Estimating the Crime Effects of Raising the Age of Majority: Evidence from Connecticut

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Abstract

Recent empirical research has shown that juveniles do not achieve complete psycho-social maturity until post-adolescence and that processing juveniles as adults in the criminal justice system can be associated with elevated rates of criminal recidivism. In response to these as well as other concerns, a number of states have recently raised their legal ages of majority in the hopes of reducing juvenile offending rates. The State of Connecticut enacted one such law change when its age of juvenile majority was raised from 16 to 17 in 2010 and then from 17 to 18 in 2012 for all but the most serious offenses. The present study examines the effect of Connecticut's policy changes on juvenile crime. To discern between changes in juvenile offending and changes in the propensity of police to arrest youthful offenders in the aftermath of a law change, we utilize two methodological approaches. Synthetic control methods are used to generate triple differences estimates of the effect of Connecticut's policy change on juvenile crime using other U.S. states as a natural comparison group. Next, using NIBRS data for a subset of Connecticut jurisdictions, we compare changes in juvenile arrests to changes in juvenile offending. The resulting evidence suggests that juvenile criminal offending was unaffected by the law change. In addition, evidence exists that in some Connecticut jurisdictions officer rather than juvenile behavior was impacted by this law change, and future efforts to alter offender behavior should anticipate likely adaptive behavior by all crime and justice actors.

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Introduction

Recent research has documented that juveniles, particularly males, do not reach psycho-social maturity until well into their twenties (Steinberg and Cauffman 1996; Cauffman 2012). This finding has implications for the culpability and the reformability of juvenile offenders, particularly older juvenile offenders, who are often tried as adults and sent to adult prisons. If juveniles do not fully mature until post-adolescence, scholars have argued, then juveniles who offend at this age should not be subject to the adult criminal justice system with its greater emphasis on punishment rather than rehabilitation (National Academy of Sciences 2013; Farrington, Loeber, and Howell 2012; Scott and Steinberg, 2009). In light of these arguments, over the last decade, a number of states with sub-eighteen ages of majority have actively considered expanding the jurisdiction of their juvenile courts by increasing the maximum jurisdictional age to eighteen for all but the most serious criminal offenses (Brown 2012). Several states, including Illinois, Connecticut, Massachusetts, and Mississippi, and New Hampshire have already done so in whole or in part. Proponents of these changes argue that raising the age of majority, and thus, expanding the juvenile justice system, will reduce crime by increasing affected juveniles' access to the more abundant treatment opportunities available in the juvenile justice system and by limiting their exposure to the more harmful aspects of the adult justice system. They further argue that the decrease in crime will, on balance, produce cost savings that will offset the higher costs of processing juvenile arrestees as juveniles. Opponents counter that at least some older teen offenders are more appropriately handled in the adult justice system, and that raising the age of majority may increase juvenile crime by reducing the deterrence value of an arrest (Gibson and Krohn 2012).

Beyond the particulars of these changing jurisdictional boundaries, altering the age of majority raises important questions regarding processing effects for the large number of first-time and non-serious offenders who are shifted between these two judicial systems. While a substantial literature on juvenile transfer has developed in recent years documenting the consequences of shifting serious and/or repeat juvenile felony offenders into the adult justice system (Fagan 1996; Bishop et al. 1996; Lanza-Kaduce, Bishop, and Frazier 2005; McGowan et al. 2007; Loughran et al., 2010; Redding 2010; Mulvey and Schubert 2012), far less is known about the public safety consequences of moving more commonplace juvenile offenders between these two systems. If newly retained juveniles benefit from these changes, then crime can potentially be reduced by raising the age of majority. However, the opposite could be true if the absence of a credible deterrent leads juveniles to engage in more and more serious criminal activity. Several recent attempts have been made to estimate the effects of "raise the age" policies (Justice Policy Institute, 2012; Loeffler and Grunwald, 2014b), but their effects on the total crime equation remain unknown due in part to continuing estimation challenges.

The primary challenge to identifying the reduced form effect of any such state-level policy change is the credibility of the policy counterfactual used to substitute for the unobservable condition of what would have happened in the adopting jurisdiction if it had retained the existing policy regime. The most common solution to this estimation problem is to compare the jurisdiction with itself (before-and-after) as well as neighboring jurisdictions while assuming that the timing of the policy shock is as good as random. While regression-based approaches typically account for national time trends and average differences between states, there is reason to doubt that state-level criminal justice policies are, in fact, random with respect to underlying crime trends. A further complication in the estimation of the crime effects of "raise the age" legislation is the long-noted difficulty of separating changes in offending from changes in the likelihood of police arrest that arise due to the absence of age-specific offending statistics in most jurisdictions (Perlman 1949; Wilson 1951; Black 1970; Black 1971; Smith and Visher 1981). Scholars have expressed the concern that changes in police behavior could be mistaken for changes in offender behavior, particularly if shifting arrest patterns are correlated with the demographic characteristics of offenders. While much of this scholarship focuses on the influence of impermissible factors, such as race, ethnicity or gender, this concern might be even greater for factors such as the age of the offender, where both the civilian and the law enforcement communities may have strong preferences regarding the appropriateness of arresting children and adolescents (Goldman 1963; Landau 1981; Novak et al. 2002; Piliavin and Briar 1964; Smith and Visher 1981). A particularly concerning version of this story involves an officer who is less favorably inclined towards arresting a 16-year old in a world in which that 16-year old will be adjudicated in the juvenile justice system and is perceived to be less likely to be punished for a given offense. If officers believe that such an arrest is less likely to lead to punishment and, in turn, make fewer arrests of juveniles, then resulting models will be biased towards a finding that raising the age of majority reduces juvenile offending. Ruhland, Gold, and Hekman (1982) raise just such a possibility in their analysis of juvenile offending in states with different ages of majority—highlighting longstanding concerns that differences in statutory age boundaries could influence officer behavior as much or more than they influence juvenile offender behavior. Such a concern also figures prominently in the applied microeconomics literature that has attempted to estimate the deterrence value of adult sanctions and represents a substantial limitation in drawing firm policy conclusions from state-level case studies (Lee and McCrary 2005; 2009; Hjalmarsson 2009). Despite these concerns, most information on individual criminal offending behavior, particularly age-related criminal offending patterns, is derived from police-generated arrest statistics, which are themselves generated from information gathered after the arrest of an individual. This process filters the underlying offending data, convolving officer behavior with offender behavior. If no other source of information on the prevalence of crime is available, which it often is not, then it can be difficult, if not impossible, to distinguish these two different behavioral responses.

In this paper, we provide two distinct tests for measuring the crime effects of state- and age-specific criminal justice policies, which we illustrate by estimating the crime effects of “raise the age” policies in the State of Connecticut, which adopted a higher age of majority in two discrete policy shifts—raising the age of majority from sixteen to seventeen in 2010 and seventeen to eighteen in 2012. First, we utilize the method of synthetic controls (Abadie, Diamond and Hainmuller 2010) to examine whether arrests among juveniles in treated age groups changed relative to arrests among juveniles in untreated age groups in Connecticut compared to a comparison group of other U.S. states. Specifically, we test whether arrests of 16-year olds changed discretely in January 2010 and whether arrests of 17-year olds changed discretely in July 2012 in Connecticut relative to other states. Comparing Connecticut to other states with similar pre-intervention juvenile crime trends, we account for national trends in juvenile crime. Similarly by comparing 16-year olds in 2010 and 17-year olds in 2012 to other juveniles, we adjust for trends in juvenile arrests within Connecticut. Thus this methodology represents the synthetic control analog to a triple differences estimator using linear regression. To bolster the synthetic control analysis, we also present estimates from a series of differences-in-differences regression models in which we compare individuals in treated versus untreated age groups within Connecticut. The primary innovation in these models is to use information on the age of offenders that is provided by victims in cases that do not result in arrest. Since this information is not sensitive to the police officer's choice to make an arrest, it is a purer measure of juvenile behavior than corresponding data on arrest, which is most often used to estimate the crime effects of juvenile justice policies on crime. The application of these two empirical tests for age- and state-specific criminal justice policies illustrate the utility of these methodological approaches to the estimation of policies such as "raise the age" that are often enacted in the midst of shifting crime and policy regimes that bias simpler linear trend based estimators. The results of these tests provide no evidence that raising the age of majority resulted in an increase in either juvenile arrest or juvenile offending. While point estimates are not sufficiently precise to make strong claims, if anything, the majority of the evidence suggests that juvenile offending either remained unchanged or possibly declined slightly as a result of the policy change. These findings make several contributions to the existing literature. First, they add to a growing literature in applied microeconomics that documents the relative insensitivity of juvenile offending behavior to the sanctions to which juveniles are exposed, suggesting that studies in the criminology literature that report substantial effects may have been estimated on a different margin or that these studies may suffer from selection bias. Second, these results provide evidence in a key policy debate within the criminological literature, indicating that shifting the age of majority is unlikely to have a significant effect on criminal offending (Cauffman 2012; Farrington, Loeber, and Howell 2012; Gibson and Krohn 2012). Finally, they demonstrate a novel approach to separate officer from offender behavior, a first-order and, to date, often unaddressed problem in the evaluation of age-specific crime policies.

