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Charles E. Loeffler

Ben Grunwald

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**Processed as an Adult:  
A Regression Discontinuity Estimate of the Crime Effects of Charging  
Non-Transfer Juveniles as Adults**

Charles E. Loeffler<sup>1</sup>  
University of Pennsylvania

Ben Grunwald  
University of Pennsylvania

Abstract

A sizable literature examines the effect of transferring the most serious and persistent juvenile arrestees from the juvenile justice system to the adult justice system. Few studies, however, have tested whether processing juveniles in the adult system has a similar effect on those who are *not* eligible for transfer. Recent legislative changes to the exclusive jurisdiction of the juvenile justice system justify greater scholarly attention on this population. Using a regression discontinuity design, this study estimates the effects of the juvenile and adult justice systems on recidivism for non-transfer-eligible juvenile offenders arrested just weeks before and after the age of majority for drug distribution, a common felony charge. Our results suggest that processing these juveniles as adults slightly reduces the probability of recidivism by between 3 and 5 percent. Based on the rapid onset and limited change in size of these effects over the duration of a 4-year follow-up, and based on the concentration of the effects within a sub-group having the lowest risk of incarceration, we attribute this finding to a combination of enhanced deterrence and incapacitation in the adult justice system. These results suggest that processing juveniles in the adult system may not uniformly increase offending and may reduce offending in some circumstances. Implications for recent state-level changes to the jurisdiction of the juvenile justice system are discussed.

Keyword(s): Juvenile Justice, Regression-Discontinuity, Criminal Recidivism

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<sup>1</sup> Direct correspondence to author at Suite 483, McNeil Building, 3718 Locust Walk, Philadelphia, PA.

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## **I. Introduction**

The primary boundary between the juvenile and adult justice systems is defined by age. During some historical periods, policymakers have lowered the age boundary, believing that exposing juveniles to the adult system would deter crime. In other periods, policymakers have raised the age boundary, believing that rehabilitation in the juvenile system would better foster desistence (Bishop 2000; Griffin 2010). Today, the age of majority varies across jurisdictions in the United States from fifteen to eighteen. Most states also empower courts and prosecutors to transfer serious and repeat offenders below the age of majority into the adult system.

A number of states have recently contemplated or enacted legislation raising the age of majority based, in part, on evidence that juveniles processed in the adult system are harmed by diminished treatment, physical danger, and exposure to criminogenic adult institutions (Baker 2011; Dixon 2009; Ingram 2007; Farrington, Loeber, and Howell 2012; Moore 2011). These findings derive from two academic literatures examining the effects of policies that allow or require the prosecution of serious juvenile offenders in the adult system. The first literature tests whether these transfer policies have any general deterrent effect on the arrest rate of all juveniles in the population, and generally finds little evidence of an effect, perhaps because most juveniles are never arrested nor transferred (Jensen and Metsger 1994; Risler, Sweatman, and Nackerud 1998; Singer and McDowall 1988). The second literature examines whether transferring a specific juvenile in the adult system will affect his or her own probability of recidivism. With some recent exceptions (e.g., Loughran et al. 2010), this literature generally finds that prosecuting a juvenile as an adult increases recidivism by 20 to 30 percent (Bishop et al. 1996; Fagan, Kupchik, and Liberman 2007; Fagan 1996; Podkopacz and Feld 1995, 1996; Winner et al. 1997).

In this study, we investigate a related but unanswered question—what is the effect on recidivism of adult processing for juveniles who are not serious enough offenders to qualify for transfer? We examine the recidivism of felony drug offenders who are processed in the juvenile system if they are less than 17 years old, and are processed in the adult system if they are over 17. Age is thus a plausibly exogenous assignment mechanism that can be used to identify the effect of adult processing on recidivism.

The design of the current study is similar to Lee & McCrary (2009), a working paper that estimates the effects of the age-18 boundary in Florida using a regression discontinuity. In contrast to this previous implementation of the regression discontinuity design, which examined an arrest cohort of all types of juvenile offenders, we examine a sample that includes only non-transfer-eligible juvenile offenders with a range of prior arrests. This feature of our study is important as the effects of criminal justice interventions can vary by prior contact (Zweig, Yahner, & Redcross, 2010). It is also important because the vast majority of juveniles affected by recent changes to the boundaries of the juvenile system are not eligible for transfer to the adult system.

Our results suggest that processing the arrestees in our sample in the adult system decreases their rate of re-offending by 5 percent. We attribute this unexpected finding to two principal differences between the present and past studies. First, many previous studies have examined the effect of juvenile transfer statutes, which usually only apply to juveniles with serious charges (e.g., armed robbery, burglary) or long criminal histories (Puzzanchera and Addie 2014). The results of most studies in the literature may, therefore, not generalize to more typical juvenile crimes like drug, property and simple assault offenses (Puzzanchera 2013). Second, our regression discontinuity design employs a different identification strategy than the

matching design used by most studies. Several systematic reviews have highlighted that matching designs are vulnerable to selection bias stemming from the non-random assignment of juveniles to the juvenile and adult system (Mulvey & Schubert, 2012; McGowan et al., 2007). The regression discontinuity design, by contrast, can overcome this problem using quasi-experimental variation of treatment assignment generated by the timing of juveniles' arrests around their seventeenth birthday.

The remainder of this paper is organized as follows. Section II reviews the prior empirical literature on the effects of processing juveniles in the adult justice system. Section III details the data and methods used in the present investigation. Section IV provides the results of this analysis. And Section V offers a discussion of the principal findings and their implications for future research on the life-course effects of juvenile and adult justice system contacts.

## **II. Prior Research**

Prior work examining the effects of processing juveniles in the adult justice system falls into two main categories. First, studies in the general deterrence literature examine the effect of processing adolescents in the adult system on total juvenile offending. Second, studies in the specific deterrence literature examine whether processing a juvenile in the adult system affects his or her own recidivism. Due to different estimation strategies and populations, these literatures have reached nearly opposite conclusions on the effects of processing juveniles as adults.

### *General Deterrence*

Studies examining the general deterrent effect of processing juveniles in the adult system have used two basic research designs. First, they have examined whether the enactment of a law increasing the probability that juveniles are processed in the adult system affects offending rates

among populations and crimes subject to the law. Three such studies have examined the effect of mandatory transfer statutes. Using a nearby state as a control, Singer and McDowall (1988) used an interrupted time series design to examine the effect of a 1978 New York State law that lowered the age of exclusive jurisdiction for the juvenile court to thirteen for the most serious charges (i.e., murder, kidnapping, rape, burglary, robbery, aggravated assault). Jensen and Metsger (1994) applied a similar design to test the effect of a 1981 law in Idaho, which required juveniles charged with serious crimes (i.e., murder, robbery, forcible rape, mayhem) to be transferred to the adult system. Finally, Risler et al. (1998) employed a single difference design to estimate the effect of a 1994 Georgia law requiring that juveniles over thirteen be transferred for the most serious criminal charges (i.e., murder, manslaughter, rape, child molestation, and armed robbery). All three studies found little evidence of a deterrent effect on the targeted offenses.<sup>2</sup>

Second, prior work has also examined the aggregate density of offending on either side of the age of majority. A 50-state panel study by Levitt (1998) found that juvenile arrest rates above the age of majority were substantially lower in states where juveniles were punished less harshly than adults, where harshness was measured as the ratio of adult prisoners to adult violent crimes over the ratio of juvenile delinquents to juvenile violent crimes. This is the only study in the general deterrence literature to report a sizable drop in violent offending linked to lower statutory boundaries. Two other recent studies have reported limited evidence of a deterrent effect. Using data from Florida, Lee & McCrary reported a small drop in offending at the age of majority (i.e., eighteen) for juveniles with a prior arrest before age seventeen (Lee & McCrary, 2005, 2009).

