Age-Dependent Sentences and Crime Bunching

Empirical Evidences from Swedish Administrative Data

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Abstract: In the Swedish penal code there is a discontinuity for convicted individuals at age 21: before the 21st birthday, there is a "rebate" on all prison sentences and life time sentences cannot be used. We exploit this age-discontinuity to investigate how individuals respond to incentives. We use a large Swedish data set, including dates for all crimes leading to convictions for cohorts born 1973-1993. We find evidence of "bunching" in the sense that more crimes are committed during the week prior to the birthday, followed by a reduction in crime during the week subsequent to the birthday. We do, however, not find that the crime rate permanently falls when there are harsher punishments.

1. Introduction

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The question how punishment affect crime has preoccupied the public policy debates as well the academic discussion in many different fields for a long time. Following Becker (1968), many economists have been prone to believe that criminals follow rational behavior in the sense that they weigh expected costs and benefit of a crime against each other. This implies that incentives matter and that harsher punishment should reduce the inclination of the general public to commit crime. An alternative view is that the behavior of criminals is determined by emotional, psychological and social factors that are not directly affected by punishment. While this ultimately is an empirical question, research has been held back by lack of high quality data and empirical research methods.

In this paper, we use a research design that exploits a feature in the Swedish legal system whereby punishments increase discontinuously at birthdays for juveniles under the age of 21. In particular, below age 21, age should be considered as a mitigating circumstance when the punishment is decided. Life imprisonment can, for example, not be used, and the convicted person is given a "rebate", in particular if sentenced to prison. These rules create a discontinuity in the sentences around the 21st birthday that we use as identifying variation in punishments to estimate the effect on the probability to commit crimes.

Our research design requires day-to-day individual data on criminal behavior. We use the Swedish National Conviction register linked with the Swedish Census providing demographic information of the included individuals. The Swedish National Conviction register contains detailed individual information on all convictions in the Swedish legal system. That is, we know when each crime in the register were committed and its relation to the convicted individual's 21st birthday. The large size of the data base allows us to obtain large enough sample in the age group of interest to obtain estimates with high precision.

We find a large sorting effect amounting to approximately a 15-percent increase in the crime rate approximately one week prior to this birthday, which is followed by a reduction of about the same size the week after. In sum, the density "jumps" by 30 % close to the threshold. We do, however, not find any long-term reductions in crime following the 21st birthday.

The result is consistent with a model of where individuals bunch their decisions to commit crime and the exact timing of the crimes committed depend on opportunities (for the theory of crime opportunity developed by criminologists, see e.g. Felson and Clark 1998). Avoiding the optimal opportunity to commit a crime may come at a cost, which is increasing in the time distance from the optimal opportunity. If crime opportunities during a certain period are limited, and criminals take incentives into account, then there will be a local displacement effect in time around the 21th birthday such that crimes are front loaded. However, there will not necessarily be a long-term effect since it is too costly, or even impossible, to frontload crime over larger time spans. This model rests on the assumption that individuals commit crimes repeatedly. Interestingly, our results show that it is exactly prolific criminals, and those without any legal income, that generate the results.

The analysis is closely related to the concept of crime displacement. When crime control increases in one area, it may reduce crime there, but increase crime rates elsewhere. Alternatively, law enforcement policies in one area may have positive diffusion effects such that crime is reduced in other areas. A large literature has analyzed this topic and found results in both directions (see e.g. Weisburd et al., 2006 and Braga and Brenda, 2008). In our study, we show that displacement of crime takes place in time rather than space. This result is to our knowledge a new insight in the literature, with the exception of Jacob et al. (2007) who instrument crime with weather and show that weeks with above average crime rates are followed by weeks with below average crime rates.

There are three earlier papers in the literature exploiting age thresholds at criminal majority which vary between the age of 16 and 19 across US states. Levitt (1998) first used annual data and cross-state differences in the harshness of adult sanctions relative to those for juveniles and found a large general deterrent effect of harsher sentences. Lee and McCrary (2016) instead used daily data from Florida around the 18th birthday and found a very small (2 percent) but significant effect around this discontinuity. Hjalmarsson (2009) found that offenders' perception of the changes in punishments at the age of majority was much smaller than the actual changes, and that there was no evidence of deterrence in self-reported data.

The age of criminal majority in the US tend to coincide with other changes potentially related to crime, such as laws on firearms, curfews, graduated drivers' licenses, drop outs from school and gambling. In Florida, for example, at 18, individuals are able to legally drop out of school without parental consent. It is possible that the earlier findings of small or no effects at criminal majority are biased by these cofounders. We contribute by analyzing a threshold, the 21th birthday in Sweden, when nothing else changes discretely.

Another benefit is that our analysis is the first to study the behavior of individuals who are no longer juveniles. We moreover study a country where the prison population per capita is about one tenth of the US prison population, but more similar to many other countries in Western Europe.

There are other studies, which instead analyze general deterrence by examining various law changes. Kessler and Levitt (1999), Kovandzik (2001) and Vollard (2013) all exploited increases in sentence length for prolific offenders. Apart from Kovandzik, they found evidence of deterrence. Raphael and Ludwig (2003) and Abrams (2012) studied the effect of harsher weapon laws where the overall result was mixed.

Another strand of the literature studies the deterrent effect of harsher penalties for those who have already been imprisoned earlier in life. Helland and Tabarrok (2007) examined the "three strikes and you're out" reform in California where those convicted a third time for a serious offence received draconian punishments. They found large (but not cost-efficient) deterrent effects of such penalties (see also Iyengar, 2008). Drago et al. (2009) instead analyzed random amnesties in Italy and also found a general deterrent effect among those who had already been imprisoned.

The outline of the paper is the following. Section 2 describes the structure of the penalty reductions for juveniles in Sweden. Section 3 describes the data and Section 4 the empirical strategy. In Section 5 the results are presented. Section 6 provides a discussion of the results and Section 7 concludes.

2. Juvenile Punishments in the Swedish Judicial System

The Swedish law states that the age of juveniles committing crimes must be taken into account when sentences are decided. In particular, "The age of the juvenile should be particularly considered at the determination of the penalty when the crime is committed before the age of 21" and "Nobody must be sentenced to life time in prison for crimes committed before the 21st birthday" (The Swedish Penal Code, chapter 29, § 7). There is also a sharp distinction at the 18th birthday because juveniles below that age cannot be sentenced to normal prisons. There is moreover an informal practice, which is not part of the law, to give sentence reductions before juvenile birthdays, in particular for crimes leading to prison sentences. The rebate for juveniles is somewhat non-linear and the punishment is approximately 1/5 for individuals who are still 15 years old, ¼ for 16, 1/3 for 17, ½ for 18, ¾ for 19 and ¾ for individuals who have not yet turned 21 (Jareborg and Zila 2007).

If incentives matter, we would therefore expect reductions in crime subsequent those birthdays, but not afterwards. However, 18 is the age of majority when individuals for example are allowed to take driver's license and buy alcohol in pubs, which may affect criminal behavior. At 20, alcohol may be bought in stores and also be consumed outside restaurants. Moreover, being a decennial birthday, we might suspect partying effects in the time period surrounding the birthday. 16 is also a problematic threshold to study since individuals are allowed to practice driving and obtain driver's license for some vehicles. We therefore attempt to isolate the effects at the 21st birthday when there is a large change in punishment (around 25 percent), but we can also analyze the 17th birthday when the jump is approximately 10-percent jump and the 19th birthday when the jump is approximately 15 percent. The birthdays above 21 are used as placebos.

