

**Effects of Juvenile Incarceration:  
Evidence from Randomly-Assigned Judges**

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**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).<sup>1</sup> 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.<sup>2</sup> 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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<sup>1</sup> 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.

<sup>2</sup> 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.<sup>3</sup>

This paper aims to estimate causal effects of juvenile incarceration on the likelihood of adult incarceration. Estimation of such a relationship is complicated by the fact that those who are incarcerated likely differ from juveniles who are not. First, these juveniles have committed more serious crimes and their underlying propensity for adult crime may be higher than juveniles who were not committed: this would bias OLS estimates of the relationship between juvenile incarceration and adult incarceration upwards. Second, effects for juveniles on the margin of juvenile incarceration may differ from the average juvenile, and this is the group most likely to be affected by policy changes.

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.

The analysis uses administrative data for over 90,000 juveniles over 16 years who came before a juvenile court in a large, urban county in the U.S. These data were linked to adult

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<sup>3</sup> 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).

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 60% more likely to enter adult prison later in life compared to those who came before the juvenile court but were not incarcerated. When we instrument for juvenile incarceration, the estimate is 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 juveniles aged 15 and 16—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.<sup>4</sup>

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).<sup>5</sup> 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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<sup>4</sup> 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.

<sup>5</sup> 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).<sup>6</sup>

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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<sup>6</sup> 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,  $JJ$ , along with characteristics of the juvenile,  $X$ :

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

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 history with the juvenile court system. 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 such that  $\text{cov}(JI, \varepsilon) > 0$ , which would bias OLS estimates of  $\gamma_1$  upward.

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. That is, judges choose the sentence, and if they tailor sentences with the idea of deterring future criminal activity, then a selection bias could understate the causal effect of juvenile incarceration for cases on the margin of commitment—those cases most likely affected by policy.

Our empirical strategy uses 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 Vytlačil, 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.<sup>7</sup> In this context, the effective randomization of judges to cases suggests that  $Z$  should be independent of  $\varepsilon$ . 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.

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<sup>7</sup> Chang and Schoar (2008) employ a similar strategy using judges assigned to Chapter 11 cases.

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**

One way to explore the source of the instrumental variable results across the distribution of cases is to use the continuously measured instrument to estimate marginal treatment effects (Heckman and Vytlačil, 2005). 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. 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. In particular, for relatively lenient judges, their marginal cases are likely to have unobserved characteristics that are more highly related to recidivism compared to marginal cases for relatively strict judges. Second, the slope of the function provides evidence as to whether the selection bias concern described above 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 1990-2006. The time period for adult incarceration data is from 1990-2008. The initial cases of juveniles aged 13-16 are considered, as these cases are eligible for juvenile detention. 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.

One issue is that juvenile custody may be related to adult incarceration in ways that are not due to criminality as an adult—if sentences are harsher for these individuals who spent time in custody as a youth. To investigate this, we have adult arrest data from 1993-2005. When we consider adult arrest as an outcome, we restrict the sample to those who were at least 18 years old by the end of this sample period.

The final dataset consists of 92,119 juveniles to consider adult incarceration and 76,197 when considering adult arrests. 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 it includes only adult outcomes 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 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).

## **B. 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:

$$Z_{ijt} = d_{ijt} \left( \frac{1}{n_{jt}-1} \right) \left( \left( \sum_{k=1}^{n_{jt}} I_k \right) - I_i \right)$$

Here,  $d_{ijt}$  is an indicator that the judge  $j$  corresponds to the one assigned to juvenile  $i$  who came before the court in year  $t$ ;  $n_{jt}$  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 was incarcerated during the first case. Thus the instrument is the incarceration rate for judge  $j$  based on all cases except the juvenile's own case in a given year. Algebraically, this is the judge fixed effect in a model of custody estimated in a "leave-out" regression estimated for each year. 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 among the judges that the juvenile could have been assigned in a given year. We refer to this instrument as the "judge incarceration rate" of the judge in what follows. There is variation in this measure: the mean is 0.11 and the standard deviation 0.05. Results will be shown with alternative measures of the instrument as checks on robustness as well.

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 76 judges that cover 38 calendars. The average number of initial cases seen by these judges in a given year is 669 (median: 653). Further, more

than one judge often sees each juvenile within a case over time.<sup>8</sup> 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**

The characteristics of juveniles charged with their first criminal offense in these data are presented in Table 1 across judges with different incarceration rates.<sup>9</sup> 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

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<sup>8</sup> 35% 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.