Raising the Age of Majority

In 2006 and 2007, the Connecticut legislature considered extending the juvenile court jurisdiction to include sixteen and seventeen year olds, who were, at the time, given youthful status but were nevertheless processed through the adult justice system. Proponents argued that the greater access to rehabilitative programming in the juvenile justice system was essential to address the underlying causes of teenage offending (Poitras 2007a). Opponents, including many Connecticut police chiefs, argued that the shifting of sixteen and seventeen year-old arrestees into the juvenile justice system would overwhelm the already overcrowded juvenile system as well as expose younger children in the juvenile system to victimization (Melone 2007). As in many jurisdictions considering "raise the age" legislation at this time (NCRSJJS, 2013), proponents not only pointed to the additional rehabilitative needs of still maturing juveniles as justification for treating juveniles as juveniles, but also referenced the higher recidivism rates observed among juveniles processed as adults in several academic studies (Poitras 2007b; Aizer and Doyle 2013). These same studies were also interpreted as suggesting that crime rates would be lower in Connecticut after the age of majority was raised, more than making up for the additional costs of processing juveniles as juveniles (Roman 2006).

Once the law change was passed in 2007, concerns remained about the capacity of the juvenile justice system to absorb the influx of sixteen year olds when the first stage of the law went into effect in 2010 (Morganteen 2009). Some jurisdictions reported lack of guidance and police confusion over its implementation, which led some officers to release juveniles with warnings in lieu of arrest (Backus 2011). Despite these implementation challenges, proponents pointed to a number of positive changes in the crime situation involving sixteen year olds as evidence of the new law's success. Proponents likewise pointed to the slightly lower rates of recidivism among arrested sixteen year olds newly processed in the juvenile justice system (Weinreb 2014; See also State of Connecticut 2012). They also highlighted decreases in juvenile arrests coinciding with the 2010 change as evidence of the policy's effects (Justice Policy Institute 2013). To date, no outcome evaluation has yet been undertaken to determine whether these crime reductions are real or whether they are sufficiently large so as to be inconsistent with sampling error. Moreover, to the extent that the observed crime reductions represent a real effect of the policy changes, it is not clear whether they are the result of changes in juvenile offending behavior, officer arrest behavior, or some other mechanism leading to year-to-year fluctuations in juvenile crime rates.

Juvenile Offending and Age

The enactment of changes to the age of majority raises a fundamental theoretical question in criminology—are offenders, particularly young offenders, sensitive to changes in statutory penalties or other changes in how their cases are handled by the justice system? If so, consistent with deterrence theory (Nagin 1998; Zimring 1976), juveniles processed in the adult system as well as juvenile offenders threatened with processing in the adult system, should be less likely to offend, less frequent in their offending, or less likely to engage in serious offending. If, on the other hand, processing juveniles as adults denies them essential services or otherwise exposes them to particularly harmful aspects of the adult justice system, then secondary deviance in the form of escalated or additional offending could be generated, cancelling out or even swamping any deterrent effects (National Academy of Sciences 2013). Finally, differential processing of juveniles could produce no discernable effect on crime if juveniles are unaware or indifferent to the existence of these alternatives and the systems differ less, for the modal offender, then anticipated (Wolfgang, Thornberry, and Figlio 1987; Feld 1997).

Changes to the statutory age of majority also implicate a foundational measurement problem in criminology—separating the possible behavioral responses of juveniles from the possible behavioral responses of law enforcement officers—since changes to policies, penalties, and procedures can simultaneously alter the costs of responding to and committing crime. Despite widespread recognition among scholars that the filtering of criminal statistics through the recording behavior of police officers poses a complex measurement challenge (Perlman 1949; Wilson 1951; Black 1970; Black 1971; Smith and Visher 1981), there has been little quantitative research on the possible adaptive behavior of law enforcement officials to the existence or modification of juvenile jurisdictional boundaries. Recently, however, a series of applied microeconomics papers examining juvenile offending on either side of a jurisdictional boundary have examined both of these questions.[2]

The earliest such paper, by Levitt (1998), examined whether the fraction of the statewide crime rate attributable to juveniles was sensitive to the location of the age of majority or the relative punitiveness of a jurisdiction's juvenile and adult justice systems. In a mixed finding for deterrence theory, Levitt reported sizable declines in age-specific crime rates just after the age of majority, but little evidence that the relative punitiveness of a state's juvenile or adult justice system explained this result. To overcome the unavailability of an age-specific offending rate, this paper imputed the age-specific

[2] For an early criminological precursor of this applied microeconomic work, See Ruhland, Gold, and Hekman (1982).

offending rate from the age-specific arrest rate and the overall non-age-adjusted crime rate. Hjalmarsson (2009) resolved this measurement problem without resorting to imputation when she examined self-report data on offending and perceived probability of punishment from a nationally representative longitudinal sample of juveniles. She reported elevated perceptions of the probability of punishment after the age of majority. However, the magnitude of her finding was much smaller than that reported in Levitt. She also reported no evidence of differences in self-reported offending behavior on either side of the age of majority.

Lee and McCrary, in two working papers, which first applied the regression discontinuity methodology to the question of juvenile offending (Lee and McCrary 2005; Lee and McCrary 2009) reported minimal effects of the age of majority on the density of offending near the age boundary in a sample of arrestees from Florida. They also reported only slight evidence in favor of the effect of the age of majority on juvenile re-arrest rates. Similarly, Loeffler and Grunwald (2014a) in their study of juvenile offending in Chicago, provided evidence of slightly lower recidivism rates, but no indication of substantial differences or density changes. The absence of substantial arrest density changes across the jurisdictional boundaries in both of these studies, as in the study by Hjalmarsson, suggests that the behavioral effects of these adjacent jurisdictions may be smaller than predicted by deterrence theory. However, given the possibility of changes in officer behavior across these jurisdictional boundaries, the effects of the boundary on juvenile behavior will likely remain under-identified unless such density tests have access to information on both juvenile offending and officer arrest behavior.

These more recent studies also contrast with earlier work, which found robust, albeit heterogeneous, evidence of elevated levels of re-offending among juveniles processed as adults (Fagan 1996; Bishop et al. 1996; Lanza-Kaduce, Bishop, and Frazier 2005; See also Loughran et al. 2010 and Drake 2013). Much of this earlier work, however, focused on juvenile transfer populations, the relatively small number of more serious or frequent offenders who were subject to adult prosecution and punishment even though they fell below the relevant age of majority. While these earlier studies contributed to the policy discussion surrounding raising the age of majority by providing evidence that processing at least some juveniles as adults could produce counterproductively elevated juvenile re-offending rates, the policies adopted in Connecticut and elsewhere generally exclude many of these juvenile offenders from the juvenile justice system. For this reason, their experience is of unknown generalizability to the much larger population of juvenile offenders subject to the expanded jurisdiction of the juvenile court.

