_k is equal to 1 if the juvenile were ever incarcerated as a youth. Thus the above equation simply calculates the average probability of incarceration for judge j based on all cases except the juvenile's own case. Algebraically, this is the judge fixed effect in a model of custody estimated in a "leave-out" regression. The resulting two-stage estimator is a Jackknife Instrumental Variables estimator (JIVE), which is recommended for models when the number of instruments (the fixed effects) is likely to increase with sample size (Stock, Wright, and Yogo, 2002). We also include a vector of calendar fixed effects in the first and second stages of the IV regressions. With the inclusion of these controls, we can interpret the within-cell variation in the instrument, Z_{ij}, as variation in the propensity of a randomly assigned judge to incarcerate a juvenile relative to the types of other cases seen in the calendar. We refer to this instrument as the "ever incarcerate rate" of the judge in what follows. The initial judge will affect this rate in two ways: (1) by sentencing the juvenile to be committed by the Department of Corrections or to the juvenile detention center; and (2) by affecting any future cases and hearings with the

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investigation into the initial case. There is variation in this measure: the mean is 0.18 and the standard deviation 0.06.

Results will be shown with an alternative measure of I_k: whether the child were incarcerated as a result of the initial case, which is tied more directly to the initial judge: a measure with a mean of 0.09 and a standard deviation of 0.04.

The instrumental variable calculation is not conditional on characteristics of the juvenile or the crime in order to allow a direct examination of whether judge assignment results in incarceration propensities (the instrument) that are unrelated to the characteristics of a given juvenile's case. A model that controls for characteristics of the crime or juvenile may mask the possibility that judges are assigned based on the characteristics of the juvenile or the case.

The sources of variation in our data are the 77 judges that cover 38 calendars. The average number of initial cases seen by these judges is 1255 (median: 542). Further, more than one judge often sees each juvenile within a case over time.[8] The instrument is based on the incarceration propensity of the first judge assigned for the juvenile's first offense, thus capturing what is referred to as the intention-to-treat effect. While this may lead to a weaker estimated relationship between the judge's propensity to incarcerate (the instrument) and an individual juvenile's incarceration status, it has the advantage of not capturing any (potential) non-random changing of judges.

V. Results

A. Instrument & Observable Characteristics

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[8] 34% of the initial cases have the same initial and final judge across all of the hearings. If the initial judge is missing in the data as it is in 37 percent of the cases, we assign the juvenile to the second judge of record. Over the course of the criminal proceedings, which often involve multiple hearings, the judge may change either temporarily or permanently and it is not known to the researcher what has caused the change in judge.

The characteristics of juveniles charged with their first criminal offense in these data are presented in Table 1 across judges with different incarceration rates.[9] While it is not possible to test whether children with unobservably high risks for incarceration later in life are assigned to particular types of judges, it is possible to test whether there are differences in observable characteristics.

The first column presents the unconditional mean of the covariate for those assigned to judges with an ever-incarcerated rate less than the median in the sample. High incarceration-rate judges may preside over particular court-calendars with juveniles at higher risk of incarceration. The second column reports the predicted mean that is adjusted to net out the effects of the court calendar using separate OLS regressions of each covariate on an indicator for an above-median judge and calendar fixed effects.

The means are remarkably similar across the two groups. Roughly 16% of the cases involve females, the average age is 15 years old, 57% are African American. A risk index is available for most cases, an index created by the department of probation that assigns each charge a score – the higher the score the more likely the department of probation will recommend detention. Again, this is similar across the two groups. In terms of charges, the most common ones are drug law violations (17%), simple assault (12%) and aggravated assault (10%). The main models below will include these variables as controls—with age disaggregated into year indicators, and more charge indicators listed in the appendix.

B. First Stage: Judge Assignment and Juvenile Incarceration

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[9] For the full set of means, see the online appendix.

To provide evidence of a first stage and consider the statistical strength of the relationship, we estimate the following equation for juvenile i assigned to judge j in calendar part c and year t using a linear probability model:

$$(2)\ JI_{ijct} = \beta_0 + \beta_1 Z_{ij} + \beta_2 X_i + \delta_c + \lambda_t + \upsilon_{ijct}$$

Similar results are found for both the first stage and the instrumental variable results when probit models are used, which is unsurprising given that the outcome variables are relatively far from zero.

In the above equation, Z_{ij} refers to the relative custody rate of judge j, the vector X_i includes juvenile characteristics described above. We present results of models estimated with and without controls as another examination of whether instrument is plausibly exogenous. If the judge's relative custody rate were a function of characteristics of either the juvenile or the crime, we would observe the estimate β_1 to fall when we include controls for the crime or juvenile characteristics. This would suggest a violation of the exclusion restriction. Calendar fixed effects concentrate the analysis within the set of judges that a juvenile could have received, and year indicators capture changes in policies and practices across all juveniles in a given year. All standard errors are clustered at the judge level to reflect the source of variation in the instrument.

The results presented in Table 2 show that the judge's "ever-incarcerated" rate and the initial-case incarceration rate are highly predictive of whether an individual juvenile will be incarcerated. The estimates decline in value somewhat with the full controls, largely due to the year indicators rather than characteristics of the juvenile. Column (3) reports a coefficient of 0.349. A two standard deviation increase in the ever-incarcerated rate would imply an increase in the likelihood of juvenile incarceration by 4.2 percentage points—or 23% of the mean

juvenile-incarceration rate. The initial-case incarceration rate shows a larger coefficient: 0.573, which means that a two standard-deviation increase in this measure implies a 4.5 percentage point increase in the juvenile incarceration rate. Moreover, all first stage estimates are precise, with t statistics close to 6.

C. Juvenile Incarceration and Adult Incarceration

We estimate the impact of incarceration at any time as a juvenile on the probability of being imprisoned as an adult in the same state where they were a juvenile offender according to the equation below that echoes (1) above:

(3) $AI_{ict} = \gamma_0 + \gamma_1 JI_i + \gamma_2 X_i + \delta_c + \lambda_t + \varepsilon_{ict}$

Where AI_{ijct} is an indicator for whether juvenile i in calendar c in year t for his first juvenile crime is imprisoned as an adult by 2005, and JI_i is an indicator for whether juvenile i was ever incarcerated as a juvenile. We present both OLS regression results and results in which we instrument for JI_i using the relative custody rate of the initial judge j assigned to the juvenile for his first case, Z_{ij} . As with the first stage, we present results both with and without controls (X_i). Calendar and year fixed effects are also included in the model, as in the first stage.

