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Walk Like a Man: Do Juvenile Offenders Respond to Being Tried as Adults?*

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Abstract

Juvenile crime continues to be a challenging problem in the United States, with a juvenile murder rate that is nearly five times higher than in other developed countries. Due to the large number of violent crimes, many states have adopted laws that allow youth under age 18 to be prosecuted, tried, and sentenced as adults for particular violent crimes. In this paper we consider whether such laws are effective in deterring juvenile crime utilizing Measure 11 in Oregon, a public referendum that imposed mandatory minimum punishments for violent crimes for all offenders over the age of 15. We test whether adult prosecution of juveniles deters crime using the dates of birth and offense from administrative records on all juvenile crimes committed from 1998-2010 in Oregon. We find some evidence that harsher punishments deter crime and that the decreases in crime are concentrated in assaults and robberies, with no deterrence effect for sexual crimes.

ToDo:
- figures at +/-100
- re-estimate table 2 and 3 specifications across bandwidths 50(10)1000, plot point estimate and CI across bw for figures
- placebo cutoffs across 14(.5)17, ECL plot
- writeup: robustness of point estimates across bw (fingers crossed)

JEL Codes: K4, D8
Key Words: Deterrence, Juvenile Crime, Law and Economics, Regression Discontinuity

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1 Introduction

Juvenile crime continues to be a problem both for the United States and almost all other countries. In the U.S., arrest rates are higher among those aged 15 to 17 than in any other age range. Indeed, although those aged 15 to 19 account for only seven percent of the population, they represented 20 percent of the arrests for violent crime (Levitt and Lochner, 2011). In terms of treatment, juvenile offenders are typically separated from adult offenders—often formally through juvenile divisions of departments of correction—where judicial treatment, education, and job-training services are catered toward providing young offenders with the opportunity to learn personal responsibility and develop the skills and behaviors they need to make positive choices for themselves. In some instances, however, crimes are sufficiently severe that the juvenile offenders are treated as adults. In fact, 45 states currently allow judges the discretion to treat juvenile offenders as adults in court, while an increasing number exclude certain serious or violent crimes from juvenile court, subject to the offender meeting a minimum age requirement.[1] In this paper we estimate the effect of GRW: can we drop? ***introducing*** adult-classifications on juvenile crime.

In 1994, Oregon passed a citizens' initiative, known as Measure 11, to establish mandatory-minimum sentences for each of 16 violent crimes. The measure applies to all defendants aged 15 and older and requires that those charged with any of these 16 crimes be tried as adults.

[1]Regarding the judges discretion, note that almost every state has statutory judicial waiver provisions which grant judges the authority to transfer juvenile offenders out of the juvenile system. In some states, the decision to transfer is solely at the judge's discretion. In others, there is a presumption in favor of transfer, subject to being rebutted by the child's attorney. In still others, transfer is mandatory once the judge determines that certain criteria have been met.

Juveniles charged with a Measure 11 offense await trial in adult jails, are tried in adult courts, and face adult mandatory minimums if convicted. Likewise, a conviction in adult court remains on the youth's criminal record even after reaching the age of majority. While the combination of these potential deterrents prevents us from identifying which of these may be most effective in deterring criminal behavior among youth, their combination well represents the differences between juvenile and adult court systems. As such, our results should be viewed as testing whether or not the potential for prosecution under the adult court system deters juvenile crime.

While other states have similar laws, the age-15 cutoff we exploit is particular useful as it does not coincide with other major changes that could influence criminality. Using the discontinuity at age 18 in Florida, Lee and McCrary (2009) find a two-percent decline in the log-odds of offending at the age of 18, which suggests that longer punishments may not have large deterrent effects on crime. However, several complications arise in considering changes in crime rates around age 18. For example, curfews and graduated drivers' licenses—or laws generally intended to prevent juvenile crime—do not apply to adults and thus change discontinuously at age 18. Students are also able to legally drop out of school without parental consent at age 18 in Florida, which also coincides more broadly with potential limits on a parent's or guardian's ability to encourage positive behavior in young adults. Importantly, while one might expect increases in punishment severity at age 18 to have a deterrent effect on crime, other confounding factors may well contribute to increased criminality. Either way, the estimated deterrent effect at age 18 may not uniquely identify the effect of punishment severity on criminal behavior. As an alternative, then, at a policy-induced discontinuity where little else changing discretely we estimate the deterrent effect of adult punishment and the associated increases in punishment severity on juvenile crime.