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<sup>2</sup> Somewhat surprisingly given the public safety focus of the statute, Jensen & Metsger (1994) reported a jump in violent crime after the post-waiver-enactment period in Idaho, which could reflect a labeling or a related criminogenic amplification process stemming from the processing of juveniles in the adult system.

And using a nationally representative sample from the National Youth Longitudinal Survey, Hjalmarsson (2009) 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).

Contrary to early research on juvenile offender perceptions (Glassner et al. 1983), the present body of research suggests that increasing the probability that juveniles are prosecuted in the adult justice system does not affect the rate of juvenile offending in general. It also suggests that offending behavior does not respond to the particular location of the jurisdictional boundary. However, general deterrence studies only test whether juveniles alter their behavior in response to sanctions they might encounter if they continued offending past the relevant age boundary. Left unaddressed is whether processing a juvenile in the adult system will affect his or her own probability of recidivism.

### *Specific Deterrence*

Studies that measure the specific deterrent effect of adult processing typically compare the recidivism of juveniles who were waived into the adult system with the recidivism of juveniles who were not waived (Bishop 2000; Griffin 2010).<sup>3</sup> Waiver can be mandated by statute or left to the discretion of judges or prosecutors. Regardless of their exact form, they all result in the transfer of a select group of juveniles below the age of majority into the adult system based on some combination of age, prior criminal record, and instant offense seriousness.

In the earliest study in this literature, Podkopacz and Feld (1995, 1996) found that approximately 58% of waived juveniles in Minnesota were re-convicted in two years while only 42% of retained juveniles were re-convicted. The authors cautioned that “selection biases

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<sup>3</sup> The academic literature sometimes refers to these studies as “micro-level studies.”

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).

To address this estimation problem, Fagan (1996) compared recidivism in cases from four adjacent counties in New York and New Jersey. New York State law requires juveniles that are fifteen years of age or older and charged with burglary and robbery to be prosecuted in the adult system. In New Jersey, 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) than in New York (where they are processed in the adult system). The study observed no effect in burglary cases. By controlling for key observables, Fagan (1996) bolstered support for the finding that waiving juveniles into the adult system increases recidivism. In a later follow-up, Fagan reported additional evidence that juveniles processed in the adult system recidivate more frequently, and that the effect varies by different subgroups (Fagan et al. 2007).

Bishop and colleagues examined the effect of juvenile transfer in Florida by matching transferred and non-transferred juveniles on offense severity, criminal history, and key demographic variables (Bishop et al. 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, although the opposite results were reported for property offenders (Winner et al. 1997). Similarly, Myers (2003) examined a sample of transferred and retained juveniles charged with violent offenses in Pennsylvania. After controlling for observed differences between these two groups and



introducing a two-step selection model to address selection bias, Myers reported a 38% re-arrest rate for waived juveniles and a 29% re-arrest rate for retained juveniles.

More recently, Loughran and colleagues (2010) used matching to compare the effects of juvenile transfer in a sample of serious adolescent offenders drawn from Pennsylvania and Arizona. Contrary to previous studies that matched on fewer pre-transfer covariates, the authors reported an overall null finding with some evidence of treatment effect heterogeneity, including reduced recidivism for certain sub-groups.

Finally, Lee and McCrary (2009) used a regression discontinuity design to compare the recidivism of adolescents arrested in Florida just before and after they passed the age of majority (18). They found that juveniles processed in the adult system were slightly less likely to be re-arrested. A similar finding was also recently reported by Hansen and Waddell (2014) in their study of juveniles just before and after the minimum juvenile transfer age (15) in Oregon.

Two observations about the literature bear note. First, scholars have suggested that adult prosecution increases recidivism for both transfer-eligible and non-transfer-eligible populations (Redding, 2010; Farrington, Loeber, and Howell 2012). But the literature reveals significant evidence of treatment effect heterogeneity (Fagan 1996; Fagan et al. 2007; Hansen and Waddell, 2014; Lee & McCrary 2009; Loughran et al. 2010), suggesting that the negative effect of adult processing may not generalize to all juvenile populations. Second, most of the evidence that adult processing increases recidivism derives from matching studies that compare transferred and retained juveniles. Recent systematic reviews (McGowan et al., 2007; Mulvey & Schubert, 2012) have expressed longstanding concerns (Smith & Paternoster, 1990) that some of the effects observed in these studies reflect selection bias. These concerns are bolstered by Lee & McCrary (2009) and Hansen & Waddell (2014), both of which report small negative effects of adult

processing on recidivism using analytic methods that are more resistant to unobserved bias. In this study, we examine the effect of adult processing on non-transfer-eligible juveniles to address questions of generalizability, while applying a regression discontinuity to address selection bias.

### **III. Data and Methods**

The research setting for this study is Chicago, Illinois. Prior to 2014 the age of majority in Illinois was seventeen for most felonies. Thus, most felony suspects under seventeen were processed in the juvenile system and all felony suspects over seventeen were processed in the adult system. We exploit this feature of Illinois law to conduct a quasi-experiment comparing the recidivism of suspects arrested just before and after their seventeenth birthday.

Illinois also has a range of transfer rules, including excluded jurisdiction, mandatory transfer, presumptive transfer, and “once an adult always an adult” transfer provisions. Unlike the age-seventeen boundary, most of these transfer statutes are discretionary, and are therefore less amenable to rigorous causal analysis. For this reason, we examine cases subject to the exclusive jurisdictions of the juvenile and the adult courts based on observable case characteristics (i.e., arrest charges and age).<sup>4</sup>

A second feature of the Chicago criminal justice system narrowed our focus further. In the Cook County District Attorney’s Office, a 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. Since prosecutorial felony review in the adult system could alter the characteristics of cases near the age of majority either by rejecting filed arrest charges or causing officers to alter their arresting practices in the shadow of felony review,

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<sup>4</sup> As of January 1, 2010, Illinois treated all youth 17 years of age or younger charged with a misdemeanor on or after that date as juveniles. A similar legislative change for 17-year-old youths charged with felonies went into effect January 1, 2014. Since the present analysis only examines cases prior December 31, 2009, this statutory change should have no impact on our findings.

we limited our analysis to felony drug crimes—an offense unaffected by the coincident change in prosecutorial policy.<sup>5</sup> This ensures that cases handled on either side of the jurisdictional boundary are comparable except for differences in post-arrest processing that result from the adjacent judicial systems at the heart of this inquiry.

The data for this study were obtained 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. 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. In order to prevent data censoring, all cases after 2008 were excluded from the analysis sample.<sup>6</sup> 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 excluded since we could not determine whether they were processed in the juvenile or adult system. After excluding additional arrests lacking an identification number, charge grade, or birthdate, our analytic data file consisted of 78,142 felony drug arrests between 2005 and 2008.

### *Regression Discontinuity Design*

The regression discontinuity (RD) is a quasi-experimental research design that estimates causal relationships under weaker assumptions than other common approaches in criminology. This design has been applied in criminology (Berk and Leeuw 1999; Berk and Rauma 1983; Berk et al. 2010), education (Thistlewaite and Campbell 1960), political science (Lee 2008), and

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<sup>5</sup> The specific felony drug offenses include—720 ILCS 570.0/401-C-2 (e.g., <15 grams of cocaine), 720 ILCS 570.0/401-D (e.g., manufacture/deliver <1 gram of cocaine or 10 grams of heroin), 720 ILCS 570.0/402-A-2 (e.g., possession of >15 grams of cocaine), 720 ILCS 570.0/402-C (e.g., possession <15 grams of cocaine or heroin; <30 grams of PCP; <200 grams of methamphetamine).