3. Data

Our data set is obtained by matching several different national Swedish registers. The frame for obtaining the sample is consecutive years of the Swedish census. Data on criminality is obtained from the Swedish conviction register provided by Swedish National Council for Crime Prevention (Brå). This register contains data on all convictions in the Swedish judicial system. The information we use is the date when the crime was committed, type of crime and the length of the potential prison sentence.

Given our research design, the exact date of birth and it's relation to the date when the crime was committed is of key importance. The data from the census are not very helpful on this, since it only contains information on month of birth. It is, however, possible to use information from the Swedish birth register to obtain a prediction on the exact date of birth. The information we use in the National birth register is date of last menstruation and gestation length in days. Since gestation lengths were residually determined as the number of days after last menstruation and date of birth until those born in late 1980s, we get a very accurate prediction of the birthday.

The lowest age threshold that can be analyzed with our data is 16, since crimes committed before age 15 were not recorded in the National conviction register and our approach requires data both to the left and to the right of the age threshold under study. Since our data ends in 2010 we need to make sure that we are not technically imposing a jump in the density due to censoring. For example, say we analyze the number of convictions around the 20-year birthday and the window around the threshold is one year. Since our data ends in 2010 we could only use cohorts born 1989 or before. If we would use the cohort born also in 1990, then we would construct a jump in the density of number of convicted mechanically since the last cohort could never have reached the age above the threshold. When pooling cohorts we allow only the cohort that have reached the specific age threshold analyzed plus one year. This means that our sample size decreases when studying thresholds for older ages. We use the cohorts from 1973-1993 when studying the age of 16 threshold, 1973-1992 when studying the age of 17 threshold and so on. Table 1 shows the number of observations that could potentially be used for each threshold analysis.

The maximum sample size we have when analyzing the 16th year threshold, is 909,291. Since we are matching with the birth register we can only use Swedish born convicted. When matching with the Statistic Sweden country of birth 184,561 are born abroad and 76 have unknown country of birth. Thus, we now have as 724,654 observations as documented in Table 2.

When matching this data set with the birth register we lose 60,930 observations which leaves us with 663,724 convictions, i.e., our attrition is approximately 9 percent. However, since we have 76,068 empty cells with respect to date of crime we are down to 587,656 numbers of convictions were we can determine the age at the date of crime. Since the population is the Swedish born verdicts, the final attrition is approximately 19 per cent. Importantly, we have no reason to believe that the attrition is systematically related to exact birth dates.

Finally, a criminal might have committed many crimes. In Sweden, more than one crime might be handled during a court session. However, the most severe of the crime in terms of punishment length is alone determining the harshness of punishment. We use the information of the most severe crime committed only and the according date.

4. Empirical Strategy

We follow the reasoning in Lee and McCrary (2016). We are studying the jump in the density of age at the date of offending, which means that the strategy is different from a standard Regression Discontinuity (RD) design. For the approach in Lee and McCrary (2016) to be causally valid it is not needed that the factors determining criminal behavior to be constant over the lifetime. However, it is assumed that no other criminal determinant, other than the severity of punishments, changes discontinuously at the birthday thresholds. Since this is typically not true, except at some age thresholds, we focus on the age threshold of 21, where no other major factors change discontinuously. Basically, it is assumed that all factors of criminal behavior are evolving continuously as the potential criminal is turning 21 except for the severity punishment, which increases discontinuously at 21. In order to detect discontinuities in the density we need high frequency data. As argued in Lee and McCrary (2016) "all other factors" are likely only constant when examining offense rates in relatively short intervals. Pooling larger intervals (such as annual comparisons across thresholds) many factors affecting criminal activity are changing in ways that could affect underlying criminal propensities. Compared to previous studies, the 21 year threshold in Sweden seems to be better suited for causal analysis, since 21 is simply a reminiscent of old days eligibility of voting.

In order to estimate a jump in the density we use standard regression discontinuity methods, following Lee and Lemieux (2010), where the outcome *j* is the number of verdicts, *i*, that committed a crime at a certain age, measured in days, denoted *Verdict*. For the ease of interpretation (percentage difference) we use the natural log.

Hence, we will estimate a standard regression discontinuity specification as:

$$\log(Verdict_{\mathrm{ij}}) = \alpha + \beta Above_{\mathrm{ij}} + \mathrm{f}(W_j) + \varepsilon_{\mathrm{ij}} \qquad (1)$$

where *Above* is an indicator variable taking the value of one if the verdict is the of the same age or above the age threshold studied and zero otherwise. The parameter of interest is β, which measures the difference in number of verdicts at the threshold in percentages. W is the forcing variable, age at crime measured in days, however normalized to be zero at the threshold, positive above and negative below. In other words, if the age threshold of 21 is studied then $\mathrm{W}_j = \mathrm{Age}_{\mathrm{at}_{\mathrm{crime}}} - 21$.

Equation (1) is estimated using local linear regressions (LLR) as suggested by Hahn et al. (2001) and Porter (2003). As Lee and Lemieux (2010) suggest, we use a rectangular kernel, which is equivalent to estimating a standard linear regression over the interval of the selected bandwidth on both sides of the cut-off point. There are many ways of choosing optimal bandwidth (see for example Imbens and Kalyanaraman 2012, Calonico et al. 2013). Thus, we are agnostic and choose to report many. For our main results of the 21 threshold, we always compute and report the optimal bandwidth from, Calonico et al. 2013. We also include estimation based on higher order of the polynomial function, $\mathrm{f}(W_j)$.

Since the forcing variable is discrete (age in days at crime), we cluster the standard errors at the forcing variable following Card and Lee (2016). Lastly, our smallest bandwidth presented is only 0.02, which means that we compare number of crimes carried out by criminals roughly one week before the birthday with crimes committed one week after. This gives us only 15 clusters, and hence the standard error for the first column should be interpreted cautiously. However, following the logic of a standard regression discontinuity design, the point estimate is still informative and should be unbiased.

5. Results

Consider first the effects on index crimes (murder and non-negligent manslaughter, forcible rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft and arson). Figure 1 shows the crime pattern one year prior to and one year subsequent to the 21th birthday. There is a clear local specific deterrence effect close to the 21th birthday. The week before the 21th birthday, there is a large increase in crime, which is followed by a sharp reduction the week following the birthday.[2]

[2] The figure shows the non-linear relation where we have local polynomial estimation for the interval (20, 22) to the large bandwidth.

Crime then reverts to the original trend. Figure 2, which instead depicts a smaller window with one month on each side of the 21th birthday, shows a strong local deterrence effect.