Based on recent examinations of the age and crime relationship around the age of majority, the anticipated effects of raising the age of majority are likely to be small. The one recent study of the effects of raising the age of majority in another jurisdiction, found little evidence of all the but the smallest of possible effects of raising the age of majority for misdemeanors on re-offending (Loeffler and Grunwald 2014b). That study, however, focused on only one element of the crime equation—criminal re-offending. It is possible that juvenile offending could be affected beyond those directly processed. Evaluations of prior and more limited changes to the exclusive jurisdiction of the juvenile justice system provide limited evidence for such cohort-wide behavioral effects (Jensen and Metsger 1994; Risler, Sweatman, and Nackerud 1998; Singer and McDowall 1988), but given the narrower focus of those changes, it remains unclear whether the broader changes of shifting the entire age of majority will produce similar results. It is also possible that felony defendants, or potential felony defendants, might be less influenced by a higher age of majority than misdemeanor defendants. However, beyond this predicted small magnitude, any observed effects could go in either direction given that the estimated effect of such a law change is the averaging of the opposite signed causal mechanism of unknown relative magnitude predicted by deterrence and rehabilitative theories.

Data and Methods

In 2010, with the implementation of the first-phase of the Connecticut's modified juvenile offender law, the state saw a decline in the number of sixteen year old juveniles who were arrested (See Figure 1). This decline appeared to contemporary observers to be steeper than the decline for other age-groups, particularly fifteen year old offenders (Justice Policy Institute 2013). However, Connecticut, like most other states in the U.S., experienced a substantial secular decline in juvenile arrests throughout this period, one that accelerated in 2009, complicating a simple trend analysis using yearly data (Puzzanchera 2013). To determine the average treatment effect of both the 2010 and 2012 law changes and to resolve the measurement problem caused by missing age-specific offending information in many of the studies in the existing literature, we employ two different estimation strategies—synthetic matching at the state level and a difference-in-differences estimator at the micro-level using age-specific offender information.

Synthetic Controls

There is a large literature in criminology that examines the effects of aggregate level policy interventions using observational data. The standard approach to estimating the treatment effect of such policy interventions is to compare an outcome, typically crime, before and after the intervention in geographic units that were treated to the evolution of the outcome in a sample of geographic units that were not treated. This approach, known as "differences-in-differences" (D-D) is easily implemented using least squares regression and has become ubiquitous throughout empirical social science research. The standard approach to computing D-D estimates of the effect of a state-level policy shock is to regress a state- and time-varying outcome, Y_{it} on a treatment dummy, D_{it}, a vector of time varying covariates, X_{it} and place state and time fixed effects. Accordingly the coefficient on D_{it} can be interpreted as the causal estimate of the effect of the treatment so long as the timing of the intervention is conditionally random. When the intervention is implemented at the same time in all states that received the treatment the D-D estimator simply compares the crime rate in treated state-time periods to the crime rate in comparison state-time periods, net of fixed effects and covariates. Another way to state the identifying assumption of differences-in-differences is that the treated states and the comparison states would have experienced parallel trends in the dependent variable but for the treatment. That is, Connecticut and other states would have experienced the same change in the juvenile arrest rate after 2010 for sixteen-year-olds and after 2012 for seventeen-year-olds, in the absence of the decision to raise the age of criminal majority. Standard methods of constructing a comparison group are not designed to address the validity of this assumption. In general, models will be either estimated unweighted, which assumes that comparison states and treatment states experience common trends, or with population weights, which assumes that population-weighted comparison states are a good comparison group for Connecticut.

The synthetic control method was developed to choose a comparison group in a data-driven way. In particular, in the context of a state-level intervention, the methodology works by assigning an analytic weight to each U.S. state that has not implemented a given policy where the weights are computed such that the difference in a given pre-intervention outcome, such as juvenile arrest between treated states and the pool of available comparison states, is minimized. In this way, the methodology generates a comparison group which, conditional on pre-treatment observables, meets the assumption of parallel trends – at least prior to implementation of the treatment.

In the present application, the synthetic control method empirically determines the mix of states that manifest a similar pre-2010 and pre-2012 trend in juvenile arrests and reported crimes. This approach avoids the need to handpick specific states that could serve as controls for theoretical reasons, but might, in reality, be poor proxies for the counterfactual of Connecticut without a raised age of majority due to different underlying crime regimes or other differences in juvenile justice policy. The procedure works as follows: Let the index j = (1,2,,J) denote the J states in the United States that have not recently raised the age of juvenile majority. The value j=1 corresponds to Connecticut while j=(2,,J) corresponds to each of the other U.S. states in the donor pool. Define Y_0 as a kx1 vector with elements equal to arrest rates in the pre-treatment period and likewise define the kxJ matrix Y_1 as a stack of similar vectors for each of the other J states in the donor pool. The synthetic control method identifies a convex combination of the J-1 states in the donor pool that best approximates the pre-intervention data vectors for Connecticut. Define the Jx1 weighting vector W=(w_1, w_2, , w_J)’ such that:

(A1) $\sum_{i=1}^{J} w_j = 1$

(A2) $w_j \geq 0$ for j=(1,,J)

Condition (A1) guarantees that the weights sum to 1, while condition (A2) constrains that the weights are weakly positive. The product Y_1W then gives a weighted average of the pre-intervention vectors for all states in the donor pool, with the difference between Connecticut and this average given by $Y_0 - Y_1W$. The synthetic control method selects values for the weighting vector, W, that result in a synthetic comparison group that best approximates the pre-intervention crime trend in Connecticut. Once the optimal weighting vector W* is computed, both the pre- intervention path as well as the post-intervention values for the dependent variable in “synthetic” Connecticut can be tabulated by calculating the corresponding weighted average for each year using the donor states with positive weights. The post-intervention values for the synthetic control group serve as the counterfactual outcomes for Connecticut.

Our principal estimate of the impact of each of Connecticut’s policy shocks on crime and arrests uses the pre- and post-treatment values for both Connecticut and its synthetic control group to calculate a simple D-D estimate. Specifically, define Y_{PRE}^{CT} as the average value of the violent crime rate in Connecticut for the pre-intervention period (2001-2009) and Y_{POST}^{CT} as the corresponding average for a defined post-treatment POST period, 2010-2013. Y_{PRE}^{SYNTH} and Y_{PRE}^{SYNTH} are the corresponding quantities for Connecticut's synthetic control group. Then the synthetic D-D estimate is given as follows:

$$DD^{CT}=(Y_{POST}^{CT}-Y_{POST}^{SYNTH})-(Y_{PRE}^{CT}-Y_{PRE}^{SYNTH}) \quad (1)$$

To formally test the significance of any observed relative change in the crime rate of Connecticut cities, we apply a permutation test suggested by Abadie et al. (2010) and implemented by Bohn, Lofstrom and Raphael (2013) to the D-D estimator given in equation (1). Specifically, for each state in the donor pool, we re-compute weights to generate a synthetic control group for that state. Next, we re-compute the synthetic D-D estimates under the assumption that the other states, in fact, raised their age of majority in 2010. Because the causal effect of the placebo laws must be zero by construction, the distribution of these "placebo" D-D estimates then provides the equivalent of a sampling distribution for the estimate DD^{CT} (see Abadie et al. 2010 for a detailed discussion).

To estimate this synthetic control model, we use monthly state-level Uniform Crime Report (UCR) panel data for the period 2001-2012. To ensure that the selected and weighted control cases do not include states that experimented with different versions of the raise the age legislation during this period, we exclude Colorado, Illinois, Missouri, Mississippi, Oklahoma, and Rhode Island from the donor pool. We first implement this method with the fraction of 15-18 arrestees that are sixteen years old.[3] This metric captures any change in the relative contribution of sixteen year-old arrestees above and beyond secular changes for other youthful offenders. In order to assess whether any changes in arrests are observable in changes in the crime rate, we then re-estimate the synthetic control estimates using overall state crime rates. Due to the unavailability of offender age information in the UCR reported crime data, we are unable to estimate age-specific offending rates. Instead, we examine overall statewide crime rates to see whether there is any evidence of corresponding shifts in the overall crime rate.