<sup>6</sup> These cases were still used to compute recidivism for individuals with arrests prior to that date.

economics (Angrist and Lavy 1999; Imbens and Kalyanaraman 2012; Ludwig and Miller 2007). An RD is often appropriate where treatment assignment is determined by a subject's location on a quantitative variable, often called a running variable. In Illinois, for example, individuals are processed in the juvenile system for crimes committed before they turn seventeen years of age, and they are processed in the adult system for crimes committed after they turn seventeen. The RD estimates the local average treatment effect (LATE) of adult processing by comparing the recidivism of offenders that are just a few weeks younger, and a few weeks older than seventeen. It thus provides a robust causal estimate for juveniles arrested just around their seventeenth birthday, but does not provide an estimate for much younger or older arrestees.

The RD has three main assumptions. First, as noted already, treatment assignment must be a function of a subject's position on a quantitative variable with respect to some relevant threshold (e.g., age seventeen). Second, treatment assignment cannot be undermined by strategic behavior correlated 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. Likewise, police officers should not be able to alter arrestees' treatment assignment by reconsidering their arrest or charging decision upon learning a suspects' age. Third, the threshold cannot coincide with any other discontinuity that is correlated with the dependent variable. If these basic assumptions are met and the correct functional form of the relationship between the running variable and the outcome is known, then the RD design can produce an unbiased causal estimate, 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 and de Leeuw 1999; Berk et al. 2010).

Ideally, we would use age at time of *offense* as the relevant threshold. Instead, we use age at time of *arrest* for several reasons. First, the record linkage for determining age at time of offense is less reliable than the linkage for age at time of arrest. Second, we suspect that the drug arrests in our sample will almost all be “on-view” arrests where the offense and arrest date will be identical. Third, we have excluded all warrant arrests, which are the most likely cases in which there would be a large difference between the arrest date and the offense date.

Following 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. We also estimated this re-arrest model using linear probability models and models with pre-assignment covariates for improved precision, both of which yielded similar results (Appendix Tables A.1 and A.2). Standard errors are calculated at the case level in all models.

Because the relationship between age and crime in our sample is non-linear and unknown, we fit seven different models to test for sensitivity to alternative functional forms. In model (1),  $\beta_2 f(x_i)$  is set to zero. Thus, model (1) estimates the simple difference in means. This model is potentially vulnerable to bias if the relationship between age and re-offending has any non-zero slope. Model (2) addresses this problem by fitting a logistic model, with a linear parameter for age. Model (2) is only plausible if the relationship between age and re-arrest is linear, which is unlikely given past research on the age crime curve (Berk et al. 2009; Gendreau, Little, and Goggin 1996). To address this concern, we model the relationship between age and recidivism

through a series of higher-order polynomials. In models (3), (4) and (5) we substitute a quadratic, cubic and quartic polynomial for  $\beta_2 f(x_i)$ , respectively. These models allow for considerable non-linearity in the relationship between age and recidivism. Additional polynomials, not shown, were fit and the results didn't differ significantly from those reported. In models (6) and (7), we apply a General Additive Model (GAM) to fit the relationship between age and the dependent variable (Berk 2008). GAM models are flexible nonparametric regression models that make less demanding assumptions about the underlying relationship between the assignment covariate and the dependent variable.

We also assess model sensitivity to different bandwidths. We first applied a bandwidth of 60 days, meaning we only included suspects arrested 60 days before and 60 days after their 17<sup>th</sup> birthday. These models are least resistant to bias, but they are also the least precise due to smaller sample size. We also applied 180-, 365- and 564-day bandwidths.

#### *Testing the RD Assumptions*

We began by checking whether the conditions required for a valid RD were present in our sample of cases. 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 confirms treatment compliance as virtually all cases were processed in the juvenile system up until age 17 and none were processed in juvenile court afterwards. Data on adult court processing was not available. We assume it would form a mirror image of Figure 1. Figure 1 also confirms that using age at arrest rather than age at offense is unlikely to bias our results.

Treatment compliance does not guarantee that comparable cases were assigned to the treatment and control conditions. We, therefore, conduct a series of additional density and

balance tests to examine whether strategic behavior by juveniles or police officers might lead to differences in the number or characteristics of cases on either side of the age threshold. If juveniles alter their offending behavior or lie about their age after passing the age of majority, then the density of juveniles arrested at the age threshold would be noticeably lower. If police officers alter their arrest and charging behavior at the age threshold, then the density of arrests for specific charges would change discontinuously. Figure 2a depicts a histogram of the frequency of arrests by age with 60-day bins. The figure shows rapid changes in the frequency of arrests as is typical of the age-crime curve in the late teens. Figure 2b zooms in on the 600 days on either side of the seventeenth birthday, and plots the frequency of arrests with one-week bins. This figure also reveals no evidence of a discontinuous change at the seventeenth birthday (or anywhere else along the age-crime curve). Following McCrary (2008), a test of the log difference in case density on either side of the discontinuity resulted in a p-value of 0.289. A second density test, recently proposed by Frandsen (2014), resulted in a much smaller p-value of 0.033. The disagreement between these two density tests likely reflects the greater weight given to points of support far from the age boundary in the McCrary test as the Frandsen test only focuses on the 3 bins on either side of the assignment threshold. As a compromise between these two approaches, we re-estimated the Frandsen test using weekly rather than daily bins to make it less sensitive to noise. The resulting p-value was 0.092, consistent with the visual evidence reported in Figure 2b. Similarly, a placebo test on alternative thresholds ranging from 100 days before seventeen to 100 days after resulted in 5% of p-values with similar or smaller magnitudes.

Taken together, these tests provide little evidence of strategic behavior, but they do not preclude the possibility of subtler changes in the composition of cases around the threshold. We therefore also conducted balance tests on observable covariates among individuals arrested less

than 60 days before and 60 days after their 17<sup>th</sup> birthday (See Table 1 and Appendix Figure A.1). Table 1 shows that both groups have similar characteristics on race, sex, charge class, number of prior arrests, and any prior arrests. The table also reveals that the sample of cases is overwhelmingly African-American (94%), male (93%) and has a skewed distribution of prior arrests—a quarter have no prior arrests and three-quarters have 1 or more. While this sample is by no means representative of juvenile offending throughout the United States, it is broadly comparable to urban juvenile offender populations (Puzzanchera 2009). Together, these treatment compliance, density and covariate balance tests provide evidence that the basic assumptions of the RD are satisfied.

## **IV. Results**

### *Basic RD Results*

We first examine the effect of adult processing by comparing the recidivism of juveniles arrested in the 60 days before their seventeenth birthday, and the 60 days after. Our first model contains only one independent variable that indicates whether a given subject is below or above age 17. As shown in the first row of Table 2, processing juveniles who are close to their 17<sup>th</sup> birthday in the adult system is associated with a roughly 28 percent reduction in the relative odds of re-arrest or a 5 percent reduction in the probability of recidivism.

This differences-in-means model is vulnerable to bias by a slope in the relationship between age and recidivism around the threshold. We therefore fit a model with a linear parameter for age. The second row of Table 2 shows a slightly smaller negative effect of adult processing (3 percent recidivism reduction) than observed in the differences-in-means model. As the top left panel of Figure 3 reveals, however, the linear model does not properly account for the non-linear relationship between age and recidivism (Berk et al. 2009; Gendreau et al. 1996).



