Tried as an Adult:  
Evidence of the Crime Effects of Juvenile Transfer  
Using a Regression Discontinuity Design

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Abstract
Many states have enacted juvenile transfer laws that dictate the age at which youth are prosecuted in adult criminal court rather than juvenile court. While a sizable literature examines the effects of these statutes on aggregate crime rates, only a handful of studies have examined whether processing juvenile offenders in the adult criminal justice system reduces subsequent reoffending at the individual level. Using a regression discontinuity design, this study examines the comparative effects of the two justice systems on recidivism for individuals charged with a felony drug offense just weeks before and after the age of majority. Contrary to previous estimates, the results suggest that prosecuting juveniles as adults reduces recidivism. The implications for future empirical research on the juvenile justice system are discussed.

Keyword(s): Juvenile, Quasi-Experiment, Regression-Discontinuity, Recidivism

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Introduction

At the end of the 20th century, rising juvenile crime rates prompted state legislatures to exclude certain adolescent offenders from the juvenile justice system. All fifty states have enacted a variety of discretionary, presumptive, and mandatory juvenile transfer statutes that dictate when juvenile arrestees can or must be moved to the adult criminal system. Legislatures set these thresholds based upon age, offense seriousness, and prior criminal history (Griffin, 2010). A dozen states have also lowered the age of exclusive jurisdiction for the juvenile court from 18 to 17, 16 or 15. These developments reflect the belief that certain juvenile offenses and offenders are better handled by the adult system (Bishop & Frazier, 2000). Some policymakers have argued that the juvenile justice system is too lenient, and that the punitive character of the adult system will better serve the public interest by reducing crime (Bennett, 1996).

Unfortunately, only a handful of scholars have tried to test the empirical assumptions underlying this widespread policy development (Feld, 1987).

Most empirical studies in this area have tested whether a particular statutory boundary between the adult and juvenile system affects juvenile crime in general. The earliest studies examined whether juvenile offending dropped after the statutory age boundary between the juvenile and adult system was decreased (Jensen & Metsger, 1994; Risler, Sweatman, & Nackerud, 1998; Singer & McDowall, 1988). Subsequent studies explored whether states with a higher punitiveness between their adult and juvenile system predicted the trend of offending across their own statutory boundary and whether the passage of direct-file waiver statutes affected juvenile crime rates (Levitt, 1998; Steiner & Wright, 2006). More recently, scholars have examined whether offending rates change as individuals pass the relevant statutory
threshold (Hjalmarsson, 2009; Lee & McCrary, 2005, 2009). In practice, all but one of these studies report minimal evidence of a sizable drop in offending at the statutory boundary.

Scholars have also examined whether a statutory boundary between the adult and juvenile system exerts a specific effect on juvenile reoffending at the individual level—that is, whether a juvenile who was prosecuted in the adult system is less likely to recidivate than if processed in the juvenile system. Generally, these studies have found that prosecuting juveniles as adults increases recidivism (Bishop, 2000; Fagan, 1991, 1995, 1996; Myers, 2003; Podkopacz & Feld, 1996; Winner, Lanza-Kaduce, Bishop, & Frazier, 1997). These studies provide important contributions to the literature, but they also suffer from some methodological limitations. In particular, all of the prior studies have estimated the effect of prosecuting a juvenile as an adult by matching on observable variables. None of the studies can rule out the possibility that the observed correlation between recidivism and adult processing is the result of unobserved selection bias. The present study builds on prior research by exploring the same question with a more stringent empirical test.

This study estimates the effect on recidivism of prosecuting a juvenile in adult court using a regression-discontinuity design. Specifically, we test whether individuals arrested at an age just below the cutoff for the exclusive jurisdiction of the juvenile court are more or less likely to recidivate than individuals arrested just over the age cutoff. Assuming that the true offending rate and the likelihood of arrest do not change across the age boundary—an assumption that can be tested empirically—any changes in recidivism can be attributed to the system in which the individual is processed.
Prior Research

Prior research has focused on two related empirical questions—How differently do the juvenile and adult criminal justice systems treat youth, and do these differences affect subsequent offending? Historically, criminal justice scholars conducted little research on these questions because the differences between the adult and juvenile systems appeared clear. But since the 1967 Supreme Court case *In re Gault*, many of the most obvious distinctions have diminished. First, a long line of Supreme Court cases after *Gault* imported new constitutional protections from the adult system to the juvenile system. And second, juvenile justice has become more punitive and less rehabilitative (Feld, 1997). Lemmon et al. (2005) have shown, for example, that the severity of sentences in juvenile and adult courts in Pennsylvania are similar after controlling for certain legally relevant variables.

