

**DECRIMINALIZING DELINQUENCY:
THE EFFECT OF RAISING THE AGE OF MAJORITY ON JUVENILE
RECIDIVISM**

Abstract

In the last decade, a number of states have expanded the jurisdiction of their juvenile courts by increasing the maximum age of jurisdiction to eighteen. Proponents argue that these expansions reduce crime by increasing access to the beneficial features of the juvenile justice system. Critics counter that these expansions risk increasing crime by reducing deterrence. In 2010, Illinois raised the maximum age of the juvenile court for misdemeanor offenders. By examining the effect of this law on juvenile offenders in Chicago, IL, this paper provides the first empirical estimates of the consequences of recent legislative activity to raise the age of criminal majority. Applying a difference-in-differences design with multiple control groups, we find no evidence of an effect of the law on recidivism. Our results suggest that—contrary to the expectations of both advocates and opponents—increasing the maximum age of the juvenile court does not affect juvenile recidivism.

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I. INTRODUCTION

Historically, children over 14 were presumptively culpable for their crimes. In the late 19th century, juvenile justice reformers advocated for the creation of a separate judicial system that emphasized rehabilitation rather than punishment, and judicial discretion rather than procedural formality. This movement was driven by concerns that juvenile offenders were different than adult offenders, and might benefit from alternative forms of judicial processing (Tanenhaus 2004, 2002). The juvenile court system spread rapidly throughout the United States. From its humble origins in Illinois in 1899, it could be found in nearly all fifty states just a generation later (McCord, Widom, and Crowell 2001).

Despite this rapid and widespread adoption, the juvenile justice system began contracting in the 1970s. Growing fears of juvenile crime and skepticism about rehabilitation led many states to reconsider the wisdom of processing serious juvenile offenders in what appeared to be a lenient juvenile justice system (Feld 1999, Fox 1996, Zimring 1998, Cook and Laub 2002). During this period, states moved more and more juveniles into the adult justice system through three different legislative changes. First, they created statutes that required or permitted juvenile judges to transfer serious adolescent offenders into adult court. Second, a number of states created direct file procedures, which empowered prosecutors to charge serious juvenile offenders in adult court without judicial approval. And third, a number of states lowered the maximum age for juvenile court—often called the age of majority—for some or all offenses. Taken together, these changes precipitated a large-scale re-criminalization of juvenile offending, transferring tens of thousands of cases each year to the adult criminal justice system (Rubin 2003, P2-2).

We are now beginning to see a shift in tides. There is significant expansionist advocacy to push the age of majority back up to eighteen, and thus, to bring offenders between the ages of sixteen and eighteen back into the juvenile court (Brown 2012, Moore 2011, Schwartz 2013). Several states, including Illinois, Connecticut, Mississippi, Massachusetts, and New Hampshire, have already done so.

Empirical questions play an important role in the public debate on age of majority legislation. Proponents argue that raising the age of majority, and thus, expanding the juvenile justice system, will reduce crime by increasing access to treatment opportunities that are more readily available in the juvenile system and by decreasing the harmful effects of processing juveniles in the adult system (e.g., victimization, public criminal records) (2013, Ingram 2007, Hlavach 2013, Chicago Sun Times Editorials 2013, Ferdinand 2002, Dixon 2009, Byrd 2008, Amelinckx and Redmond 2013). They further argue that the decrease in crime will, on balance, produce cost savings for society and offset the increased costs of processing additional juveniles in the more expensive juvenile justice system (Morse 2007, Roman 2006, Timberlake 2009). Opponents counter that older teen offenders engaged in serious or repeated offending are more appropriately handled in the adult justice system (Lord 2008), and that the law change may increase juvenile crime (Birckhead 2008, Backus 2011). They further argue that criminal justice budgets will increase because of the higher costs of juvenile processing (Wiser 2009, Morse 2007).

This paper explores these empirical questions by providing the first estimates of the effect of recent “raise the age” legislation on juvenile recidivism. In 2010, Illinois raised the age of majority for misdemeanor crimes from 17 to 18. As a result, 17-year-old misdemeanor offenders who would have been processed in the adult system before 2010 are now processed in the juvenile system. We apply a difference-in-differences (DID) design to estimate the effect of

the law on the recidivism of 17-year-old arrestees. Specifically, we compare the change in recidivism among 17-year-old arrestees before and after 2010, with the change in recidivism among 16- and 18-year-old arrestees during that same period (who were unaffected by the law change). The results suggest that—contrary to the expectations of both its advocates and opponents—the law change had no effect on recidivism.

To ensure the robustness of our results, we apply the DID design on two different subsamples of the data. First, we fit the model on the subset of suspects arrested within 60 days of their 17th or 18th birthday. Second, we fit the model to a subset of suspects arrested within 60 days of the law change (i.e., January 1st, 2010). Both of these alternative specifications increase the comparability of the treatment and control groups and remove some potential unobserved sources of bias.

Our analysis improves on the research design of prior studies, and reinforces and extends their findings. First, prior studies of changes to the jurisdiction of the juvenile court have formed control groups with juveniles from other states or juveniles in the same state with substantially different ages. Our detailed individual-level data permit control groups with offenders from the same city who are no more than 120 days apart in age. Second, the current study examines the effect of raising the age of majority on the subset of offenders who were actually processed in the juvenile system (rather than the adult system) as a result of the law change instead of examining the effect on all juveniles in the jurisdiction. Our results suggest that offending rates remain unchanged even for this subset of juveniles who are directly affected by the law. Third, the current study extends the findings of prior research to a new population. Until now, scholars have only evaluated the effect of changing the age of majority for serious felonies. Yet, misdemeanors account for the vast majority of all juvenile offenses.

II. PRIOR LITERATURE

Two areas of academic research provide evidence on the effects of raising the age of majority on juvenile crime. The first examines the effect on aggregate offending rates of legislative enactments that shrank the jurisdiction of the juvenile justice system during the contractionary period that began in the 1970s (Risler, Sweatman, and Nackerud 1998, Steiner and Wright 2006, Jensen and Metsger 1994, Zimring and Rushin 2013). Several of these studies have examined the effect of legislative enactments that required certain juvenile offenders to be transferred to the adult criminal justice system.