To better account for this non-linearity, we also fit models with higher-order polynomials for age. Row 3 of Table 2 shows that our quadratic model estimates an effect that is similar to the linear model. Once again, Figure 3 shows that the quadratic polynomial poorly accounts for the relationship between age and recidivism. Rows 4 and 5 in Table 2 present the results of cubic and quartic models. The magnitude of the estimated effects are consistent with each other and are slightly smaller than the difference in means model, but they point in the opposite direction. As shown in Figure 3, the cubic and quartic models appear to fit the data relatively well.<sup>7</sup>

Row 6 in Table 2 presents the results for our first GAM model, which estimates that adult processing is associated with a reduction in the probability of offending (3 percent) that is similar to the linear and quadratic models and slightly smaller than the difference in means model. Row 7 shows that the results are similar for a second GAM that provides greater flexibility for the age parameter.

To assess whether these results are sensitive to bandwidth size, we re-estimated all seven models with three alternative bandwidths (180-day, 365-day, and 564-day). The difference-in-differences estimator for all three alternative bandwidths generates a slightly larger estimated effect (7 percent reduction) with improved precision due to larger sample size. Virtually all of the remaining models generated similar coefficients as the 60-day bandwidth difference-in-differences estimator regardless of bandwidth (4-5 percent recidivism reduction). The 180-day higher order polynomials had the least precision, but the 365- and 564-day models in the linear, quadratic and GAM specifications all had sufficient precision to support a statistically significant result consistent with the 60-day model.

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<sup>7</sup> We also tested several higher order polynomials and found no substantive change in the results (results not shown).

To check the robustness of our results, we conducted two random shuffle tests for the differences in means model with a 60-day bandwidth. The first shuffle test randomly selected 500 alternative “hypothetical” age thresholds within a bandwidth of -1000 to +2000 days, and refit the model assuming each of these hypothetical thresholds. The shuffle test assesses whether the magnitude of the estimated coefficient at the true threshold is larger than the coefficients observed at these other randomly selected age thresholds (Angrist and Pischke 2009; Borjas 2005). If the magnitude of the former is general larger than the latter, then the effect is less likely to be the result of noise. However, if the magnitude of the estimate is similar to those generated by hypothetical thresholds, then the result is likely spurious. Figure 5 shows that the estimated coefficient at the true threshold is equal to or smaller than the estimated coefficient at 9 percent of the random shuffles. Figure A.2 reports the results for a second shuffle test examining all possible thresholds between -180 and 180. The coefficient at the true threshold is equal to or smaller than 12% of the hypothetical thresholds, but importantly, 80% of those were within 60 days of the true threshold (i.e., overlap in sample). This indicates that when alternative thresholds are far enough away from the true threshold that they only include subjects below seventeen or only include subjects above 17, they are rarely bigger than the true coefficient. Given the calculated result, it seems likely that the estimated effect is not spurious.

#### *Testing Possible Causal Mechanisms*

In order to examine the possible mechanisms underlying the observed differences in recidivism, we re-estimate the differences-in-means model for subsets of the data by prior criminal history. If a behavioral mechanism is at work, then we would expect a larger estimate for first-time offenders in our sample relative to offenders with prior contacts with the justice system. As is common in subgroup analyses, disaggregating our sample leads to small sample

sizes and instability in our estimates. Table 3 shows that while the estimates for each of the groups differ in size and some are not statistically significant, they are all negative. Perhaps most importantly for our purposes, a simple deterrence explanation for our results would posit the largest effects for individuals with no prior contact with the criminal justice system. The results of our subgroup analysis are not consistent with this expectation providing some evidence against deterrence.

Next, we re-estimate our difference in means model using four different follow-up periods (1 year, 2 years, 3 years, and 4 years) to determine how quickly the observed effects adhere. Table 4 shows that treatment effects do not vary by length of follow-up period, indicating that the effect adheres early and persists. While this early adherence is consistent with either a deterrence or incapacitation hypothesis, the persistence of the effect across all four follow-up periods provides initial evidence that incapacitation alone is an insufficient explanation.

To further investigate the timing of these effects, we examined both the cumulative and non-cumulative re-arrest rates of treatment and control groups after the initial arrest (see Figure 4). The cumulative arrest graph clarifies why model estimates do not differ by follow-up period—a large treatment gap emerges within 1 year and grows only slightly in subsequent years. The non-cumulative arrest graph tells a complementary story: a large gap forms immediately, but the lines quickly converge soon after. This suggests that some form of incapacitation is contributing to the initial difference between the treatment and control groups. However, the fact that offending rates never converge in the cumulative models is hard to explain based solely on incapacitation. The majority of the sample was charged with a Class 4 felony, for which the most common disposition is probation for first-time offenders and a short prison spell for repeat

offenders. One possible explanation for the absence of cumulative convergence is that some adult processed offenders are incapacitated for the duration of the study period. Alternatively, some adult processed offenders may desist entirely as a result of their experience.

In order to separate these two explanations of our results, we re-estimated our models for each charge class separately. If the effects are observed only in the charge class with the highest average and maximum sentence, Class 1, then incapacitation would be the simplest explanation for the observed effects. However, if the observed effects appear in the charge class with the lowest average and maximum sentence, Class 4, then a behavioral effect is the best explanation. Table 5 shows that the effect of adult processing appears in the subsample of Class 4 cases that receive the least exposure to incarceration. First-time-Class-4 offenders are most likely to receive a probationary sentence and the statutory maximum period of imprisonment for these offenses is 3 years (Illinois SPAC 2011). The class 1 cases, by contrast, appear to have a crime differential in the opposite direction, providing further evidence of treatment effect heterogeneity.

## **V. Discussion**

Past studies have found that serious juvenile offenders transferred into the adult system as part of automatic, presumptive, or discretionary transfer mechanisms reoffend more often than similar juvenile offenders retained in the juvenile system. On the basis of this evidence, scholars have concluded that exposing youth to the adult system is harmful (Bishop and Frazier 2000; Farrington, Loeber, and Howell 2012; National Academy of Sciences 2013) and policymakers have begun moving the boundary between the juvenile and adult justice systems for all juvenile offenders (Rubin 2003). However, the existing empirical literature is primarily focused on the effects of juvenile transfer statutes, which generally apply to the most serious or frequent

juvenile offenders. As a result, we have relatively little information about processing more typical juveniles in the adult system. Recent studies, mainly in the field of applied microeconomics, have sought to address this gap in the literature. In general, they have reported little evidence of large increases in offending for the general population of adolescent offenders just beyond the legal age of majority and some evidence for decreases, suggesting that the placement of the exclusive jurisdiction boundary between the juvenile and adult systems may not be as harmful as previously assumed on the basis of the juvenile transfer literature.

The present study was designed to estimate the effect of processing less serious juvenile offenders in the adult criminal justice system using a methodology that addresses longstanding concerns about selection bias in the transfer literature. Our regression discontinuity models estimate that adult processing leads to a 25-30% reduction in the relative odds of re-arrest or an approximately a 3-5% reduction in the probability of re-arrest. Intriguingly, this effect appears in the first few months after arrest, and endures in size for at least four years. While our quasi-experimental research design limits our ability to definitively determine the mechanism underlying this estimated effect, the rapid and sustained difference is not easily explained by a classic rehabilitation or incapacitation theory. Instead, adult processing may exert a behavioral change in some juveniles who would otherwise have continued offending for only a short period of time. If this interpretation is correct, then felony drug offenders, and perhaps other young adult offenders, may benefit from adult processing.

Our results diverge from those of most studies in the recidivism literature. We attribute this divergence to the sample of offenders examined and the estimation strategy employed. The vast majority of recidivism studies examine juvenile transfer, which only applies to the most severe juvenile offenders. It is plausible that adult processing has a different effect on the less

serious felony drug offenders in our sample. Recent transfer studies, which find significant treatment effect heterogeneity, support this conclusion (Fagan et al. 2007; Loughran et al. 2010; Winner et al. 1997). Moreover, our results are similar to those reported in Lee and McCrary (2009) and Hansen and Wadell (2014) as well as being directionally consistent with Levitt (1998). Together, this body of work suggests that processing juveniles as adults may not always increase recidivism, and that for certain young adult offenders near the peak of the age/crime curve, adult processing might reduce offending rates slightly.

