LOWERING THE AGE ON CRIME: AN ASSESSMENT OF JUVENILES' RESPONSIVENESS TO HEIGHTENED CRIMINAL SANCTIONS

An Undergraduate Research Scholars Thesis

by

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Submitted to Honors and Undergraduate Research
Texas A&M University
in partial fulfillment of the requirements for the designation as an

UNDERGRADUATE RESEARCH SCHOLAR

Approved by
Research Advisor: Dr. Jason Lindo

May 2015

Major: Economics

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ABSTRACT

Lowering the Age on Crime: An Assessment of Juveniles’ Responsiveness to Heightened Criminal Sanctions. (May 2015)

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Nationwide, the 1990s saw an increase in the rate of violent crimes committed by individuals under the age of 18. States responded to this rise with various policies including the lowering of the age of criminal majority, the age at which juveniles fully enter the adult criminal and judicial systems. This paper tests whether this policy caused a decrease in crime among the juveniles whose jurisdiction changed. I find that it did not. This conclusion is important since it sheds light on the question of whether juveniles respond rationally to heightened criminal sanctions. It is also an important conclusion in light of the social costs imposed by stiffer sanctions on juvenile crime including lower high school completion rates, higher recidivism rates, and higher expenses for taxpayers. This paper differs from prior research by using panel data and several applications of a difference-in-differences design to estimate this policy’s causal effect. In particular, this model tracks the crime rate over time of youths in Wisconsin and Wyoming whose jurisdiction changes from the juvenile to the adult system. The model compares the crime rates of each of these treatment groups to rates in various control groups in the same or a different state which do not undergo the same jurisdictional change. The results show that any deterrent effect of this policy change is too small—both in the long-term and the short-term—to be detected by these methods.

SECTION I

INTRODUCTION

Nationwide, the 1990s saw an increase in the rate of violent crimes committed by individuals under the age of 18. In 1995, the juvenile crime rate stood at .49 percent, up from .33 percent in 1988 (OJJDP Statistical Briefing Book). States responded to this rise in a number of ways including the lowering of the maximum age of juvenile jurisdiction, the highest age at which individuals can be tried in juvenile courts. This nationwide shift in policy was part of an effort to "get tough" on crime and stood on the assumption that more punitive sanctions deter crime (Hartston & Richtelli, 2006; Redding, 2010). In 1993, Wyoming lowered its maximum age of juvenile jurisdiction from 18 to 17, thereby excluding all 18-year-olds from the juvenile court system. In 1995, New Hampshire and Wisconsin followed suit by lowering their respective maximum ages of juvenile jurisdiction from 17 to 16 years of age (Torbet et al., 1996).[1] I find that the deterrent effect of this policy change is negligible.

This study examines these policy changes by using difference-in-differences designs to measure the effectiveness of these changes.[2] These designs observe the change in crime rates among the group of juveniles whose jurisdiction changes and compares it to the change in a control group that serves as a good counterfactual for the changes in the treatment group that would occur in

[1] New Hampshire changed its maximum age of juvenile jurisdiction back to 17 in 2014 in an effort to focus more on rehabilitation than punishment, to promote lower recidivism rates, and to preserve juvenile rights (Conley, 2014)

[2] Butts and Roman (2014) examine the effectiveness of these states' policy changes and find that only New Hampshire outperformed the 10-year national average decline. Their analysis, however, does not establish whether these states were experiencing fluctuations in crime rates similar to the national average before the policy change. Furthermore, their analysis does not analyze the effect of these policies specifically on those age groups for which the policies apply but instead on all juveniles, even those not subject to the adult criminal system.

the absence of the treatment. The results show that these policies do not have an observable impact on the crime rate both in the short-term and the long-term.