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). The authors used 13- to 15-year-olds in New York as the treatment group, and used 16- to 19-year-olds in New York and 13- to 15-year-olds in Philadelphia as controls. Jensen and Metsger (1994) examined the effect of a 1981 law in Idaho, which required juveniles between the ages of 14 and 18 charged with serious crimes (i.e., murder, robbery, forcible rape, mayhem) to be transferred to the adult system. The authors compared the rate of total juvenile arrests in Idaho in the four years before and after the statute was enacted to the rate of total juvenile arrests during that period in Wyoming and Montana. Risler et al. (1998) examined the effect of a 1994 Georgia law requiring the transfer of juveniles over thirteen years of age who were charged with the most serious criminal offenses (i.e., murder, manslaughter, rape, child molestation, and armed robbery). The authors compared the total juvenile arrest rate in Georgia in the two years before and after the law went into effect. One final study examined the effect of direct file statutes enacted in fourteen states (Steiner and Wright 2006). These statutes empower

prosecutors to transfer serious juvenile offenders to the adult system without judicial approval. The authors applied a multiple interrupted time series model, comparing the violent juvenile arrest rate in the fourteen states that enacted direct file statutes with ten states that did not. All four of these studies found little evidence of a deterrent effect on the targeted offenses.

Taken together, the null findings in the literature provide little evidence that the exact location of the age boundary between the juvenile and adult system has a large effect on juvenile offending (Bishop 2000, Redding 2010). However, two methodological limitations temper this conclusion. First, the counterfactuals used in this literature are limited. One study in this literature did not use a control group. Two of the studies used juveniles in other states that did not enact a transfer or direct file statute as controls. And the final study used 16- to 19-year-olds in the same state as controls for 13- to 15-year-olds, and also used 13- to 15-year-olds in another similar jurisdiction. It is difficult to determine whether these control groups provide valid information about the trend the treatment groups would have taken in the absence of the relevant legislative enactment. Second, prior work on legislative changes to the jurisdiction of the juvenile court has only examined the effect of legislative changes to the age of majority on the offending rate for *all* juveniles rather than examining the effect on the recidivism of *arrested* juveniles. There are strong grounds to expect a greater effect on the subset of juveniles who were actually processed in the juvenile system (rather than the adult system) as a result of the law change.

While no studies have yet examined the effect on *recidivism* of a legislative change that raises or lowers the age of majority for all misdemeanor or felony crimes, a second related literature has compared the recidivism of juveniles transferred to the adult system with those retained in the juvenile system. In one of the earliest studies, Fagan examined re-offending

among juveniles arrested for robbery and burglary in two adjacent jurisdictions with different age thresholds for mandatory transfer (Fagan 1996, Bishop et al. 1996, Winner et al. 1997). After controlling for differences in observable case characteristics (e.g., sex, race, number of prior offenses, age at first offense, offense severity), Fagan found that 15- and 16-year-old robbery suspects tried as adults in New York City had higher recidivism than robbery suspects tried as juveniles in New Jersey. No effect was observed for burglary suspects.

Bishop and colleagues have published a series of studies comparing the recidivism rates of juveniles who are transferred to the adult system with those who are not transferred (Bishop et al. 1996, Winner et al. 1997). In one study, they matched transferred and non-transferred juveniles on observable covariates (e.g., number of prior offenses, offense seriousness, sex, race, and age). The authors found substantially higher 1-year re-arrest rates among transferred juveniles (Bishop et al. 1996). With the exception of property offenders, the authors reported similar results in a subsequent 7-year follow-up (Winner et al. 1997, p. 558).

More recent matching studies have examined a wider range of offenses covered by transfer statutes. These studies have found higher recidivism among transferred youth (Myers 2003, Lanza-Kaduce et al. 2005, Fagan, Kupchik, and Liberman 2007) and some evidence of treatment effect heterogeneity (Loughran et al. 2010, Fagan, Kupchik, and Liberman 2007).

Based on the methodological approaches in this literature, it is difficult to determine whether differences between transferred and non-transferred juveniles reflect a true effect of transfer, or unobserved selection bias (McGowan et al. 2007, Mulvey and Schubert 2012). To address this issue of selection bias, scholars have applied a regression discontinuity design. Recognizing that nearly all adolescents under eighteen years of age arrested in Florida are processed in the juvenile system, and nearly all adolescents over eighteen are processed in the

adult system, Lee and McCrary examined the re-arrest rates of adolescents who were arrested just a few weeks before and a few weeks after their eighteenth birthday (Lee and McCrary 2009). The authors reported relatively little evidence that processing juveniles in the adult system has a large effect on recidivism, a finding echoed by another recent discontinuity study (Hjalmarsson 2009).

Taken together, most individual-level recidivism studies find that processing juveniles in the adult system increases recidivism. This empirical result has played an important role in contemporary debates about age of majority reform. Advocates frequently cite these findings to argue for raising the age of majority for all juvenile offenders (2013, Ingram 2007, Hlavach 2013, Editorials 2013, Ferdinand 2002, Dixon 2009, Byrd 2008, Amelinckx and Redmond 2013). But, their applicability is unknown given their focus on juveniles transferred to the adult system. Transferred juveniles represent only a small fraction of all juvenile offenders. In 2010, less than one percent of all juvenile court petitions and less than one percent of all delinquency cases resulted in a waiver to the adult system (Puzzanchera and Robson 2014). The results of the literature on transferred juveniles may not generalize to the larger population of felony and misdemeanor juvenile offenders affected by current legislation that raises the age of majority for all misdemeanor or felony offenders.

III. RESEARCH AND METHODS

A. Data

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. They include the following information on each arrest: the date of arrest, the most serious offense charged, and the date of birth, gender and race of the arrestee. We excluded all arrests that lacked a fingerprint identification number, a charge grade, or a birthdate. The remaining observations consisted of 22,779 misdemeanor arrests of 16-, 17-, and 18-year-olds between the years 2009 and 2010. Cases from before 2009 were also used to provide descriptive trends on recidivism before the law went into effect.

We created a binary outcome variable indicating whether an individual was re-arrested by the Chicago Police Department within 2 years of the present offense. We also created a second outcome variable indicating whether an individual was re-arrested for a *violent* crime.¹ Our two-year follow-up period represents a compromise between several competing methodological considerations. First, the observed effect of a treatment can vary depending on follow-up period, and longer follow-up periods are typically preferable. However, the law change went into effect on January 1st, 2010 and our data cover all arrests until 2012. As a result, the longest possible follow-up period is three years (January 1st, 2010 to December 31st, 2012). Second, to avoid censoring problems, all subjects must have an equal length of time to recidivate after the initial arrest. Third, a longer follow-up period disqualifies subjects from the potential analysis sample to avoid censoring. As a reasonable tradeoff of these considerations, we use a two-year follow-up period, which allows all arrestees in 2010 to be included in the analysis.