As expected, in OLS regressions incarceration as a juvenile is strongly related to adult imprisonment (columns 1-3, Table 3). Incarceration as a juvenile increases the probability of imprisonment as an adult by 18.6 percentage points in the model without controls and declines slightly to 16.1 percentage points when we control for observable characteristics. This implies that those who were incarcerated as juveniles are 3 times more likely to be found incarcerated as

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an adult at some time during the 1990-2005 period.[10] When we instrument for juvenile

incarceration (columns 3 - 4, Table 4), the point estimate increases to approximately 38

percentage points.

The instrumental-variable point estimate is large, as is its standard error, and some caution in

the interpretation is warranted. First, the IV and OLS estimates are not statistically significantly

different, but both can be characterized as large. Second, taken at face value, the instrumental-

variable point estimate suggests that the children on the margin of incarceration—compliers

where the judge assignment induces a change in the incarceration decision—may have even

larger effects of juvenile incarceration on adult incarceration than the average incarcerated

juvenile. This pattern is seen in some of the subgroup analyses reported below as well. Third,

the treatment of interest is binary: an indicator if the juvenile were ever incarcerated. The

instrumental-variable estimate extrapolates the change in this indicator from zero to one from a

change in the propensity to be incarcerated. This extrapolation can lead to large point estimates.

In the end, we regard the point estimate as evidence of large effects of juvenile incarceration on

adult incarceration for marginal cases.

E. Heterogeneous Treatment Effects Across Observable Characteristics

In this section we explore potential heterogeneity in the IV treatment effects. In particular,

we present first-stage and 2SLS estimates stratified by gender, age, and severity of the first

juvenile offense. Stronger first stages point to the types of children who are most affected by the

instrument—the "compliers". Differences in the IV results are suggestive of differential impacts

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[10] A mean juvenile incarceration rate of 0.18, an adult incarceration rate of 0.26, and a 16 percentage point
difference is achieved with adult incarceration rates of 23% and 7% for those incarcerated as juveniles and those not
incarcerated as juveniles, respectively. Also, note that juveniles are mostly released by their 21st birthday, if not
sooner, these results are net of any incapacitation effect that incarceration might have which would lead to a
negative relationship between incarceration and future arrest.

of the incarceration on the propensity for adult incarceration. Given the data requirements of the approach, differences across subgroups are rarely statistically significantly different and should be regarded as suggestive.

In terms of the first-stage estimates, males, those with a low-risk index, and cases from the early 1990s have the largest point estimates and are contributing the most to the overall IV estimates.

When we estimate effects of juvenile incarceration on adult incarceration across these groups, we find that female offenders have slightly higher point estimates despite a much lower mean adult-incarceration rate. This is consistent with a negative stigma associated with juvenile detention, especially among females given the relative rarity of the occurrence. The main results stem from juveniles aged 15-17 rather than the younger offenders, whose point estimate is negative and not statistically significant. This is consistent with long-lasting effects precisely at the time when these youths decide to drop out of high school.

We find larger point estimates for those with low levels of the risk assessment index, a result that suggests the effects of incarceration are especially problematic for these groups.[11] The main results appear to stem from the earliest time period as well. This is notable because reforms were implemented in the mid-1990s to allow judges to keep more juveniles out of detention. Alternatives introduced include the now more-widespread program of electronic monitoring and strictly enforced curfews complete with "check-in" centers set up in particular communities.[12]

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[11] The observations do not sum to the overall sum because the juveniles who do not have a risk index calculation are not included. That subset is shown in Table A5, wehre the results again suggest large increases in the likelihood of adult incarceration for those who are incarcerated as juveniles.

[12] Future research as part of this project will investigate the impact of the availability of these programs across time and across communities.

The last row reports results for a subsample that focuses more on the swing judges by restricting the sample to exclude judges who preside primarily in one calendar in any given year. In particular, cases heard by judges with more than 88% of cases in a single calendar-year were excluded.[13] The results are nearly the same for this group of judges who are thought to be most likely to be effectively randomized to the court cases.

E. Marginal Treatment Effects

Our continuous instrument allows us to estimate marginal treatment effects to explore the source of our results. Again, if the MTE function is relatively flat, then it is less likely that a correlated random coefficient problem affects the interpretation of our results. To the extent that there is an upward or downward slope, the estimates would suggest heterogeneous treatment effects as the marginal offender changes with the instrument.

To graph the relationship between adult incarceration and the propensity for juvenile incarceration, it is first necessary to factor out the influence of court calendar, which are defined by neighborhoods. Residuals from separate OLS models of adult incarceration, juvenile incarceration, and the instrument on calendar fixed effects were first calculated. The juvenile-incarceration residuals were then regressed on the instrument residuals to calculate an estimated $P(z)$: the predicted juvenile incarceration rate, where the variation stems from the instrument.

Figure 1 provides local quadratic estimates of the relationship between the adult incarceration measure and $P(z)$. A local quadratic estimator is used because it is known to have superior properties when the derivative of the relationship is sought. The estimator is evaluated at each percentile of $P(z)$. Similar to the first-stage results in Table 1, a first result is that the instrument

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[13] Each judges maximum-calendar share was calculated for each ear, and 88% is the median of this measure.

induces changes in the propensity of juvenile incarceration from 14% to 22%--a relatively narrow range and far from the full unit interval. This is the slice of the variation used to identify the causal effects of juvenile incarceration on adult incarceration.

As the propensity of incarceration increases with the instrument (relative to other judges in the same year-calendar cell), so does the propensity of adult incarceration. The slope of this function relates the change in adult incarceration to the change in the probability of incarceration stemming from the judge assignment—a ratio that is a Wald estimator at each point where the MTE function is evaluated.

Figure 1 reveals a fairly linear function, the slope of which is roughly 0.4, as expected from the results in Table 3. The slope varies from 0.3 to 0.5 across the P(z) distribution (Figure 1B). This suggests that the estimates do not come from one particular part of the P(z) distribution. If anything, there is some evidence of an increasing slope in the MTE function, which would suggest that juveniles who come before the most strict judges have even larger disruptions due to the incarceration. These are cases where the marginal cases that identify the effect are those with unobservable characteristics that should make them relatively less likely to enter custody. This is consistent with the comparison of the IV and OLS results, and the results in Table 4 where juveniles with lower risk measures had larger effects of juvenile incarceration on adult incarceration: juveniles who may have the most to lose from incarceration.[14]

F. Robustness

A number of robustness checks were undertaken, which are shown in Table A5. For a large number of cases the initial judge is missing from the coding, in which case we used the second

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[14] For similar figures for the first stage and the reduced form, see Appendix Figure A1.

judge code assigned to a particular case. When the data were restricted to cases where the initial judge is not missing, the results are very similar to the overall results.