The question of whether juveniles responded to these policy changes is important for several reasons. First, this research demonstrates the effectiveness of these policies in these three states. These states were three of many which implemented some kind of policy change in the late 1980s and early 1990s in response to rising juvenile crime, especially to crime associated with gangs (Torbet et al., 1996; Hartston & Richtelli, 2006). This study highlights whether these changes have met their intended goal. This policy question is important since research indicates that adult sanctions impose large costs upon juveniles themselves and society at large. Juvenile incarceration, for example, causes lower rates of high school completion and higher rates of adult recidivism (Aizer & Doyle, 2013). The adult correction system is also more expensive to taxpayers than the juvenile system. For fiscal year 2012 in Texas, for example, it cost $50.04 per inmate per day to run the adult corrections system. The most punitive juvenile corrections system, however, cost only $29.78 per inmate per day. The less punitive juvenile corrections system cost only $22.42 per inmate per day (Texas Legislative Budget Board, 2013). This paper helps to determine whether these policies provide a comparable benefit to society in the form of decreased crime to help outweigh these social costs.

Furthermore, this research shows whether juveniles respond rationally to the use of adult sanctions on juvenile crime. A great body of crime research and policy formulation rests upon the theoretical formulation of Becker (1968). This theory asserts that individuals act rationally in committing crimes, weighing the expected marginal costs and benefits of each criminal act.

Under this theory, a rise in the cost of crime should theoretically decrease the amount of crime committed. The adult criminal system raises the cost of crime by not sealing any criminal records and by generally giving more punitive criminal sanctions (Levitt, 1998; Lee & McCrary, 2005). My paper helps to analyze whether higher costs of crime, observed to decrease crime among adults, has a similar effect for juvenile crime (Hansen, 2013).

Literature Review

This paper builds on the literature on the effectiveness of using stiffer sanctions on juveniles, which has reached mixed results. Levitt (1998) uses several measures to determine the behavioral response of juveniles to increased sanctions. First, he uses an ordinary least squares (OLS) regression that takes juvenile crime rates as a function of the punitiveness of a state's juvenile justice system.[3] This regression controls for a number of other variables including the percentage black, the percentage residing in urban areas, the state unemployment rate, the legal drinking age, and age demographics. The results show that states do see a decrease in juvenile crime in time periods following the presence of a more punitive juvenile justice system.

Levitt also uses an OLS regression to assess the relationship of the change in punitiveness when an individual reaches the age of criminal majority and the observed change in crime at that age. This relative punitiveness between the two systems is calculated by dividing the ratio of adult prisoners to adult crimes with the same ratio for juveniles. He finds that states who have a greater

[3] Levitt measures the punitiveness of a state's juvenile justice system both as the proportion of juveniles in custody to the total amount of juvenile crimes reported and as the proportion of juveniles in custody to the entire population of juveniles aged 15-17 in the state.

difference in punitiveness between the adult and juvenile systems experience greater decreases in crime when juveniles reach that age.

Hansen and Waddell (2014) find that minimum sentencing laws applicable to juveniles in Oregon do have a deterrent effect, but only for certain types of crimes and only for those sentences with the harshest penalties. They use the identification strategy of a regression-discontinuity design. These designs observe changes in outcome at a certain cut point at which a treatment starts to apply. The observations immediately on either side of this cut point are considered comparable *ex ante* and any difference in outcome is therefore judged to be a result of the treatment. Specifically, Hansen and Waddell observe behavioral changes at the age of 15 when juveniles become subject to the exogenous impact of the state’s minimum sentencing laws. This strategy is effective since other variables, including the legal ability to drive, curfews, and dropout laws, do not change at the age of 15.

Lee and McCrary (2005) also use a regression-discontinuity design and use the age of 18, the age of criminal majority in Florida, as the cut point. They find, however, that there is no significant change in criminal behavior at this cut point. They use an identification strategy very similar to that used by Hansen and Waddell. However, the age of 18 does not isolate the variables of smoking, gambling, and attending college, which could very well distort the impact of these adult sanctions. These latter variables could reasonably outweigh or compensate for any deterrent effect of the increased sanctions.