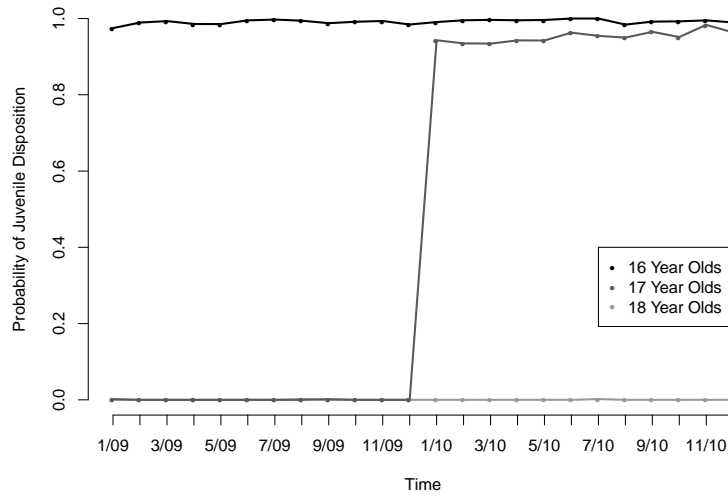
¹ Violent offenses include homicide, robbery, aggravated assault, simple assault, and sexual assault.

We also calculated the number of days until subjects are rearrested to assess whether timing varies across sub-groups in our sample. An analysis of timing may provide some insight about theoretical mechanisms. For example, if pre-trial detention or incarceration affects one group more than the others, we would expect to see delays in the time to rearrest.

B. Analytic Strategy

On January 1st, 2010, the state of Illinois raised the criminal age of majority for misdemeanor crimes from 17 to 18. Before that date, 17-year-olds arrested for a misdemeanor were prosecuted in the adult criminal justice system. Since that date, they are processed in the juvenile system. The law did not affect 16- or 18-year-old arrestees. To verify that the law was implemented as enacted, we graphed the monthly probability of juvenile case disposition post-arrest for 16-, 17-, and 18-year-olds from January 2009 until December 2010 (Figure 1). Consistent with the law change, the probability of juvenile case disposition (i.e., juvenile station adjustment, referral to juvenile court, or juvenile detention) went from zero to nearly one immediately after the law was implemented while the probabilities of a juvenile case disposition for 16- and 18-year olds remained unchanged at one and zero respectively.

Figure 1: Probability of Juvenile Case Disposition by Age and Time



Given the absence of age or temporal treatment spillover, we apply a DID design on pooled cross-sections over time to estimate the effect of the law change on recidivism. DID designs have been extensively used to evaluate policy changes that affect some but not all units subject to similar conditions (Card 1992, Gruber 2000). In our first set of DID models, we compare the change in the re-arrest rate of 17-year-old arrestees (treatment group) from 2009 to 2010, with the change in the re-arrest rate of 18-year-olds (control group) during that period. The model is as follows:

$$\Delta Y_i = \beta_0 + \beta_1 \text{Age17}_i + \beta_2 \text{Year2010}_i + \beta_3 \text{Age17}_i \cdot \text{Year2010}_i + \mu_i \quad (1)$$

where Y_i is a 2-year measure of re-arrest for arrested suspect i , Age17_i is a dummy variable indicating that the arrested suspect was 17-years-old, and Year2010_i is a dummy variable indicating that the arrest took place in 2010. β_0 reflects the average re-arrest rate for 18-year-olds in 2009. β_1 captures any possible differences between 17- and 18-year-olds before the policy change, and β_2 captures the difference between the probability of re-arrest in 2009 and 2010 for 18-year-olds. β_3 is the relevant DID estimate of the effect of the law on recidivism. We estimate

these models using ordinary least squares on the sub-population of 17- and 18- year-olds arrested in 2009 and 2010 (Angrist and Pischke 2009).

In our second set of models, we use 16-year-olds rather than 18-year-olds as the control group. In these models, the relevant treatment group is 17-year-olds arrested in 2010. β_0 reflects the average re-arrest rate for 16-year-olds in 2009. β_1 captures any differences between 16- and 17-year-olds before the policy change, and β_2 captures the difference between the probability of re-arrest in 2009 and 2010 for 16-year-olds. β_3 is the relevant DID estimate of the effect of the law on recidivism as it captures the difference between re-offending for 16- and 17-year-olds.

The primary assumption in the DID context is the *parallel trends assumption*: there can be no variables that change during the study period that affect the recidivism of one group differently from the other. Due to differences in developmental maturation, it is possible that 17-year-olds are different from 16- and 18-year-olds in a way that introduces bias into the model.

We assess the plausibility of using 16- and 18-year-olds as controls for 17-year-olds in two ways. First, we check for balance on pre-treatment covariates. Table 1 provides a comparison of pre-treatment covariates for 17- and 18-year-olds arrested in 2009. Though there are statistically significant differences, the table shows that 17- and 18-year-olds are relatively similar on demographic characteristics and the composition of charges for which they are arrested. As a general rule of thumb, standardized mean differences of 0.1 or less suggest adequate balance. The standardized mean differences for nearly all covariates in Table 1 are below or just above 0.1. Seventeen-year-old arrestees are just slightly less male and less white, and commit slightly more violent and property crimes. But, Table 1 also shows a substantial difference between 17- and 18-year-olds on prior arrest history. Eighteen-year-olds tend to have, on average, one more prior arrest than 17-year-olds because they have had one more year of

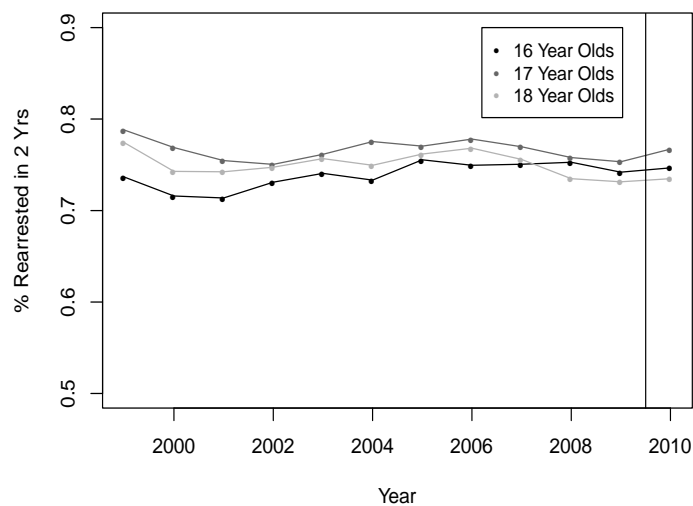
adolescence to commit crime. The same general patterns are observed when comparing 16-year-olds to 17-year-olds (see Appendix Table A.1).