The regression results reported in Table 3 (without control variables) and Table 4 (with control variables such as birth month and year and previous crime history) show a very large and significant robust negative effect of sorting at the 21th birthday. For example, for the second bandwidth 0.06, where we also can trust the standard errors, the effect is around 30 percent. When using a wider bandwidth, the effects decrease indicating a non-linear effect. It is therefore reassuring that the point estimates are similar when adding second- and third-order polynomials for the larger bandwidths.[3]

We would expect that incentives matter more for planned crimes than other crimes. Index crimes reported in Table 1 in fact tend to be planned. Moreover, as Tables 5 and 6 show, the results are mostly driven by aggravated assaults, burglary and larceny, which are all planned crimes. On the other hand, we do not expect to find any deterrent effects when crimes that are less planned are considered. Traffic-related crimes and drug-related crimes, for example, may be less planned since they may be a result of addiction. Table 7 and 8 show that the results are indeed not driven by these types of crime. Table 9 shows that there is no behavioral effect for non-index crimes, which also tend to be less planned.

Moreover, for crimes which lead to maximum 6 months in prison, individuals are in practice usually fined. For such crimes, the jump in penalties is less clear at 21 than for crimes leading to more than 6 months in prison. We therefore do not expect significant any results. Table 10 confirms this hypothesis.

Table 11 and 12 finally show the effects for men and women separately. The results are entirely driven by men.

[3] We have also run the STATA RD-package for optimal bandwidth and it gives a bandwidth of 0.117 cct. The search was made plus minus 0.9 of the 21st birthday in order not to include the 20th birthday.

As mentioned above, the 17th and the 19th birthday can also be studied since there are no other confounding factors. While we do not find an effect at 17, at 19, when the jump in penalty is larger (15 percent), we do in fact observe a tendency for deterrence as shown in Table 13. However, the power is low and there are no statistically significant effects. It might also be noted that there is an increase in crime subsequent to the 20th birthday, which we attribute the ability to buy alcohol freely and the decennial birthday.

Lastly, in Table 14 we present the result for linear and 2nd order polynomial control function for the thresholds 22 to 31. In general, there is little evidence of any robust and significant sorting, except at 30, where there is a weak evidence of positive sorting which again fits with the tradition of celebrating decennial birthdays. We conclude that this placebo analysis strongly supports the findings at 21.

6. Discussion

What, then, can explain the very large, but short-term, behavioral response to the changed incentives at the 21th birthday? As mentioned above, we believe that a model of prolific offenders taking advantage of the best opportunity to commit crime can help explaining the result. Consider the example of car theft or burglary. In a certain time period, opportunities, or costs of committing crime, vary across days depending on e.g. the day of the week. People are, for example, more at home or on the streets during certain days. The total number of opportunities during a period may be limited because there are only so many cars and houses in an area. Another feature of the model is that committing a crime in the period may come at an increased cost to commit a similar crime later on, either because of a higher attention by car and house owners, or because the opportunity is simply exploited. In this model, the best opportunity to commit a crime in a certain time period will be foregone only if the benefit, in our case due to reduced punishment before 21, dominates the increased cost of displacing the crime in time. However, if this is the case, crime will be displaced and increase before the 21th birthday and consequently be reduced afterwards. But why is the effect so temporary? The cost of changing the crime may be increasing in the time span from the optimal opportunity. Adjusting the date from one day to the previous day may not be so costly, but committing many car thefts or burglaries which should have taken place weeks later according to the optimal opportunities may be very costly.

Note that this model rests on the assumption that criminals commit crimes repeatedly. Interestingly, as Figure 3 shows, it is prolific offenders (those being sentenced more than once) that are responsible for the reallocation of crime in time. Figure 4, in contrast, shows that there is little evidence of reallocation for offenders being sentenced for the first time. These results are confirmed in the regression results reported in Table 15 and Table 16. Prolific offenders may also work little or not at all on the legal market. Figure 5 to 10 shows that the individuals generating our results are precisely those that do not have a legal income or student aid, but instead receive social cash transfers (with no obligations in return). The overall pattern shows that the individuals sorting around the 21st birthday are more likely to be outsiders to the formal society.

7. Conclusions

The results from the previous empirical literature addressing the causality problem between punishments and crime are very mixed. Based on a number of American studies, Durlauf and Nagin (2011a and 2011b), Nagin et al. (2009) and Nagin (2013) have argued that longer prison sentences have no or very small general deterrent effect. The previous studies most similar to ours, Hjalmarsson (2009) and Lee and McCrary (2016) finds no or a very small deterrent effect of punishment.

Using Swedish data and a legal system that generates a sharp increase in the penalties at the 21th birthday we find very large deterrent effect from harsher punishments one week after the 21th birthday. We also find a corresponding increase in crime the week before the 21st birthday. This indicates that individuals take incentives into account and bunch their decision to commit crime within a certain time period. In other words, they reallocate, or displace, crime in time, rather than in space. Our identification strategy is not suitable for evaluating long-term effects. However, if anything can be said, we do not observe such effects. We suggest that an explanation for the large but short-term effects is that it is costly to reallocate crime in time. Our results reveal that it is not worth, or even possible, to frontload crime from a more distant future. An implication is that the reductions in punishments for juveniles which we have analyzed does not have seem to have negative welfare consequences in terms of larger costs of crime.

A topic not discuss here is specific deterrence, i.e., the effect the punishment has on the convicted's inclination to commit new crime. Longer duration of imprisonment may reduce future crime through specific deterrence, but also increase crime since incapacitated may learn from each other. Needless to say, more research, and in particular research on European data, is needed in order to provide policy makers with more evidence.

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Figures

All figures except for Figure 2 are based on data windows where individuals are 20 to 22 years of age. Figure 2 zooms in the major variable of interest for local analysis plus minus 0.1 years around the threshold (roughly 5 weeks on both sides). Zero denotes 21-years old at the date of the crime. The dependent variable is the logarithm of the number of sentenced that committed the crime a certain date. For all figures, except for Figure 2, plotted points are means with a binwidth of 0.2 of a year. The solid line is the predicted values of local polynomial estimator with a fifth degree smoother and a rectangular kernel with a bandwidth of 0.7. For Figure 2 plotted points are means with a binwidth of 0.02 of a year. The solid line is the predicted values of a local linear smoother with a rectangular kernel.

Figure 1. Number of index crimes (log scale) one year before and one year after the 21st birthday