My paper contributes to the literature by using panel data to observe detailed fluctuations in crime rates over time. To this point, the literature on adult sanctions in general and on the age of criminal majority in particular has focused in large part on exploiting age cutoffs in an attempt to determine the causality of an already established policy. This paper, however, exploits policy *changes* and compares juvenile crime frequency to a control group both before and after these changes. This approach documents the extent to which juveniles respond immediately at the policy change. Past research indicates that juveniles are knowledgeable of increased sanctions as they reach the already existing age of criminal majority (Hjalmarsson, 2009). This paper, however, shows whether there is a time lag in knowledge of and responsiveness to these policies when these policies change. It is possible that juveniles may take a fair amount of time before learning about these policy changes and changing their behavior in response to them.[4]

[4] The use of a difference-differences design to study juvenile crime policy is not entirely new. Jensen and Metsger (1994) use a difference-in-differences approach to study Idaho's mandatory waiver law, finding no effect on deterring juvenile crime. However, my paper studies the specific policy of changing the maximum age of juvenile jurisdiction rather than mandatory waiver laws. Furthermore, my paper uses a richer analysis by following juvenile crime on a monthly level, whereas Jensen and Metsger simply compare Idaho with a neighboring state only once before the policy change and once after the policy change.

SECTION II

METHODOLOGY

This paper uses difference-in-differences designs to measure the effectiveness of the lowering of the maximum age of juvenile jurisdiction in Wisconsin and Wyoming.[5] These designs observe two groups which exhibit similar fluctuations in outcomes leading up to the policy change. At the policy change, one of these groups is subject to a certain treatment while the control group is not. The underlying intuition is that the change observed over time in the control group provides a good counterfactual for the treatment group. The control group shows the changes of outcome that would occur in the treatment group in the absence of the treatment. If there is a difference in outcome between the groups after the policy change, it indicates that the policy is effective. If, however, both groups exhibit similar outcomes after the policy change in the treatment group, the policy likely has no effect.

For Wisconsin, the treatment group is 17-year-olds living in that state. Starting in January of 1996, the law excluded these individuals from the juvenile court system and directed them to adult courts when apprehended for any crime (Torbet et al. 1996). I compare this treatment group to the 17-year-olds in Nebraska, New York, and Iowa.

[5] I exclude New Hampshire from this paper even though it did lower its age of criminal majority in 1995. This is because data for New Hampshire is missing leading up to the policy change and is impacted by reporting issues in the years immediately following the policy change. Hence, the only effective control groups for New Hampshire would be other age cohorts within the state, subject to these same data shortcomings. None of these groups, however, satisfy the identifying assumptions for a difference-in-differences design.

For Wyoming, the treatment group is 18-year-olds. Before the state legislature changed the age of criminal majority in 1993, the state had the highest maximum age of juvenile jurisdiction in the nation (Torbet et al., 1996). Starting in January of 1994, these 18-year-olds were excluded from the juvenile court system. I compare this treatment group to the control group combining 17-year-olds and 16-year-olds in the state, who are subject to the juvenile court system throughout the time period studied.

The regression formula for this approach is as follows:

$$(1)\ ln(Y_{gt}) = \alpha_1 group_g + \alpha_t + \alpha_3 treatment_{gt} + \epsilon_{gt}$$

where Y_{gt} is the number of crimes per capita committed by those in group g at time t. The variable $group_g$ is a dummy variable that identifies whether the observation is in the treatment group or the control group of its respective design. This variable takes a 1 if the observation is in the treatment group. α_t designates time period fixed effects (month and year). The variable $treatment_{gt}$, the variable of interest, is a dummy variable indicating whether the treatment of adult jurisdiction has been applied. Observations take a 1 for this variable if they are in the treatment group and after the policy change in that group.

The most important identifying assumption of a valid difference-in-differences design is that the control and treatment groups have similar trends before the policy change. This establishes that the control group provides a good counterfactual for the changes in the treatment group in the absence of the treatment. If the two groups are not trending similarly before the policy, any observed difference after the policy change is likely to be the result of factors acting upon the crime rate before the policy was applied. I choose control groups to satisfy this identifying assumption and I check for the similarity of trends in two ways. First, I examine the fluctuation in quarterly or yearly graphs to see whether there is any noticeable divergence or convergence between the groups before the change.