[3] Due to the unavailability of 2013 UCR, these estimates are limited to sixteen-year-old arrest rates that were subject to the 2010 law change.

Incident-Level Data on Offenders and Arrestees

To address the unavailability of age-specific offending rates at the state-level, we use NIBRS incident-level data from participating Connecticut law enforcement agencies (75 of 102 as of 2008) to determine whether any changes in the more precisely estimated arrest information is mirrored by changes in offending behavior or is better explained by changes in officer arrest, recording, and reporting behavior (See Figure 2). NIBRS data has been used previously to better understand the connections between offenders, victims, and the resulting arrest events—connections that were significantly more difficult to understand when only Uniform Crime Report (UCR) data was available (Akiyama and Nolan 1999; Thompson, Saltzman, and Bibel 1999). A comparison of offender and arrestee age data has also been proposed and tested (Chilton and Jarvis 1999), but has not been incorporated in the literature on the effects of criminal justice policies on juvenile offending, which has primarily relied on either self-reported offending information *or* official arrestee data.

Unlike these other forms of data on offenders, the NIBRS data series contains an annual account of the ages of arrestees as well as estimated yearly ages of non-arrested offenders for all Group A incidents, corresponding to Part I crimes in the UCR program. Non-arrest offender ages in the NIBRS data are derived from incident reports taken by police officers responding to Group A incidents. Since self-reported ages are not available, as they are for incidents leading to an arrest, suspect ages are generated from victim, officer, or other witness statements. These age estimates are, of course, subject to witness error, but past record check studies where both self-reported and victim-reported age estimates are available show a high degree of correspondence (Hindelang, 1981). More importantly for the present application, as long as the errors in witness age estimation are smooth across the age threshold of the policy shock, systematic errors in witness' age identification will not compromise our ability to draw inferences from the data. More specifically, as there is no *a priori* reason to believe that victims or witnesses should change their reporting of an offender's age to the police as a function of the policy shock, an abrupt change in the ages of offenders as identified by victims and witnesses constitutes evidence in favor of a reason change in age-specific offending. Using these data, changes in age-specific police arrest behavior can be separated from age-specific changes in offender behavior. If age-specific arrest rates and age-specific offending rates change equally after the implementation of an age-specific policy, then these changes likely reflect offender behavior without evidence of officer filtering behavior. If age-specific arrest rates and age-specific offending rates change unequally after the implementation of an age-specific policy, then some combination of officer and offender behavioral change is likely present. The full set of possible interpretations of this inequality is explored below.

To estimate the effect of the 2010 law change for sixteen year olds and the 2012 law change for seventeen year olds, we fit two least squares regression models using monthly NIBRS data for all Connecticut agencies that participated in the NIBRS program from 2008 through 2013. For the 2010 law change, we estimate a series of models that accord with the following basic equation:

$$ln(Y_{at}) = \beta_0 + \beta_1 Time + \beta_2 Age_{15} + \beta_3 Age_{16} + \beta_4 Age_{17} +$$

$$\delta_1 T_{2010} \cdot Age_{16} + \delta_2 T_{2012} \cdot Age_{17} + \epsilon_{at} \quad (2)$$

In (2), $ln(Y_{at})$ is the logged number of arrests in month t for age group a, T_{2010} is a dummy variable indicating whether the month falls during or after January 2010 and T_{2012} is a dummy variable indicating whether the month falls during after July 2012. $Time$ is a vector of the count of months from 1 to 60 and accordingly β_1 accounts for statewide time trends in arrests. For simplicity, in (2) time is modeled as a linear process; in practice, we estimate the model more flexibly allowing time to vary according to a quadratic, cubic and quartic time trend. We also interact linear and quadratic measures of time with age to allow each age group to have its own time trend. Finally, we re-estimate (2) using a dummy variable for each of the 60 months in the data to directly model the evolution of statewide time trends.

While the time dummies do not allow each age group to have its own time trend, this model ends up having the greatest explanatory power in explaining log monthly arrest counts.

In (2), the coefficients β_2 through β_4 provide estimates of the number of arrests for each age group relative to 18-year olds, the omitted group. Finally, δ_1 and δ_2 provide an estimate of the treatment effect of the law changes for the sixteen-year-olds after the 2010 and the seventeen-year-olds after for the 2012 law change, respectively. For this estimator, eighteen-year-olds form the counterfactual comparison group and are preferred due to the greater stability of their time trend and the fact that both sixteen- and seventeen-year-olds are eventually treated. All age groups contribute to the time trend and therefore provide enhanced precision. To determine the effect of the law changes on juvenile offending behavior, equation (2) is re-estimated with logged number of offenders in each age group in each month substituted for the logged number of arrests per month. For the estimation of these models, data are limited to the years 2008 through 2013 for agencies participating for the full period.

Formalizing the inequality test outlined above, $\delta_{(Incidents)}$ is compared to $\delta_{(arrests)}$for all treatments, in this case the law change of 2010 for sixteen-year-olds and 2012 for seventeen-year-olds. Assuming the coefficients are both significant and the outcomes are logged, then $\delta_{(Incidents)} \cong \delta_{(arrests)}$indicates that all percentage changes in arrests are matched by similar percentage changes in incidents. $\delta_{(Incidents)} \neq \delta_{(arrests)}$ indicates a disjunction between changes in arrests and incidents. The simplest such disjunction can be seen in the case where $\left|\delta_{(incidents)}\right| < \left|\delta_{(arrests)}\right|$, corresponding to the situation in which the magnitude of the arrest change is larger than the magnitude of the incident change. This pattern is most consistent with a change in officer arrest or recording behavior, assuming both coefficients are similarly signed or $\delta_{(Incidents)} \cong 0$. The same inequality with opposite signed coefficients is less interpretable, while the $\left|\delta_{(incidents)}\right| > \left|\delta_{(arrests)}\right|$ inequality corresponds to the situation in which offender behavior is unmatched by similar changes in arrest statistics. In the extreme, when $\delta_{(arrests)} \cong 0$, officer arrest behavior would appear to be entirely insensitive to the changes in offender behavior.

Robustness

To ensure the robustness of our synthetic and difference-in-differences estimation strategies, we implement several sensitivity tests. For the synthetic control models, we re-estimate our models for two different categories of offenses which we identify as "major crimes" and "discretionary crimes" to assess treatment effect heterogeneity.[4] This is important as police typically have a great deal of discretion regarding whether to make an arrest in the case of a property or drug offense but far less discretion when the offense is violent or, in the case of a property crime, reaches the threshold of a felony or an offense involving substantial use or threat of force. We also run a series of placebo tests substituting non-Connecticut states as "treatment" states and calculate a distribution of differences between these state trends and the synthetic comparison group. Finally, we use American Community Survey (ACS) data to determine whether there is any evidence that birth cohort effects explain changes in age-specific crime and arrest patterns, a proposition which has no support in the data.

Results

We begin with a description of the synthetic control analyses that compare the evolution of arrests among treated age groups in Connecticut to other U.S. states.[5] Figure 3a depicts the primary results of the synthetic control comparison showing the fraction of all youthful arrestees (15-18 year olds) that are sixteen years of age leading up to and following the 2010 law change. Three notable

[4] Major crimes include assault, burglary, murder, motor vehicle theft, robbery, sex offenses and weapons offenses. Discretionary crimes include drug offenses, theft, prostitution, public order offenses as well as family offenses.
[5] For each outcome, Appendix Table 1 presents the weights assigned to each state in Connecticut's donor pool. Across all models, New Hampshire is the most prominent contributor.

findings emerge from this graph. First, the synthetic control methods successfully identify an extraordinarily similar comparison group that closely tracks the trajectory of Connecticut's arrest trends in the pre-2010 period. Second, the trends for Connecticut and its synthetic comparison group sharply diverge in 2010, which is consistent with the dip in 16-year-old arrests observed in Figure 1. Finally, there is a convergence of trends beginning approximately one year after the initial divergence. The magnitude of the arrest difference between Connecticut and all other states in 2010 is 2.7 percentage points (representing a 14% reduction) and falls to under one percentage point in 2012. Figure 3b plots the counterfactual distribution of treatment effects by studying the effect of a placebo intervention for each state in the donor pool. Comparing the actual difference in arrests observed for Connecticut to the placebo comparisons reported in Figure 3b, it appears as though the magnitude of the estimated difference for Connecticut is initially larger than all but two of the placebo estimates, suggesting that Connecticut's decline in arrests is unlikely to be spurious. However, by 2012 the estimated difference for Connecticut is very close to zero and falls well within the counterfactual sampling distribution.