As only particular crimes are subject to mandatory-minimum punishments, we can also use crimes not newly subject to Measure 11 to evidence that our estimates of the deterrent effect are not picking up other factors that may still shift discretely at age 15.

The remainder of the paper proceeds as follows. In Section 2, we provide background on previous research regarding deterrence and discuss Measure 11 in more detail. In Section 3, we discuss the data and econometric models we employ. In Section 4, we report the main findings while delivering concluding remarks in Section 5.

2 Background

Oregon's Measure 11 came into effect through public referendum in 1994. Measure 11's intent was to establish mandatory-minimum punishments so that egregious offenders would face certain punishment that were also on average greater in length, as shown in Table 1. Whether this actually happened in practice is an open question, primarily because mandatory-minimum punishments shift power from judges to lawyers, who may have been more inclined to offer plea bargains for lower offenses that would earlier have not been pleaded—the mandatory minimums change the price of pleading, essentially. In addition to increasing the length of punishment adults received for all applicable offenses, Measure 11 also set guidelines***GRW: just guidelines? We should use "rules" if we can.*** whereby offenders are to be prosecuted as adults. Namely, juveniles 15 and over charged with these crimes are prosecuted through the adult judicial system rather than through juvenile courts.

Previous research on deterrence has produced mixed evidence concerning whether offenders, particularly juveniles, are deterred by harsher punishments. While there are several studies that use the endogenous transferring of juveniles to adult court to identify the effect of treatment, we are not inclined to interpret estimates from such approaches as causal, given that endogeneity. Previous studies that have used difference-in-difference methods to study state-wide changes in punishments have found deterrence elasticities that range from -.73 (Drago, 2009), to -.07 (Helland and Tabborak, 2007). Using the increase in punishments Florida juveniles face upon reaching 18 years of age, Lee and McCrary (2009) find evidence that deterrence elasticities are no larger than -.13 for juvenile offenders, while Levitt (1998) finds juvenile deterrence elasticities as large as -.38. Regression-discontinuity approaches exploiting the change in treatment at age 18 can produce unbiased estimates if no other factors shift at age 18. However, if other factors (e.g., curfews, graduated driver's licenses, and dropout rates) also change discretely with age, then using the age-18 cutoff may underestimate the true deterrent effect of the adult criminal system.[2]

3 Data and Econometric Models

In a regression-discontinuity (RD) design, we consider the effect of minimum sentencing on arrests (officially labeled "referrals" in the Oregon Youth Authority system), using the

[2]See Anderson (2012).

Measure-11 classification at age 15 as an exogenous source of variation. This approach offers several advantages. First and foremost, other factors (e.g., the ability to drive, curfews, dropout laws) do not likewise change at age 15 in Oregon. Second, the Measure-11 classification should only affect a subset of crimes—those that are particularly violent crimes under the Measure-11 statute—which yields other crimes as natural placebo tests to confirm that our results are not being driven by other unobservables or any discontinuities that might still occur at age 15.

Using administrative, longitudinal data on juvenile referrals to the Oregon Youth Authority from 1998 to 2010 (n = 1,088,225), we exploit this discontinuous increase in the punitiveness of criminal sanctions at age 15 to estimate the deterrent effect associated with adult treatment of juvenile offenders.[3] The administrative data include the date of birth of the offender, the date of the referral, the original crime listed at the time of the referral, demographics of the offender, and details regarding the final disposition of the referral. The exact date of birth of the offender and day of the offense provide the critical elements needed to determine if criminal behaviors shift discretely at age 15, while the original crime listed at the referral provides the necessary information to determine if the original crime is among those with penalties and judicial treatment being influenced by the introduction of Measure 11.