Second, I test the statistical significance of an observation being in the treatment group both one year and two years before the policy change. Thus, in addition to Equation (1), the table for each application of the design also lists the results for the two following regressions expressed in general form:

$$(2)\ ln(Y_{gt}) = \alpha_0 + \alpha_1 group_g + \alpha_t + \alpha_3 treatment_{gt} + \alpha_4 group_g * oneyearprepolicy_t + \epsilon_{gt}$$

$$(3)\ ln(Y_{gt}) = \alpha_0 + \alpha_1 group_g + \alpha_2 month_t + \alpha_3 treatment_{gt} + \alpha_4 group_g * oneyearprepolicy_t + \alpha_5 group_g * twoyearsprepolicy_t + \epsilon_{gt}$$

In these regressions, coefficients α_4 and α_5 show if there is statistically significant divergence or convergence between the two groups one year or two years before the policy is applied, respectively. If there is not a significant effect, the regression satisfies the identifying assumption for a difference-in-differences design.

Difference-in-differences designs like this do not need to take explicitly into account a large number of control variables. This model uses the control group as a counterfactual which captures the trend that would occur in the absence of the policy change. Thus, factors like the national crime rate, local crime rate, economic conditions, and demographics of offenders should equally affect observations in both the control and treatment groups, as they have been shown to do in the years leading up the policy change. The design does not need to include these other control variables since the control group itself captures the effect of all these variables.

Data

This paper uses the Uniform Crime Reports' (UCR) agency-level data on age, sex, and race. Wyoming's data are available throughout the time period studied, making its analysis the most reliable in this regard. Data are missing, however, for Wisconsin after December 1997, confining the post-treatment window of analysis. Thus, the lack of data poses a limitation on the effectiveness of this approach.

It should not make a difference, however, that this data only includes crimes that go reported. This would be problematic only if there was reason to believe that crime is understated to different degrees in the treatment and control groups. If both groups underreport crime at the same rate, the data can still serve as an effective proxy for analyzing the fluctuation in crime (Lindo & Schaller, 2014). There does not appear to be anything inherent to the data that would indicate that misreporting is different between the control and treatment groups.

The population data used for the per capita statistics in the analysis are from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program. Because these data are yearly and the UCR data are monthly, I linearly interpolate the population data at the monthly level.

SECTION III

RESULTS

Wisconsin

The results for the graph comparing 17-year-olds in Wisconsin to their 17-year-old counterparts in Nebraska are shown in Figure 1. This graph indicates that crime rates among these cohorts experienced similar fluctuation leading up to the policy change, indicating that Nebraska 17-year-olds are an appropriate comparison group for the Wisconsin 17-year-olds who transfer jurisdictions. The reference line at January of 1996 designates the time at which Wisconsin implemented the policy of trying 17-year-olds in the adult system. At this point, the Wisconsinites fully enter adult jurisdiction. Nebraska 17-year-olds are under juvenile jurisdiction for the entire time period studied (Torbet et al., 1996).

In analyzing the graph, two things appear. First, it seems that this design is effective since there is strong synchronization between the two groups leading up to the policy change. Second, it appears that the policy did not have an effect since the synchronization carries through the policy change into the post-treatment window of analysis.