Figure A1 shows the first-stage and reduced-form relationships with respect to the ever-incarcerated instrument, and there are a few judges with especially high or low incarceration rates. Table A5 shows that the results are largely the same when the instrument is trimmed. When this model was estimated without controls, however, the result is weaker, and in both cases the results are no longer statistically significant. As suggested by Figure 1, we rely on variation within a relatively narrow range, and discarding variation in the instrument clearly affects the precision of the results.

The effective randomization of judges to cases happens at a point in time. A related approach to the main results would be to include court-calendar by year fixed effects, rather than entering them in separately. The results are largely the same when these are used.

When the juvenile is part of the welfare system, the set of identifiers to link the data sources is especially rich. This can affect the ability to match the data with the adult incarceration sample. The results are similar on the subset of juveniles where these identifiers are available, although this may be expected given the high rate of contact with the welfare system (over ¾ of the data), and that the instrumental variable strategy should correct for spurious match rates.

Last, the results are also similar when a probit model is used instead of the 2SLS estimator.

VI. Interpretation & Conclusions

Juvenile incarceration is expensive, as is adult incarceration, with estimated marginal costs of roughly $20,000 per year per inmate (NIJ, 2007). If juvenile incarceration deterred future crime and incarceration, a tradeoff could be considered. Rather, we find that juvenile incarceration

leads to an increase in adult incarceration, and it appears welfare enhancing to use alternatives to juvenile incarceration. This state has an array of such policies, including electronic monitoring and well-enforced curfews that serve as substitutes for incarceration. Indeed, these substitutes have been growing in popularity. Results are similar when the models were estimated when these alternatives were in use, which suggests that their continued expansion could reduce the likelihood of adult crime still further.

One tradeoff that is difficult to measure is the reduction in crime due to the incapacitation effect while a juvenile. Presumably a benefit of juvenile incarceration is that criminal activity by that offender is at least delayed. To the extent that the alternatives such as the strict curfews reduce juvenile recidivism without the negative effects of full-fledged incarceration, this should be less of a concern. Another consideration is that a move away from juvenile detention may reduce its deterrence effect and lead to an increase in juvenile crime. Recent evidence suggests that juveniles' criminal propensity is particularly inelastic with respect to penalties (Lee and McCrary, 2006), which implies that this may be of second order importance compared to the large increases in adult incarceration found here. If this is the case, then the results suggest that a continued move toward less restrictive juvenile sentencing would lower the propensity of these juveniles to become incarcerated as adults without an increase in juvenile crime.

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Table 1: Means Comparison

		Unconditional Means for: Z<Median (1)	Conditional Means for: Z>=Median (2)	p-value (3)
	female	0.162	0.171	(0.131)
	age	15.0	15.1	(0.073)
	white	0.256	0.245	(0.213)
	African American	0.560	0.574	(0.251)
	other race	0.184	0.181	(0.763)
	Risk Index = 2	0.235	0.230	(0.449)
	Risk Index = 3	0.073	0.074	(0.817)
	Risk Index = 5	0.219	0.209	(0.164)
	Risk Index = 7	0.072	0.074	(0.555)
	Risk Index = 10	0.117	0.108	(0.285)
	Risk Index = 15	0.026	0.028	(0.468)
	Risk Index = Missing	0.258	0.277	(0.220)
Most	aggravated assault	0.094	0.106	(0.070)
Serious	arson	0.005	0.004	(0.282)
Charge	burglary	0.094	0.086	(0.097)
	court order violation	0.053	0.054	(0.821)
	drug law violation	0.174	0.172	(0.775)
	larceny theft	0.084	0.082	(0.561)
	automobile theft	0.095	0.097	(0.597)
	robbery	0.042	0.040	(0.429)
	simple assault	0.124	0.126	(0.718)
	vandalism	0.069	0.062	(0.044)*
	weapons offense	0.054	0.063	(0.104)
	initial judge missing	0.503	0.542	(0.527)
	Observations	96672		
	Number of Judges	77		

Z is the Judge's "Ever-Incarcerted" rate as described in the text. Column (2) is the predicted value from an OLS regression of the characteristic on the indicator that the instrument is above the median with court-calendar fixed effects. p-values are for the difference between the two columns, which was calculated using standard errors clustered at the judge level. The number of observations for race: 33,133.

Table 2: First Stage

Dependent Variable: Ever Incarcerated as a Juvenile

Judge instrument:	(1)	(2)	(3)	(4)	(5)	(6)
Ever-incarcerated rate	0.449	0.381	0.349			
	(0.060)**	(0.064)**	(0.063)**			
Initial-case incarceration rate				0.707	0.616	0.573
				(0.091)**	(0.093)**	(0.091)**
Controls	None	Year	Full	None	Year	Full
Observations	96672					
Mean of dependent var.	0.183					

Standard errors clustered at the judge level in parentheses. All models include court-calendar fixed effects. * significant at 5%; ** significant at 1%

Table 3: Adult Incarceration

Dependent Variable: Adult Incarcration

	OLS			2SLS: Z=ever-incarcerated rate			2SLS: Z=initial-case incarceration rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Ever incarcerted as a Juvenile	0.186	0.186	0.161	0.345	0.395	0.383	0.366	0.386	0.370
	(0.005)**	(0.005)**	(0.004)**	(0.132)*	(0.111)**	(0.109)**	(0.131)**	(0.108)**	(0.107)**
Controls	None	Year	Full	None	Year	Full	None	Year	Full
Observations	96672								
Mean of dependent var.	0.261								

Columns (4)-(9) standard errors clustered at the judge level in parentheses. All models include court-calendar fixed effects. * significant at 5%; ** significant at 1%

Table 4: Compliers & Heterogeneous Treatment Effects

Dependent Variable:	1st Stage (OLS): Ever in Juvenile Incarceration			2SLS: Adult Incarceration			
Subgroup	on Judge Ever-Incarcerated Rate	Standard Error	Mean of Dep. Var.	Coefficient on Ever in Juvenile Incarceration	Standard Error	Mean of Dep. Var.	Observations
Full Sample	0.349	(0.063)**	0.183	0.383	(0.109)**	0.261	96672
Male	0.364	(0.063)**	0.201	0.368	(0.111)**	0.298	82126
Female	0.221	(0.085)*	0.086	0.405	(0.284)	0.053	14546
Age < 15	0.387	(0.100)**	0.222	-0.102	(0.163)	0.230	26725
Age >= 15	0.336	(0.063)**	0.169	0.598	(0.163)**	0.273	69947
White	0.152	(0.137)	0.168	-0.111	(0.776)	0.159	6430
African American	0.457	(0.120)**	0.275	0.197	(0.134)	0.327	20649
Other Race	0.500	(0.116)**	0.224	0.232	(0.279)	0.211	6054
Low Risk Index	0.372	(0.068)**	0.161	0.489	(0.151)**	0.268	47511
High Risk Index	0.232	(0.077)**	0.185	0.282	(0.247)	0.254	24252
Years: 1990-1994	0.425	(0.064)**	0.205	0.398	(0.108)**	0.280	39501
Years: 1995-1999	0.158	(0.093)	0.164	0.272	(0.438)	0.266	35275
Years: 2000-2005	0.094	(0.099)	0.174	1.219	(1.919)	0.219	21896
"Swing Judges" Only	0.377	(0.083)**	0.180	0.375	(0.198)	0.249	48313