In order to estimate if crime rates shift at the age-15 threshold, we aggregate the number

[3]In Oregon, juvenile offenders receive "referrals" (i.e., they are arrested) and adult offenders receive "adjudications" (i.e., they are charged with a crime). As such, the Oregon Youth Authority is the equivalent of the Department of Corrections for juvenile offenders in Oregon, where a referral to the Oregon Youth Authority is identical to an arrest for an adult offender.

of crimes committed into bins—initially, we consider bins of 14 days—capturing the number of days from age 15. This approach is common, having been used to consider such outcomes as shifting mortality rates across the age-21 cutoff when alcohol becomes available (Carpenter and Dobkin, 2009) or across the age-65 cutoff when universal health is accessible (Dobkin *et. al*, 2009). We then estimate regressions based on Equation (1),

$$Y_i = \alpha_0 + \alpha_1 1(A_i \geq 15) + f(A_i - 15) + \epsilon_i, \tag{1}$$

where i indexes a bin, Y_i is the natural logarithm of offenses in bin i, A_i is the age bin i such that observations with $A_i \geq 15$ are treated as adults and subject to mandatory-minimum penalties. $f(\cdot)$ represents a second-order polynomial with slope allowed to vary over the age-15 threshold. We thereby allow for the non-linear curvature which results naturally in the age-crime curve for juvenile offenders, demonstrated using all potential crimes in Figure 1. In (1), ϵ_i is a random error. As usual, this model is motivated as measuring the local average treatment effect by estimating the difference in the conditional expectations of Y_i as we approach the treatment threshold from either side.

The identifying assumptions supporting a regression-discontinuity design are discussed in Thistlethwaite and Campbell (1960) and are laid out in more detail in Hahn, Todd, and Van der Klaauw (2003). However, one limitation associated with aggregating to bins is that some of the specification tests common in regression-discontinuity designs are not possible. For example, we cannot test for the smoothness of the density of the running variable, or the stability of covariates across the threshold. That said, we can test for the stability of crimes—those that shouldn't be affected by the law, such as running away from home, staying out passed curfew, illegally possessing cigarettes—at age 15 as a test to confirm that other behaviors which should not be affected by the harsher punishments are not also shifting at age 15. To the extent there are identifiable shifts in other crimes at the age-15 cutoff we should resist concluding that we have retrieved an unbiased estimate of the causal parameter of interest among treated crimes.

4 Results

Initially, we estimate linear regression models with the natural log of offenses as the left-hand-side variable. As such, the point estimates can be considered semi-elasticities, capturing the percentage drop in offenses for ages greater than 15. As the data have been aggregated to offense counts within bins, we also specify Poisson regression models. Because the Poisson regressions assume $E(Y|X) = \exp(x'\beta)$, the estimated coefficients in the Poisson specifications also are interpreted as semi-elasticities. In order to explore the sensitivity of the results, we estimate the models across three different bandwidths: within one year of age 15, within two years, and within three years. The estimated standard errors allow for heteroskedasticity and the Poisson models use sandwich standard errors to allow the conditional variance to differ from the conditional mean.

In Table 2 (also represented graphically in Figure 2) we report the estimates for all offenses, and separately for Measure 11 and non-Measure 11 offenses. Across all three bandwidths, we find a small downward shift in all offenses of one to two percent, although we cannot reject the null hypothesis that the true values of the point estimates are zero. When restricting the offenses to Measure-11 offenses, we find the estimated decrease is sizably larger, ranging between nine and 12 percent. While at smaller bandwidths the estimates are not statistically different from zero, the point estimates are essentially stable across the three different bandwidths. For the offenses not implicated in Measure-11, the estimates are again small, stable across bandwidths, and not distinguishable from zero. For all of the models, log-linear regressions and Poisson-regression models produce nearly identical estimates. Overall, we interpret these results as consistent with the hypothesis that punishment in the adult court system deters violent crimes for juvenile offenders.

In Table 3, we separately report estimates for assaults, robberies, and sexual assaults, respectively. The main specifications are also represented graphically in Figure 3. In large part, we consistently find little evidence of decreases or increases in non-measure 11 violent crimes (i.e., lesser felony or misdemeanor charges for similar crimes). However, we do find some evidence that Measure-11 assaults and robberies decrease substantially. While the size and significance is somewhat sensitive to the bandwidth, the point estimates suggest the Measure-11 assaults decrease by 13 to 33 percent. Similarly, point estimates suggest that Measure-11 robberies decline 23 to 68 percent when individuals turn 15. However, Measure-11 sexual assaults exhibit little signs of a decrease at 15, with point estimates ranging from a four-percent increase to a four-percent decrease. Once again, the results are nearly identical across log-linear and Poisson regression models.

In Table 4, graphically represented in Figure 4, we test if plausible placebo offenses such as runaway, curfew violations, and illegal possession of tobacco shift at age 15. We find little evidence of any change in these offenses which we would not expect to be directly affected by the Measure-11 statute. This supports that other factors or behaviors in teens are not shifting simultaneously and introduce bias in our main results.