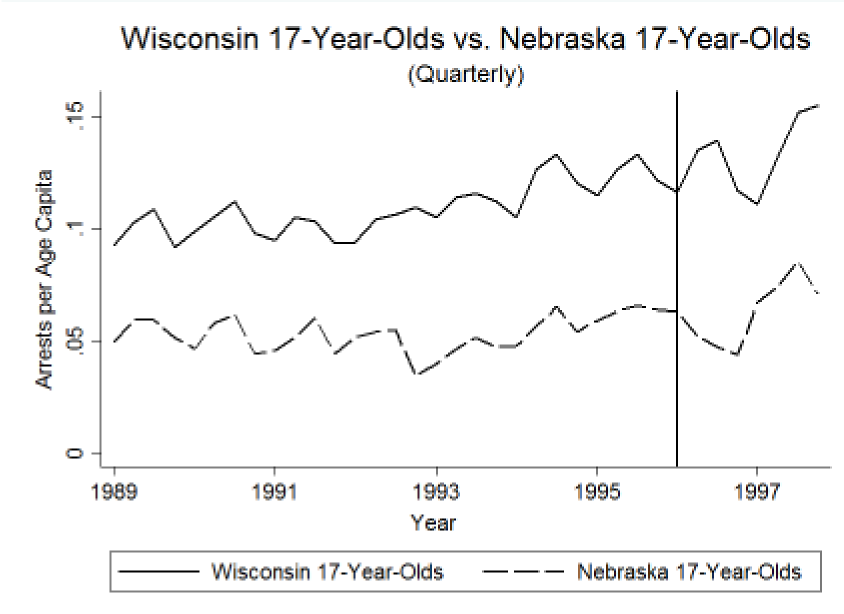

Figure 1: This graph shows the fluctuation in crime rates among 17-year-olds in Wisconsin and Nebraska. At the policy change, marked by the vertical reference line, 17-year-olds in Wisconsin fully enter the adult judicial system while their Nebraskan counterparts are still under juvenile jurisdiction.

The regression results are shown below in Table 1. The results in Column (1) are for the regression containing only those variables delineated in Equation (1). This equation includes only the group designator, the time fixed effects, and the treatment designator. In this regression, *Age17*Wisconsin*PostPolicyChange*, the treatment variable, is not statistically significant. This casts doubt on the idea that trying juveniles in the adult system causes a decrease in crime.

Column (2) shows estimates based on Equation (2), adding an interaction term designating whether an observation is both in the treatment group and in the year prior to the policy change. This variable indicates whether there is convergence or divergence between the two groups in the year preceding the policy change. Since *Age17*Wisconsin*1YearPrePolicyChange* is not statistically significant, Nebraska 17-year-olds are a valid control group. Column (2) also highlights that *Age17*Wisconsin*PostPolicyChange* is again statistically insignificant.

Column (3) corresponds to Equation (3), adding a similar interaction term designating whether an observation is both in the treatment group and two years prior to the policy change. The statistical insignificance of *Age17*Wisconsin*2YearPrePolicyChange* confirms this design's validity. In fact, in comparing it to the other designs involving Wisconsin or the other two states, this design is probably the most effective because of the almost perfect synchronization leading up to the policy change.

The regression results, then, support the same conclusion from the graphical analysis. In all three iterations of the regression, the variable of interest, *Age17*Wisconsin*PostPolicyChange,* is not statistically significant. Hence, if there is an effect of the policy in the two years following its implementation, it is too small to be observed with these statistical methods. Columns (2) and (3) support the effectiveness of this model as there is not a statistically significant effect of being in the treatment group in the years leading up to the policy change.

Table 1: Wisconsin vs. Nebraska Analysis[6]

Variable	(1)	(2)	(3)
*Age17*Wisconsin*PostPolicyChange*	.0435 (.0781)	.0384 (.0794)	.0473 (.0811)
*Age17*Wisconsin*1YearPrePolicyChange*		-.0387 (.0948)	-.0299 (.0963)
*Age17*Wisconsin*2YearPrePolicyChange*			.0585 (.0945)

[6] Each cell shows the value of the coefficient listed and the standard error in parentheses. This robust regression consists of 357 monthly observations.

Figure 2 shows a similar comparison of Wisconsin 17-year-olds to New York 17-year-olds. Again, this graph shows similar fluctuations in crime rates leading up to the policy change. The dip in crime among New York 17-year-olds does seem to disrupt this pre-policy consistency, but the fluctuation in these groups is similar for a full three years leading up to the change.