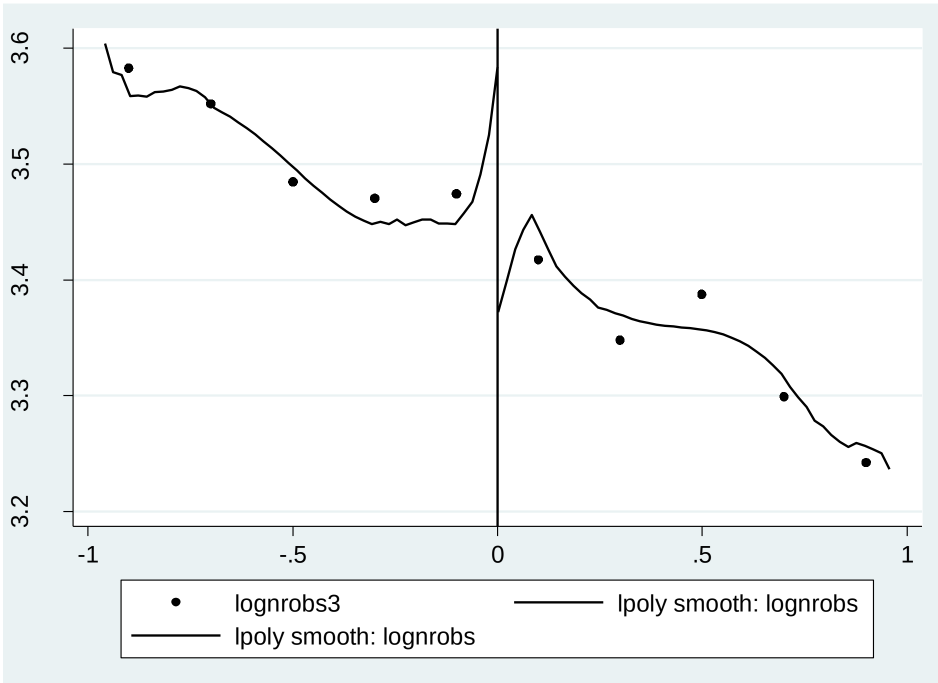


Figure 2. Number of index crimes (log scale) one month before and after the 21st birthday

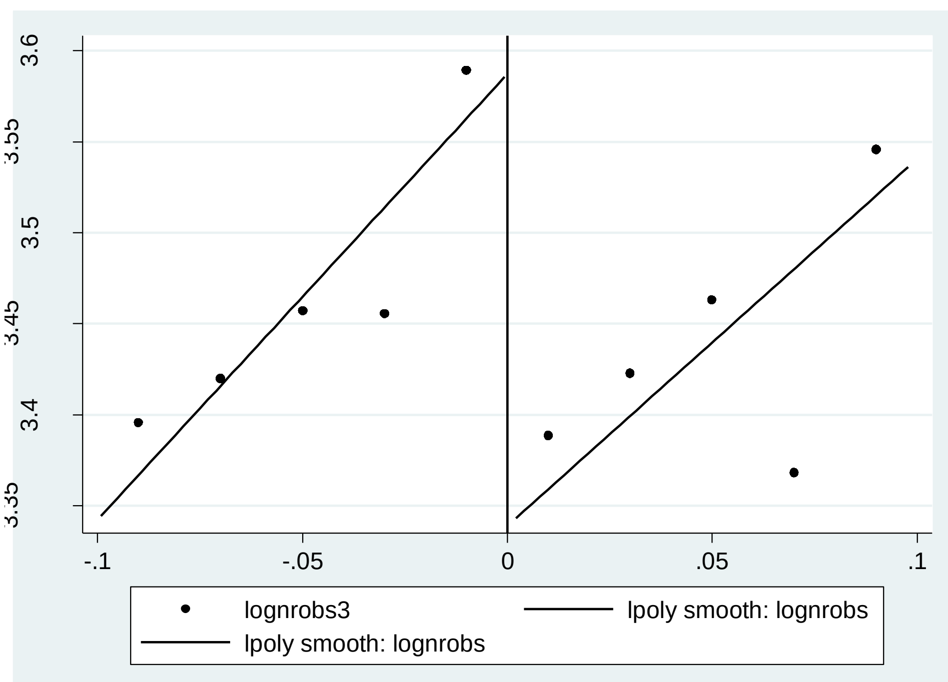


Figure 3. Prolific offenders, index crime

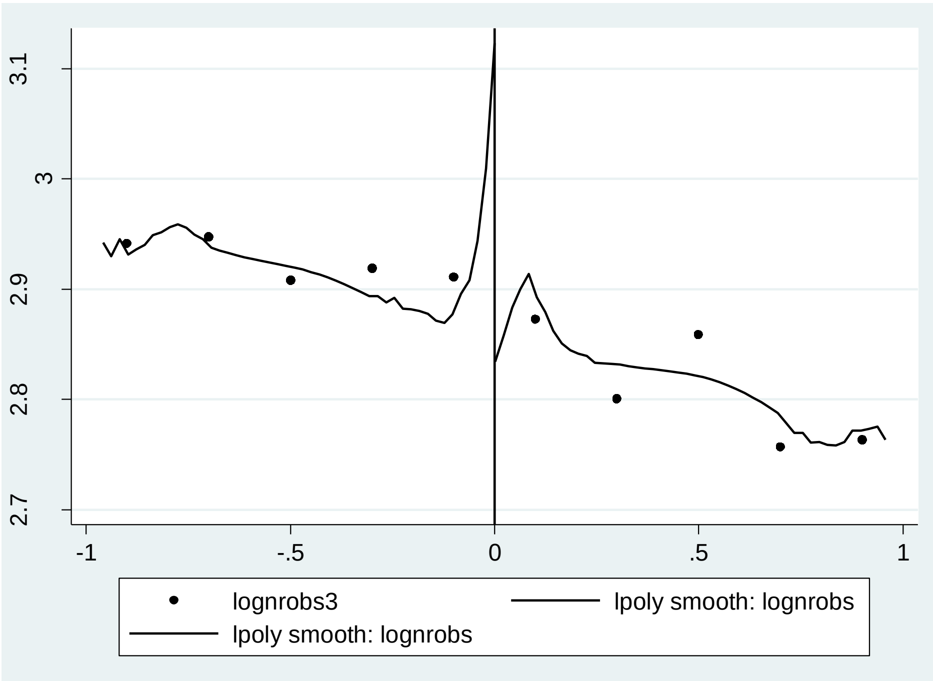


Figure 4. First-time offenders, index crime

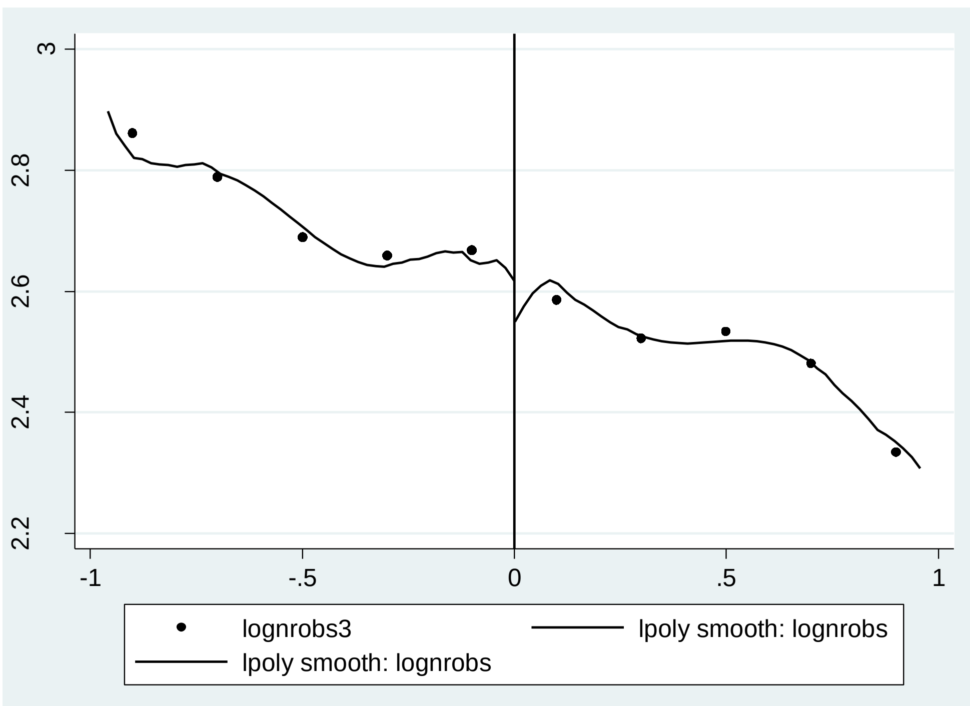


Figure 5. Individuals having no legal income the previous year, index crime.