Each row represents separate regression for different subsamples labeled in Column (1). All models include full controls and court-calendar fixed effects. Standard errors clustered at the judge level are reported in parentheses. * significant at 5%; ** significant at 1%

Figure 1A: Adult Incarceration vs. P(Juvenile Incarceration| Z)

Figure 1B: Marginal Treatment Effects: d(Adult Incarceration)/d(P(z))
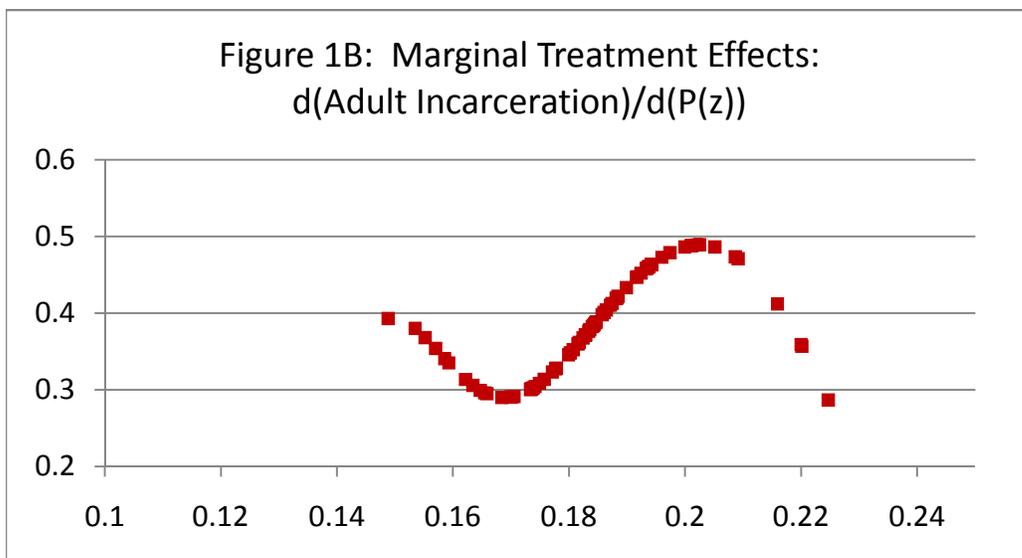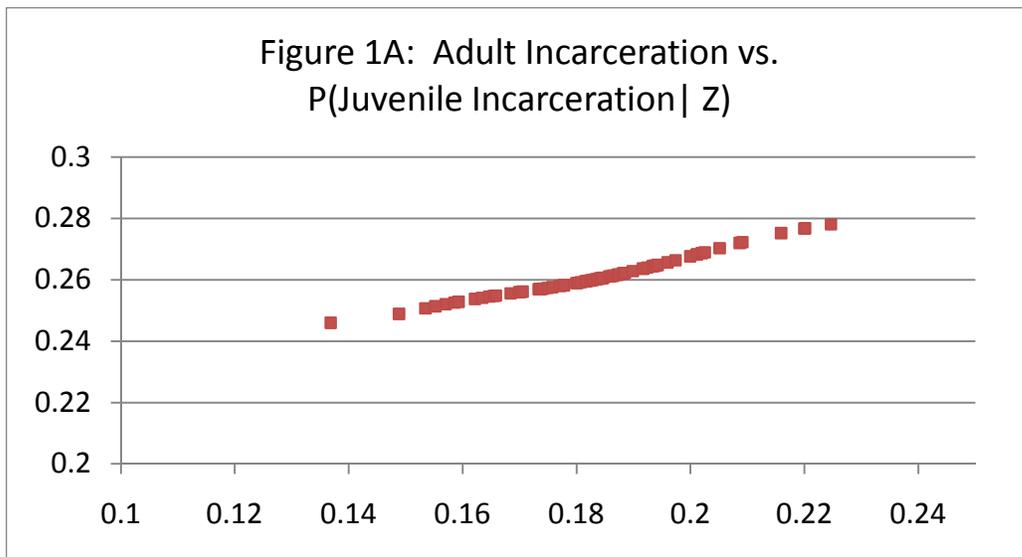
Figure 1A reporst local quadratic regression with a normal kernel and a pilot bandwidth of 0.02. Figure 1B reports the first derivative of these regression estimates. Measures of Adult Incarceration and P(z)--the predicted P(Juvenile Incarceration | Z = z)--are residuals from models with court-calendar fixed effects