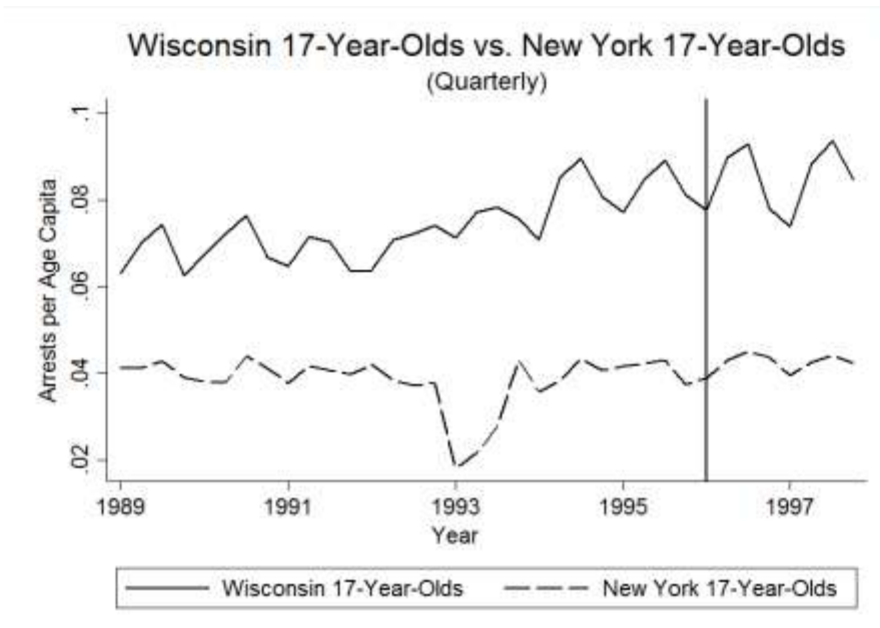

Figure 2: This graph shows the fluctuation in crime rates among 17-year-olds in Wisconsin and New York. At the policy change, marked by the vertical reference line, 17-year-olds in Wisconsin fully enter the adult judicial system while their New Yorker counterparts do not.

The regression results for this design are shown below in Table 2. In Column (1), the effect of the policy is not statistically significant. In Column (2), the policy's effect is still insignificant and *Age17*Wisconsin*1YearPrePolicyChange* shows that the identifying assumption of pre-policy consistency is met. Column (3) reiterates the failure to observe an effect of the policy as well as the confirmation of the identifying assumption.

Table 2: Wisconsin vs. New York Analysis[7]

Variable	(1)	(2)	(3)
*Age17*Wisconsin*PostPolicyChange*	.0299 (.0318)	.0364 (.0355)	.0481 (.0404)
*Age17*Wisconsin*1YearPrePolicyChange*		.0497 (.0383)	.0614 (.0430)
*Age17*Wisconsin*2YearPrePolicyChange*			.0765 (.0472)

Figure 3 shows results for yet a third difference-in-differences design studying the effectiveness of Wisconsin's change in the age of criminal majority. This design compares the treatment group to the control group of Iowan 17-year-olds. Figure 3 shows that, while the general trends in these two groups are roughly similar, Wisconsin seems to vary more.

[7] Each cell shows the value of the coefficient listed and the standard error in parentheses. This robust regression consists of 236 monthly observations.

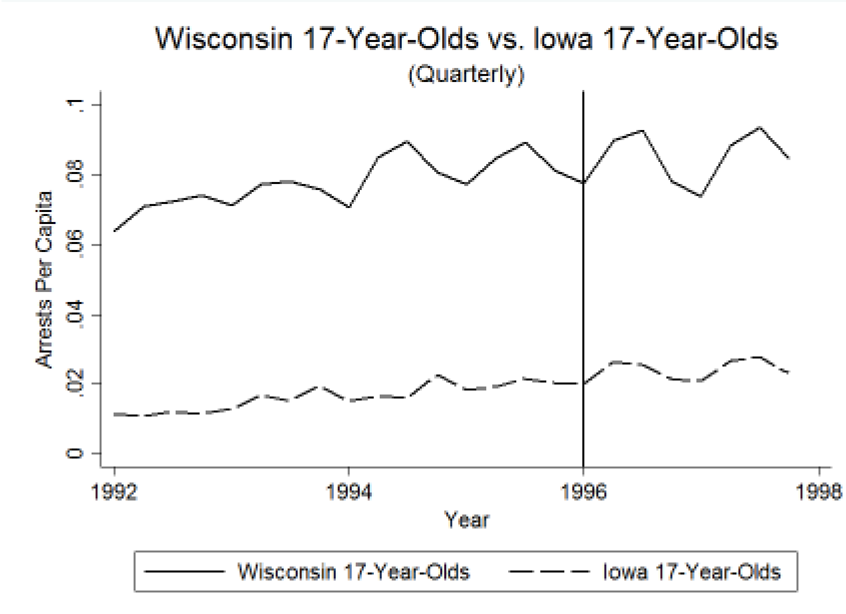

Figure 3: This graph shows the fluctuation in crime rates among 17-year-olds in Wisconsin and Iowa. At the policy change, marked by the vertical reference line, 17-year-olds in Wisconsin fully enter the adult judicial system while their Iowan counterparts do not.