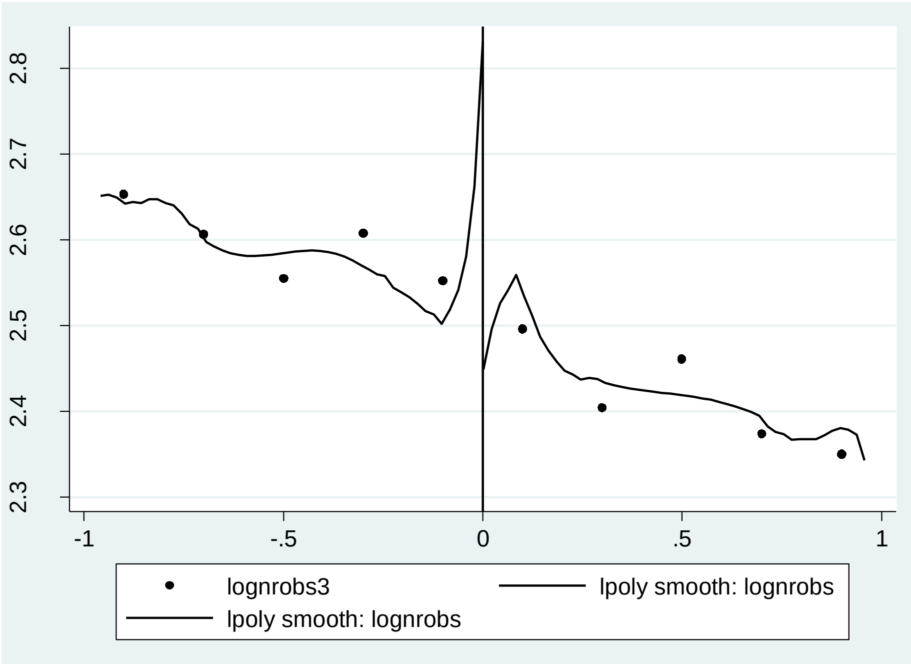


Figure 6. Individuals with legal incomes the previous year, index crime.

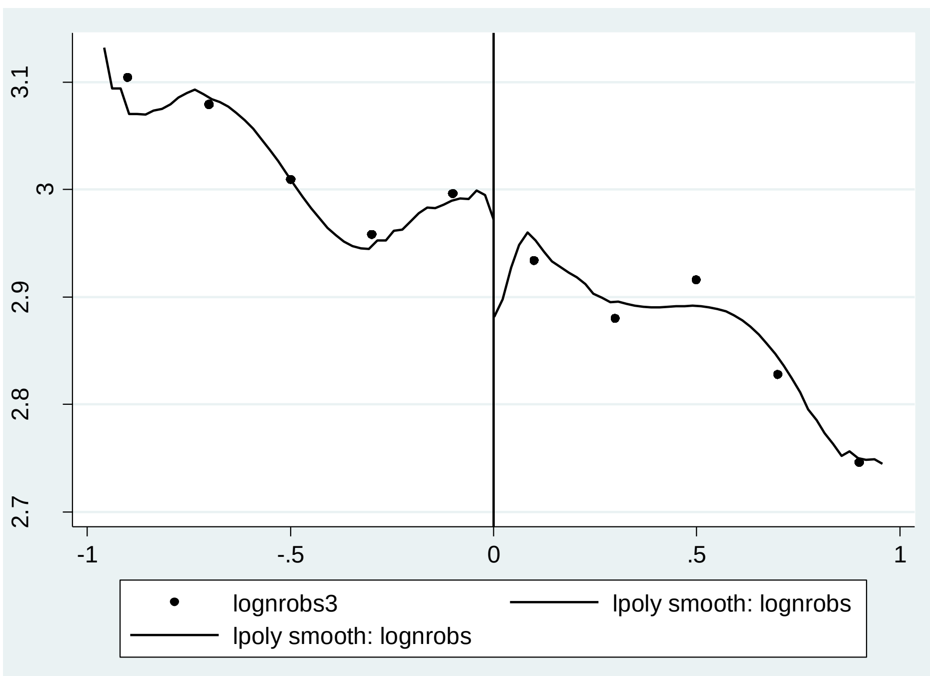


Figure 7. Individuals with social transfers the previous year, index crime.

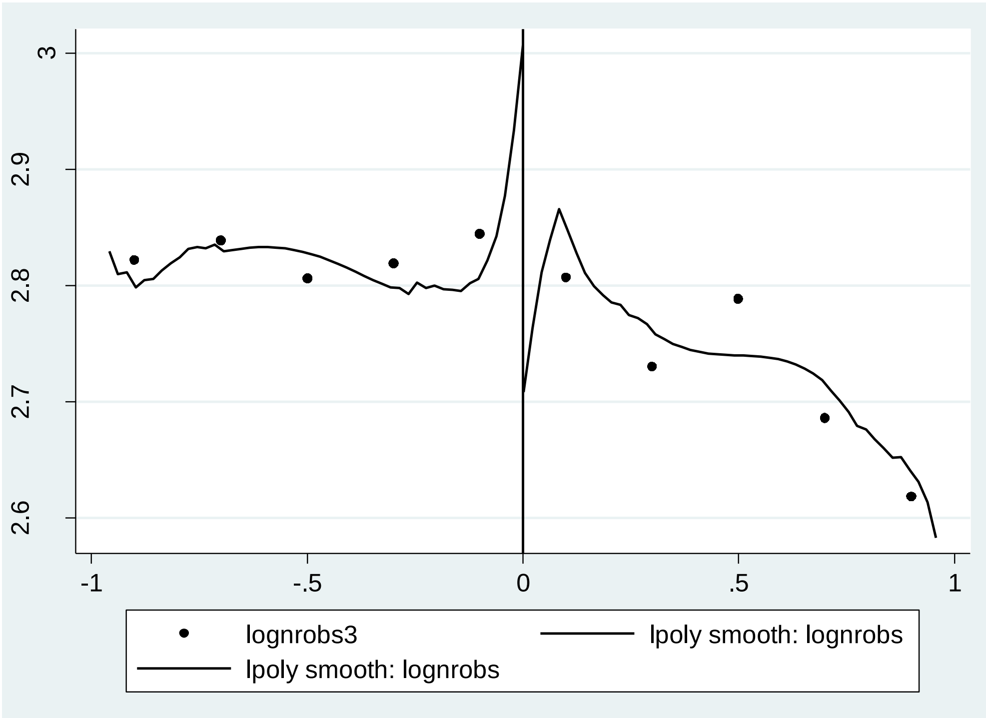


Figure 8. Individuals without social transfers the previous year, index crime.