Still, differences remain between the two systems. Juvenile courts retain some non-adversarial features, provide more rehabilitative programming, and impose lower maximum penalties than the adult criminal justice system (Office of Juvenile Justice and Delinquency Prevention, 1999). These realities are reflected in the perceptions of young offenders. Qualitative research has shown that young offenders believe the adult system is more punitive, but less transformative than “deep-end” juvenile justice placements (Lane, Lanza-Kaduce, Frazier, & Bishop, 2002).

The empirical literature has examined the effect of juvenile transfer policies in two ways. First, prior work has estimated the *general effect* on offending by examining the offending rates of all juveniles who would be prosecuted in the adult system if charged with a crime covered by the policy. Second, studies have explored the *specific effect* on juveniles who have already been prosecuted as adults due to the policy. We discuss prior work in each of these groups separately.
General Offending Studies

The earliest research in this area examined legislative enactments that restricted the jurisdiction of the juvenile court (Jensen & Metsger, 1994; Risler et al., 1998; Singer & McDowall, 1988). Singer and McDowall (1988) assessed the effect of a 1978 New York State law that decreased the age of exclusive jurisdiction for the juvenile court for certain offenses. The authors applied an interrupted time series analysis on data from New York City using Philadelphia as a comparison site. Singer and McDowall (1988) found little evidence of an effect on crime. Jensen and Metsger applied a similar research design to Idaho’s 1981 juvenile mandatory transfer statute, and found no drop in offenses targeted by the law (Jensen & Metsger, 1994). Somewhat surprisingly, given the public safety focus of the statute, they reported a jump in violent crime after the post-waiver-enactment period in Idaho, which could reflect a labeling or related criminogenic amplification process from processing juveniles in the adult system. Finally, Risler et al. employed a single difference estimator to examine the effect of a 1994 juvenile waiver statute in Georgia (Risler et al., 1998). All of these studies share a common interrupted time-series or panel research design that tests for a change in offending in an age group of juveniles who previously were processed in the juvenile system, but would now be prosecuted as adults due to the legislative enactment. Two of the studies reported no change in offending, while one found an increase. Together, they provide only weak evidence of a causal effect.

Scholars have also estimated the relative effect of the juvenile and adult systems on offending by comparing crime rates among individuals just under and over the age of majority threshold. In a 50-state study, Levitt (1998) found that adult offending rates are lower relative to
juvenile offending rates in states that are comparatively more punitive towards adults than juveniles relative to other states. This study represents a notable contribution to the literature because it examines all fifty states, and uses state-by-state variation in statutory boundaries to estimate the crime effects of prosecuting juveniles as adults. It is also the only aggregate-level study to report a sizable drop in violent offending linked to lower statutory boundaries.

More recently, three new studies have reported little evidence that prosecuting juveniles in the adult system affects crime. Steiner and Wright examined direct file statutes, which empower prosecutors to prosecute certain juveniles without independent judicial review. Based on a 14-state interrupted time series, the authors find little evidence that the direct file states affected crime. Nine of the fourteen states experienced no change.2 Similarly, using a longitudinal sample from Florida, Lee and McCrary reported a small drop in offending after the age of majority (18) for juveniles with at least one prior arrest before the age of 17 (Lee & McCrary, 2005; 2009). Finally, Hjalmarsson (2009) examined a nationally representative sample of juveniles in the National Youth Longitudinal Survey, and reported “little evidence of a discontinuous change in delinquent behavior, over and above general aging trends, at the age of criminal majority” (Hjalmarsson, 2009, p. 245).

Taken together with earlier studies, the weight of available evidence is most consistent with the absence of a large discontinuity in aggregate offending at statutory boundaries, providing support for the statement by Wolfgang and colleagues that “[c]riminal behavior, like any other form of human behavior, is continuous and develops independent of legal boundaries such as the switch from juvenile to adult criminal status” (Wolfgang, Thornberry, & Figlio, 1987, p. 6). However, the studies discussed in this section only test whether juveniles alter their

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2 Only two states experienced a clear increase in crime after enactment, and only one state experienced a clear decrease.
behavior in response to the different sanctions that they could encounter if they offended after passing the relevant statutory age boundary. They cannot answer how juveniles would respond after actually encountering the sanctions. The studies in the following section address this question.