Given the magnitude of the drop in 16-year old arrests observed in 2010, it is possible that a corresponding drop in non-age specific crime rates should be visible if changes in offender behavior are driving this observed drop in sixteen-year-old offenders. Figure 4a presents a series of synthetic control models testing this hypothesis with statewide UCR reported crimes replacing youthful arrests as the dependent variable. These new models achieve a similar level of pre-2010 tracking and also show what, at first glance, are sizable divergences between Connecticut and its synthetic comparison for both violent and property crime trends. However, these drops appear to be unremarkable as a majority of placebo estimates (Figure 4b) are of similar or larger magnitudes.[6]

[6] Similar results (not shown) were found for other crime categories (e.g., drug offenses, other offenses)

We next turn to Table 1 which reports regression-based estimates of the effect of raising the age of criminal majority using Connecticut NIRBS data. These models are estimated using the basic functional form represented in equation (2). Estimates are presented for both the 16-year old and 17-year old policy changes using NIRBS incident as well as arrest data. For each incarnation of the model, we report estimates of δ_1 and δ_2, the estimated treatment effects, having accounted for independent age and time effects. We begin by estimating models with a global time trend, modeled linearly as well as using polynomials of orders up to and including 4. We next allow each age group to follow its own linear or quadratic time trend. Finally, we model time in a fully flexible manner using a vector of 59-month dummies, one for each month in the data exclusive of a reference month. Robust standard errors are reported in parentheses below the parameter estimates.

Referring to model 1 in which time is entered linearly, we observe a 0.7% reduction in offending among 16-year-olds and a 7.3% reduction in offending among 17-year-olds as a function of the law change. Neither coefficient is significant at conventional levels though standard errors are not sufficiently small to rule out potentially important effects. Repeating this analysis with higher order polynomials added to the regression equations (Models 2-4) going from quadratic up to quartic models to capture more complex trends in the secular time logged arrested relationship reveals small fluctuations in the coefficient estimates although all still negative, relatively small, and insignificant. Turning to the even more flexible specifications, we next consider models in which we allow each age group to have its own time trend, which was modeled first as a linear trend and also as a quadratic. Here, estimated coefficients are larger, indicating that linear trends potentially mask some of the identifying variation. In particular, the quadratic age-specific model produced a much larger coefficient for sixteen-year-olds, but not seventeen-year-olds. In the model with age-specific quadratic time trends, we observe a 14% reduction crime among the 16-year-olds, a result which nearly reaches significance at conventional levels. However, while the model is more flexible, standard errors are also quite a bit larger due to multicollinearity. Finally, we model time in a maximally flexible way using a set of dummy variables for each month of data in the series. Standard measures of fit (the adjusted R^2 and the AIC) indicate that this model fits the data best. Here, we observe little change in offenses for either the 16- or the 17-year olds with estimates of a 1% and 3% decline in offending respectively, neither of which is statistically significant.

Next, we turn to the models which estimate the effect of the law change on the number of juvenile arrests. These models are presented in columns (3) and (4) of Table 1. Overall, the pattern of the coefficients is very similar to those presented in columns (1) and (2). With one exception, coefficients are fairly small and imprecisely estimated indicating no evidence of large changes as a function of the law change. As the estimates are similar to and not statistically distinguishable from those we obtained for offending, there is little evidence that patterns in the estimated treatment effects are driven by changes in police behavior. However, the large p-values in each set of tests preclude us from formally testing to see whether each set of coefficients are different from each other.

Table 2 presents similar estimates except that we next estimate the effect of Raise the Age on "major" crimes, offenses which are likely to yield an arrest when the available evidence allows police to identify an offender. While police typically have a great deal of discretion in making arrests for more minor crimes, they have little discretion in making an arrest for violent crimes or felony property crimes. If coefficients reported in Table 2 are larger in magnitude than those reported in Table 1, this will tend to support the idea that police behavior has masked important changes. Given that the coefficients reported in Table 2 are very similar to those reported in Table 1, the table provides additional evidence that the generally small and statistically insignificant effects reported in Table 1 are not attenuated as a result of changes in police discretion.

To better understand these observed patterns, we examined the trends in youthful arrests for the top ten reporting agencies in Connecticut. These included Bridgeport, East Hartford, Hartford, Manchester, Meriden, New Britain, New Haven, Norwalk, Stamford, and Waterbury. For seven of the ten largest agencies, the trends for each age group are quite similar and reveal little indication of a discontinuity for sixteen-year-old arrestees in 2010 or seventeen-year-old arrestees in 2012. New Haven provides the clearest evidence of a discontinuity for sixteen-year-olds in 2010, but even this change is paralleled by a drop for fifteen-year-olds in the same year. However, Hartford and East Hartford each manifest unusual patterns of arrests in 2010. In the case of East Hartford, there is a noticeable drop in arrests for sixteen-year-olds in 2010, which is followed by a recovery in this trend in 2011. At the same time, the number of arrests for fifteen-year-olds correspondingly jumps and then returns to level—suggesting that some form of substitution process could be at work. The trends for Hartford are starker still. Hartford's sixteen-year-old arrest trends manifest an immediate reduction in all but the most serious youthful arrests in the period after the 2010 law change—a drop-off that does not recover until 2012. This reduction, with its sharply discontinuous pattern, is much more consistent with a change in police arrest behavior than a change in juvenile offending behavior. Intriguingly, this result may help explain why the drop in juvenile arrests for Connecticut as a whole observed in the synthetic control models was not mirrored in the micro-level analysis. However, exclusion of Hartford from the synthetic control models did not significantly change the results of these models.

Discussion

Recent research has shown that juveniles, including juvenile offenders, continue to mature well into their twenties. These findings, when coupled with related research showing the adverse effects of prosecuting certain juvenile offenders as adults, has given rise to a multi-state policy initiative to raise the legal age of majority in states with a sub-18 age of majority. To date, several states have already enacted such changes and legislatures in several more are actively considering doing so. Despite this considerable research and policymaking agenda, no previous study has systematically evaluated the effects of these policy enactments to determine whether, as proponents of these policies have postulated, youthful crime will be diminished, or as critics have feared, will be exacerbated. However, some commentators have reasoned that police changes were effective given continuing and possibly accelerating decline in juvenile arrests, particularly 16-year-olds after Connecticut's modified age of majority went into effect in 2010.

To assess this possibility, and demonstrate the utility of synthetic-control and age-specific offender/arrest inequality tests for resolving situations in which simpler estimation techniques and tests produce ambiguous results, the present study examined how these observed changes in arrests for affected juveniles co-varied with national trends and age-specific juvenile offending rates within Connecticut. Using these two complementary approaches, we observe that Connecticut experienced a sizable but temporary drop in their arrests of sixteen year olds compared to other states, which was not mirrored by a change in the overall reported crimes for Connecticut versus other states. Looking more closely at these patterns, we observed that while the drop in juvenile arrests for sixteen-year-olds is greater than expected based on the pre-existing arrest trends for youthful offenders, when age-specific offending rates are available, it does not appear that these trends are present, at least to the same degree. Finally, after exploring the heterogeneity across agencies within Connecticut, we observed that while many agencies have age-specific arrest trends that show a steady decline beginning in 2008, two years before the law change went into effect, at least one large agency has trends that sharply diverge after the 2010 law change only to converge again circa-2012. This temporary drop was observed in a non-NIBRS ORI, which could explain why this pattern was also observed in statewide synthetic control estimates but not in the age-specific offender/arrest tests that rely on NIBRS information. These results support several conclusions.