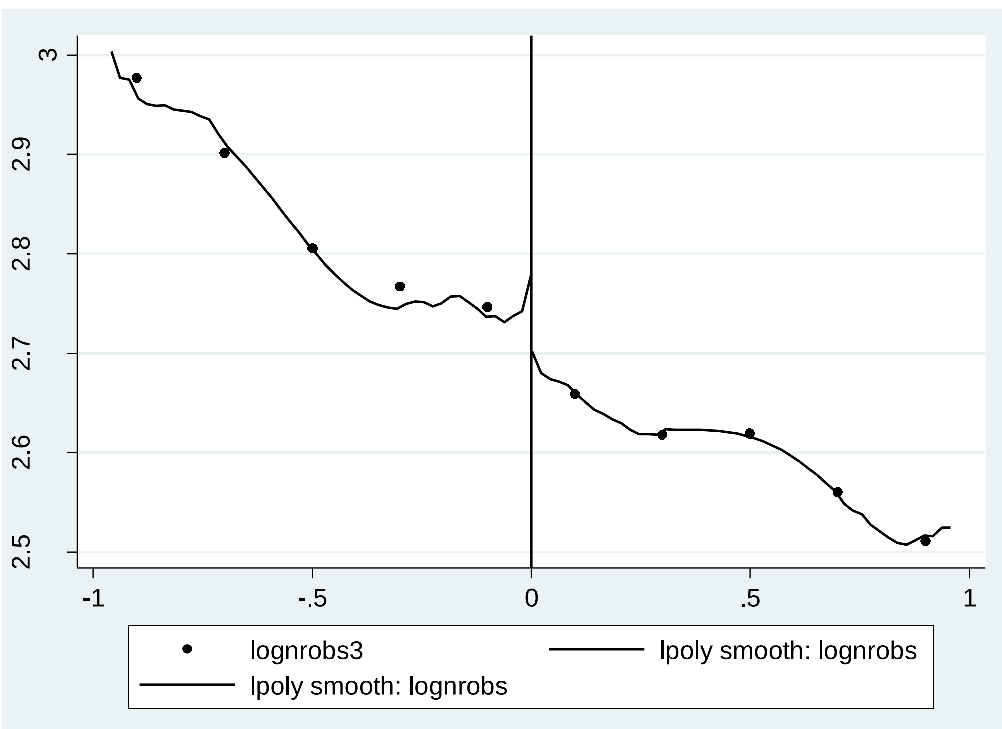


Figure 9. Individuals without student aid the previous year, index crime.

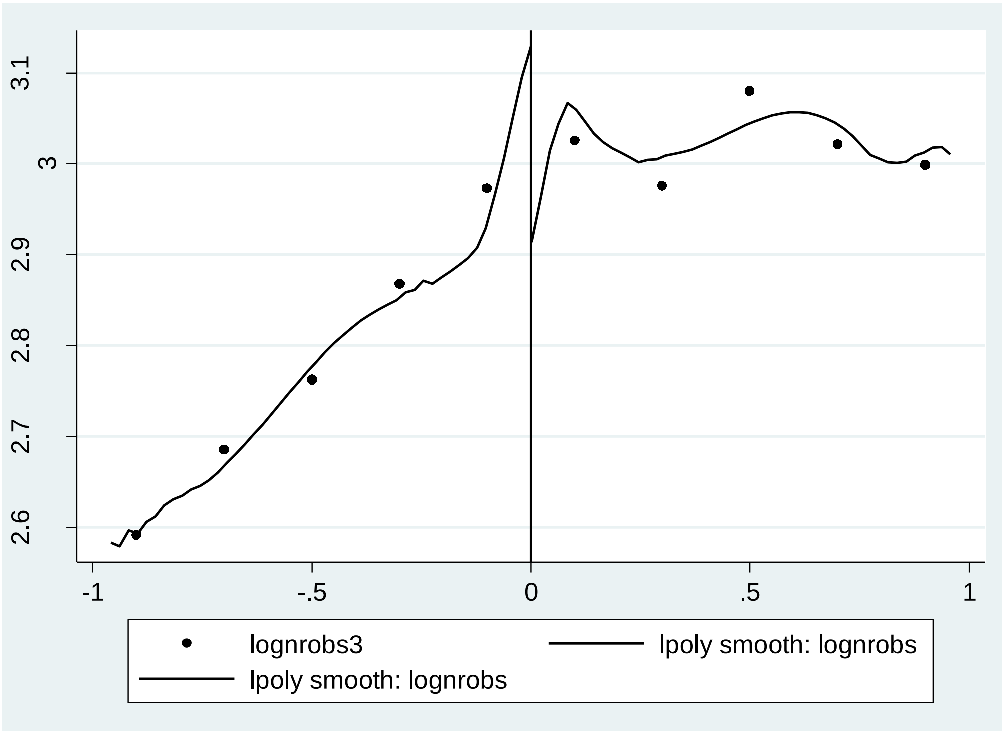
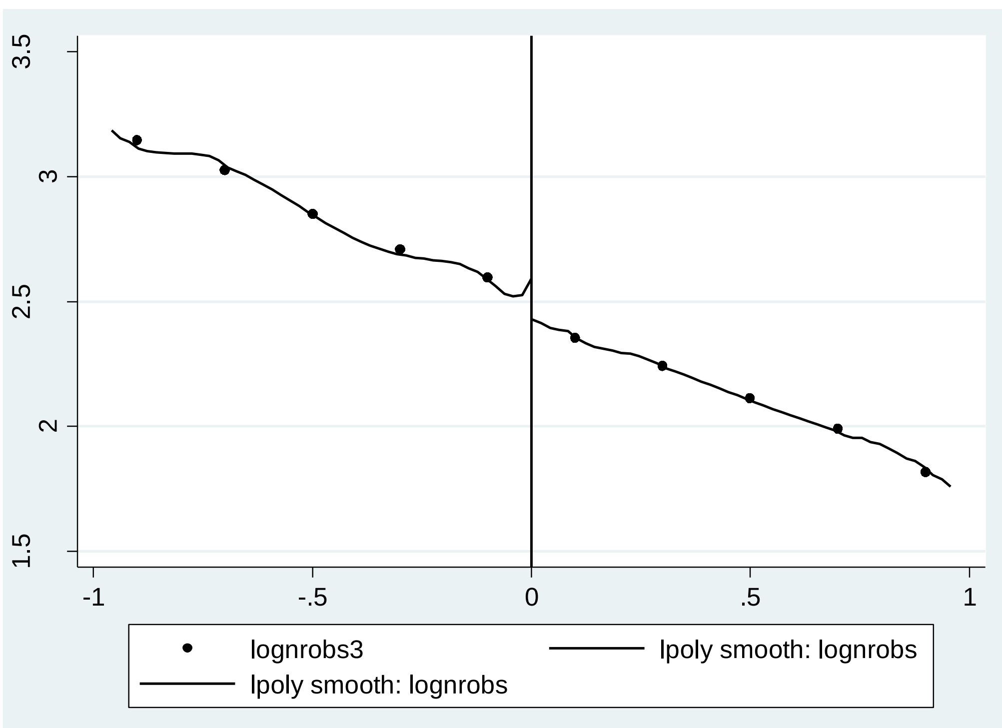


Figure 10. Individuals with student aid the previous year, index crime.