**Individual Level Re-offending Studies**

The earliest individual-level study examining the effects of prosecuting juveniles as adults compared the recidivism rates of juveniles who were waived into the adult system against juveniles who were retained in the juvenile system (Podkopacz & Feld, 1995, 1996). The authors reported sizable differences in re-conviction (or re-adjudication). Approximately 58% of waived juveniles were re-convicted in two years while only 42% of retained juveniles were re-convicted (Podkopacz & Feld, 1996). As the authors note, however, “the population selection biases inherent in the waiver process and the absence of a [comparable] control group make it difficult to attribute differences in recidivism rates . . . to ‘treatment’ effects.” (1996: 491). In other words, the study could not ensure that the observed effects were not the result of pre-existing differences between waived and retained juveniles.

To address this estimation problem, Fagan (1996) compared recidivism in cases from four adjacent counties in New and New Jersey. New York State excludes burglary and robbery cases for defendants over fifteen-years-old from the jurisdiction of the juvenile court. The authors compared similar cases in New Jersey, where burglary and robbery cases are processed in the juvenile court until age seventeen. Controlling for observable covariates, the study reported significantly lower re-offending rates in robbery cases for fifteen and sixteen-year-olds in New Jersey (where they are typically processed in the juvenile system). By controlling for key
observables, Fagan (1996) bolstered support for the theory that waiving juveniles into the adult system increased re-offending. But, several methodological limitations remain. First, the study could not control for unobservable differences between waived and non-waived youth. In particular, it is possible that the juvenile/adult threshold induced upstream changes in police or prosecutorial charging decisions which would alter the kinds of offenders included in the sample. For example, police officers in New York or New Jersey choosing to bring different charges for the same underlying offense conduct based in part on the different legal age of majority in their respective jurisdictions. In the absence of an exogenous assignment mechanism (e.g., random assignment or assignment by covariate), it is impossible to assure that upstream actors did not alter the flow of cases in subtle ways that correlate with recidivism.

Bishop and colleagues examined the effect of juvenile waiver in Florida by matching waived and non-waived juveniles on offense severity, criminal history, and key demographic variables (Bishop, Frazier, Lanza-Kaduce, & Winner, 1996; Winner et al., 1997). Juveniles transferred to the adult system were re-arrested more quickly and more frequently than retained juveniles (Bishop et al., 1996). These differences persisted with a seven year follow-up window (Winner et al., 1997).

More recently, Myers (2003) examined a sample of waived and retained juveniles charged with violent offenses in Pennsylvania. After controlling for observed differences between these two groups as well as introducing a two-step selection model to address the problem of selection bias, Myers reported a 38% re-arrest rate for waived juveniles and a 29% re-arrest rate for retained juveniles.

Taken together, all published studies on the effects of processing a juvenile in the adult system show an increase in recidivism. The results imply that doing so is counterproductive
because it denies youth the benefits of the juvenile system, or exposes them to the harmful consequences of the adult system. But, widespread recognition that selection effects may explain the observed results (Myers, 2003; Smith & Paternoster, 1990) has not yet lead to stronger research designs.

The present study builds upon prior work by examining the effect of processing 17-year-old offenders charged with a felony drug crime in the adult system. This analysis rests upon a regression discontinuity research design, which can distinguish the treatment effect of the adult system from observed and unobserved selection mechanisms. This paper thus offers a more stringent empirical test of the effect on recidivism of transferring juveniles to the adult system.

_Empirical Tests and Causal Theories_

Prior studies have framed the relevant empirical question in terms of deterrence—i.e., does prosecuting juveniles as adults deter crime (Bishop, 2000; Jordan & Myers, 2011). But, in general, the research designs and empirical findings in the literature can only show that offending is or is not affected by the system in which youth are processed. They cannot distinguish between different causal theories about why one system affects offending. For example, studies finding that offending decreases when juveniles are prosecuted as adults provide equal support for multiple theories. The reduction in offending might be explained by increased deterrence or by increased incapacitation. Similarly, studies that find offending increases when juveniles are prosecuted as adults provide evidence consistent with the proposition that the treatment focus of juvenile court is beneficial and the proposition that the punitive nature of the adult system amplifies criminality. The current study is subject to the same limitation: the analytic design can rigorously estimate the bundled effects associated with

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3 For one exception, see Lee & McCrary (2009).
prosecuting a juvenile as an adult, but it cannot identify the causal origin of each of the effects in the bundle.