First, these results add to growing evidence that statutory boundaries between the juvenile and adult justice systems as well as changes to these boundaries have few observable effects on offender behavior (Lee and McCrary 2009; Hjalmarsson 2009; Risler, Sweatman, and Nackerud 1998; Singer and McDowall 1988; Jensen and Metsger 1994). This may be because of the absence of meaningful treatment differences or, consistent with other research on juvenile decision-making, the insensitivity of juveniles to these incentives.

Second, these results suggest that in research that examines how legislative changes or boundaries affect criminal behavior, it is essential to look closely at possible effects on official behavior—who gets arrested, and as other research has shown—what they get arrested for. This paper provides some of the first empirical estimates to document that what may seem like changes in offending behavior can result from changes in officer arrest behavior. Due to the absence of age-specific offending rates in many large municipalities, because of non-participation in the NIBRS program, the effects of age-specific policy changes will likely be difficult to disentangle. However, when information on arrestees and offenders is available, as this study proposed, officer and offender behavior can be readily separated.

For the policy discussion in Connecticut, these results suggest that there is no discernable effect of raising the age on crime or juvenile crime. And while there is some evidence of changes in the number of arrests by age, there is also huge variation by agency, reflecting the implementation challenges noted contemporaneously with the policy change in 2010. The change in the age of majority, however, was not the primary driver of declining juvenile crime rates, nor did the change halt these pre-existing declines, as was initially feared. As such, the policy may have contributed to additional processing costs without providing contaminant cost-saving in the form of reduced recidivism. Beyond Connecticut, these results also suggest that public safety may not be the primary lens through which this policy initiative should be viewed (Brown 2012; NCRSJJS 2013; Rubin 2012). While this study is the first such study to examine “raise the age” and therefore to show no impact on the overall crime equation, it is the second study to show no impact on a measure of criminal offending (Loeffler and Grunwald 2014b). Future research on “raise the age” and other related policy initiatives (e.g., juvenile decarceration) could usefully focus on the non-public safety effects of these policies. For example, little is known about the effects of shifting the age of majority on schooling or labor market outcomes, though recent work by Aizer and Doyle (2013) suggests that changes in juvenile justice processing or punishments could generate substantial increases in human capital among affected juveniles. This is despite the fact that many of the features of the juvenile justice system that are most obviously different than the adult juvenile system, such as routine access to educational opportunities and limited dissemination of criminal history record information, are more directly tied to these untested life-course outcomes rather than the more commonly studied metrics of crime and criminal recidivism.

A final possible consequence of this finding is that the current policy focus on simply shifting juveniles between parallel justice systems may not be as impactful as either proponents hoped or critics feared (Cauffman 2012; Farrington, Loeber, and Howell 2012; Gibson 2012). Instead, policy could focus on those aspects of the juvenile justice system that are thought to be helpful, demonstrate evidence of these benefits, and then pilot importation of these policies into the adult system or visa-versa. Recent research, for example, has found extraordinarily promising evidence that providing at-risk youth with cognitive behavioral therapy can have profound effects on future criminality (Cook et al 2014). This focus on identifying specific practices that are beneficial to juveniles and making them available to juveniles regardless of the specific judicial system in which they are prosecuted reconciles the need to ensure juveniles receive adequate services with the emerging evidence that the distinction between juvenile and adult justice systems may be less meaningful than previously thought. Such an emphasis echoes elements of past policy proposals to abolish the juvenile justice system altogether (Feld 1997), which generally focused on the reduced procedural safeguards for juveniles in the juvenile justice system rather than the exchangeability of the two systems.

Whether these alternative policies are adopted, we will soon have even more evidence of the effectiveness of the current "raise the age" policies. Massachusetts, New Hampshire and Illinois all raised their respective ages of majority in whole or in part in 2014. New York is currently considering doing so as well. As studies of these law changes are undertaken, similar or disparate results may be found. The policy differential between the juvenile and adult justice systems in each of these jurisdictions may not be perfectly substitutable. Similarly, differences in the juvenile offending population could generate different results. Even if the results reported here do not hold for other jurisdictions, the present investigation suggests that careful attention to measurement issues is warranted, and that the methods demonstrated in this paper can resolve many of the most common estimation challenges encountered in the evaluation of single-jurisdiction age-specific policies.

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Figures and Tables

Figure 1. Trends in Connecticut Youth Arrests by Age, 2003-2013

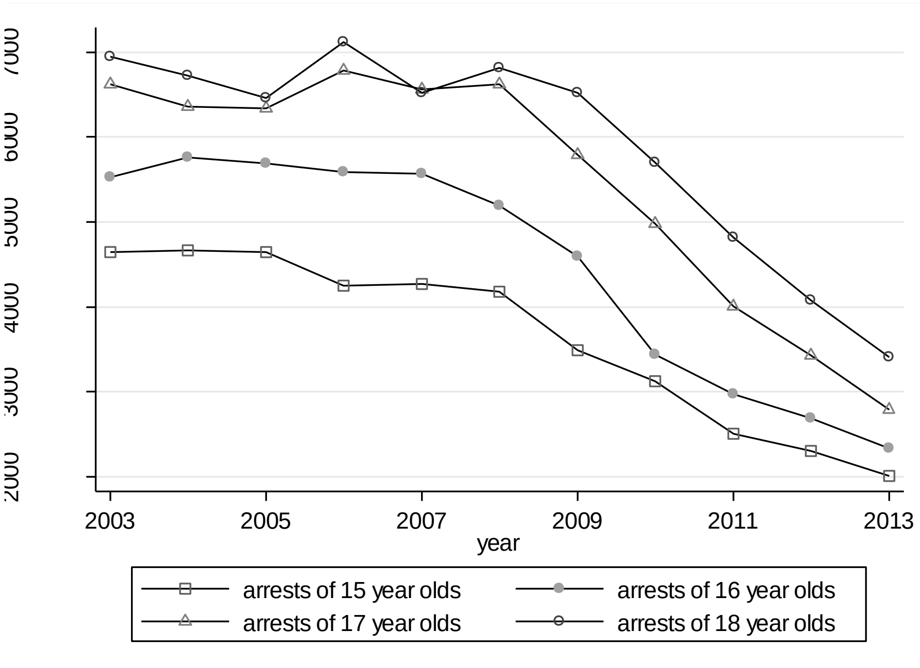
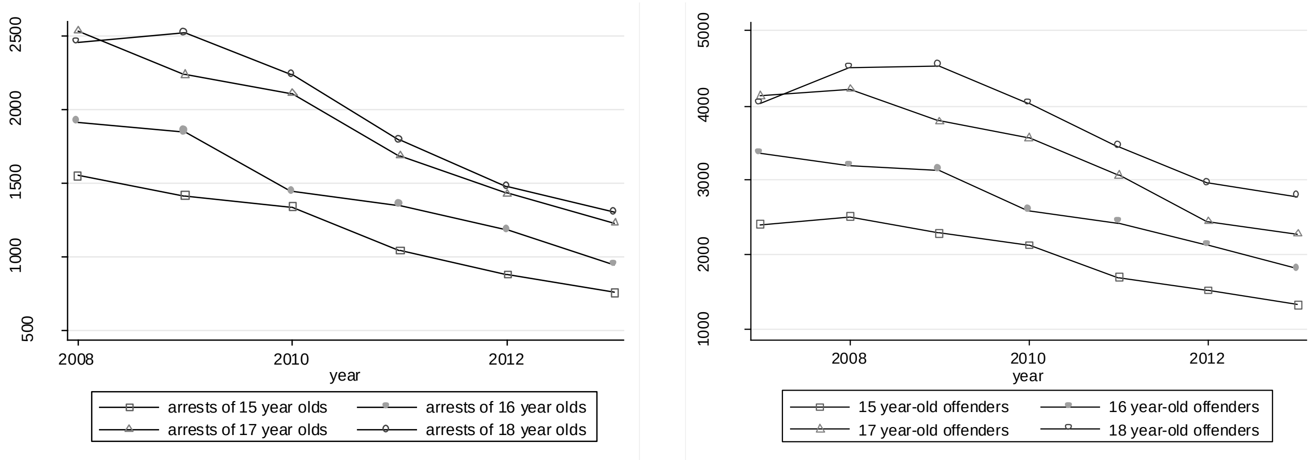
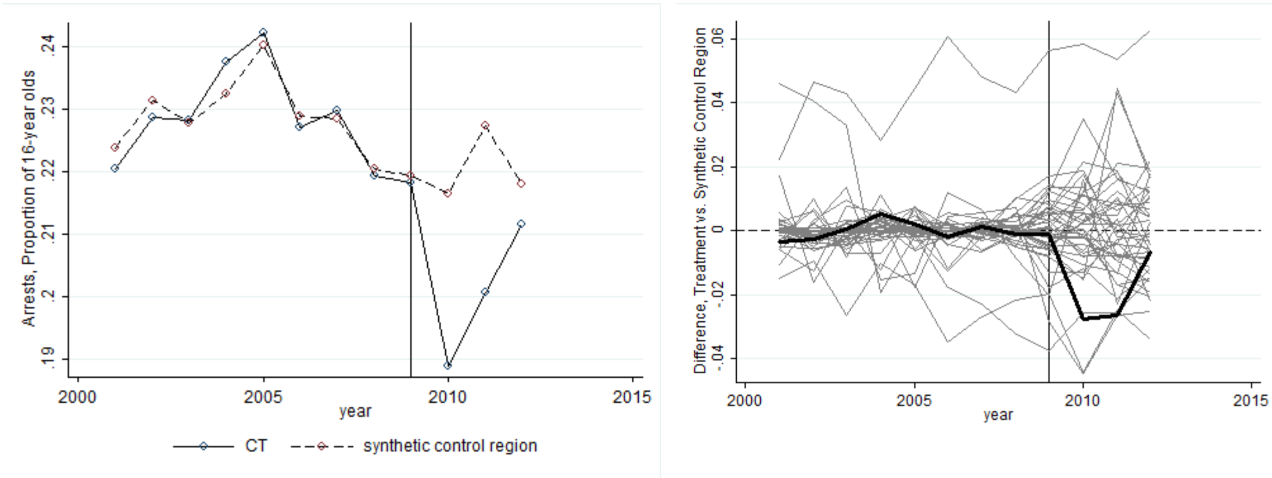