5 Conclusion

High rates of juvenile crimes remain a major challenge, despite the large decreases in crime since the 1990's. During that time, many states have adopted statutes introducing mandatory-minimum punishments, which often include provisions allowing youth to be charged as adult offenders. In this paper, we investigate whether harsher adult punishments deter violent juvenile crime. We find some evidence that some types of the most violent youth crimes—assaults and robberies—decrease at age 15. However other violent crimes such as sexual assaults remain unchanged despite the large increase in punishments facing potential offenders.

One explanation for the differences in the estimated effects across the different crimes is the ease in distinguishing between a Measure-11 assault/robbery vs. a non-Measure-11 assault/robbery. The difference between these violent crimes boils down to the use of a gun, knife, or another object that can inflict fatal injury in the commission of the crime. Indeed, that assault and robbery rates are not falling suggests that youth may continue to commit such crimes, but respond to the age 15 cutoff by foregoing the use of a weapon in the commission of those crimes.[4] This interpretation is supported by recent research by Abrams (2012), that finds sentencing enhancements for committing crimes with handguns reduces robberies.***GRW: This doesn't support that we would not see assault/robbery fall, with the age 15 action essentially coming only along the weapon margin. *** With that in mind, Measure-11 punishments for youth amount, essentially, to a sizable sentence enhancement. For sexual assaults, no such clear delineations exist for the behaviors which results in Measure-11 sexual assaults and lesser sexual assaults, perhaps preventing the sentence enhancement from deterring more-severe sexual assaults.

While we find evidence that harsher punishments may reduce certain violent crimes in juvenile offenders, this does not necessarily justify their imposition. Indeed, Aizer and Doyle (2013) find that incarcerating youth can lead to substantially worse long-run outcomes, which in turn could increase the overall crime rate due the negative criminogenic effects associated with longer incarceration periods and eventual incarceration in adult facilities. With that in mind, non-cognitive behavioral interventions such as the recent pilot experiment by Heller, Pollack, Ander, and Ludwig (2013) would likely be preferable, both at reducing youth crime and improving future labor-market success through enhanced non-cognitive skills.

[4] Another potential explanation is that arresting officers may be exercising discretion in initially charging or reporting a youth offense as a Measure-11 offense for those barely over 15. We intend to investigate this possibility using officer fixed effects and characteristics in the future.***

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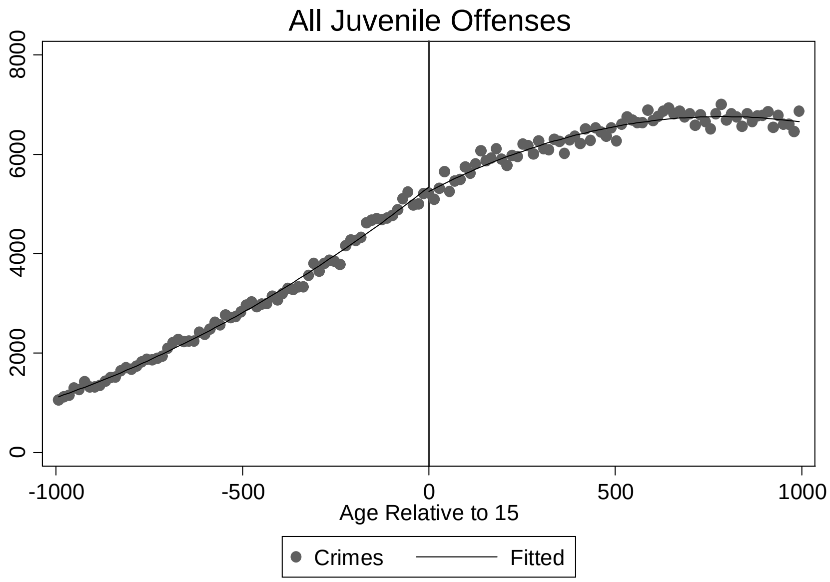
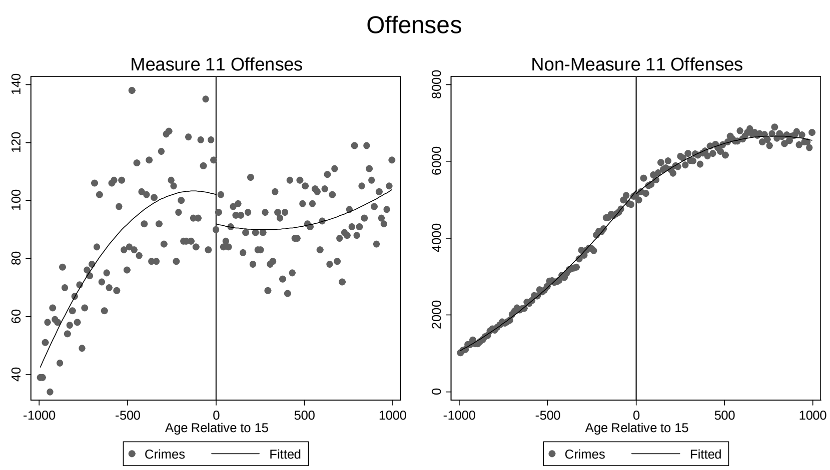
6 Tables and Figures