**Data and Methods**

The research setting for this study is Chicago, Illinois, the site of the nation’s first juvenile court. In Illinois, the age of majority is seventeen for most felonies. Thus, most accused felons under seventeen years-of-age are processed in the juvenile system, and all accused felons over seventeen are processed in the adult system. Illinois also has statutory exclusion laws which specify when the combination of offense, age, and prior record require cases involving juvenile offenders to be tried in the adult system. Illinois has a range of transfer thresholds, including excluded jurisdiction, mandatory transfer, presumptive transfer, and “once an adult always an adult” juvenile transfer provisions. Most of these transfer statutes are discretionary, and are therefore less amenable to rigorous causal analysis. For this reason, the present study examines cases subject either to the exclusive jurisdiction of the juvenile court, or the adult criminal justice system based on observable case characteristics (i.e., charges and age). In practice, this restricted

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4 Nine other states also use seventeen as the age of majority. Most states (39) set their boundary at 18. Two states (New York and New Mexico) set it at 15 (Griffin 2010).
5 Twenty-eight other states also have statutory exclusions. A detailed description of the relative statutes and policies can be found in (Bostwick, 2010).
6 Cases covered by mandatory transfer statutes include all cases in which a juvenile, who is at least 15 years of age, is alleged to have committed 1) a forcible felony (e.g., murder, criminal sexual assault, robbery, burglary, arson, kidnapping, aggravated battery) related to gang activity having been previously adjudicated a delinquent for a felony, 2) a felony related to gang activity having been previously adjudicated a delinquent for a forcible felony, 3) an offense eligible for presumptive transfer having been previously adjudicated a delinquent for a forcible felony, or 4) an aggravated discharge of a firearm in or near (within 1,000) feet) a school, at a school activity, or in a school vehicle.
7 Cases covered by the presumptive transfer statute include juveniles (15 or older) charged with a Class X felony other than armed violence, aggravated discharge of a firearm, or other enumerated charges.
our analysis to a subset of felony arrests that are processed in the juvenile system for suspects under seventeen, and in the adult system for suspects over seventeen.8

A second feature of the Chicago criminal justice system narrowed our focus further. In the Cook County District Attorney’s Office, a senior prosecutor reviews all non-drug felony cases before charges are officially filed (Chicago Police Department, 2012). This policy is in effect for the adult system, but not the juvenile system. Based on related work showing that felony review in the adult system may alter charging practices near the age of majority (Grunwald & Loeffler, 2013), we limited our analysis to felony drug crimes—an offense unaffected by the coincident change in prosecutorial policy.9

The data for this study were requested and received from the Research and Evaluation Division of the Chicago Police Department in early 2013. The data cover all arrests from January 1999 until February 2013. After excluding misdemeanor cases as well as non-drug felony cases from the sample, a preliminary sample was formed of felony drug cases from 1999 to 2008. Recidivism was calculated as a binary indicator of whether an individual was re-arrested by the Chicago Police Department for a felony charge within 4 years of the present offense.10 In order to prevent data censoring, all cases after 2008 were excluded from the analysis sample.11 Upon further inspection, it was determined that missingness for juvenile disposition information was more prevalent for arrests occurring prior to 2005. Therefore, all cases prior to 2005 were

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8 As of January 1, 2010, Illinois treated all youths 17 years of age or younger charged with committing a misdemeanor on or after that date as juveniles. Since the present analysis only examines cases prior December 31, 2009, this statutory change should have no impact our findings.

9 The specific felony drug offenses include—720 ILCS 570.0/401-C-2, 720 ILCS 570.0/401-D, 720 ILCS 570.0/402, 720 ILCS 570.0/402-A-2, 720 ILCS 570.0/402-C.

10 A more expansive recidivism definition including misdemeanor arrests was considered but ultimately rejected. The inclusion of misdemeanor arrests led to certain age groups in the sample having insufficient variation in the dependent variable to allow for reliable estimation.

11 However, these cases were still used to compute recidivism for individuals with arrests prior to that date.
excluded since we could not determine whether these arrests were processed in the juvenile or adult system. This missing data problem did not affect later years.

The resulting analysis file includes 81,629 arrests, representing 9.4% of all felony arrests from the extract of all felony arrests from 1999 to 2013 and 33.3% of all felony arrests from the years 2005 to 2008.