Table 1: Descriptive Comparison of 17- and 18-Year-Olds, 2009

	17 Mean	18 Mean	17 SD	18 SD	Mean Diff	Stan Mean Diff	T Test
Male	0.845	0.857	0.362	0.350	-0.012	-0.034	0.060
White	0.285	0.300	0.451	0.458	-0.015	-0.034	0.062
Black	0.709	0.695	0.454	0.461	0.014	0.032	0.080
Other Race	0.005	0.004	0.072	0.065	0.001	0.014	0.433
Total Priors	4.972	6.140	6.111	7.185	-1.168	-0.175	0.000
Property	0.192	0.189	0.394	0.392	0.003	0.007	0.678
Violent	0.200	0.157	0.400	0.364	0.043	0.113	0.000
Drug	0.254	0.308	0.436	0.462	-0.054	-0.120	0.000
Other Crime	0.354	0.346	0.478	0.476	0.008	0.017	0.353
Recid 2 Years	0.753	0.731	0.431	0.443	0.022	0.050	0.005
Arrest Days from 17	183.8	176.6	105.1	106.1	7.2	0.068	0
N	6479	5845					

Second, we assess whether the recidivism of 17-year-olds follows a similar trend over time to that of 16- and 18-year olds. Figure 1 presents the average rate of recidivism by age of juveniles arrested over time. There are a few periods when the trends diverge. There are also small differences in the level of each curve on the y-axis. However, in general, the 16- and 18-year-olds tend to follow the same general trend as the 17-year-olds.

Figure 1: Recidivism of 16-, 17- and 18-Year-Olds Over Time



Together, the comparison of pre-treatment covariates and the assessment of trends over time suggest that 16- and 18-year-olds may be comparable control groups for 17-year-olds, but there are some non-trivial differences. To improve the comparability of the treatment and control groups, we fit the DID models on two different subsets of the data. First, we fit an *age-limited model*. When using 18-year-olds as the control group, we fit the DID model on subjects arrested within sixty days of their 18th birthday. When using 16-year-olds as the control group, we fit the DID model on subjects arrested within sixty days of their 17th birthday. This ensures that any two people in the model are no more than 120 days apart in age. The vast majority of subjects will be closer in age.

Table 2 compares the pre-treatment covariates for 17- and 18-year-olds in the age-limited models, and shows improved balance on most covariates. Most importantly, the substantial difference in criminal history observed in the previous table virtually disappears, and the standardized mean differences are all well below 0.1. The results are substantively similar when comparing 17- and 16-year-olds arrested within 60 days of the 17th birthday (see Appendix Table A.2).

Table 2: Descriptive Comparison of 17- and 18-Year-Olds Arrested within 60 Days of 18th Birthday, 2009

	17 Mean	18 Mean	17 SD	18 SD	Mean Diff	Stan Mean Diff	T Test
Male	0.840	0.851	0.367	0.356	-0.011	-0.032	0.466
White	0.312	0.301	0.463	0.459	0.011	0.023	0.598
Black	0.679	0.692	0.467	0.462	-0.013	-0.029	0.505
Other Race	0.009	0.006	0.092	0.075	0.003	0.034	0.430
Total Priors	5.885	6.188	7.372	7.016	-0.304	-0.042	0.333
Property	0.218	0.208	0.413	0.406	0.010	0.024	0.577
Violent	0.174	0.172	0.380	0.378	0.002	0.006	0.899
Drug	0.247	0.276	0.431	0.447	-0.029	-0.065	0.135
Other Crime	0.360	0.344	0.480	0.475	0.017	0.035	0.426
Recid 2 Years	0.738	0.742	0.440	0.438	-0.005	-0.010	0.811
Arrest Days from 17	30.5	28.7	17.3	17.5	1.8	0.104	0.017
N	1049	1056					

Figures 2 and 3 present the trend in recidivism for the 17- and 18-year-olds and the 17- and 16-year-olds in the age-restricted models. Figure 2 shows that the 17- and 18-year-olds share the same level on the y axis and a very similar trend over time. The divergence in trends between 2003 and 2005 is likely the result of statistical error due to smaller sample size. The trends for the 17- and 16-year-olds in the age-restricted models also track each other, but perhaps less well.

Figure 2: Recidivism of 17- and 18-Year-Olds within 60 Days of the 18th Birthday

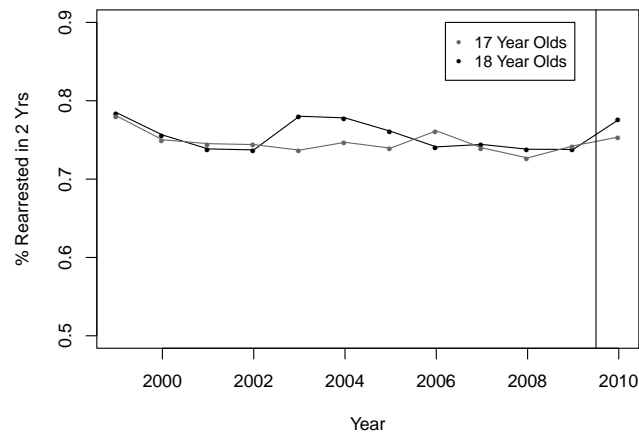
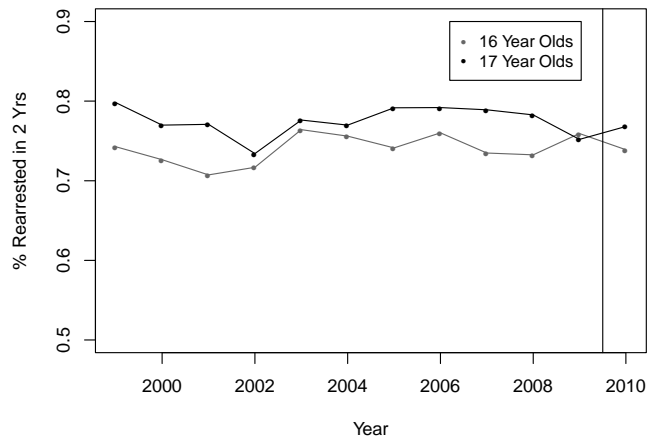


Figure 3: Recidivism of 17- and 16-Year-Olds within 60 Days of the 17th Birthday



We also examined the number of suspects arrested sixty days before and sixty days after their 17th or 18th birthday to test for any shifts in density that might coincide with the 2010 law change (see Appendix Charts A.1 and A.2). We do observe a subtle shift. A stable gap in arrests

between those just under 17 and those just over 17 closes after 2010. Similarly, a stable gap in arrests between those just under 18 and those just over 18 emerges after 2010. This pattern suggests that the law change may trigger some selection processes in the decision to arrest. However, this process appears pseudo-random given the absence of any covariate imbalance conditional on arrest, as observed in Tables 2 and A.2. We suspect this trend is best explained by a lack of interest among a subset of officers in arresting juvenile misdemeanor offenders. This shift in the number of arrestees is a cause for concern, but the near perfect covariate imbalance suggests that comparable juveniles were arrested on either side of the age threshold both before and after 2010.