Table 3 below shows the results for the design comparing Wisconsin and Iowa. Column (1) shows a statistically significant effect of the policy. Column (2) also shows a significant effect of *Age17*Wisconsin*PostPolicyChange*. However, Column (2) also shows that the identifying assumption is violated since there is an effect in the year prior to the policy change. Column (3) shows that the identifying assumption holds for two years prior to the policy change, but the effect for a year prior to the change is still present.

Table 3: Wisconsin vs. Iowa Analysis[8]

Variable	(1)	(2)	(3)
*Age17*Wisconsin*PostPolicyChange*	-.327 (.0416)	-.383 (.0486)	-.428 (.0513)
*Age17*Wisconsin*1YearPrePolicyChange*		-.225 (.0511)	-.270 (.0537)
*Age17*Wisconsin*2YearPrePolicyChange*			-.134 (.100)

The weight of the evidence from the Wisconsin results shows that there is not an observable impact of these policies on the crime rate. While results comparing Wisconsin to Iowa do point at the possibility of the adult system causing a decrease in crime, the identifying assumption does not hold. This is because there are not consistent trends between the two groups in the years before the policy change. Greater weight should be given to the Nebraska and New York designs, which cast doubt on this policy's effectiveness.

Wyoming

Figure 4 shows the comparison between Wyoming 18-year-olds, who move to the adult system at the vertical reference line, and the group consisting of their 17- and 16-year-old counterparts. Because of complicating "noise" in the quarterly fluctuation, Figure 4 presents fluctuations on the yearly level rather than the quarterly level.

[8] Each cell shows the value of the coefficient listed and the standard error in parentheses. This robust regression consists of 156 monthly observations.

The graph indicates that this control group is a good counterfactual for the treatment group since the trends in outcome are similar in the years leading up to the policy change. Furthermore, it appears from the graph that if there is an effect of the policy, it is relatively small.

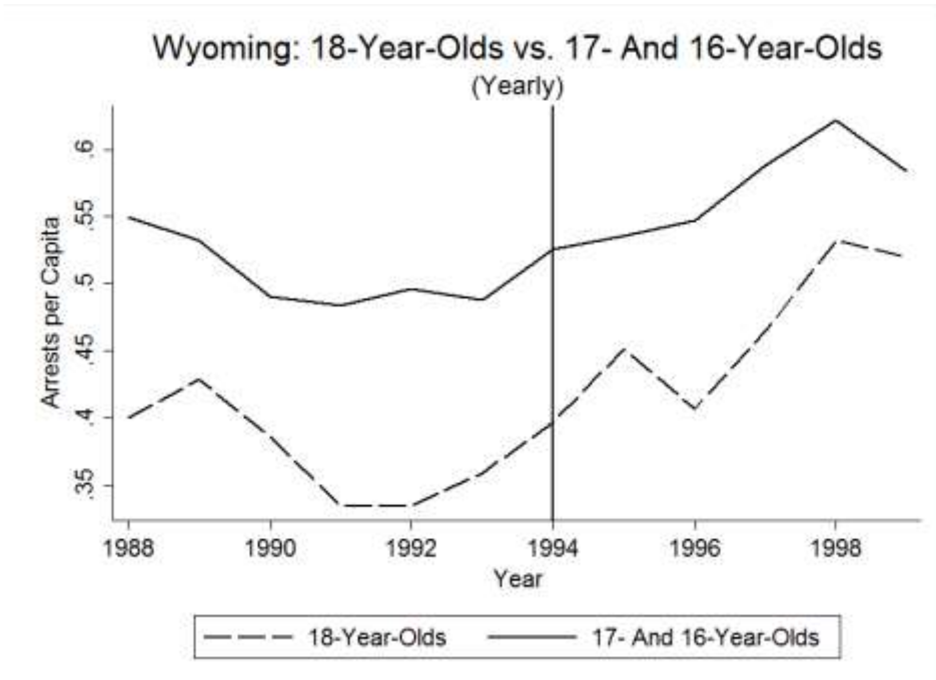

Figure 4: This graph shows the yearly fluctuation in crime rates among 18-year-olds and the combined group of 17- and 16-year-olds in Wyoming. At the policy change, marked by the vertical reference line, 18-year-olds fully enter the adult judicial system. 17- and 16-year-olds are subject to juvenile jurisdiction throughout this time period.