Tables

Standard errors clustered at the running variable, age at crime. *10%, **5%, and ***1%.

Table 1: Potential Sample depending on age threshold analyzed

Threshold	16	17	18	19	20	21
Cohorts that can be used	1973-1993	1973-1992	1973-1991	1973-1990	1973-1989	1973-1988
Total number of convicted	909,291	897,153	879,606	857,946	831,935	803,832

Table 2: Attrition when matching the different data sources

Max observations	Sample after matched with country of birth	Sample after matched with birth register	Sample after using only verdicts with known date of crime
909,291	724,654	663,724	587,656

Table 3. Index crimes

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.501**	-0.283**	-0.248***	-0.093	-0.089	-0.101*
		(0.180)	(0.106)	(0.083)	(0.076)	(0.063)	(0.059)
	2	-0.443	-0.396**	-0.230*	-0.313***	-0.208**	-0.151
		(0.302)	(0.154)	(0.130)	(0.107)	(0.102)	(0.092)
	3	-1.013***	-0.535***	-0.496***	-0.335**	-0.349***	-0.281**
		(0.222)	(0.180)	(0.163)	(0.141)	(0.123)	(0.118)
Obs.		488	1,392	2,275	3,170	4,001	4,899
No. days		15	44	73	102	131	161

Table 4. Index crimes with control variables

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.494**	-0.281***	-0.250***	-0.102	-0.099	-0.108*
		(0.180)	(0.103)	(0.081)	(0.074)	(0.062)	(0.058)
	2	-0.388	-0.391**	-0.231*	-0.316***	-0.211**	-0.160*
		(0.296)	(0.151)	(0.126)	(0.105)	(0.100)	(0.090)
	3	-0.974***	-0.532***	-0.499***	-0.344**	-0.355***	-0.285**
		(0.221)	(0.177)	(0.161)	(0.139)	(0.122)	(0.116)
Obs.		486	1,384	2,262	3,151	3,973	4,862
No. days		15	44	73	102	131	161

Notes: Standard errors clustered at the running variable date of birth*10%, **5%, and ***1%

Table 5. Violence, aggravated assault

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.700**	-0.299*	-0.346**	-0.188	-0.192*	-0.229**
		(0.255)	(0.177)	(0.153)	(0.124)	(0.106)	(0.104)
	2	-0.005	-0.395**	-0.197	-0.365**	-0.294*	-0.227
		(0.164)	(0.168)	(0.190)	(0.182)	(0.165)	(0.150)
	3	-0.215	-0.560**	-0.508***	-0.289	-0.319*	-0.307
		(0.269)	(0.236)	(0.179)	(0.185)	(0.189)	(0.190)
Obs.		167	443	748	1,051	1,323	1,639
No. days		15	44	73	102	131	161

Table 6. Burglary and larceny

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.461	-0.340**	-0.254**	-0.123	-0.056	-0.055
		(0.384)	(0.160)	(0.116)	(0.100)	(0.088)	(0.082)
	2	-0.632	-0.477	-0.288	-0.306**	-0.261*	-0.173
		(0.689)	(0.292)	(0.195)	(0.153)	(0.135)	(0.121)
	3	-1.625**	-0.557	-0.611*	-0.459*	-0.387**	-0.353**
		(0.697)	(0.438)	(0.319)	(0.235)	(0.193)	(0.167)
Obs.		272	782	1,257	1,726	2,176	2,639
No. days		15	44	73	102	131	161

Table 7. Traffic (including drunk driving and driving under the influence of drugs)

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.389	0.139	0.174	0.106	0.088	0.054
		(0.233)	(0.192)	(0.141)	(0.113)	(0.097)	(0.086)
	2	-0.760**	-0.024	0.083	0.190	0.129	0.122
		(0.271)	(0.273)	(0.217)	(0.191)	(0.159)	(0.140)
	3	-0.543*	-0.326	-0.033	0.018	0.202	0.198
		(0.267)	(0.287)	(0.271)	(0.241)	(0.231)	(0.202)
Obs.		248	752	1,266	1,716	2,221	2,701
No. days		15	44	73	102	131	161

Table 8. Drug-related crime

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.211	0.049	0.044	0.041	0.167*	0.198**
		(0.191)	(0.136)	(0.118)	(0.107)	(0.098)	(0.088)
	2	-0.073	-0.073	-0.026	-0.010	-0.064	0.019
		(0.185)	(0.195)	(0.149)	(0.135)	(0.128)	(0.121)
	3	0.125	0.024	0.015	0.038	-0.001	-0.086
		(0.266)	(0.204)	(0.191)	(0.165)	(0.154)	(0.139)
Obs.		261	763	1,239	1,768	2,272	2,796
No. days		15	44	73	102	131	161

Table 9. Non-index crimes

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.232*	0.081	0.086	0.082	0.098**	0.089**
		(0.109)	(0.086)	(0.063)	(0.051)	(0.045)	(0.039)
	2	-0.295*	-0.134	-0.003	0.056	0.053	0.067
		(0.146)	(0.111)	(0.092)	(0.080)	(0.070)	(0.064)
	3	-0.410*	-0.184	-0.052	-0.029	0.036	0.060
		(0.217)	(0.134)	(0.121)	(0.102)	(0.096)	(0.087)
Obs.		1,094	3,190	5,310	7,388	9,537	11,660
No. days		15	44	73	102	131	161

Table 10. All crimes that yield less than 6 months in prison

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.118	0.055	0.094	0.113	0.089	0.063
		(0.227)	(0.131)	(0.097)	(0.077)	(0.067)	(0.060)
	2	-0.400	-0.053	0.015	0.060	0.090	0.092
		(0.232)	(0.179)	(0.145)	(0.125)	(0.110)	(0.097)
	3	-0.267	-0.043	-0.000	-0.018	0.052	0.107
		(0.449)	(0.255)	(0.189)	(0.159)	(0.147)	(0.136)
Obs.		655	1,831	3,039	4,181	5,339	6,489
No. days		15	44	73	102	131	161

Table 11. Women Index crimes

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.355	-0.158	-0.127	-0.033	0.007	-0.070
		(0.521)	(0.228)	(0.151)	(0.146)	(0.136)	(0.118)
	2	-0.292	-0.588	-0.187	-0.258	-0.203	-0.028
		(1.066)	(0.406)	(0.262)	(0.208)	(0.199)	(0.175)
	3	-0.692	-0.272	-0.584	-0.184	-0.179	-0.311
		(1.830)	(0.598)	(0.418)	(0.325)	(0.263)	(0.234)
Obs.		69	217	351	498	649	795
No. days		15	44	73	102	131	159

Table 12. Men Index crimes

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.502***	-0.303***	-0.264***	-0.109	-0.105	-0.105
		(0.154)	(0.112)	(0.091)	(0.081)	(0.067)	(0.064)
	2	-0.400	-0.368**	-0.242*	-0.322***	-0.215*	-0.172*
		(0.241)	(0.142)	(0.131)	(0.112)	(0.109)	(0.097)
	3	-1.021***	-0.548***	-0.476***	-0.353**	-0.374***	-0.284**
		(0.175)	(0.140)	(0.149)	(0.138)	(0.123)	(0.126)
Obs.		419	1,175	1,924	2,672	3,352	4,104
No. days		15	44	73	102	131	161