As a further check on the robustness of our estimates, we also fit a *time-limited model*. We fit the DID model on arrests that took place within sixty days of January 1, 2010, the date the law went into effect. Restricting the data within this 60-day window may help remove bias resulting from secular trends over time. However, it is also possible that the time-restricted subsample biases the model downward if the Chicago Police Department did not fully implement the law in the first 60 days of 2010.

IV. RESULTS

Table 3 presents the results of the models that use 18-year-olds as controls for 17-year-olds. The first column presents the full model, which includes all 17- and 18-year-olds. The primary estimate of interest is the interaction between Seventeen and 2010, which estimates the effect of the law change. The full model finds that the effect of the law is essentially zero and statistically insignificant. The second column presents the results of the model with just those subjects arrested within 60 days of their 18th birthday. While the coefficient is slightly larger, it

remains small and statistically insignificant. The third column presents the model with 17- and 18-year-olds arrested within 60 days of January 1, 2010, the day the law went into effect. The coefficient of interest is slightly larger than the estimates in the other models, but it remains small and statistically insignificant.

Table 3: Models on Any Rearrest with 18-Year-Olds as Controls

Variables	Full		Age Limited		Time Limited	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.731 (0.006)	0.000	0.742 (0.013)	0.000	0.712 (0.016)	0.000
Seventeen	0.022 (0.008)	0.005	-0.005 (0.019)	0.808	0.046 (0.021)	0.030
2010	0.003 (0.008)	0.668	0.011 (0.019)	0.553	0.021 (0.022)	0.324
Seventeen*2010	0.010 (0.011)	0.366	0.027 (0.027)	0.325	-0.044 (0.030)	0.138
N	23576		4009		3566	

Note: Bolded coefficients are statistically significant at the .05 level.

Table 4 presents the results of our models that use 16-year-olds as controls for 17-year-olds. The results are substantively similar. For this comparison, the estimated coefficients are small and statistically insignificant with all but the age-limited model between 0.01 and 0.00 in magnitude. Re-estimating each of the models in Tables 3 and 4 with covariates produces nearly identical results (see Appendix Tables A.5 and A.6).

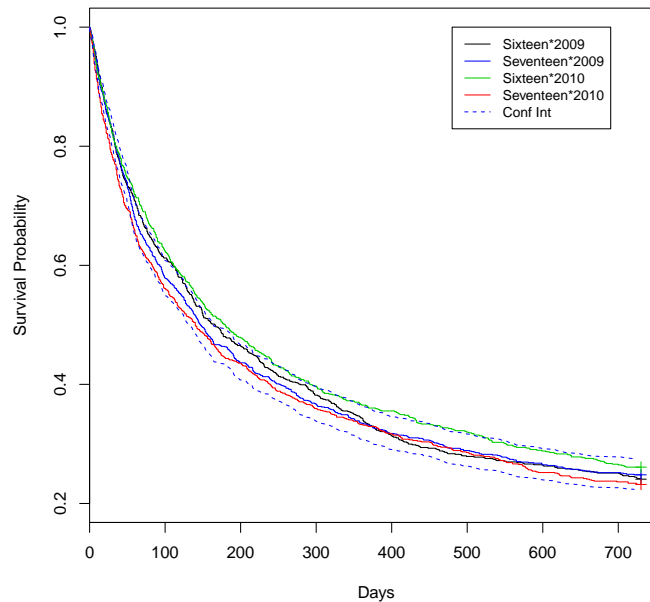
Table 4: Models on Any Rearrest with 16-Year-Olds as Controls

Variables	Full		Age Limited		Time Limited	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.742 (0.006)	0.000	0.759 (0.015)	0.000	0.739 (0.015)	0.000
Seventeen	0.011 (0.008)	0.157	-0.007 (0.020)	0.706	0.019 (0.021)	0.361
2010	0.005 (0.009)	0.585	-0.020 (0.021)	0.334	-0.018 (0.021)	0.392
Seventeen*2010	0.009 (0.012)	0.442	0.037 (0.029)	0.201	-0.004 (0.029)	0.885
N	21943		3653		3609	

Note: Bolded coefficients are statistically significant at the .05 level.

Given the high rates of rearrest observed in our sample, we also examined the timing of rearrest using hazard models. Figure 4 shows the probability of survival for 17-year-olds arrested in 2010 along with each of the three comparison groups. There are no statistically significant differences between the timing of failure for either the treatment or control groups in our age-limited models.

Figure 4: Time-Until-Re-arrest 17- and 16-Year-Olds within 60 Days of the 17th Birthday



As a further sensitivity analysis, we re-estimated our models to examine the effect of raising the age of majority on violent recidivism. Table 5 reports the results of the models that use 18-year-olds as controls. In the full model, the coefficient is 0.00 and statistically insignificant. The age- and time-limited models estimate slightly larger coefficients, but both are still between 0.01 and -0.01 and statistically insignificant. For the 16- and 17-year-old comparison, reported in Table 6, the full model estimates a statistically insignificant coefficient of 0.01. Both the age- and time-limited models are larger in magnitude but they remain statistically insignificant. These models provide little evidence of an effect, but if anything, they

suggest that the law resulted in a slight increase in violent recidivism under a less stringent standard of statistical significance.

Table 5: Models on Violent Rearrest with 18-Year-Olds as Controls

Variables	Full		Age Limited		Time Limited	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.246 (0.006)	0.000	0.253 (0.014)	0.000	0.253 (0.016)	0.000
Seventeen	0.054 (0.008)	0.000	0.040 (0.020)	0.043	0.046 (0.022)	0.033
2010	0.019 (0.008)	0.021	0.022 (0.020)	0.264	0.011 (0.022)	0.629
Seventeen*2010	0.000 (0.012)	0.972	0.005 (0.029)	0.848	-0.006 (0.030)	0.851
N	23576		4009		3566	

Table 6: Models on Violent Rearrest with 16-Year-Olds as Controls

Variables	Full		Age Limited		Time Limited	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.364 (0.007)	0.000	0.351 (0.016)	0.000	0.384 (0.016)	0.000
Seventeen	-0.064 (0.009)	0.000	-0.053 (0.022)	0.013	-0.085 (0.022)	0.000
2010	0.005 (0.009)	0.623	0.010 (0.023)	0.678	-0.052 (0.023)	0.023
Seventeen*2010	0.014 (0.013)	0.266	0.055 (0.031)	0.081	0.057 (0.031)	0.07
N	21943		3653		3609	

Note: Bolded coefficients are statistically significant at the .05 level.