Table 4 below shows the regression results for this design. Column (1) includes those variables delineated in Equation (1). This equation includes only the group designator, the time fixed effects, and the treatment designator. Column (1) shows a statistically insignificant effect of the policy. Column (2) adds the interaction of the variable for the treatment group and the variable designating that an observation is a year prior to the policy change. Column (2) shows that the policy continues to be statistically insignificant. Furthermore, *Age18*1YearPrePolicyChange* shows that the identifying assumption holds. Column (3) adds a similar variable for two years prior to the policy change. In this column, the statistical insignificance of the treatment variable continues to hold. The identifying assumptions continue to hold as well.

Column (4) takes a look at the dynamics in the years after the policy change. *Age18*2Year-PostPolicyChange* takes a value of 1 if the observation is in the treatment group and in the window two years immediately after the implementation of the policy. *Age18*2YearPlus-PostPolicyChange* takes a value of 1 for the remaining observations in the treatment group after the policy change. Column (4) removes the *treatment* variable from Equation (1) and adds these two variables in its place.

Column (4) shows that the statistical insignificance of the policy holds in both the short- and long-term after the policy change. This casts doubt on the idea that juveniles may adjust their behavior to heightened sanctions after time. It also casts doubt on the opposite idea that juveniles may first adjust their behavior before resuming pre-policy behavior. Instead, the results show that these policies do not affect juvenile criminal behavior at any time after the implementation of the policy.

Table 4: Wyoming 18-Year-Olds vs. Wyoming 17- And 16-Year-Olds[9]

Variable	(1)	(2)	(3)	(4)
*Age18*PostPolicyChange*	.0437 (.0615)	.0418 (.0655)	.0138 (.0720)	N/A
*Age18*1YearPrePolicyChange*		-.0114 (.130)	-.0393 (.134)	-.0393 (.134)
*Age18*2YearPrePolicyChange*			-.134 (.121)	-.140 (.121)
*Age18*2YearPostPolicyChange*				.0218 (.0979)
*Age18*2YearPlusPostPolicyChange*				.0102 (.0748)

[9] Each cell shows the value of the coefficient listed and the standard error in parentheses. This robust regression consists of 307 monthly observations.

SECTION IV

CONCLUSION

The weight of the evidence supports the position that trying juveniles in the adult judicial system does not cause a decrease in crime. If these policies do have an effect, it is too small to be perceived with these methods. In three of the four difference-in-differences designs, the policy fails to have a statistically significant effect and the identifying assumption of similar pre-policy trends is met. In the comparison of Wisconsin and Iowa, the only application of the design that shows a significant effect of the policy, the trends before the policy change differ between the control and treatment groups. This means that the observed difference between the two groups after the policy change is very likely the result of factors acting upon crime rates before the policy change.

The results for Wyoming also show that this ineffectiveness holds in both the short-term and the long-term. There does not appear to be a lag before finally observing juvenile responsiveness nor a brief decrease in crime followed by a resumption of previous behavior.

This conclusion provides valuable information on whether lowering the age of criminal majority meets the intended goal of causing a decrease in crime. However, it is unable to specify why these policies fail to deter crime. It could be the case that juveniles are simply unaware of the policy changes. It could also be the case that, while they are knowledgeable of the policy changes, they fail to respond to the heightened “cost of crime” in a way that we would expect.

Additionally, it is important to note that lowering the age of criminal majority is a broad, wide-sweeping approach since it applies to all crimes. It could be the case that heightened sanctions for certain crimes are met with more juvenile responsiveness than other crimes. These questions require further research.

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