Figure 1
Discontinuity in all offenses at age 15



Note: GRW

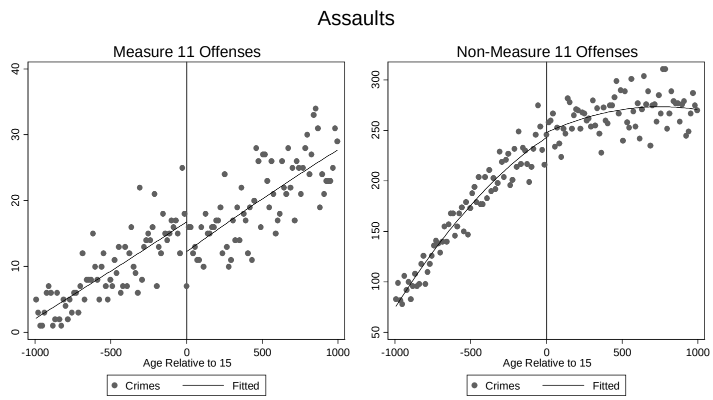
Figure 2
Discontinuity at age 15, by Measure-11 status



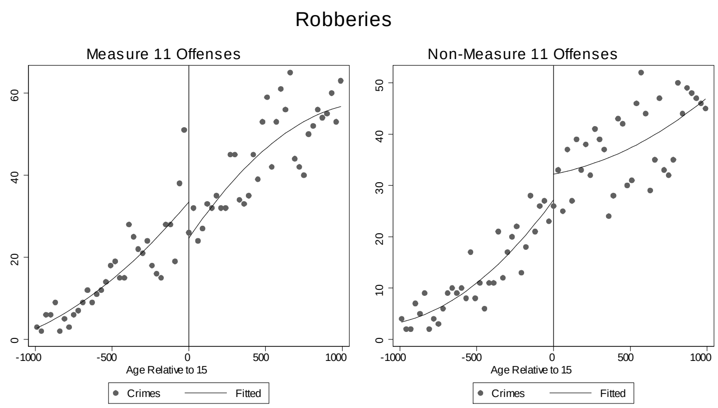
Note:

Figure 3
Discontinuities in individual Measure 11 offenses at age 15

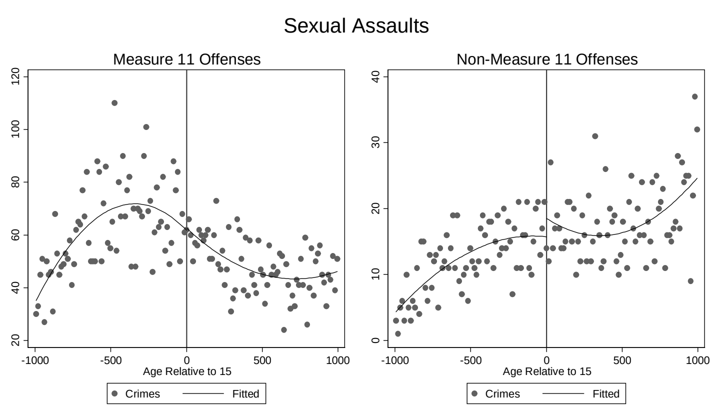
Panel A: Assaults



Panel B: Robbery

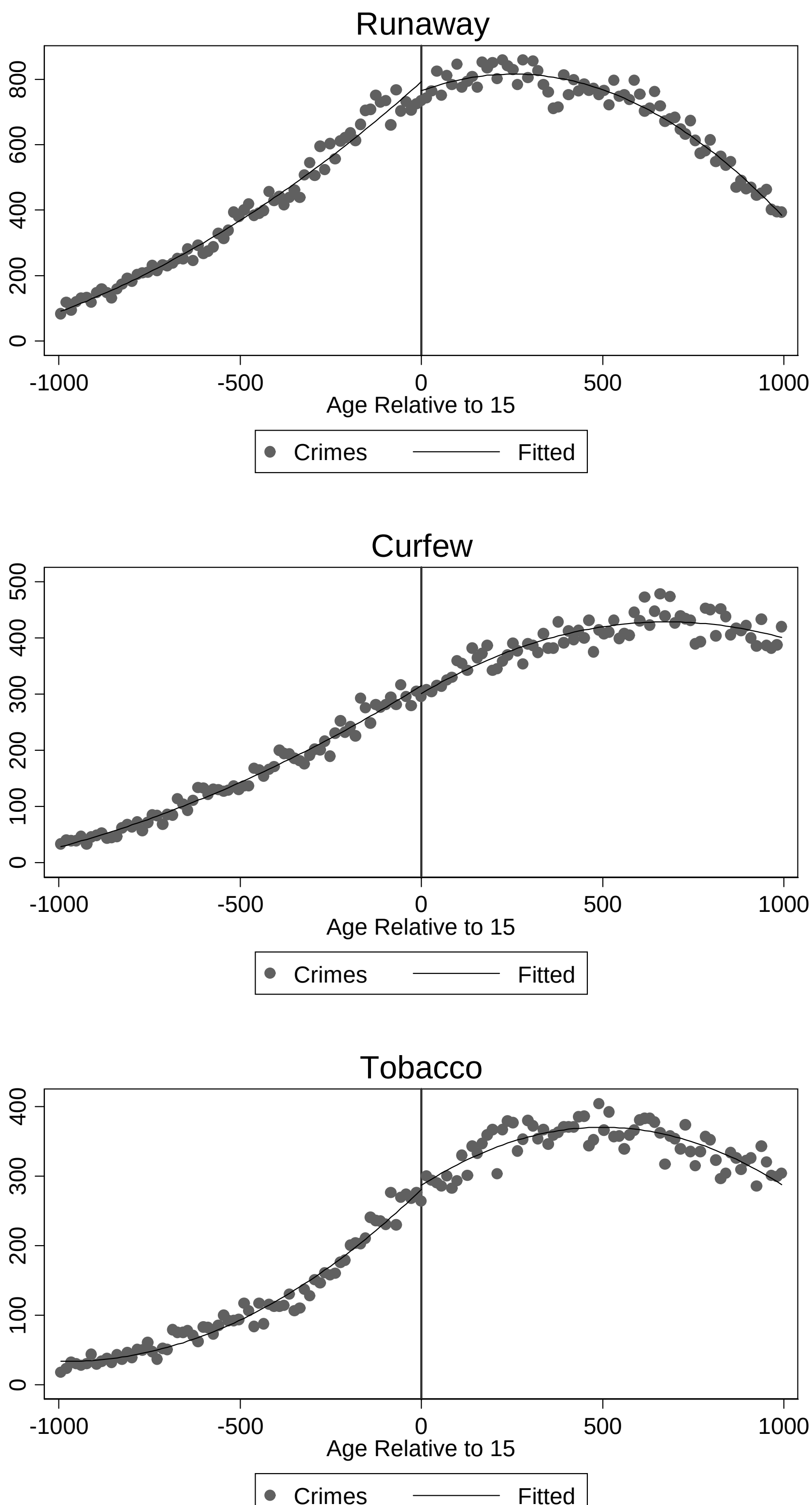


Panel C: Sexual assaults



Note:

Figure 4
Discontinuities in placebo crimes at age 15



Note:

Table 1
Mandatory-minimum sentences under Oregon's Measure 11

Crime	Minimum sentence length
Murder	25 y, 0 m
1st degree Manslaughter	10 y, 0 m
2nd degree Manslaughter	6 y, 3 m
1st degree Assault	7 y, 6 m
2nd degree Assault	5 y, 10 m
1st degree Kidnapping	7 y, 6 m
2nd degree Kidnapping	5 y, 10 m
1st degree Rape	8 y, 4 m
2nd degree Rape	6 y, 3 m
1st degree Sodomy	8 y, 4 m
2nd degree Sodomy	6 y, 3 m
1st degree Unlawful sexual penetration	8 y, 4 m
2nd degree Unlawful sexual penetration	6 y, 3 m
1st degree Sexual abuse	6 y, 3 m
1st degree Robbery	7 y, 6 m
2nd degree Robbery	5 y, 10 m