Taken together, our results provide little evidence of a change in recidivism after the law went into effect. The most plausible explanation is that the law had no effect. But, it is also possible the law had multiple countervailing effects of similar magnitude that mask each other. It is possible, for example, that processing 17-year-old misdemeanor offenders in the juvenile system decreases deterrence, but also increases incapacitation. To test this hypothesis, we refit our models for subsets of the sample with different levels of criminal history and instant offenses. The rate of pre-trial detention is very low for both juvenile and adult misdemeanor arrestees who have no criminal history, but we suspect that pre-trial detention rates may differ

more dramatically for juveniles and adults with longer criminal histories. In Table 7 we refit our models for subsets of the data with different levels of criminal history. While the estimates are less precise, they provide little evidence of treatment effect heterogeneity across criminal histories. We found similar results for models estimated across instant offense categories (See Appendix Tables A.7 and A.8). This finding, combined with the similar timing of re-arrest in our hazard models, reinforces our conclusion that the law had no effect on recidivism.

Table 7: Models on Any Rearrest by Criminal History with 18-Year-Olds as Controls

Variables	All		0 Priors		1-2 Priors		3-10 Priors	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.742 (0.013)	0.000	0.311 (0.033)	0.000	0.687 (0.032)	0.000	0.883 (0.015)	0.000
Seventeen	-0.005 (0.019)	0.808	0.058 (0.046)	0.210	-0.052 (0.045)	0.242	0.010 (0.022)	0.633
2010	0.011 (0.019)	0.553	0.052 (0.048)	0.279	-0.031 (0.045)	0.485	0.018 (0.022)	0.415
Seventeen*2010	0.027 (0.027)	0.325	0.039 (0.069)	0.569	0.054 (0.064)	0.400	-0.017 (0.032)	0.585
N	4009		788		897		1551	

Note: Bolded coefficients are statistically significant at the .05 level.

Table 8: Models on Any Rearrest by Criminal History with 16-Year-Olds as Controls

Variables	All		0 Priors		1-2 Priors		3-10 Priors	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.759 (0.015)	0.000	0.447 (0.033)	0.000	0.727 (0.032)	0.000	0.93 (0.015)	0.000
Seventeen	-0.007 (0.020)	0.706	0.013 (0.045)	0.776	-0.016 (0.041)	0.692	-0.016 (0.020)	0.418
2010	-0.020 (0.021)	0.334	-0.004 (0.049)	0.932	-0.057 (0.044)	0.188	-0.014 (0.022)	0.525
Seventeen*2010	0.037 (0.029)	0.201	-0.012 (0.067)	0.862	0.07 (0.060)	0.242	0.011 (0.030)	0.711
N	3653		893		951		1397	

Note: Bolded coefficients are statistically significant at the .05 level.

V. DISCUSSION AND CONCLUSION

After an extended period of contraction at the end of the 20th century, a number of states have expanded the jurisdiction of the juvenile court by raising the age of majority to eighteen. Proponents argue that these legislative changes reduce juvenile recidivism, and they cite the

continued decline in juvenile crime as evidence that they do not harm public safety. Critics, on the other hand, have voiced concerns about the cost of juvenile processing, and the risk of increased crime. However, no empirical study has systematically tested whether raising the age of majority actually has an effect on juvenile offending.

The present study examined the effect on recidivism of a 2010 Illinois law that raised the age of majority for misdemeanor crimes from 17 to 18. Using a difference-in-differences design, we compared the change in recidivism among 17-year-old arrestees before and after 2010, with the change in recidivism among 16- and 18-year-olds arrestees during that period (who were unaffected by the law change). The results suggest that—contrary to expectations of both advocates and opponents of age of majority reform—the law had no discernable effect on juvenile recidivism. While some of our standard errors leave room for a change in recidivism in the high single-digit range, most of our estimates are much closer to zero. Effects of this size are smaller than the projected recidivism reductions offered by proponents of raise-the-age legislation who anticipated a double-digit reduction in recidivism (e.g. Deitch, Breeden, and Weingarten 2012, Roman 2006).

Our results are consistent with and reinforce previous evaluations of legislative changes to the jurisdiction of the juvenile court. This prior work, which was largely unmentioned during policy debates on recent raise-the-age legislation, found little consistent evidence that legislative changes to the jurisdiction of the juvenile court affect the aggregate offending rate of all juveniles. This finding is unsurprising as the vast majority of juveniles are likely unaware of such legislative changes. However, there are strong theoretical reasons to expect a greater effect on the subset of juveniles who are actually processed in the juvenile system (rather than the adult system) as a result of a legislative change, such as the one examined in the present study. And

yet, our findings provide little evidence of a change in offending even for this subset of juveniles who are directly affected by the law. Our results thus reinforce previous findings that changing the jurisdiction of the juvenile court does not affect the recidivism of misdemeanor offenders.

At first glance, our results are harder to reconcile with the literature on juvenile transfer, which has repeatedly found higher recidivism among juveniles transferred into the adult system—a finding that has served as the empirical basis for claims that raising the age of majority lowers juvenile recidivism. We offer several potential explanations. First, all of the studies in this literature have focused on serious felonies, which are relatively rare among juveniles. Our analysis focused on misdemeanor offenders, who account for the vast majority of all juvenile offenders. Moving these less serious offenders into or out of the juvenile system may not have the same effect on recidivism. Second, the transfer literature has focused on the effects of processing individual offenders in one system or the other. It is possible that a legislative enactment that moves thousands of juveniles from one system to the other all at once has a different effect. Finally, consistent with our findings, a number of recent studies have found little evidence of an effect (Lee and McCrary 2009, Hjalmarsson 2009). It is possible that these studies provide a stronger estimate of the effect of juvenile/adult processing by avoiding certain selection problems—an issue identified by two recent systematic reviews of this literature (McGowan et al. 2007, Mulvey and Schubert 2012).

The present study is subject, however, to several important limitations. First, our analysis focuses on one city, suggesting the need for broader analyses in Illinois and elsewhere. Jurisdictions with greater treatment differentials between their juvenile and adult justice systems may experience a larger effect due to raise-the-age legislation.

Second, our analysis focuses on recidivism because public safety plays a central role in the national policy debate on the age of majority. Our study, therefore, leaves open many questions about the effect of raising the age of majority on other life-course outcomes such as employment and health.

Third, the present investigation only examined misdemeanor offenders. We are therefore unable to draw conclusions about the effect of raising the age of majority on felony offenders. Future research may extend our work to felonies by examining other legislative changes. In 2014, for example, Illinois raised the age of majority for felonies from 17 to 18. Connecticut offers another natural experiment as the state raised its age of majority for all crimes from 16 to 17 in 2010, and then from 17 to 18 in 2012.

Finally, the present study lacks data on sentences received by offenders in our sample. This is a common problem in the literature (Bishop et al. 1996, Lee and McCrary 2009), at least partially because juvenile court data is difficult to access. Such data would provide only limited value to our analysis anyway. Because sentencing in juvenile court is indeterminate, sentencing data would provide little information about the length of time subjects spend behind bars.