ONLINE APPENDIX

Figure A1A: 1st Stage:
Ever in Juvenile Incarceration vs Z

Figure A1B: Reduced Form:
Adult Incarceration vs Z
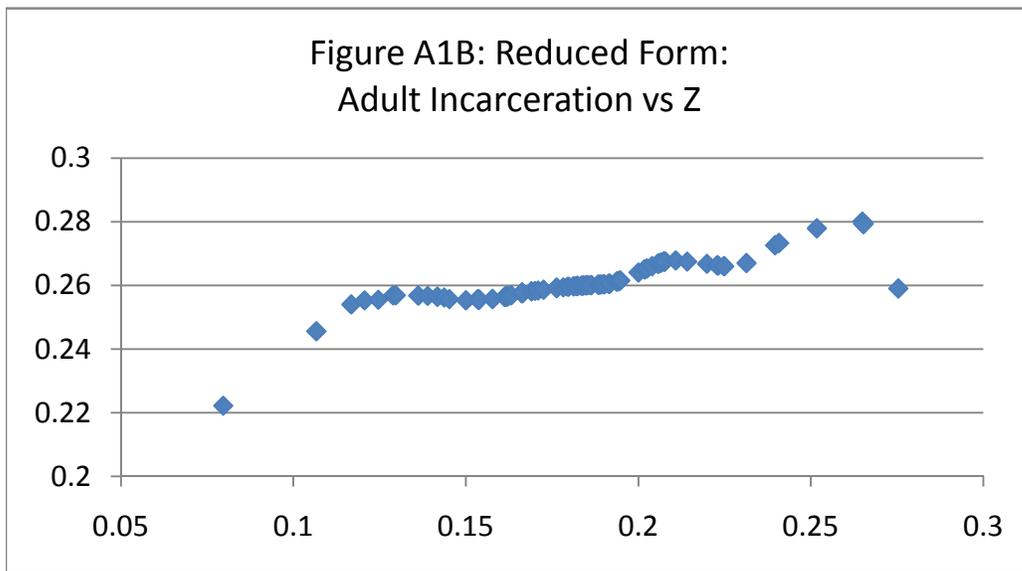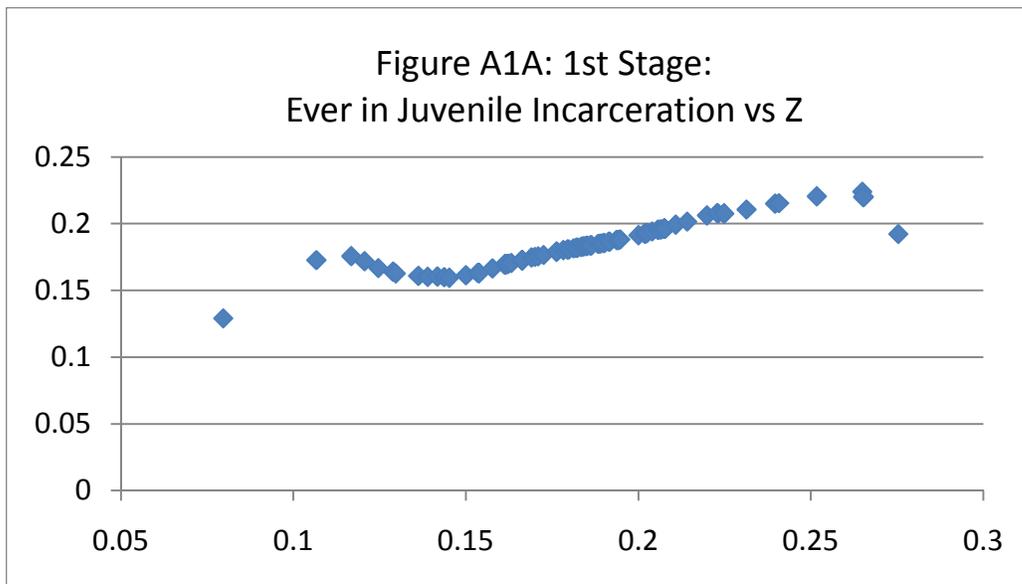
Z = ever incarcerated rate. Local Linear Regression with pilot bandwidth of 0.03.
Measures are residuals from regressions on court-calendar fixed effects.

Figure A2A: Adult Incarceration vs. P(Juvenile Incarceration| Z) Bandwidth: 0.03

Figure A2B: Adult Incarceration vs. P(Juvenile Incarceration| Z) Bandwidth 0.01
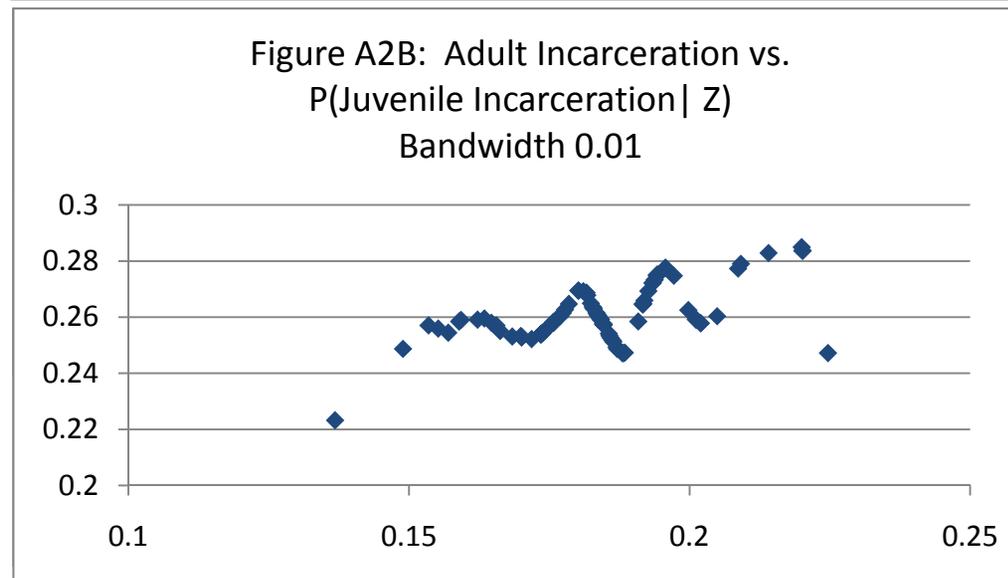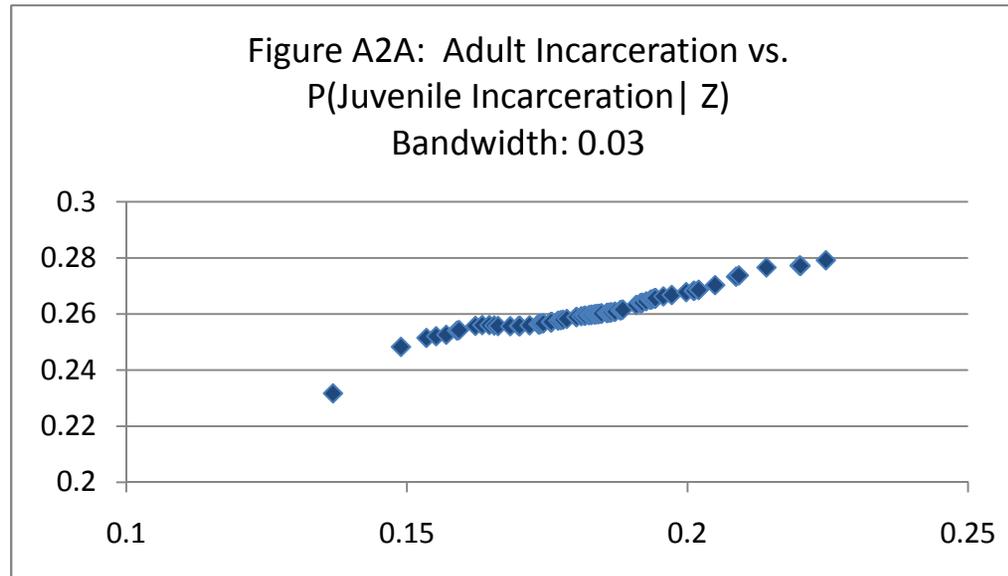
Local quadratic regressions. Measures are residuals from a model with court-calendar fixed effects