Regression discontinuity

The regression discontinuity (RD) is a quasi-experimental research method designed to estimate causal relationships under weaker methodological assumptions than other common methods in criminology. This design has been applied in criminology (Berk & Leeuw, 1999; Berk & Rauma, 1983), education (Thistlewaite & Campbell, 1960), political science (Lee, 2008), and economics (Angrist & Lavy, 1999; Imbens & Kalyanaraman, 2012; Ludwig & Miller, 2007).

An RD is often appropriate where treatment assignment is determined by a subject’s location on a quantitative variable with respect to some specified threshold value. In our study, for example, subjects are processed in the juvenile system if they are under seventeen years of age, and they are processed in the adult system if they are over seventeen years of age. The RD estimates the comparative effect of processing a juvenile in the adult system by comparing the recidivism of arrestees that are just a few weeks younger, and a few weeks older than seventeen.

An RD has three main assumptions. First, as described above, treatment assignment must be a function of a threshold on a quantitative variable (e.g., age). Second, treat assignment cannot be violated by strategic behavior that correlates with the outcome variable. Arrestees, for example, should not be able to “re-assign” themselves to the juvenile system by lying about their age to a police officer. Third, the threshold (e.g., age seventeen) cannot coincide with any other
discontinuity that is correlated with the dependent variable. In some cases, a fourth assumption is also necessary—that the correct functional form of the relationship between the assignment covariate and the outcome is known. If these basic assumptions are met, then an unbiased estimate of the effect of the treatment can be estimated, at least for cases near the threshold. Prior work in criminal justice has shown that, when the assumptions are satisfied— the RD performs similarly to a randomized experiment (Berk, Barnes, Ahlman, & Kurtz, 2010; Berk & de Leeuw, 1999).

The design of the current study is similar to Lee & McCrary (2005; 2009), which estimated the short-term incapacitation effect of prosecuting juveniles as adults (i.e., thirty days to one year) as part of a larger analysis of density changes in offending at the legal age of majority. Due to our interest in the effect of the adult system on recidivism more generally, we use a substantially longer follow-up period of four years. Thus, our analysis measures the joint effects of deterrence and incapacitation. Our analysis also focuses on drug felonies in particular.

Following Berk (Berk et al., 2010; Berk, 2010), we employ the general linear model to estimate the log-odds of re-arrest:

\[
\log \left( \frac{p_i}{1 - p_i} \right) = \beta_0 + \beta_1 t_i + \beta_2 f(x_i)
\]

where \( \beta_0 \) is the intercept, \( t_i \) is a binary indicator that takes the value of 1 if a case is above the statutory age threshold, \( \beta_1 \) is the average treatment effect of being processed as an adult, and \( \beta_2 f(x_i) \) represents the relationship between age and the outcome measure.

To assess sensitivity to alternative specifications, we fit seven different models. In model (1), \( \beta_2 f(x_i) \) is set to zero and all offenders arrested more than 60 days before their 17th birthday, or more than 60 days after their 17th birthday are excluded from the analysis. Thus, model (1) estimates the simple mean difference on a restricted sample of offenders arrested within just two
months of their 17\textsuperscript{th} birthday. Model (2) includes all 81,629 offenders in the sample, and assumes that age and re-arrest are related linearly. Model (2) is implausible because the relationship between age and re-arrest is non-linear (Berk, Sherman, Barnes, Kurtz, & Ahlman, 2009; Gendreau, Little, & Goggin, 1996). To address this concern, we fit model (3) on a restricted sample that excludes observations more than two years from the threshold.

Alternatively, we can account for the non-linearity between age and crime by modeling age as a higher-order polynomial. In model (4) we substitute a quadratic polynomial for $\beta_2 f(x_i)$, and in model (5) we substitute a quartic polynomial.\footnote{A quartic polynomial takes the form: $f(x_i) = x_i^4 + x_i^3 + x_i^2 + x_i$.} A flexible nonparametric regression model can also be estimated that makes fewer assumptions about the underlying relationship between the assignment covariate and the dependent variable. In models (6) and (7), we apply a General Additive Model to fit the relationship between age and the dependent variable (Berk 2008).\footnote{Any of these models can be combined with interactions. However, a significant interaction does not provide strong evidence for a treatment effect at a discontinuous threshold.}