<sup>9</sup> For the full set of means, see the online appendix [to add].



judge and calendar fixed effects. The first row shows that the value of the instrument is 13% for high-incarceration-rate judges and 7.4% for low-incarceration-rate judges.

The means are remarkably similar across the two groups. Roughly 16% of the cases involve females, the average age is 15 years old, 61% are African American (among those with race information). 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 (18%), simple assault (12%) and aggravated assault (10%). The main models below will include these variables as controls—with age disaggregated into year indicators. The race indicators will be included as well as an indicator that the race information was missing.

## **B. First Stage: Judge Assignment and Juvenile Incarceration**

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:

$$(2) \text{JI}_{ijct} = \beta_0 + \beta_1 Z_{ijt} + \beta_2 X_i + \delta_c + \lambda_t + v_{ijct}$$

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,  $Z_{ijt}$  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  $\beta_1$  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 incarceration rate is highly predictive of whether an individual juvenile will be incarcerated in both samples. The estimates decline in value somewhat with the full controls, largely due to controlling for missing race information rather than characteristics of the juvenile or the charges. Column (3) reports a coefficient close to 0.5. A two standard deviation increase in the judge incarceration rate would imply an increase in the likelihood of juvenile incarceration by 5 percentage points—or 26% of the mean rate of juvenile incarceration. Moreover, all first stage estimates are precise, with *t* statistics over 6.

### **C. Juvenile Incarceration and Adult Crime Outcomes**

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:

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

Where  $Y_{ict}$  is an indicator for whether juvenile  $i$  in calendar  $c$  in year  $t$  for his first juvenile crime is arrested or imprisoned as an adult, 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  $J_i$  using the judge incarceration rate of the initial judge  $j$  assigned to the juvenile for his first case in year  $t$ ,  $Z_{ijt}$ . 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.

Table 3 reports results for adult arrests by 2005. The first statistics to notice is the arrest rate: 58%. This sample average is similar to the adult arrest rate for those who are aged 25 in 2005. As expected, in OLS regressions incarceration as a juvenile is strongly related to adult arrests (columns 1-3). Incarceration as a juvenile increases the probability of arrest as an adult by 12 percentage points in the model without controls and declines slightly to 10 percentage points when we control for observable characteristics. This implies that those who were incarcerated as juveniles are 18% more likely to be found incarcerated as an adult at some time during the 1993-2005 period.<sup>10</sup> When we instrument for juvenile incarceration (columns 3 - 4, Table 4), the point estimate increases to 30 percentage points in the model with no controls and 22 percentage points with controls. These estimates are large, although not statistically-significantly different from the OLS results. We view the results as suggesting that there are large effects of juvenile incarceration on the likelihood of an adult arrest, where the IV results largely corroborate the OLS results. This is discussed in more detail below.

Similar results are found when we consider adult incarceration (Table 4). The adult imprisonment rate at some time between 1990 and 2008 is 27%. The OLS results again show a strong relationship between juvenile incarceration and adult incarceration, with those who were

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<sup>10</sup> A mean juvenile incarceration rate of 0.193, an adult arrest rate of 0.584, and a 10 percentage point difference is achieved with adult arrest rates of 66.5 and 56.5% for those incarcerated as juveniles and those not incarcerated as juveniles, respectively. Also, note that juveniles are mostly released by their 21<sup>st</sup> birthday, usually much 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.

in juvenile detention having an adult incarceration rate that is 15 percentage points higher than juveniles who came before the court but were not committed. This implies an approximately 40% higher adult incarceration rate.<sup>11</sup> The instrumental variable estimates are again larger, with a point estimate close to 0.27. This estimate is similar regardless of whether or not controls are introduced.

The instrumental-variable point estimates are large, as is the standard errors, 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.

#### **D. 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

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<sup>11</sup> Similar to the arrest-rate calculations, the estimates can be reconciled with imprisonment rates of 39% and 24% for those who were incarcerated as a juvenile compared to those who were not.

instrument—the “compliers”. Differences in the IV results are suggestive of differential impacts 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, they tend to be similar across the subgroups. Slightly larger first-stages are found for males, those under the age of 15, and those with a low-risk index. The first stage has a stronger relationship in the earlier years as well, especially 1990-1994.

When we estimate effects of juvenile incarceration on adult incarceration across these groups, we find that the large IV estimates are results are found for males, those aged 15 or 16, and those with a low risk index used to determine the probation status. The large effects for older juveniles are consistent with long-lasting effects precisely at the time when these youths decide to drop out of high school. Further, to the extent that younger offenders have higher criminal propensities later in life, and that high-risk-index offenders do as well<sup>12</sup>, these results are consistent with incarceration having larger effects for those with characteristics that make them less likely to commit crimes in the future.