This growing diversity of estimates also suggests revisiting some recent policy discussions on the age of majority. The existing recidivism literature, which consists primarily of transfer and waiver studies, has been used to argue that raising the age of majority will lower juvenile recidivism (Farrington, Loeber, and Howell 2012; Ingram 2007; Melone 2007; Moore 2011). It has also been used to argue that the expense of processing juveniles in the juvenile system will not increase costs in the long run (Roman 2006). However, these benefits may not materialize if the findings of the transfer literature do not apply to more typical non-transfer-eligible juvenile offenders. To determine whether the anticipated results of these boundary shifts have in fact materialized, additional evaluations are needed.

Still, our findings should be treated with caution for several reasons. First, our shuffle test reported that 9 percent of randomly selected alternative age thresholds produced estimates that are similar or greater in size. Even if this small number of comparable shuffle estimates are a function of slope changes or model instability, their presence remains unexplained. Second, this study focuses on one jurisdiction, and only a portion of its juvenile justice system. Other jurisdictions and other boundary thresholds may reveal different effects due to the characteristics of the arrestees, the boundary rules, or the juvenile and adult justice systems more generally. It is

possible that juvenile justice systems in other jurisdictions have more positive effects on recidivism, or that adult systems in other jurisdictions have more negative effects. Additional research is needed on the use and effects of other transfer statutes. These include discretionary, presumptive, and automatic transfer statutes as well as “once an adult, always an adult” statutes. 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 is the change in public access to criminal history information that coincides with the transition from juvenile to adult justice systems. We do not expect that this will directly affect re-offending in our sample, but it is possible that changes in information availability lead individuals processed in the adult system to have greater difficulty on the labor market due to the stigma of a public record. This limitation could be addressed by examining jurisdictions with similar transfer laws but different criminal history information policies. Another solution would be to examine the effects of juvenile transfer or exclusive jurisdiction boundary on schooling and labor market outcomes as these life-course dimensions likely will be more sensitive to any criminal record differential.

Beyond the sign or magnitude of estimated effects, the question of mechanisms by which each of these systems might alter the likelihood of re-offending is a topic in need of further attention. Prior studies on processing juveniles as adults have generally framed this theoretical question in terms of deterrence (Bishop 2000; Jordan and Myers 2011). When crimes rates for non-transferred juveniles are lower, scholars have also pointed to the rehabilitative focus of the juvenile justice system and the possible labeling or stigmatizing effects of the adult system caused by greater public access to police, court, and jail records. But, in general, the research designs and empirical findings in the existing literature only show that one system has a higher

or lower level of re-offending than the other. They do not distinguish between different theoretical mechanisms. For example, studies finding that offending is lower when juveniles are prosecuted as adults provide equal support for deterrence and incapacitation theories (Nagin 1998; Zimring 1976). Similarly, studies finding that offending is higher when juveniles are prosecuted as adults provide equal support for the theory that greater treatment opportunities in the juvenile system reduce re-offending and the theory that the severity of the adult system amplifies crime.

Compounding this ambiguity about mechanisms is the ongoing empirical effort to determine whether and to what extent the adult justice system is more severe or punitive than the juvenile system (Kurlychek and Johnson 2004; Levitt 1998). Given the predominantly adverse findings in the recidivism literature, it is tempting to assume that the rehabilitative orientation of the juvenile system or the punitive focus of the adult system explains the observed differences in re-offending. However, only some empirical studies on differences in punishment administered by the juvenile and adult systems have found more severe punishments in the adult system (Fagan 1996; Levitt 1998). Others have found more mixed results (Kurlychek and Johnson 2004; Lemmon et al. 2005). Still, even if the punishment differential is minimal, these two judicial systems differ in other salient ways. 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; Redding 2010). In addition, by default, many juvenile records of arrest and adjudication are sealed while virtually all adult records are publicly accessible. This procedural difference could affect juveniles' ability to find or keep a job. It could also affect the likelihood of receiving a dismissal for future charges. Until the effects of these potential differences between juvenile and adult justice systems have



been examined, it will be difficult to know with certainty why studies observe differences in juvenile recidivism.

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## **Figures and Tables**

Figure 1. Probability of Juvenile Processing by Age

Figure 2. Histogram of Arrests by Age

Figure 3. Probability of Re-arrest within 4 years by Age

Figure 4. Cumulative and Non-cumulative Probability of Recidivism

Figure 5. Shuffle Test

Table 1. Covariates by Age (Balance Tests—Black, White, Sex, # of Prior Arrests, Any Priors)