Testing Model Assumptions

We began by checking the conditions required for a valid RD. First, we tested whether treatment assignment varied discontinuously at the relevant age threshold. Figure 1 plots the probability of being processed as a juvenile against age at arrest in days. This plot shows that virtually all juvenile cases were processed in the juvenile system up until age 17. No cases were processed in juvenile court after age 17, which confirms treatment compliance at the threshold. Adult court processing information was not available in our data. We assume that adult court processing data would form a mirror image of Figure 1.
Treatment compliance does not guarantee that comparable cases were exposed to the treatment and control conditions. We, therefore, conduct a series of additional density and balance tests. Figure 2 plots the frequency of arrests by age in days. The figure shows rapid changes in the density of arrests—as is typical of the age-crime curve in the late teens—but there is no visually discernible evidence that the density changes discontinuously at the threshold (or anywhere else along the age-crime curve).\textsuperscript{14} We also conducted balance tests on observable pre-arrest covariates for a sixty-day window on either side of the age threshold. Table 1 revealed no evidence of a systematic difference between cases on either side of the boundary for observable covariates using either parametric $T$-tests or non-parametric Wilcoxon $T$ tests. Together, the treatment compliance, density and covariate balance tests suggest that the basic requirements of the RD are met by our sample. It is therefore likely that RD provides a rigorous estimate of the effect of processing juveniles near the threshold as adults.

**Results**

Initially, the effects of moving from the juvenile to the adult justice systems along the age assignment covariate were calculated using a comparison of mean outcomes on either side of the threshold. This approach is comparable to assuming that the day upon which each juvenile was arrested was essentially random with respect to the exact location of the boundary. If this assumption is sustainable, then the difference in means provides an unbiased estimator of the treatment effect. A 60 day window on either side of the threshold was used to calculate this difference, which was implemented as a logistic regression of recidivism on a binary treatment indicator. This estimate, reported in Table 2, is both negative and sizable. The narrow window

\textsuperscript{14} For examples of what discontinuous change in charge density for this population would look like, see Grunwald & Loeffler, 2013.
produced large standard errors, but enough precision was maintained to allow for statistical significance at conventional values (P<0.05).

Since the mean differences estimator is potentially vulnerable to bias by a slope in the relationship between age and recidivism around the threshold, the second estimator used was a simple logistic regression of re-arrest on age from the threshold (in days) and a dummy variable indicating a case’s position on the left or right-hand side of the threshold. This estimator provides an unbiased estimate of treatment at the threshold, assuming the absence of non-linearity in the relationship between age and recidivism. The unexponentiated logit coefficient for this estimator, reported as Model 2 in Table 2, is still negative but much larger and even more highly significant. As mentioned previously, these results are unlikely to be accurate due to the known non-linearity in the age-recidivism relationship (Berk et al., 2009; Gendreau et al., 1996). A re-estimation of this linear model using weights, also reported in Model 2 of Table 2, confirmed that the many observations far from the threshold were biasing the coefficients. If these observations had similar re-arrest rates to observations closer to the threshold, this problem might have been less substantial, but recidivism in the late-teens and early twenties, at least for this sample, is rapidly declining with each additional year.

Model 3 reports the results of a first attempt at addressing the non-linear relationship between age and recidivism. A bandwidth of two years on either side of the threshold was used to subset the data. Within this bandwidth, the relationship between age and recidivism is well-approximated by a linear function. The results of fitting a linear model within this threshold are consistent with the mean differences model, but with much greater precision due to the additional 20 months of data on either side of the threshold.

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15 Weights were calculated by taking the inverse of the number of months before or since the offender turned 17. All offenders within one month of seventeen were given a weight of 1.
To better understand the non-linearity in the age/recidivism relationship for this sample, a double-sided quadratic model was also fit to the weighted and unweighted sample (Figure 3 and Model 4). The large differences between the resulting coefficients from each model suggested that the unweighted sample, like the unweighted linear model, was highly affected by observations far from the treatment threshold. In model 5, a quartic polynomial was fit to the relationship between re-arrest and age, with a binary indicator for treatment status included. The results of this model were quite similar to the mean differences model, but the use of the many additional observations beyond the 120 day window led to much greater statistical significance as was also found with the linear estimator in model 2. Unlike the linear or quadratic models, the quartic polynomial was unaffected by distant observations, such that inverse weighting of the observations had no impact on estimated coefficients. In models 6 and 7, non-parametric generalized additive models (GAM) were used to estimate the change in re-arrest probability at the age threshold (Hastie & Tibshirani, 1990). GAMs have previously been used in the regression discontinuity literature to avoid making strong modeling assumptions associated with less flexible parametric approaches (Berk et al., 2010). Model 6 uses a more rigid implementation of GAM and model 7 used a more flexible version. Both models resulted in very similarly sized estimated coefficients, which were somewhat larger than the coefficients from the polynomial model.