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 80% of cases in a single calendar-year were

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<sup>12</sup> 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, where the results again suggest large increases in the likelihood of adult incarceration for those who are incarcerated as juveniles.

excluded.<sup>13</sup> The results are qualitatively similar 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 court-calendar and year 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 induces changes in the propensity of juvenile incarceration from 14% to 25%, with most of the data between 16% and 22%--a relatively narrow range and far from the full unit interval. This is

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<sup>13</sup> Each judges maximum-calendar share was calculated for each ear, and 80% is the median of this measure.

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 that the juvenile could be assigned), 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. The slope of the line through the points of  $P(z)$  at 16 to 22% is similar to the overall IV estimate of 0.25.

Figure 1 shows that the increasing relationship between  $P(z)$  and adult incarceration, especially in two zones in the second quartile and the top half of the  $P(z)$  distribution. Our main take away from the figure is that the main results are not limited to one part of the judge incarceration-rate distribution. We are reluctant to interpret the marginal treatment effects (the slopes) at each point given that we do not have the statistical precision to distinguish the slopes. To the extent that the slope changes, it is roughly flat in the first quartile of the  $P(z)$  distribution, suggesting little effect among the cases assigned to the most lenient cases. These are cases where the marginal cases that identify the effect are those with unobservable characteristics that should make them relatively more 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.<sup>14</sup> That said, there is a flat region near the median of the sample as well, implying that the effects are somewhat unstable at the judge level.

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<sup>14</sup> For similar figures for the first stage and the reduced form, see Appendix Figure A1.

## **F. Robustness**

A number of robustness checks were undertaken, which are shown in Table A1. When the instrument is trimmed to exclude the 5% at the extremes of the Z distribution, the estimates are similar to the main results. A somewhat larger point estimate is found when the extreme 10% are excluded. When court calendar x year fixed effects are included, the point estimate increases slightly, but so does the standard error. When the model is estimated using a maximum-likelihood probit estimator, the point estimate again increases slightly. Last, when the sample is restricted to cases of juveniles who are also recipients of the state's welfare programs, the identifiers are particularly high in quality for matching with the adult outcomes. This should be unrelated to the instrument, but the point estimate does decrease somewhat to a 20 percentage point increase in adult incarceration using this sample.

## **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

		Cases before Judges w/ Low Incarceration Rate (1)	Cases before Judges w/ High Incarceration Rate (2)	p-value (3)
Instrument:	incarceration rate	0.074	0.131	0.001
Demographics	female	0.161	0.163	0.660
	age	14.9	14.9	0.439
	white	0.213	0.212	0.950
	African American	0.609	0.626	0.045
	other race	0.178	0.161	0.015
Risk index	risk index = 2	0.217	0.220	0.645
	risk index = 3	0.069	0.067	0.532
	risk index = 5	0.234	0.221	0.066
	risk index = 7	0.076	0.078	0.592
	risk index = 10	0.111	0.125	0.035
	risk index = 15	0.023	0.026	0.193
	risk index = Missing	0.270	0.262	0.601
Most Serious Charge	aggravated assault	0.096	0.101	0.409
	burglary	0.098	0.087	0.024
	court order violation	0.060	0.067	0.139
	disorderly conduct	0.017	0.016	0.413
	drug law violation	0.181	0.179	0.779
	larceny theft	0.075	0.068	0.141
	automobile theft	0.106	0.105	0.894
	non-violent sex offense	0.012	0.012	0.876
	public order offense	0.015	0.011	0.010
	robbery	0.045	0.043	0.315
	simple assault	0.117	0.126	0.075
	trespassing	0.016	0.014	0.036
	vandalism	0.065	0.060	0.128
	weapons offense	0.056	0.069	0.026
	other offense	0.041	0.042	0.653
	Observations	92119		
	Number of Judges	76		

Categories defined by the median of the instrument: the judge's incarceration rate. Column (2) is the predicted value from an OLS regression of the characteristic on the indicator that the judge's incarceration rate is higher than this median, controlling for 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: 33,041

Table 2: First Stage

A. Adult Arrest Sample

**Dependent Variable: Ever Incarcerated as a Juvenile**

	(1)	(2)	(3)
Judge incarceration rate	0.567 (0.080)	0.499 (0.076)	0.476 (0.075)
Controls	Year	Demographics	Full
Observations	76197		
Mean of dependent var.	0.193		

B. Adult Incarceration Sample

**Dependent Variable: Ever Incarcerated as a Juvenile**

	(1)	(2)	(3)
Judge incarceration rate	0.581 (0.076)	0.519 (0.072)	0.496 (0.073)
Controls	Year	Demographics	Full
Observations	92119		
Mean of dependent var.	0.193		

Standard errors clustered at the judge level in parentheses. Demographic controls are indicators for age, race, and sex. Full controls include indicators for the most serious charge, the risk index, and whether the first judge code was missing. All models include court-calendar fixed effects.