Table 2. Regression Discontinuity Results

Table 3. Regression Discontinuity Results by Prior Arrest History

Table 4. Regression Discontinuity Results by Length of Follow-Up

Table 5. Regression Discontinuity Results by Charge Class

Figure 1. Probability of Juvenile Processing by Days to Age Threshold

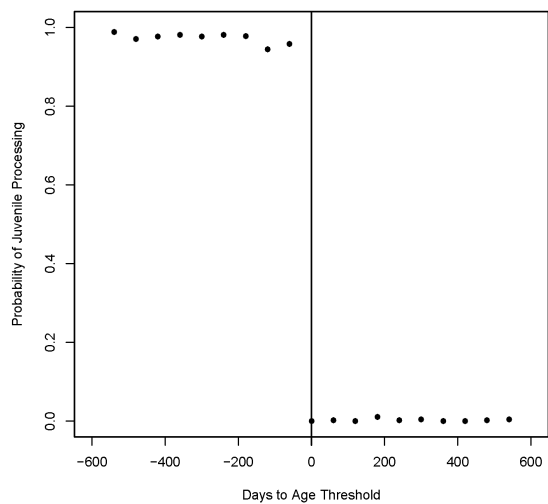


Figure 2. Histogram of Age at Arrests

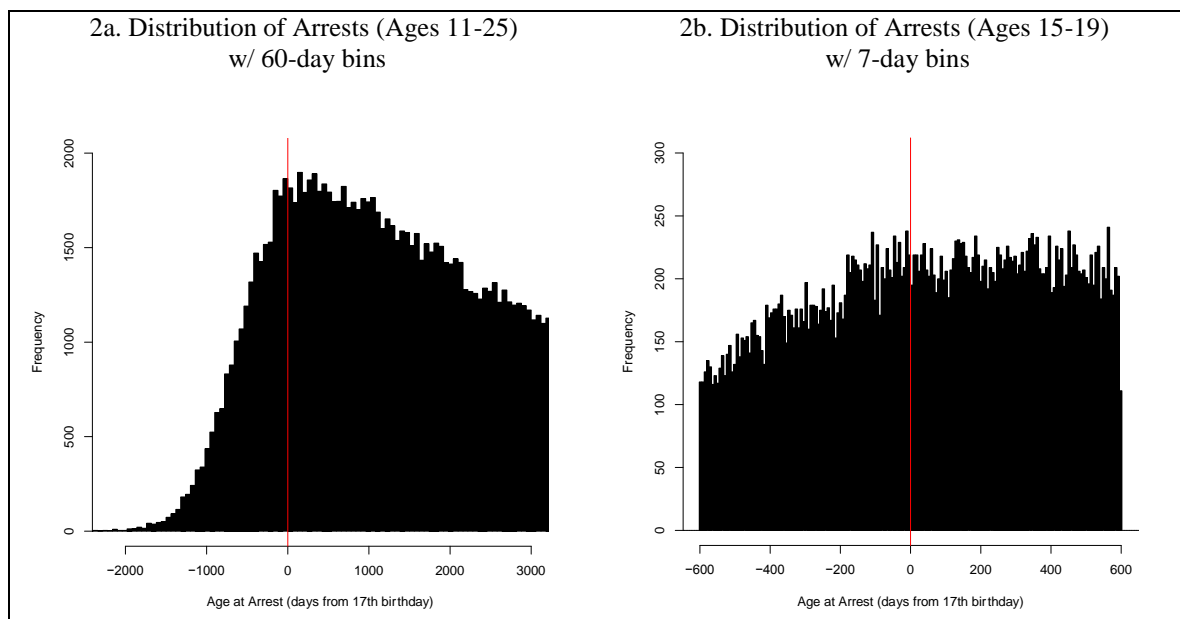




Figure 3. Probability of Re-arrest within 4 years by Age

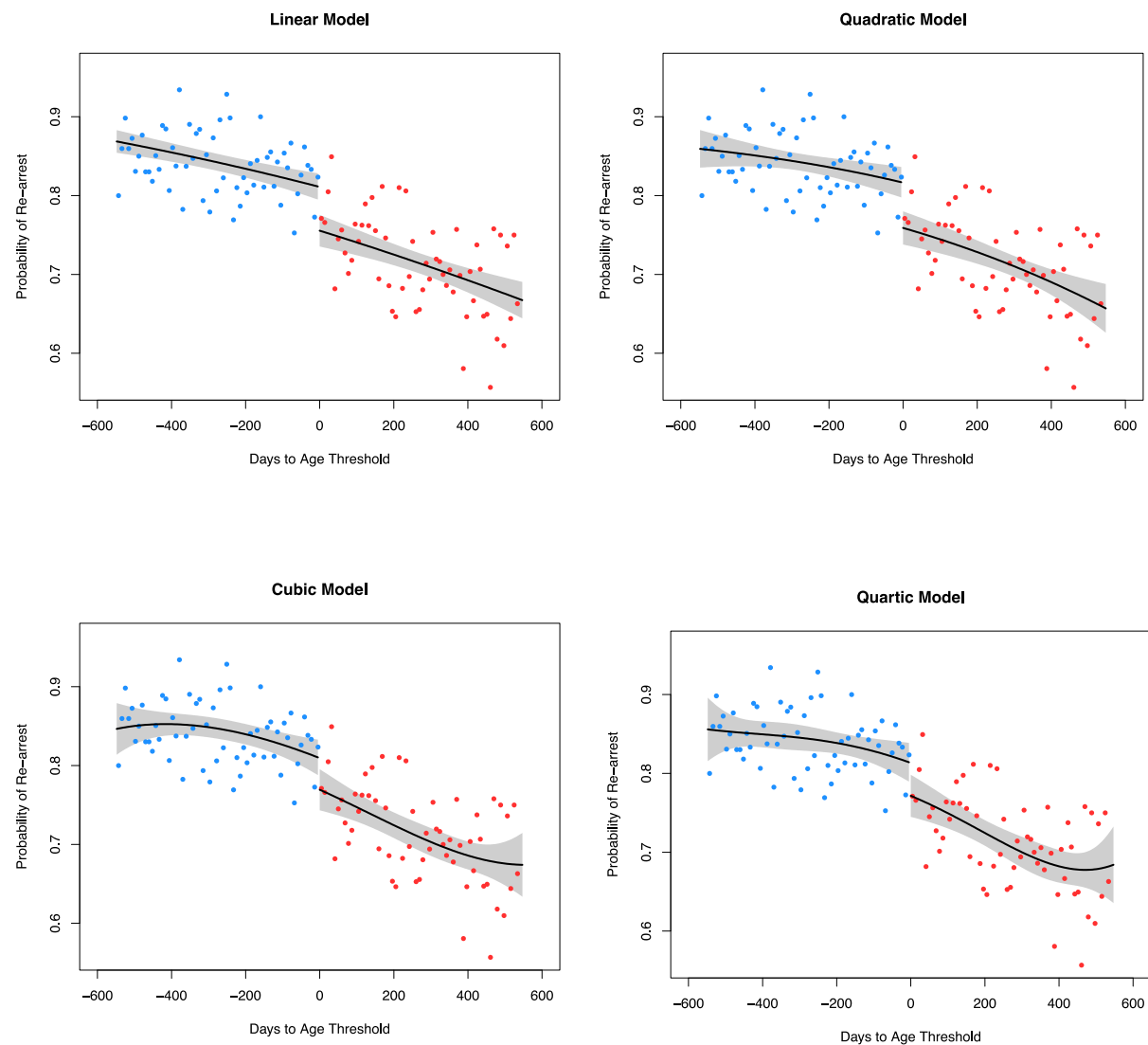


Figure 4

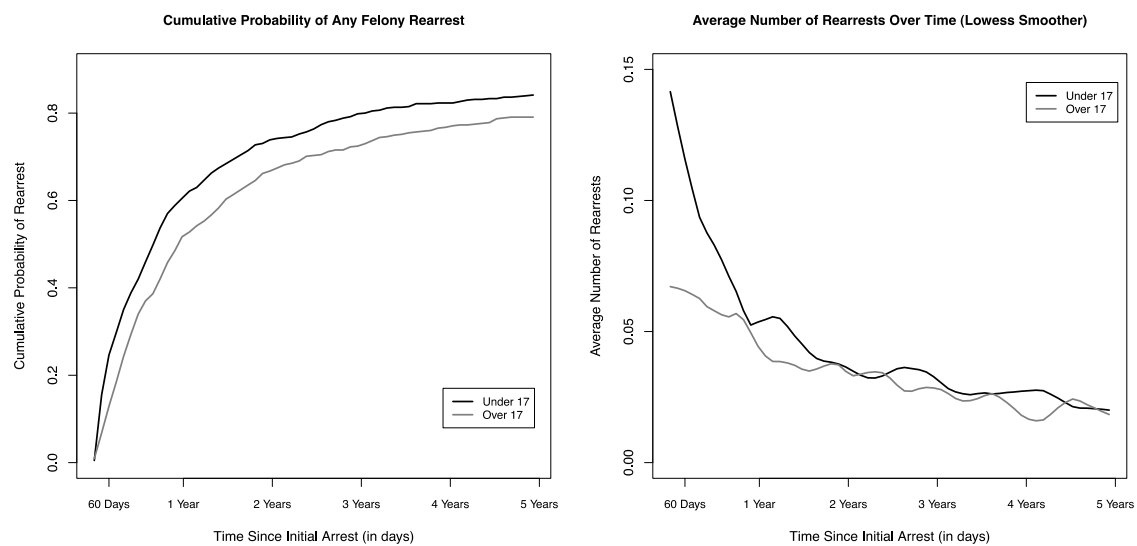


Figure 5. Shuffle Test for Difference-in-Means Estimator (-1000 to +2000 days)

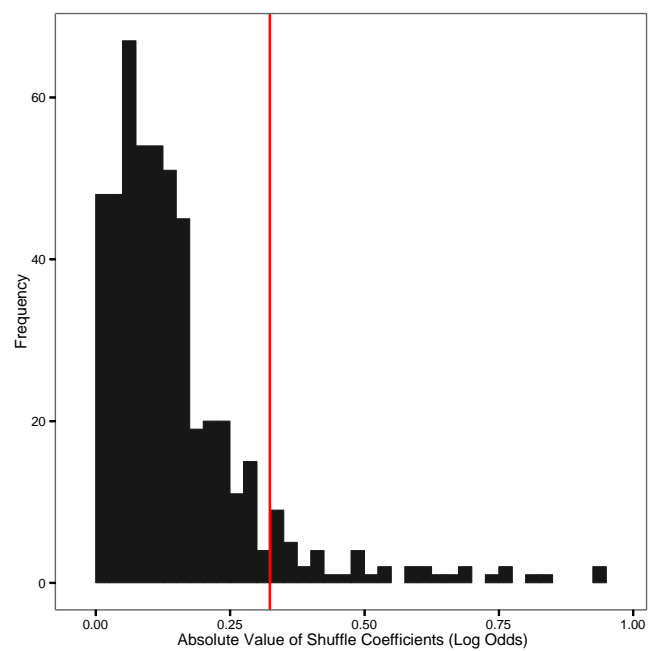


Table 1. Covariates by Age (Balance Tests—Black, White, Sex, Charge Class, # of Prior Arrests, Any Priors)