Table 1: Means Comparison

		Unconditional Means for: Z<Median	Conditional Means for: Z>=Median	p-value
		(1)	(2)	(3)
	female	0.162	0.171	(0.131)
	age10	0.006	0.004	(0.061)
	age11	0.012	0.009	(0.031)*
	age12	0.032	0.027	(0.049)*
	age13	0.075	0.070	(0.191)
	age14	0.158	0.157	(0.786)
	age15	0.280	0.282	(0.596)
	age16	0.387	0.397	(0.311)
	age17	0.051	0.055	(0.011)*
	white	0.256	0.245	(0.213)
	African American	0.560	0.574	(0.251)
	other race	0.184	0.181	(0.763)
	Risk Index = 2	0.235	0.230	(0.449)
	Risk Index = 3	0.073	0.074	(0.817)
	Risk Index = 5	0.219	0.209	(0.164)
	Risk Index = 7	0.072	0.074	(0.555)
	Risk Index = 10	0.117	0.108	(0.285)
	Risk Index = 15	0.026	0.028	(0.468)
	Risk Index = Missing	0.258	0.277	(0.220)
Most	aggravated assault	0.094	0.106	(0.070)
Serious	arson	0.005	0.004	(0.282)
Charge	burglary	0.094	0.086	(0.097)
	court order violation	0.053	0.054	(0.821)
	homicide	0.001	0.003	(0.061)
	disorderly conduct	0.018	0.017	(0.294)
	drug law violation	0.174	0.172	(0.775)
	larceny theft	0.084	0.082	(0.561)
	automobile theft	0.095	0.097	(0.597)
	non violent sex offense	0.013	0.012	(0.375)
	other offense	0.012	0.012	(0.693)
	public order offense	0.016	0.015	(0.269)
	violent sex offense	0.010	0.008	(0.138)
	robbery	0.042	0.040	(0.429)
	simple assault	0.124	0.126	(0.718)
	stolen property	0.006	0.005	(0.378)
	trespassing	0.017	0.017	(0.883)
	vandalism	0.069	0.062	(0.044)*
	weapons offense	0.054	0.063	(0.104)
	obstruction of justice	0.007	0.007	(0.255)
	minor categories	0.009	0.010	(0.346)
	initial judge: missing	0.503	0.542	(0.527)
	year	1996.6	1997.5	(0.120)
	Observations	96672		
	Number of Judges	77		

Z is the Judge's "Ever-Incarcerted" rate as described in the text. Column (2) is the predicted value from an OLS regression of the characteristic on the indicator that the instrument is above the median with court-calendar fixed effects. p-values are for the difference between the two columns, which was calculated using standard errors clustered at the judge level. The number of observations for race are: 33,133

Table A2: First Stage w/ Covariate Estimates

Dependent Variable: Ever in Juvenile Detention

		(1)	(2)	(3)	(4)	(5)	(6)
Judge instrument:	Ever-custody rate	0.449	0.381	0.349			
		(0.060)**	(0.064)**	(0.063)**			
	Initial-custody rate				0.707	0.616	0.573
					(0.091)**	(0.093)**	(0.091)**
	female			-0.098			-0.098
				(0.008)**			(0.008)**
	white			-0.029			-0.029
				(0.008)**			(0.008)**
	African American			0.038			0.038
				(0.006)**			(0.006)**
	race missing			-0.084			-0.084
				(0.012)**			(0.012)**
	age11			0.042			0.042
				(0.016)**			(0.016)**
	age12			0.071			0.071
				(0.015)**			(0.015)**
	age13			0.085			0.084
				(0.012)**			(0.012)**
	age14			0.086			0.086
				(0.013)**			(0.013)**
	age15			0.051			0.051
				(0.013)**			(0.013)**
	age16			0.004			0.004
				(0.014)			(0.014)
	age17			-0.024			-0.025
				(0.015)			(0.015)
	white			-0.029			-0.029
				(0.008)**			(0.008)**
	African American			0.038			0.038
				(0.006)**			(0.006)**
	race missing			-0.084			-0.084
				(0.012)**			(0.012)**
	judge 1 missing			-0.015			-0.014
				(0.008)			(0.008)
	risk index = 3			0.020			0.020
				(0.009)*			(0.009)*
	risk index = 5			0.006			0.006
				(0.007)			(0.007)
	risk index = 7			0.008			0.008
				(0.009)			(0.009)
	risk index = 10			-0.001			-0.001
				(0.008)			(0.008)
	risk index = 15			0.033			0.033
				(0.009)**			(0.009)**
	risk index: missing			0.011			0.011
				(0.006)			(0.006)
Most serious charge	arson			-0.044			-0.044
				(0.015)**			(0.015)**
	burglary			0.024			0.024
Aggravated Assault excluded:				(0.007)**			(0.007)**
	court order violation			0.235			0.235
				(0.009)**			(0.009)**
	homicide			0.114			0.113
				(0.024)**			(0.024)**
	disorderly conduct			-0.005			-0.005
				(0.008)			(0.008)
	drug law violation			0.061			0.061
				(0.005)**			(0.005)**
	larceny theft			0.031			0.030
				(0.009)**			(0.009)**
	automobile theft			0.047			0.047
				(0.008)**			(0.008)**
	non violent sex offense			-0.067			-0.067
				(0.009)**			(0.009)**
	other offense			0.026			0.026
				(0.013)*			(0.013)*
	public order offense			-0.012			-0.013
				(0.010)			(0.010)
	violent sex offense			-0.125			-0.125
				(0.011)**			(0.011)**
	robbery			0.012			0.012
				(0.006)			(0.006)
	simple assault			0.004			0.004
				(0.007)			(0.007)
	stolen property			-0.006			-0.006
				(0.018)			(0.018)
	trespassing			0.038			0.038
				(0.013)**			(0.013)**
	vandalism			0.010			0.010
				(0.009)			(0.009)
	weapons offense			0.032			0.032
				(0.010)**			(0.010)**
	obstruction of justice			0.021			0.021
				(0.018)			(0.017)
	minor categories			-0.011			-0.011
				(0.013)			(0.013)
	judge 1 missing			-0.015			-0.014
				(0.008)			(0.008)
	yr1991		-0.000	-0.005		0.000	-0.005
			(0.010)	(0.009)		(0.010)	(0.009)
	yr1992		0.030	0.019		0.029	0.019
			(0.010)**	(0.009)*		(0.009)**	(0.009)*
	yr1993		0.025	0.016		0.025	0.017
			(0.010)*	(0.009)		(0.010)*	(0.010)
	yr1994		0.009	-0.001		0.014	0.003
			(0.021)	(0.019)		(0.020)	(0.018)
	yr1995		-0.013	-0.026		-0.010	-0.023
			(0.022)	(0.022)		(0.021)	(0.021)
	yr1996		-0.039	-0.069		-0.036	-0.066
			(0.022)	(0.023)**		(0.022)	(0.022)**
	yr1997		-0.043	-0.068		-0.040	-0.066
			(0.019)*	(0.020)**		(0.019)*	(0.020)**
	yr1998		-0.019	-0.047		-0.016	-0.045
			(0.019)	(0.020)*		(0.019)	(0.020)*
	yr1999		-0.022	-0.048		-0.018	-0.044
			(0.020)	(0.021)*		(0.020)	(0.021)*
	yr2000		-0.031	-0.049		-0.029	-0.047
			(0.019)	(0.020)*		(0.019)	(0.019)*
	yr2001		-0.012	0.001		-0.008	0.004
			(0.020)	(0.021)		(0.019)	(0.020)
	yr2002		0.010	0.025		0.014	0.028
			(0.020)	(0.020)		(0.019)	(0.019)
	yr2003		0.005	0.019		0.009	0.022
			(0.019)	(0.019)		(0.018)	(0.018)
	yr2004		-0.026	-0.003		-0.022	0.001
			(0.020)	(0.019)		(0.020)	(0.019)
	yr2005		-0.039	-0.003		-0.034	0.001
			(0.020)	(0.020)		(0.020)	(0.019)
	Constant	0.101	0.122	0.113	0.122	0.136	0.125
		(0.011)**	(0.019)**	(0.022)**	(0.008)**	(0.017)**	(0.021)**
	Observations	96672	96672	96672	96672	96672	96672
	Mean of dependent var.	0.183	0.183	0.183	0.183	0.183	0.183