Table 3: Adult Arrest

Dependent Variable: Adult Arrest	OLS			2SLS: Z=Judge incarceration rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Ever incarcerated as a Juvenile	0.121 (0.004)	0.102 (0.004)	0.097 (0.004)	0.296 (0.094)	0.240 (0.104)	0.220 (0.110)
Controls	Year	Demographics	Full	Year	Demographics	Full
Observations	76197					
Mean of dependent var.	0.584					

Standard errors clustered at the judge level in parentheses. Demographic controls are indicators for age, race, and sex. Full controls include indicators for the most serious charge, the risk index, and whether the first judge code was missing. All models include court-calendar fixed effects.

Table 4: Adult Incarceration

<b>Dependent Variable: Adult Incarceration</b>						
	OLS			2SLS: Z=Judge incarceration rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Ever incarcerated as a Juvenile	0.180 (0.006)	0.156 (0.005)	0.148 (0.005)	0.289 (0.102)	0.267 (0.115)	0.269 (0.116)
Controls	Year	Demographics	Full	Year	Demographics	Full
Observations	92119					
Mean of dependent var.	0.268					

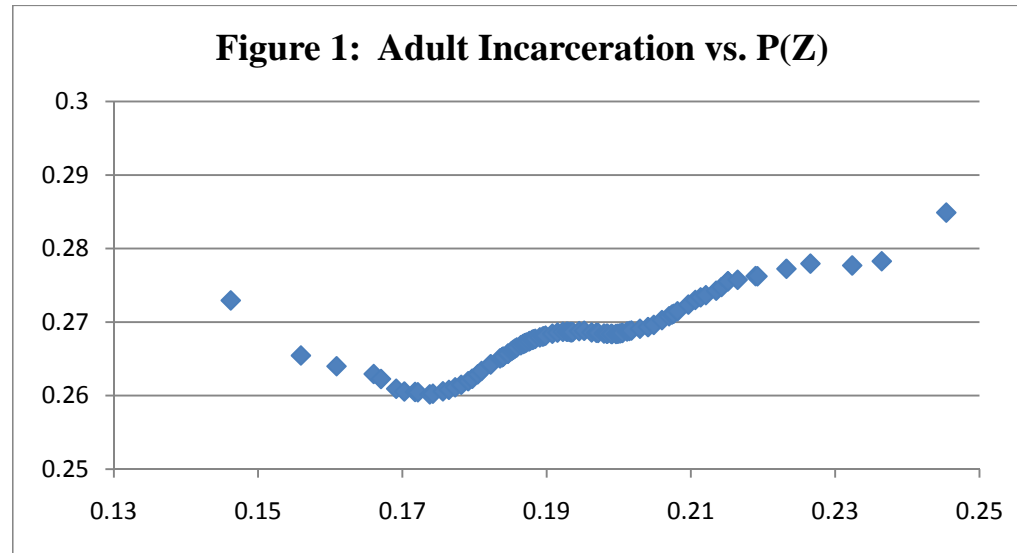
Standard errors clustered at the judge level in parentheses. Demographic controls are indicators for age, race, and sex. Full controls include indicators for the most serious charge, the risk index, and whether the first judge code was missing. All models include court-calendar fixed effects.

Table 5: Compliers & Heterogeneous Treatment Effects

Dependent Variable:	1st Stage (OLS):			2SLS:			Observations
	Ever in Juvenile Incarceration			Adult Incarceration			
Subgroup	Coefficient on Judge Ever-Incarcerated Rate	Standard Error	Mean of Dep. Var.	Coefficient on Ever in Juvenile Incarceration	Standard Error	Mean of Dep. Var.	
Full Sample	0.496	(0.073)	0.193	0.269	(0.116)	0.268	92119
Male	0.505	(0.075)	0.211	0.280	(0.122)	0.305	78449
Female	0.426	(0.103)	0.092	-0.004	(0.176)	0.056	13670
Age < 15	0.540	(0.109)	0.294	0.032	(0.127)	0.290	31766
Age >= 15	0.454	(0.074)	0.141	0.424	(0.163)	0.257	60353
Low Risk Index	0.483	(0.089)	0.167	0.335	(0.152)	0.267	43861
High Risk Index	0.435	(0.083)	0.197	0.016	(0.159)	0.270	23458
"Swing Judges" Only	0.405	(0.061)	0.217	0.342	(0.147)	0.279	45888

Each row represents separate regression for different subsamples labeled in the first column. All models include full controls and court-calendar fixed effects. Standard errors clustered at the judge level are reported in parentheses.