Figure 2. Trends in Connecticut NIBRS ORI Youth Arrests/Offenders by Age, 2008-2013



(a) (b)

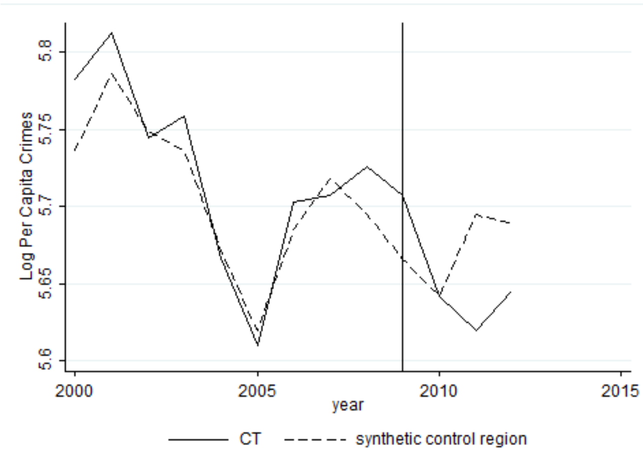
Figure 3. Synthetic Control Graph for Proportion of Youthful Arrests by Sixteen Year Olds



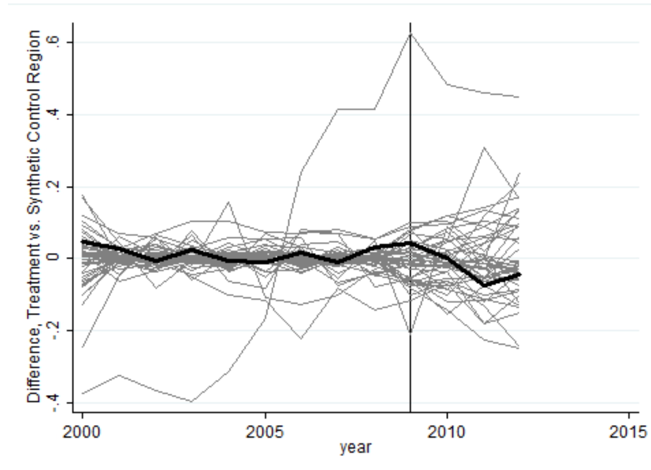
(a) (b)

Figure 4. Synthetic Control Graph for Crime Rate (Connecticut vs. U.S.)

Violent Crime

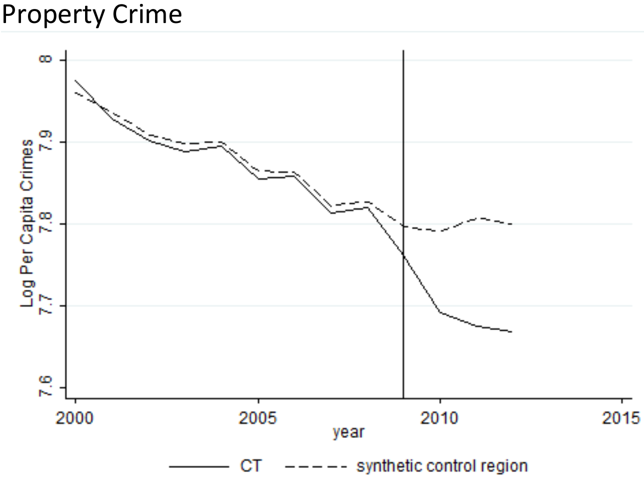


(a)

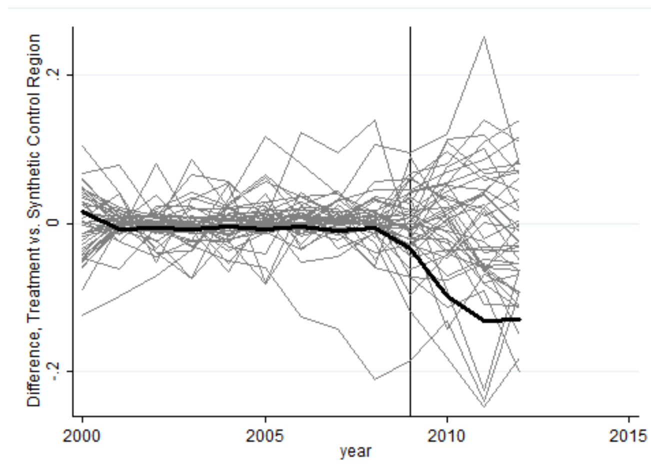


(b)

Property Crime



(c)



(d)