Variables	Mean Over 17	Mean Under 17	SD Over 17	SD Under 17	Mean Diff.	Standardized Mean Diff.	T Test	Wilcox Test
Black	0.943	0.940	0.233	0.237	0.003	0.013	0.870	0.870
White	0.054	0.056	0.226	0.23	-0.002	-0.009	0.850	0.850
Male	0.927	0.942	0.261	0.234	-0.015	-0.061	0.288	0.285
Charge Class 1	0.077	0.051	0.267	0.221	0.026	0.106	0.075	0.073
Charge Class 2	0.161	0.162	0.368	0.369	-0.001	-0.003	0.964	0.964
Charge Class 4	0.762	0.787	0.426	0.41	-0.025	-0.060	0.315	0.314
Total # Prior Arrests	2.719	2.678	2.748	2.696	0.041	0.015	0.795	0.927
Any Prior Arrest	0.750	0.774	0.434	0.419	-0.024	-0.056	0.338	0.337

Table 2. Regression Discontinuity Results

	60-Day Bandwidth			180-Day Bandwidth			365-Day Bandwidth			564-Day Bandwidth		
	Odds Ratio	P Val	Marg Eff	Odds Ratio	P Val	Marg Eff	Odds Ratio	P Val	Marg Eff	Odds Ratio	P Val	Marg Eff
(1) Diff in Means	.723	.027	-.052	.640	0	-.073	.538	0	-.104	.470	0	-.129
(2) Linear	.850	.585	-.027	.733	.069	-.051	.758	.020	-.046	.727	.001	-.054
(3) Quadratic	.851	.590	-.027	.739	.076	-.050	.751	.018	-.047	.711	.001	-.057
(4) Cubic	1.097	.819	.013	.775	.260	-.043	.756	.079	-.046	.769	.041	-.044
(5) Quartic	1.096	.820	.013	.773	.257	-.043	.748	.071	-.048	.764	.039	-.044
(6) GAM (rigid)	.850	.586	-.027	.733	.069	-.051	.758	.020	-.046	.723	.001	-.055
(7) GAM (flex)	.850	.586	-.027	.733	.070	-.051	.758	.020	-.046	.729	.005	-.053
N	1,164			3,312			6,363			9,439		

Table 3. Regression Discontinuity Results 60-day bandwidth by Prior Arrest History

Priors	Diff in Means			N
	Odds	P Val	Marg Eff	
0	.864	.552	-.034	277
1	.498	.033	-.121	222
2	.955	.922	-.005	163
3+	.668	.156	-.040	502
All	.723	.027	-.052	1,164

Table 4. Regression Discontinuity Results 60-day bandwidth by Length of Follow-up

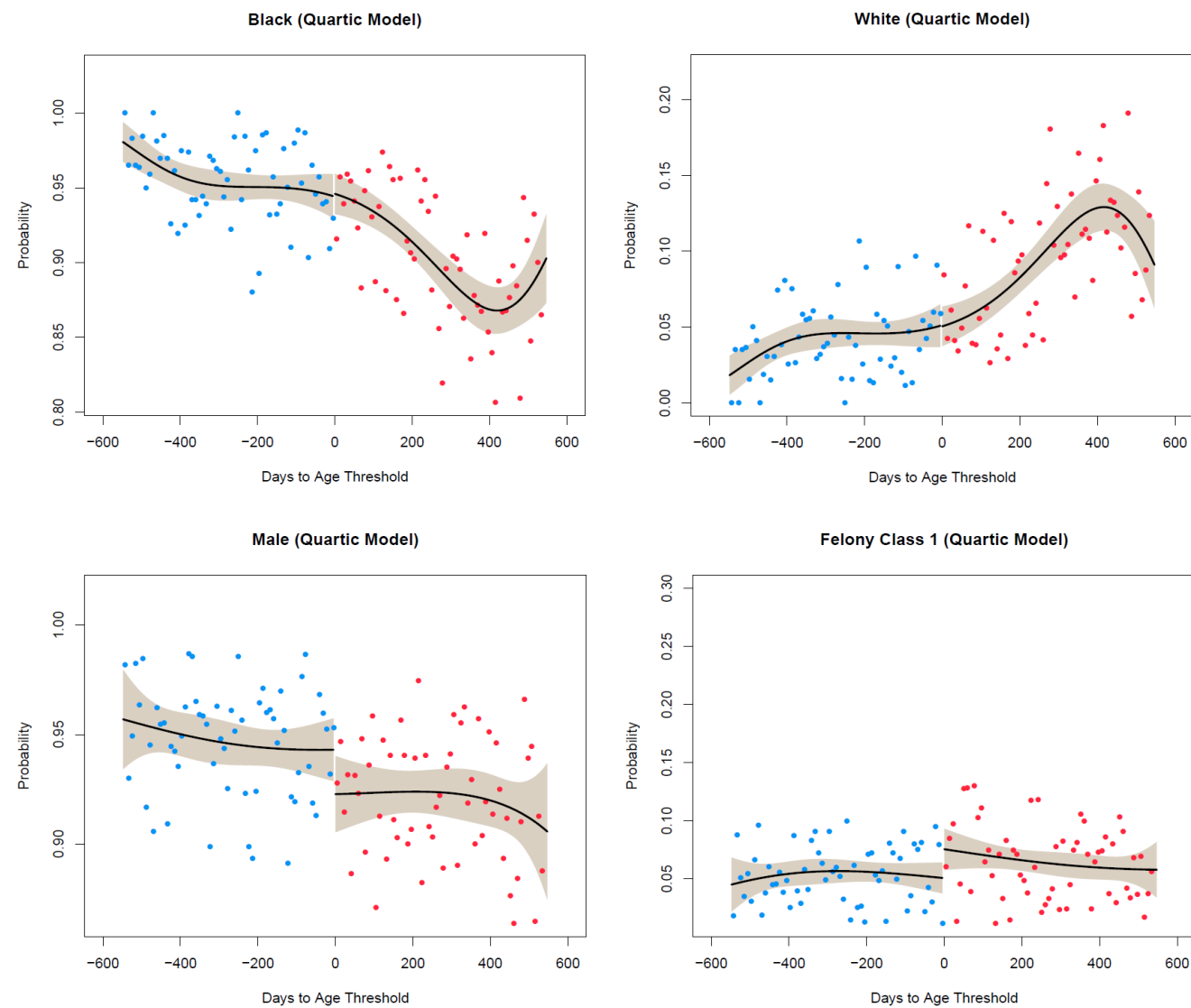
Follow up Period	Diff in Means			N
	Odds	P Val	Marg Eff	
1 Year	.699	.003	-.088	1,164
2 Year	.720	.011	-.068	1,164
3 Years	.676	.005	-.070	1,164
4 Years	.723	.027	-.052	1,164

Table 5. Regression Discontinuity Results 60-day bandwidth by Charge Class

Charge Class	Diff in Means			N
	Odds	P Val	Marg Eff	
1	2.937	.065	.183	74
2	.368	.013	-.143	188
4	.728	.055	-.052	902
All	.723	.027	-.052	1,164