Standard errors clustered at the judge level in parentheses. All models include court-calendar fixed effects. * significant at 5%; ** significant at 1%

Table A3: Adult Incarceration w/ Covariate Estimates

Dependent Variable: Adult Incarceration

		OLS			2SLS: Z=ever-incarcerated rate			2SLS: Z=initial-case incarceration rate		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ever incarcerted as a Juvenile	0.186	0.186	0.161	0.345	0.395	0.383	0.366	0.386	0.370
		(0.005)**	(0.005)**	(0.004)**	(0.132)*	(0.111)**	(0.109)**	(0.131)**	(0.108)**	(0.107)**
	female			-0.209			-0.187			-0.189
				(0.010)**			(0.012)**			(0.013)**
	age11			0.004			-0.005			-0.005
				(0.020)			(0.020)			(0.020)
	age12			0.030			0.014			0.015
				(0.017)			(0.018)			(0.018)
	age13			0.040			0.021			0.022
				(0.015)**			(0.017)			(0.017)
	age14			0.051			0.032			0.033
				(0.015)**			(0.018)			(0.017)
	age15			0.068			0.056			0.057
				(0.014)**			(0.015)**			(0.015)**
	age16			0.096			0.095			0.095
				(0.014)**			(0.015)**			(0.015)**
	age17			0.081			0.086			0.086
				(0.014)**			(0.016)**			(0.016)**
	white			-0.018			-0.012			-0.012
				(0.005)**			(0.006)*			(0.005)*
	African American			0.099			0.090			0.091
				(0.007)**			(0.009)**			(0.009)**
	race_miss			0.008			0.027			0.026
				(0.003)*			(0.011)*			(0.011)*
	judge 1 missing			-0.014			-0.010			-0.010
				(0.005)*			(0.006)			(0.006)
	risk index = 3			0.010			0.006			0.006
				(0.012)			(0.014)			(0.013)
	risk index = 5			0.032			0.031			0.031
				(0.010)**			(0.011)**			(0.011)**
	risk index = 7			0.009			0.007			0.008
				(0.010)			(0.011)			(0.010)
	risk index = 10			0.004			0.004			0.004
				(0.012)			(0.012)			(0.012)
	risk index = 15			0.026			0.019			0.019
				(0.010)*			(0.010)			(0.010)*
	risk index: missing			0.012			0.010			0.010
				(0.009)			(0.009)			(0.009)
Most serious charge	arson			-0.020			-0.010			-0.010
				(0.018)			(0.018)			(0.018)
	burglary			-0.017			-0.022			-0.022
Aggravated Assault excluded:				(0.010)			(0.009)*			(0.009)*
	court order violation			0.052			-0.001			0.002
				(0.008)**			(0.025)			(0.024)
	homicide			0.028			0.003			0.004
				(0.022)			(0.030)			(0.030)
	disorderly conduct			-0.027			-0.026			-0.026
				(0.011)*			(0.011)*			(0.011)*
	drug law violation			0.058			0.044			0.045
				(0.008)**			(0.009)**			(0.009)**
	larceny theft			0.001			-0.006			-0.005
				(0.009)			(0.010)			(0.009)
	automobile theft			0.017			0.006			0.007
				(0.008)*			(0.008)			(0.008)
	non violent sex offense			-0.055			-0.040			-0.041
				(0.011)**			(0.013)**			(0.013)**
	other offense			-0.005			-0.011			-0.011
				(0.015)			(0.015)			(0.015)
	public order offense			-0.017			-0.015			-0.015
				(0.014)			(0.015)			(0.015)
	violent sex offense			-0.068			-0.040			-0.042
				(0.013)**			(0.022)			(0.022)
	robbery			0.015			0.013			0.013
				(0.007)*			(0.007)			(0.007)
	simple assault			0.007			0.006			0.006
				(0.011)			(0.011)			(0.011)
	stolen property			-0.004			-0.002			-0.002
				(0.018)			(0.018)			(0.018)
	trespassing			0.015			0.006			0.007
				(0.015)			(0.016)			(0.016)
	vandalism			-0.005			-0.007			-0.007
				(0.014)			(0.014)			(0.014)
	weapons offense			-0.002			-0.009			-0.009
				(0.009)			(0.009)			(0.009)
	obstruction of justice			0.030			0.026			0.026
				(0.026)			(0.024)			(0.024)
	minor categories			-0.034			-0.032			-0.032
				(0.014)*			(0.014)*			(0.014)*
	judge 1 missing			-0.014			-0.010			-0.010
				(0.005)*			(0.006)			(0.006)
	yr1991		-0.028	-0.028		-0.028	-0.027		-0.028	-0.027
			(0.007)**	(0.006)**		(0.008)**	(0.007)**		(0.008)**	(0.007)**
	yr1992		-0.028	-0.035		-0.035	-0.041		-0.034	-0.040
			(0.009)**	(0.009)**		(0.011)**	(0.010)**		(0.011)**	(0.010)**
	yr1993		-0.030	-0.041		-0.035	-0.044		-0.035	-0.044
			(0.007)**	(0.006)**		(0.008)**	(0.007)**		(0.008)**	(0.007)**
	yr1994		-0.053	-0.069		-0.054	-0.069		-0.054	-0.069
			(0.016)**	(0.017)**		(0.018)**	(0.018)**		(0.018)**	(0.018)**
	yr1995		-0.033	-0.043		-0.029	-0.036		-0.029	-0.036
			(0.021)	(0.021)*		(0.024)	(0.024)		(0.024)	(0.023)
	yr1996		-0.056	-0.075		-0.046	-0.058		-0.046	-0.059
			(0.019)**	(0.019)**		(0.021)*	(0.021)**		(0.021)*	(0.020)**
	yr1997		-0.070	-0.080		-0.058	-0.062		-0.059	-0.064
			(0.020)**	(0.019)**		(0.023)*	(0.021)**		(0.022)**	(0.020)**
	yr1998		-0.041	-0.051		-0.036	-0.038		-0.036	-0.039
			(0.021)	(0.021)*		(0.024)	(0.023)		(0.023)	(0.022)
	yr1999		-0.042	-0.050		-0.034	-0.037		-0.035	-0.038
			(0.021)	(0.020)*		(0.024)	(0.023)		(0.024)	(0.022)
	yr2000		-0.061	-0.066		-0.052	-0.053		-0.052	-0.054
			(0.020)**	(0.019)**		(0.023)*	(0.022)*		(0.023)*	(0.021)*
	yr2001		-0.053	-0.068		-0.049	-0.067		-0.050	-0.067
			(0.018)**	(0.018)**		(0.020)*	(0.020)**		(0.020)*	(0.019)**
	yr2002		-0.082	-0.093		-0.083	-0.098		-0.083	-0.098
			(0.019)**	(0.019)**		(0.022)**	(0.021)**		(0.022)**	(0.021)**
	yr2003		-0.092	-0.106		-0.092	-0.109		-0.092	-0.109
			(0.019)**	(0.019)**		(0.021)**	(0.021)**		(0.021)**	(0.021)**
	yr2004		-0.142	-0.161		-0.135	-0.159		-0.135	-0.159
			(0.021)**	(0.020)**		(0.024)**	(0.022)**		(0.024)**	(0.022)**
	yr2005		-0.172	-0.205		-0.163	-0.202		-0.163	-0.202
			(0.020)**	(0.020)**		(0.022)**	(0.021)**		(0.022)**	(0.021)**
	Constant	0.227	0.277	0.166	0.000	0.000	0.000	0.000	0.000	0.000
		(0.003)**	(0.014)**	(0.017)**	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)
	Observations	96672								
	Mean of dependent var.	0.261								