Notes:

Table 2
Estimated shifts in offenses at age 15

	All Crime			Measure 11			Non-Measure 11		
Log-Linear Models	-0.022	-0.013	-0.018	-0.086	-0.106	-0.132**	-0.020	-0.011	-0.016
	(0.021)	(0.018)	(0.029)	(0.103)	(0.0768)	(0.068)	(0.021)	(0.018)	(0.029)
Poisson Models	-0.019	-0.011	-0.021	-0.102	-0.124	-0.132**	-0.018	-0.009	-0.019
	(0.020)	(0.017)	(0.024)	(0.096)	(0.076)	(0.068)	(0.020)	(0.017)	(0.024)
Bandwidth (yrs)	1	2	3	1	2	3	1	2	3

Notes: All models utilize a local second order polynomial regression discontinuity. All estimates represent semi-elasticities. Robust standard errors are reported in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3
Estimated shifts in Measure-11 crimes at age 15

Panel A: Assault / Manslaughter / Murder

	All Crime			Measure 11			Non-Measure 11		
Log-Linear Models	0.002	0.020	-0.023	-0.128	-0.187	-0.337**	0.010	0.037	-0.006
	(0.052)	(0.038)	(0.048)	(0.288)	(0.181)	(0.157)	(0.056)	(0.041)	(0.049)
Poisson Models	0.001	0.019	-0.008	-0.112	-0.247*	-0.305**	0.007	0.034	0.009
	(0.049)	(0.037)	(0.040)	(0.250)	(0.165)	(0.139)	(0.053)	(0.040)	(0.041)
Bandwidth (yrs)	1	2	3	1	2	3	1	2	3

Panel B: Robbery

	All Crime			Measure 11			Non-Measure 11		
Log-Linear Models	-0.396**	-0.089	-0.134	-0.680**	-0.235	-0.353**	-0.055	0.068	0.107
	(0.194)	(0.138)	(0.120)	(0.266)	(0.202)	(0.153)	(0.209)	(0.177)	(0.155)
Poisson Models	-0.376**	-0.163	-0.132	-0.682**	-0.411**	-0.368**	-0.005	0.081	0.102
	(0.185)	(0.135)	(0.115)	(0.259)	(0.194)	(0.158)	(0.176)	(0.151)	(0.132)
Bandwidth (yrs)	1	2	3	1	2	3	1	2	3

Panel C: Sexual Assault

	All Crime			Measure 11			Non-Measure 11		
Log-Linear Models	0.038	-0.010	0.011	0.044	-0.020	-0.029	-0.024	0.050	0.015
	(0.106)	(0.082)	(0.076)	(0.109)	(0.084)	(0.078)	(0.247)	(0.158)	(0.138)
Poisson Models	0.024	-0.019	0.010	0.0242	-0.0417	-0.024	0.018	0.048	0.014
	(0.098)	(0.079)	(0.072)	(0.102)	(0.0833)	(0.076)	(0.243)	(0.156)	(0.130)
Bandwidth (yrs)	1	2	3	1	2	3	1	2	3

Notes: All models utilize a local second-order polynomial regression discontinuity. All estimates represent semi-elasticities. Robust standard errors are reported in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4
Estimated shifts in placebo crimes at age 15

	Runaways			Curfew			Tobacco Possession		
Log-Linear Models	0.011	-0.000	-0.015	-0.021	0.019	-0.015	-0.029	-0.041	0.029
	(0.027)	(0.029)	(0.038)	(0.032)	(0.029)	(0.038)	(0.047)	(0.043)	(0.050)
Poisson Models	0.013	0.0083	-0.033	-0.013	0.014	-0.016	-0.028	-0.045	-0.009
	(0.026)	(0.027)	(0.029)	(0.029)	(0.028)	(0.031)	(0.043)	(0.038)	(0.035)
Bandwidth (yrs)	1	2	3	1	2	3	1	2	3

Notes: All models utilize a local second-order polynomial regression discontinuity. All estimates represent semi-elasticities. Robust standard errors are reported in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%