Even with these limitations, our findings support at least two theoretical interpretations. First, it is possible that raising the age of majority does not affect the recidivism of older and less serious juvenile offenders in the short-term, but does affect their recidivism later on. The effect of the law may, for example, be delayed until offenders enter the workforce and fare better on the job market because their offense is not on a publicly available criminal record. While this is certainly possible, we observe recidivism over two years, which provides time for our 16-, 17- and 18-year old subjects to enter the job market. Still, a longer-follow period of three or four years would help resolve this question.

Second, raising the age of majority may have no effect on recidivism for older and less serious juvenile offenders. One possible explanation for such a non-effect is that the rehabilitative services provided in the juvenile system, or the more punitive treatment in the adult system have little effect on recidivism. Another explanation is that the offenders in our sample receive similar rehabilitative services and punishment in the juvenile and adult systems. Indeed, scholars have found mixed evidence on the relative severity of punishment in the juvenile and adult systems (Fagan 1996, Kurlychek and Johnson 2004, Lemmon et al. 2005), leading some to conclude that these two systems differ more in their procedural elements than in their substantive effects (Feld 1999). In our view, it is particularly plausible that the subjects in our sample—older adolescents who commit less serious crime—receive similar treatment in the juvenile and adult systems. However, to fully resolve these theoretical issues, additional research with detailed data on diversion and treatment programs in the juvenile and adult system is needed.

Our preferred interpretation—that raising the age of majority has no effect on crime, at least for lower level offenders—helps resolve a key *empirical* question at the heart of the national debate on the age of majority. Yet, our findings cannot fully resolve the *normative* question of whether states should or should not raise the age of majority. Even if there is no effect on recidivism, we suspect that raising the age of majority may benefit other important life-course outcomes related to employment, health, life satisfaction and victimization. And, processing adolescents in the juvenile system may be fairer if adolescents are less culpable for their crimes than adults.

The wisdom of raising the age of majority may then hinge on the values and financial resources of each state contemplating a change to its age of majority. Since raising the age of majority poses little threat to public safety, states that value retaining adolescents in the juvenile

justice system should continue or begin doing so if they are willing to pay for it. Similarly, since raising the age of majority is unlikely to increase crime, states that do not value the approach of the juvenile justice system or do not have the requisite resources may retain or lower their current ages of majority without adversely affecting juvenile crime, as Rhode Island did when it temporarily lowered its age of majority in pursuit of cost savings.

However, if the first-order goal of policymakers is to lower juvenile recidivism, then it may prove useful to focus on other policy alternatives that possess a higher likelihood of reducing recidivism. These initiatives include cognitive behavioral therapy in juvenile justice settings (Heller et al. 2011, Landenberger and Lipsey 2005), and a swift, certain, and short sanction regime (Hawken and Kleiman 2009, Bonnie et al. 2013). The latter has shown success among adults and is currently being tested in the juvenile system.

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Appendix

Table A.1: Descriptive Comparison of Seventeen- and Sixteen-Year-Olds, 2009

	17 Mean	16 Mean	17 SD	16 SD	Mean Diff	Stan Mean Diff	T Test
Male	0.845	0.815	0.362	0.389	0.030	0.081	0.000
White	0.285	0.256	0.451	0.437	0.028	0.064	0.001
Black	0.709	0.738	0.454	0.440	-0.029	-0.065	0.000
Other Race	0.005	0.005	0.072	0.068	0.001	0.009	0.644
Total Priors	4.972	3.694	6.111	4.885	1.278	0.231	0.000
Property	0.192	0.189	0.394	0.392	0.003	0.007	0.698
Violent	0.200	0.235	0.400	0.424	-0.035	-0.085	0.000
Drug	0.254	0.219	0.436	0.413	0.036	0.084	0.000
Other Crime	0.354	0.358	0.478	0.479	-0.004	-0.008	0.673
Recid 2 Years	0.753	0.742	0.431	0.438	0.011	0.026	0.160
Arrest Days from 17	181.2	180.3	105.1	105.2	0.9	0.009	0.646
N	6479	5166					

Table A.2: Descriptive Comparison of Seventeen- and Sixteen-Year-Olds Arrested within 60 Days of Seventeenth Birthday, 2009

	17 Mean	16 Mean	17 SD	16 SD	Mean Diff	Stan Mean Diff	T Test
Male	0.833	0.823	0.373	0.382	0.010	0.028	0.546
White	0.283	0.255	0.451	0.436	0.029	0.065	0.156
Black	0.711	0.743	0.453	0.437	-0.032	-0.072	0.116
Other Race	0.004	0.002	0.061	0.048	0.001	0.025	0.574
Total Priors	4.107	3.935	4.919	4.671	0.172	0.036	0.43
Property	0.182	0.171	0.386	0.377	0.011	0.029	0.523
Violent	0.221	0.229	0.415	0.421	-0.008	-0.019	0.68
Drug	0.216	0.228	0.412	0.420	-0.012	-0.03	0.518
Other Crime	0.381	0.372	0.486	0.483	0.009	0.019	0.683
Recid 2 Years	0.752	0.759	0.432	0.428	-0.007	-0.017	0.706
Arrest Days from 17	29.6	31.5	17.7	17.4	-1.9	-0.106	0.02
N	1080	864					

Table A.3: Descriptive Statistics Before and After Law Change with 18-Year-Olds as Controls

	2009 Mean	2010 Mean	2009 SD	2010 SD	Mean Diff	Stan Mean Diff	T Test
Male	0.851	0.850	0.356	0.357	0.001	0.003	0.829
White	0.292	0.284	0.455	0.451	0.008	0.018	0.175
Black	0.702	0.710	0.457	0.454	-0.008	-0.017	0.204
Other Race	0.005	0.005	0.069	0.072	0.000	-0.006	0.621
Total Priors	5.526	5.703	6.667	6.771	-0.177	-0.026	0.043
Property	0.191	0.179	0.393	0.384	0.012	0.030	0.022
Violent	0.179	0.176	0.384	0.381	0.003	0.008	0.535
Drug	0.280	0.296	0.449	0.456	-0.016	-0.035	0.008
Other Crime	0.350	0.349	0.477	0.477	0.001	0.002	0.866
Arrest Days from 17	180.4	182.3	105.6	105.8	-1.9	0.0	0.173
N	12324	11252					

Table A.4: Descriptive Statistics Before and After Law Change with 16-Year-Olds as Controls