As a final check of the results of the quartic polynomial estimator, this model was re-estimated using a random shuffle test. The shuffle test substituted 250 randomly selected and non-theoretically relevant ages within a bandwidth of -1000 to +2000 days. The shuffle test whether the magnitude of the estimated coefficient at the true threshold was larger than the coefficients observed at these other points on the age variable (Angrist & Pischke, 2009; Borjas,
2005). Figure 4 shows that the estimated coefficient at the true threshold is equal to or smaller than the estimated coefficient at 14 percent of the random shuffles. All of these equally sized or larger shuffle estimates are concentrated in the mid-teens when rapid changes in the age-recidivism curve are occurring, which suggests that these rapid slope changes or related model instability could explain the large shuffle estimates.

In order to observe whether our estimates were sensitive to a sample member’s number of prior contacts or to the estimation problems created by individual sample members contributing multiple cases to the analysis sample, we next estimated the preferred quartic polynomial model, along with the differences-in-means and quadratic models, across different levels of prior criminal history among juveniles. Table 3 shows that the estimates are relatively consistent across prior criminal history levels as well as with the estimates for the entire sample presented in Table 2.

Finally, we re-estimated our preferred quartic model using four different follow-up periods (1 year, 2 years, 3 years, and 4 years) in order to determine whether our estimates were being driven solely by possible incapacitation effects. If our estimated effect shrunk with a lengthening of the re-offending follow-up period, it would suggest that our estimates are affected by the timing of our subjects’ release from custody. The resulting estimates reported in Table 4 confirm that our estimates are insensitive to the length of the follow-up. Incapacitation, therefore, is unlikely to be the sole source of the estimated effects and the coefficient can be reasonably interpreted as a bundled treatment effect.
Discussion

Past studies have found that juveniles prosecuted in the adult system reoffend more frequently. On the basis of this evidence, scholars have concluded that exposing youth to the adult system is harmful (Bishop & Frazier, 2000; McGowan et al., 2007). However, certain methodological features of prior work coupled with the results of the present investigation suggest that this conclusion should be approached with caution.

Most prior work has relied upon matching and multivariate regression to estimate the effect of prosecuting juveniles as adults. These methods are only as good as the observable variables that are available. Any unobserved differences are necessarily assumed to be unrelated to the estimator and this assumption then justifies the non-pursuit of any exogenous form of variation in the assignment of cases to treatment conditions. Matching designs, along with the related method of stratification, assist analysts in avoiding “inappropriate comparisons” that commonly occur in regression-based analyses, but they are not infallible (Rosenbaum, 2004). Given the visibility of the transfer boundary to police officers, prosecutors, and judges as well as the need of these actors to make a decision to transfer or not to transfer each eligible juvenile (Fagan & Deschenes, 1990), it seems prudent to assume that any correlation between transferred and retained juveniles could easily be unrelated to the effects of transfer. Even in the studies conducted by Fagan (1991,1995,1996), which exploited the plausibly exogenous variation found in the differing transfer ages in neighboring states (New Jersey and New York), the authors assumed that the different statutory boundaries had not induced any upstream case-processing effects that could then be correlated with recidivism. For example, prosecutors in the lower age boundary jurisdiction deciding to charge some subset cases, perhaps more sympathetic ones, with lessor included offenses rather than robbery (aggravated assault) or burglary (trespass or
theft). Alternatively, downstream effects consistent with the finding that transferred juveniles are more likely to re-offend could be produced if police officers were more likely to re-arrest adult-processed youths as a result of greater access to their criminal record history information, consistent with at least one version of the labeling effects paradigm (Cullen & Cullen, 1978; Lemert, 1951). Finally, processing juveniles as adults could in fact be producing heightened re-offending among affected juveniles. In the absence of some empirical examination of these different possibilities, it is difficult to adjudicate between these plausible explanations for an observational finding that transferred youths are more likely to be re-arrested than non-transferred youths.

The present study was designed to remedy this limitation by examining a potentially exogenous source of variation in the assignment of juveniles to the juvenile or adult justice systems. In contrast to past studies, which assumed the absence of law enforcement strategic behavior, this paper explicitly tested for whether there was evidence of case processing manipulation at or near the boundary between systems. Having found no evidence of such strategic behavior, the policy discontinuity was then used to estimate the likelihood of felony re-arrest within four years after being processed as a juvenile or an adult. The results of this estimation suggest that processing 17 year old juveniles facing felony drug charges as adults is associated with a 25-30% reduction in the odds of re-arrest.