Columns (4)-(9) standard errors clustered at the judge level in parentheses. All models include court-calendar fixed effects. * significant at 5%; ** significant at 1%

Table A4: Means Comparison for Initial-Case Incarceration Rate

	Z = Judge Initial-Case-Incarceration Rate		
	Z<Median	Z>=Median	p-value
female	0.163	0.169	(0.352)
age10	0.0058	0.0041	(0.061)
age11	0.012	0.009	(0.046)*
age12	0.032	0.026	(0.030)*
age13	0.074	0.069	(0.248)
age14	0.158	0.157	(0.898)
age15	0.28	0.281	(0.786)
age16	0.387	0.396	(0.334)
age17	0.051	0.056	(0.001)**
white	0.251	0.245	(0.461)
African American	0.559	0.571	(0.304)
other race	0.191	0.186	(0.558)
Risk Index = 2	0.234	0.235	(0.885)
Risk Index = 3	0.071	0.075	(0.338)
Risk Index = 5	0.221	0.208	(0.061)
Risk Index = 7	0.073	0.073	(0.892)
Risk Index = 10	0.118	0.108	(0.210)
Risk Index = 15	0.026	0.028	(0.570)
Risk Index = Missing	0.257	0.272	(0.331)
judge 1 missing	0.485	0.54	(0.435)
aggravated assault	0.096	0.105	(0.151)
arson	0.005	0.004	(0.351)
burglary	0.096	0.087	(0.087)
court order violation	0.052	0.051	(0.841)
homicide	0.001	0.002	(0.080)
disorderly conduct	0.018	0.017	(0.650)
drug law violation	0.169	0.17	(0.938)
larceny theft	0.083	0.082	(0.869)
automobile theft	0.096	0.098	(0.724)
non violent sex offense	0.014	0.013	(0.201)
other offense	0.012	0.012	(0.618)
public order offense	0.017	0.016	(0.400)
violent sex offense	0.01	0.008	(0.049)*
robbery	0.043	0.042	(0.793)
simple assault	0.123	0.129	(0.364)
stolen property	0.006	0.004	(0.210)
trespassing	0.017	0.017	(0.808)
vandalism	0.069	0.064	(0.178)
weapons offense	0.055	0.06	(0.354)
obstruction of justice	0.007	0.007	(0.614)
minor categories	0.009	0.01	(0.335)
initial judge: missing	0.485	0.54	(0.435)
year	1996.36	1997.43	(0.041)*

Z is the Judge's "Initial-case incarceration rate as described in the text. Column (2) is the predicted value from an OLS regression of the characteristic on the indicator that the instrument is above the median with court-calendar fixed effects. p-values are for the difference between the two columns, which was calculated using standard errors clustered at the judge level. The number of observations for race are: 33,133

Table A5: Robustness

Dependent Variable:	2SLS Adult Incarceration			
	Coefficient on Ever in Juvenile Incarceration	Standard Error	Mean of Dep. Var.	Observations
Judge 1 not Missing	0.382	(0.131)**	0.280	60949
Z trimmed (5-95)	0.38	(0.203)	0.265	86418
Court Calendar x Year fixed effects	0.379	(0.179)*	0.261	96672
IV Probit (MLE)	0.308	(0.073)**	0.261	96672
Identifiers from Welfare System	0.373	(0.131)**	0.312	68629
Missing Risk Index	0.249	(0.124)*	0.255	24909

All models include full controls including court-calendar fixed effects. IV Probit estimate is the average marginal effect.