	2009 Mean	2010 Mean	2009 SD	2010 SD	Mean Diff	Stan Mean Diff	T Test
Male	0.831	0.824	0.374	0.381	0.008	0.020	0.142
White	0.272	0.270	0.445	0.444	0.002	0.005	0.692
Black	0.722	0.725	0.448	0.446	-0.003	-0.007	0.622
Other Race	0.005	0.005	0.070	0.068	0.000	0.005	0.733
Total Priors	4.405	4.370	5.636	5.515	0.035	0.006	0.642
Property	0.191	0.185	0.393	0.388	0.006	0.016	0.24
Violent	0.215	0.208	0.411	0.406	0.007	0.017	0.211
Drug	0.238	0.244	0.426	0.429	-0.005	-0.013	0.354
Other Crime	0.355	0.363	0.479	0.481	-0.008	-0.016	0.233
Arrest Days from 17	180.8	179.2	105.1	104.5	1.5	0.015	0.278
N	11645	10298					

Table A.5: Models on Any Rearrest with 18-Year-Olds as Controls with Covariates

Variables	Full		Age Limited		Time Limited	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.225 (0.034)	0.000	0.188 (0.076)	0.014	0.200 (0.075)	0.008
Seventeen	0.047 (0.007)	0.000	0.006 (0.017)	0.722	0.061 (0.019)	0.001
2010	-0.001 (0.007)	0.843	0.015 (0.017)	0.361	0.007 (0.019)	0.720
Seventeen*2010	0.015 (0.010)	0.149	0.013 (0.024)	0.597	-0.034 (0.264)	0.194
Male	0.331 (0.008)	0.000	0.348 (0.018)	0.000	0.352 (0.019)	0.000
White	0.088 (0.033)	0.008	0.127 (0.074)	0.085	0.066 (0.073)	0.368
Black	0.154 (0.033)	0.000	0.186 (0.073)	0.011	0.174 (0.072)	0.017
Prior Arrest	0.018 (.000)	0.000	0.016 (0.093)	0.000	0.016 (0.001)	0.000
Property	-0.048 (0.007)	0.000	-0.044 (0.018)	0.014	-0.044 (0.019)	0.020
Violent	-0.038 (0.007)	0.000	0.001 (0.018)	0.938	-0.033 (0.020)	0.094
Drug	-0.015 (0.006)	0.000	0.000 (0.016)	1.000	-0.016 (0.017)	0.341
N	23576		4009		3566	

Table A.6: Models on Any Rearrest with 16-Year-Olds as Controls with Covariates

Variables	Full		Age Limited		Time Limited	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.309 (0.036)	0.000	0.098 (0.100)	0.327	0.386 (0.077)	0.000
Seventeen	-0.022 (0.007)	0.248	-0.012 (0.017)	0.498	-0.030 (0.019)	0.113
2010	0.009 (0.008)	0.140	-0.023 (0.018)	0.205	-0.022 (0.018)	0.255
Seventeen*2010	0.004 (0.010)	0.679	0.034 (0.025)	0.173	-0.004 (0.026)	0.866
Male	0.342 (0.007)	0.000	0.386 (0.018)	0.000	0.353 (0.018)	0.000
White	0.052 (0.035)	0.140	0.231 (0.098)	0.019	-0.025 (0.075)	0.734
Black	0.114 (0.035)	0.001	0.290 (0.098)	0.003	0.042 (0.074)	0.568
Prior Arrest	0.019 (0.000)	0.000	0.020 (0.001)	0.000	0.019 (0.001)	0.000
Property	-0.060 (0.007)	0.000	-0.044 (0.018)	0.016	-0.047 (0.019)	0.011
Violent	-0.017 (0.007)	0.018	-0.011 (0.017)	0.051	-0.017 (0.018)	0.342
Drug	0.001 (0.007)	0.874	0.007 (0.017)	0.684	0.003 (0.018)	0.835
N	21943		3653		3609	

Table A.7: Models on Any Rearrest by Offense Type with 18-Year-Olds as Controls

Variables	All		Violent		Property		Drugs	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.742 (0.013)	0.000	0.692 (0.034)	0.000	0.664 (0.032)	0.000	0.784 (0.023)	0.000
Seventeen	-0.005 (0.019)	0.808	0.002 (0.048)	0.972	-0.070 (0.045)	0.125	0.043 (0.034)	0.204
2010	0.011 (0.019)	0.553	0.043 (0.048)	0.375	-0.033 (0.050)	0.511	0.013 (0.032)	0.673
Seventeen*2010	0.027 (0.027)	0.325	-0.013 (0.068)	0.851	0.102 (0.071)	0.152	-0.001 (0.048)	0.982
N	4009		709		763		1098	

Table A.8: Models on Any Rearrest by Offense Type with 16-Year-Olds as Controls

Variables	All		Violent		Property		Drugs	
	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value	B (SE)	p-value
Intercept	0.759 (0.015)	0.000	0.722 (0.033)	0.000	0.655 (0.039)	0.000	0.853 (0.027)	0.000
Seventeen	-0.007 (0.020)	0.706	-0.078 (0.045)	0.081	0.035 (0.051)	0.496	-0.012 (0.036)	0.751
2010.000	-0.020 (0.021)	0.334	-0.036 (0.048)	0.454	-0.027 (0.056)	0.630	-0.021 (0.037)	0.573
Seventeen*2010	0.037 (0.029)	0.201	0.102 (0.066)	0.121	0.019 (0.074)	0.800	-0.031 (0.052)	0.542
N	3653		799		655		857	

Chart A.1: Number of Arrests for 16- and 17-Year-Olds Arrested within 60 Days of 17th Birthday, 2005-2012

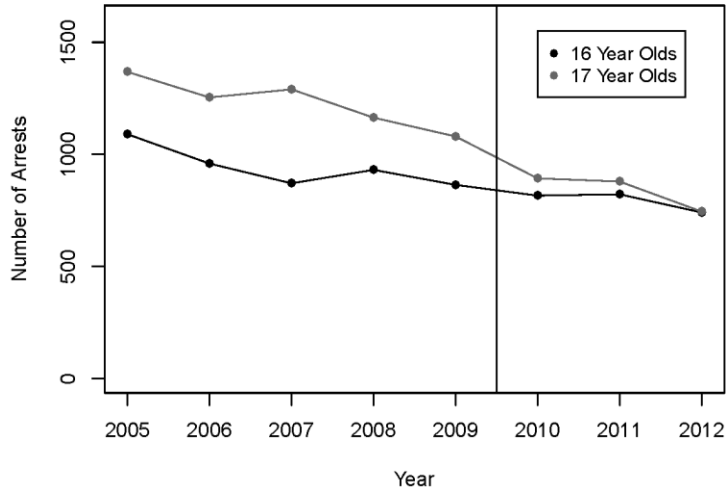


Chart A.2: Number of Arrests for 17- and 18-Year-Olds Arrested within 60 Days of 17th Birthday, 2005-2012