For a number of reasons, we believe that this finding should be treated with caution, consistent with the recommendations of a recent systematic review of the literature on juvenile transfer (McGowan et al., 2007). First, a shuffle test reported a number of hypothetical alternative thresholds (14%) in regions of the age/crime curve where no such effect was anticipated with similarly sized or larger coefficients than the threshold coefficient. Even if these
few sizable shuffle estimates are a function of slope changes or model instability over a year from the policy discontinuity, their presence was unexpected and remains unexplained.

Second, this study focuses on one jurisdiction, and only a portion of its juvenile transfer statute. Other jurisdictions and other age/transfer thresholds may reveal different effects due to the characteristics of the arrestees, the transfer rules, or the juvenile and adult justice systems more generally (Levitt 1998). Additional research is needed on the use and effects of the variety of other transfer statutes. These include discretionary, presumptive, and automatic transfer statutes as well as “once an adult, always an adult” statutes. It is also noteworthy that our treatment effect was estimated on a sample of felony drug offenders with an average of three prior felony arrests. Whether or not this is typical of other jurisdictions, it suggests that our sample had extensive prior contact with the Chicago-area justice systems. While the results hold across different levels of prior criminal history, felony drug offending is a serious charge and it is possible that the effect of processing juveniles as adults varies by prior criminal record and seriousness of current charge.

Finally, while we were able to limit our analysis to a subset of cases subject to a single jurisdictional boundary, it is possible that other aspects of that jurisdictional boundary affect individual outcomes in ways that we have not appreciated. One example of such a feature of the boundary is the change in public access to criminal history information that coincides with the transition from juvenile to adult justice systems. While it is not anticipated that this will directly affect the re-offending behavior of the sample in our study, it is possible that these changes in information availability could lead individuals arrested just above the age of majority to have a more difficult time in the labor market due to stigma associated with having a publicly available
arrest record. This potential problem could be addressed by examining jurisdictions with similar transfer laws but different criminal history information policies.

Beyond replicating the present finding for other samples and transfer laws, future research could also usefully explore the apparent disagreement between general offending and recidivism studies of the crime effects of juvenile transfer statutes. The published general offending studies, which look for evidence of jumps or drops at statutory boundary as a result of discontinuous jumps in the severity of punishment, have shown very little evidence of sizable changes (Jensen & Metsger, 1994; Lee & McCrary, 2005; Singer & McDowall, 1988) with the exception of one study by Levitt (1998). In contrast, recidivism studies report differences in re-offending between transferred and non-transferred juveniles (Bishop et al., 1996; Fagan, 1995, 1996; Myers, 2003; Winner et al., 1997). One possible explanation for this apparent disagreement could be that these two groups of studies are asking different questions. The general offending studies are examining a much larger population of individuals subject to a change in punishment expectations while the recidivism studies are examining a population actually exposed to different punishments. However, this is only one possible explanation and this inconsistency between macro- and micro-level studies is unlikely to be resolved until more studies provide estimates across two levels of analysis in the same jurisdiction (See, for example, Lee & McCrary, 2005).
References


Figures and Tables

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Figure 2. Histogram of Arrests by Age
Figure 3. Probability of Re-arrest within 4 years by Age
Figure 4. Shuffle Test (Using alternative thresholds)
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Figure 2. Histogram of Arrests by Age
Figure 3. Probability of Re-arrest within 4 years by Age
Figure 4. Shuffle Test (Using alternative thresholds)
Table 1. Covariates by Age (Balance Tests—Black, White, Sex, # of Prior Arrests, Any Priors)

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<th>c.sd</th>
<th>mean.diff</th>
<th>stan.mean.diff</th>
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Table 2. Regression Discontinuity Results

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<td>P Value</td>
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<td>(3) Linear, 2 year bandwidth</td>
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<td>(4) Quadratic</td>
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<td>(5) Quartic</td>
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<td>(6) GAM (rigid)</td>
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<td>(7) GAM (flexible)</td>
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### Table 3. Regression Discontinuity Results by Prior Arrest History

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<th>Quartic</th>
<th>N</th>
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<td>0.018</td>
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Note(s): All models are unweighted.
Table 4. Regression Discontinuity Results by Length of Follow-up

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<td>1 Year</td>
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Appendix. Fitted values from Regression Models