Figure 5. Top Ten ORI Trends in Connecticut Youth Arrests by Age, 2008-2013

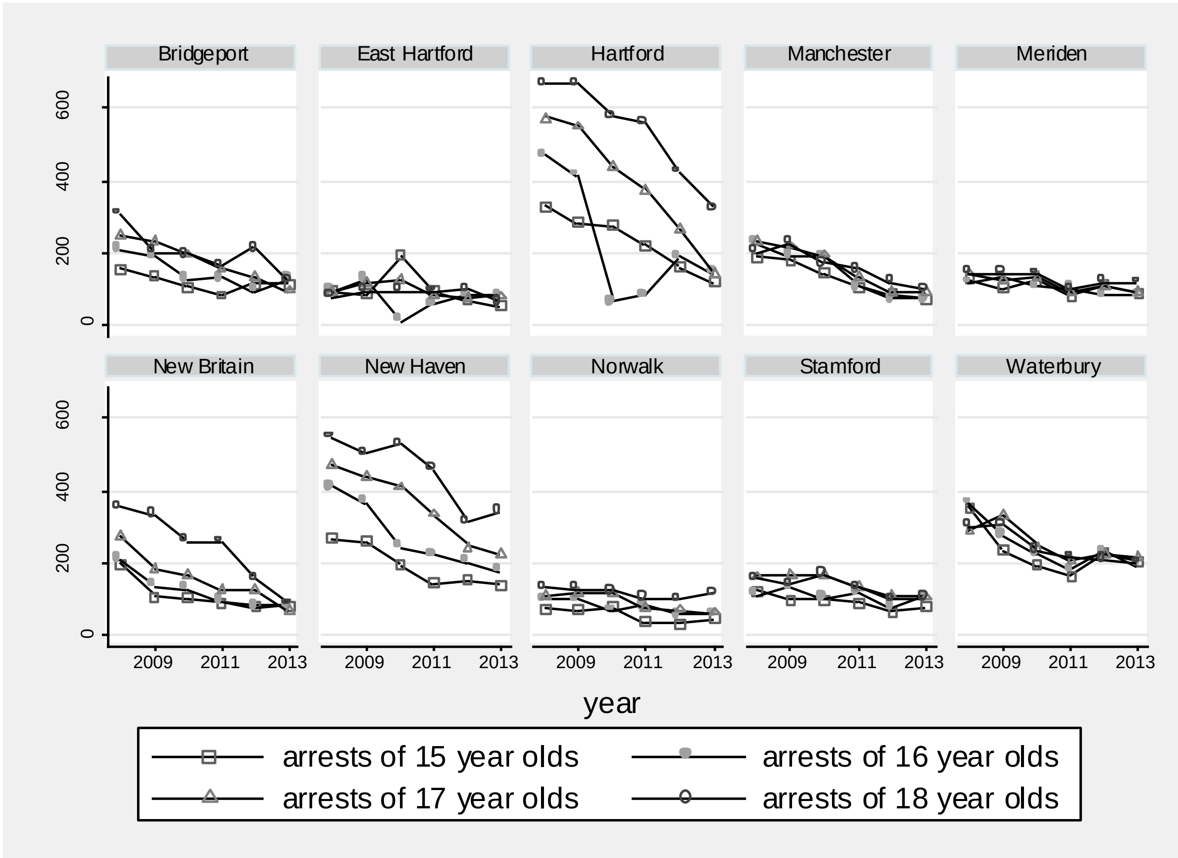


Table 1. Difference-in-Differences Log Arrest Coefficient Estimates

	Log Incidents		Log Arrests	
	Age16	Age17	Age16	Age17
Global Time Trend				
Linear	-0.007	-0.073*	-0.018	-0.051
	(0.041)	(0.036)	(0.042)	(0.040)
Quadratic	-0.022	-0.050	-0.047	-0.006
	(0.040)	(0.037)	(0.041)	(0.039)
Cubic	-0.016	-0.050	-0.041	-0.006
	(0.040)	(0.037)	(0.040)	(0.040)
Quartic	-0.017	-0.050	-0.037	-0.009
	(0.041)	(0.037)	(0.041)	(0.040)
Age-specific Time Trend				
Linear	-0.033	-0.082	-0.058	-0.070
	(0.063)	(0.058)	(0.062)	(0.053)
Quadratic	-0.142*	-0.043	-0.219***	0.037
	(0.076)	(0.081)	(0.069)	(0.068)
Time Dummies	-0.010	-0.032	-0.033	0.011
	(0.029)	(0.028)	(0.031)	(0.033)

Table 2. Difference-in-Differences Log Arrest Coefficient Estimates: Major Crimes

	Major Crimes			
	Log Incidents		Log Arrests	
	Age16	Age17	Age16	Age17
Global Time Trend				
Linear	0.002	-0.043	-0.044	-0.089
	(0.050)	(0.050)	(0.061)	(0.055)
Quadratic	-0.004	-0.032	-0.062	-0.061
	(0.051)	(0.052)	(0.061)	(0.055)
Cubic	-0.000	-0.033	-0.060	-0.061
	(0.051)	(0.052)	(0.061)	(0.055)
Quartic	0.009	-0.042	-0.050	-0.070
	(0.051)	(0.051)	(0.062)	(0.055)
Age-specific Time Trend				
Linear	-0.097	-0.015	-0.075	-0.064
	(0.078)	(0.070)	(0.086)	(0.064)
Quadratic	-0.190*	0.051	-0.244**	0.022
	(0.099)	(0.111)	(0.110)	(0.094)
Time Dummies	0.023	-0.016	-0.032	-0.034
	(0.038)	(0.038)	(0.047)	(0.047)

Appendix

Figure A1. American Community Survey Annual Population Estimates

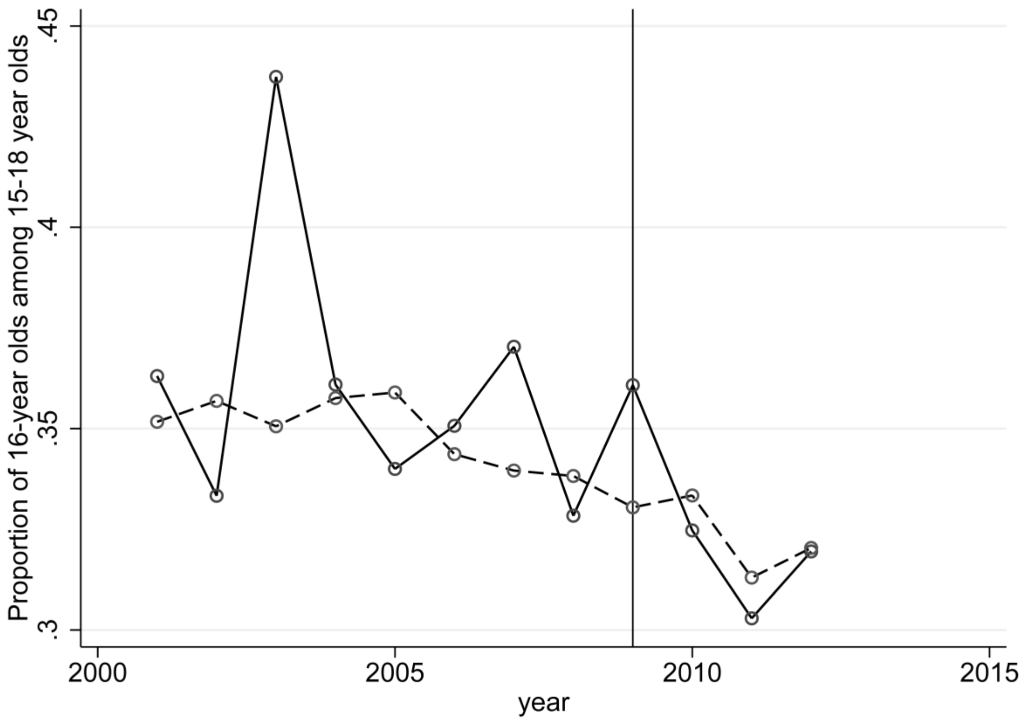


Table A1. Contributors to Connecticut's Synthetic Comparison Group

State	Arrests, Proportion of 16-year olds	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor Vehicle Theft	Count
AK						0.083			1
AL									0
AR								0.109	1
AZ									0
CA									0
DC			0.012						1
DE	0.428	0.027							2
FL									0
GA	0.178								1
HI						0.059			1
IA		0.212	0.234						2
ID							0.131		1
IN					0.530				1
KS							0.006	0.120	2
KY									0
LA				0.255				0.181	2
MA				0.118					1
MD						0.020			1
ME								0.052	1
MI									0
MN				0.120					1
MT	0.083	0.273			0.100				3
NB									0
NC									0
ND		0.178			0.003				2

NH			0.066	0.177	0.367	0.269	0.376	0.108	6
NJ			0.486			0.179			2
NM						0.009			1
NV		0.134		0.025			0.003		3
NY			0.073						1
OH									0
OR									0
PA				0.284					1
SC								0.074	1
SD						0.024			1
TN								0.292	1
TX	0.311			0.028					2
UT		0.110							1
VA						0.356			1
VT			0.130					0.064	2
WA							0.108		1
WI									0
WV		0.067					0.276		2
WY									0