Local quadratic regression with a pilot bandwidth of 0.02 evaluated at each percentile of  $P(Z)$ : the predicted probability of juvenile incarceration estimated with a model that includes only the instrument in addition to court-calendar and year fixed effects. The dependent variable is the residual from regression of the adult incarceration indicator on court-calendar fixed and year fixed effects.

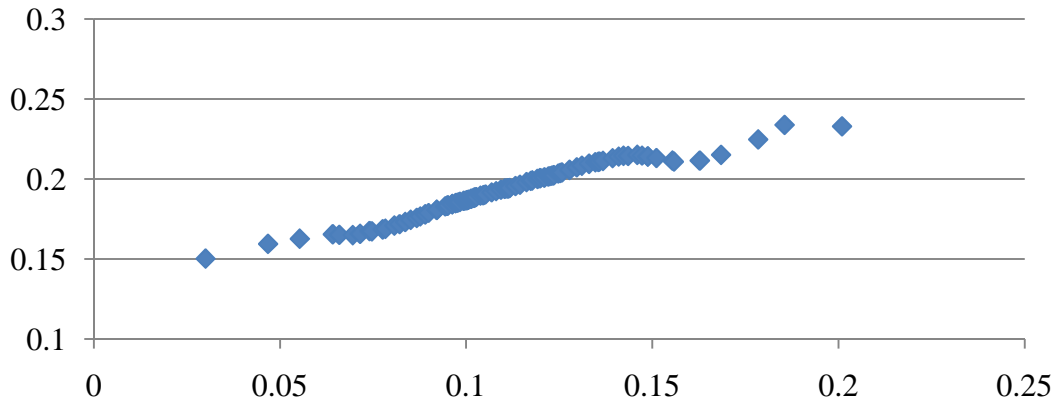
# ONLINE APPENDIX

Table A1: Robustness

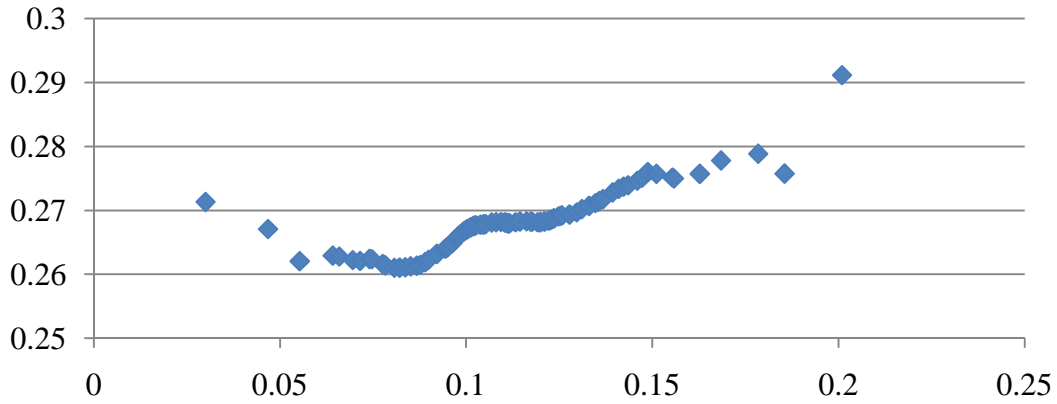
2SLS				
Dependent Variable:	Adult Incarceration			
	Coefficient on Ever in Juvenile Incarceration	Standard Error	Mean of Dep. Var.	Observations
Z Trimmed of extreme 5%	0.286	(0.143)	0.270	87736
Court Calendar x Year fixed effects	0.335	(0.164)	0.268	91023
IV Probit (MLE)	0.315	(0.109)	0.268	92119
Welfare recipient (matching variables)	0.194	(0.109)	0.318	65829

Court-calendar x year fixed effects models limited to calendars with at least 30 first cases in a given year. All models include full controls including court-calendar fixed effects. IV Probit estimate is the average marginal effect.

**Figure A1: First Stage  
Juvenile Incarceration vs. Z**



**Figure A2: Reduced Form  
Adult Incarceration vs. Z**



Local Linear Regression with pilot bandwidth of 0.02 evaluated at each percentile of Z.: the judge incarceration rate. Dependent and independent variables are residuals from regression models with court-calendar and year fixed effects.