Appendix. Figure A.1. Probability Plots for Pre-Arrest Covariates



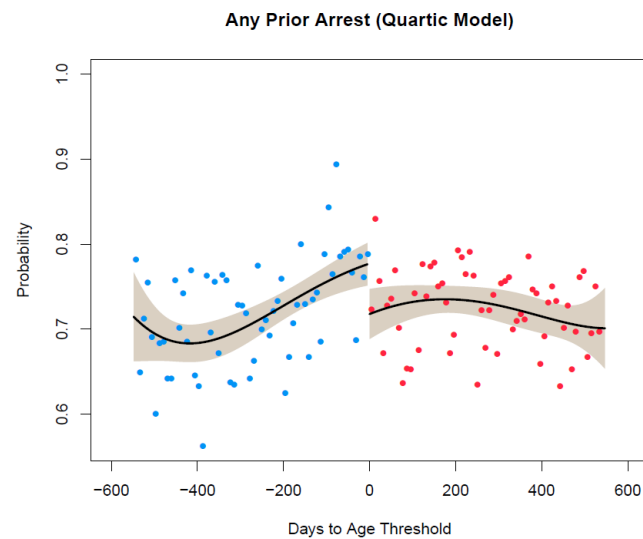
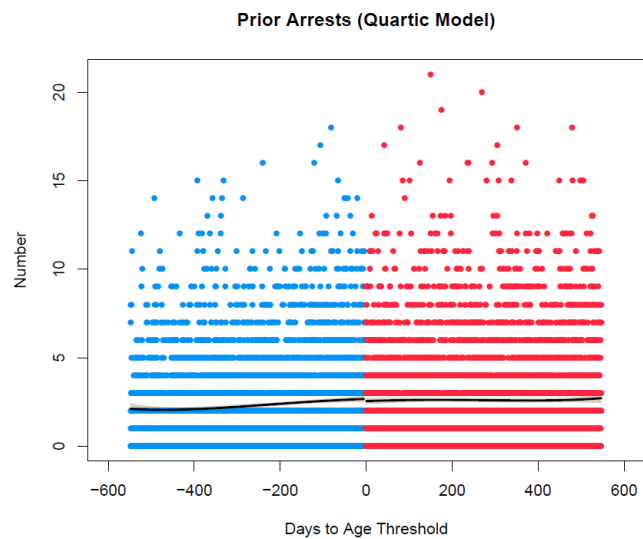
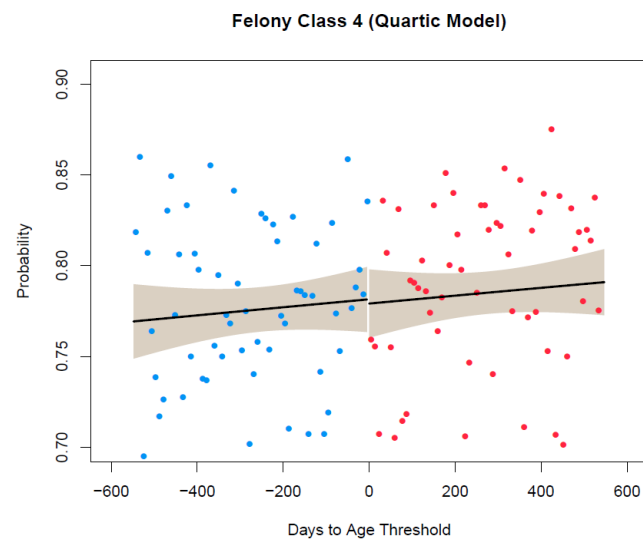
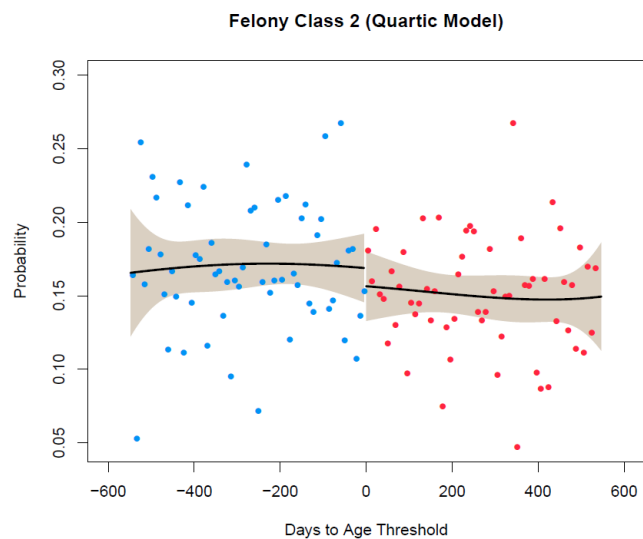


Figure A.2. Alternative Shuffle Test (-180 to +180 days)

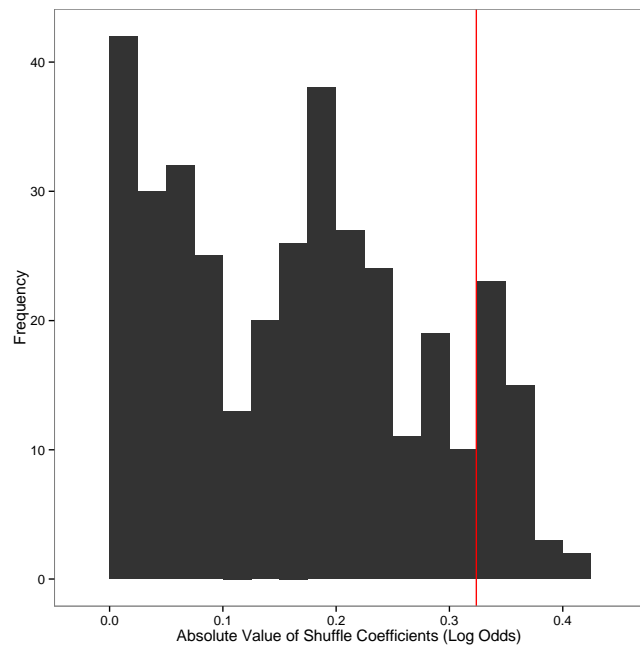


Table A.1. Regression Discontinuity Estimates 60-day bandwidth w/ Covariate Models (Logit Models)

Model	w/o covariates		w/ covariates		N
	Odds Ratio	P Value	Odds Ratio	P Value	
(1) Difference-in-Means	.723	.027	.718	.033	1,164
(2) Linear	.850	.586	.840	.585	1,164
(3) Quadratic	.851	.590	.840	.588	1,164
(4) Cubic	1.097	.819	1.068	.879	1,164
(5) Quartic	1.096	.820	1.067	.881	1,164
(6) GAM (rigid)	.850	.586	.840	.585	1,164
(7) GAM (flexible)	.850	.586	.840	.586	1,164

Note: Covariates include Race, Sex, Charge Class, and Prior Record

Table A.2 Regression Discontinuity Estimates 60-day bandwidth w/ Covariate Models (Linear Probability Models)

Model	w/o covariates		w covariates		N
	Beta	P Value	Beta	P Value	
(1) Difference-in-Means	-.052	.027	-.047	.035	1,164
(2) Linear	-.026	.585	-.023	.614	1,164
(3) Quadratic	-.026	.583	-.023	.614	1,164
(4) Cubic	.014	.825	.011	.857	1,164
(5) Quartic	.014	.829	.010	.878	1,164
(6) GAM (rigid)	-.026	.585	-.023	.614	1,164
(7) GAM (flexible)	-.026	.585	-.023	.614	1,164

Note: Covariates include Race, Sex, Charge Class, and Prior Record