Table 13, Jump in percentages, all index crime age 16 to 22

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
16	1	-0.038	0.015	0.007	-0.008	-0.016	-0.005
		(0.076)	(0.065)	(0.050)	(0.042)	(0.037)	(0.033)
	2	-0.152*	0.031	0.044	0.047	0.023	-0.001
		(0.081)	(0.082)	(0.070)	(0.063)	(0.056)	(0.051)
	3	0.003	-0.049	0.008	0.013	0.048	0.039
		(0.138)	(0.101)	(0.078)	(0.072)	(0.069)	(0.065)
Obs.		1,650	4,754	8,209	11,752	15,020	18,468
No. days		15	44	73	102	131	161
17	1	0.053	0.008	-0.022	-0.021	-0.016	-0.008
		(0.059)	(0.044)	(0.036)	(0.032)	(0.030)	(0.027)
	2	0.156***	0.015	0.025	-0.001	-0.009	-0.029
		(0.046)	(0.056)	(0.045)	(0.043)	(0.041)	(0.037)
	3	0.022	0.054	0.018	0.017	-0.003	0.020
		(0.099)	(0.063)	(0.060)	(0.049)	(0.049)	(0.045)
Obs.		1,294	3,814	6,240	8,775	11,388	14,034
No. days		15	44	73	102	131	161

18	1	0.219	0.022	-0.035	-0.015	-0.059	-0.080*
		(0.155)	(0.086)	(0.064)	(0.054)	(0.047)	(0.041)
	2	0.322	0.150	-0.016	-0.012	0.033	0.001
		(0.236)	(0.133)	(0.105)	(0.084)	(0.072)	(0.066)
	3	-0.057	0.235	0.236*	0.000	-0.039	0.035
		(0.159)	(0.189)	(0.132)	(0.123)	(0.106)	(0.089)
Obs.		844	2,588	4,433	6,254	8,216	10,052
No. days		14	44	74	102	132	160
19	1	-0.169	-0.061	-0.088	-0.062	-0.029	-0.032
		(0.171)	(0.093)	(0.069)	(0.055)	(0.051)	(0.046)
	2	-0.160	0.002	-0.021	-0.072	-0.097	-0.065
		(0.249)	(0.144)	(0.112)	(0.087)	(0.075)	(0.067)
	3	0.277	-0.171	-0.081	-0.045	-0.048	-0.089
		(0.177)	(0.197)	(0.151)	(0.126)	(0.109)	(0.094)
Obs.		723	2,196	3,663	5,142	6,570	8,049
No. days		15	44	73	102	131	161
20	1	0.197*	0.253**	0.158**	0.132**	0.128**	0.099*
		(0.096)	(0.097)	(0.072)	(0.062)	(0.057)	(0.052)
	2	0.243**	0.101	0.300***	0.197**	0.166**	0.163**
		(0.106)	(0.112)	(0.104)	(0.086)	(0.079)	(0.073)
	3	0.218	0.209	0.123	0.305***	0.251**	0.230**
		(0.172)	(0.141)	(0.108)	(0.109)	(0.099)	(0.091)
Obs.		565	1,598	2,725	3,862	4,910	5,998
No. days		15	43	73	103	131	161
21	1	-0.501**	-0.283**	-0.248***	-0.093	-0.089	-0.101*
		(0.180)	(0.106)	(0.083)	(0.076)	(0.063)	(0.059)
	2	-0.443	-0.396**	-0.230*	-0.313***	-0.208**	-0.151
		(0.302)	(0.154)	(0.130)	(0.107)	(0.102)	(0.092)
	3	-1.013***	-0.535***	-0.496***	-0.335**	-0.349***	-0.281**
		(0.222)	(0.180)	(0.163)	(0.141)	(0.123)	(0.118)
Obs.		488	1,392	2,275	3,170	4,001	4,899
No. days		15	44	73	102	131	161

Table 14. Placebo threshold 23-29

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
22	1	0.335**	-0.044	-0.085	-0.015	-0.054	-0.027
		(0.151)	(0.116)	(0.095)	(0.079)	(0.069)	(0.062)
	2	0.652***	0.071	-0.010	-0.110	-0.047	-0.070
		(0.139)	(0.165)	(0.128)	(0.124)	(0.105)	(0.095)
	3	0.610**	0.599***	0.127	0.057	-0.048	-0.039
		(0.207)	(0.148)	(0.158)	(0.141)	(0.139)	(0.127)
Obs.		354	1,021	1,778	2,442	3,150	3,847
No. days		14	44	74	102	132	160
23	1	0.044	0.006	-0.035	-0.070	-0.029	-0.001
		(0.171)	(0.115)	(0.089)	(0.079)	(0.071)	(0.063)
	2	0.491***	0.078	0.034	0.015	-0.056	-0.074
		(0.141)	(0.141)	(0.143)	(0.122)	(0.106)	(0.092)
	3	0.852***	0.105	0.072	0.042	0.050	0.007
		(0.101)	(0.190)	(0.146)	(0.158)	(0.147)	(0.126)
Obs.		297	886	1,491	2,016	2,552	3,119
No. days		15	44	73	102	131	161
24	1	0.017	0.106	-0.030	-0.039	0.019	0.021
		(0.148)	(0.116)	(0.103)	(0.093)	(0.084)	(0.076)
	2	0.460**	0.292*	0.137	0.023	-0.016	0.006
		(0.207)	(0.168)	(0.124)	(0.119)	(0.113)	(0.109)
	3	-0.065	-0.038	0.360**	0.202	0.040	-0.016
		(0.229)	(0.184)	(0.157)	(0.133)	(0.131)	(0.127)
Obs.		248	692	1,151	1,627	2,063	2,553
No. days		15	43	73	103	131	161
25	1	-0.036	0.164	0.100	0.106	0.031	0.148*
		(0.246)	(0.157)	(0.117)	(0.104)	(0.088)	(0.079)
	2	-0.125	-0.046	0.120	0.101	0.176	0.027
		(0.360)	(0.220)	(0.185)	(0.151)	(0.138)	(0.120)
	3	0.286	-0.071	0.078	0.088	0.066	0.162
		(0.390)	(0.279)	(0.235)	(0.212)	(0.174)	(0.161)
Obs.		209	586	956	1,323	1,701	2,097
No. days		15	44	73	102	131	161

26	1	0.337	0.057	0.010	-0.023	0.004	-0.023
		(0.229)	(0.164)	(0.133)	(0.113)	(0.095)	(0.088)
	2	0.038	0.279	0.187	0.053	0.016	0.059
		(0.224)	(0.212)	(0.181)	(0.170)	(0.160)	(0.137)
	3	0.072	0.151	0.196	0.303	0.117	-0.005
		(0.333)	(0.220)	(0.206)	(0.194)	(0.182)	(0.181)
Obs.		176	488	828	1,177	1,483	1,803
No. days		0.072	0.151	0.196	0.303	0.117	-0.005
27	1	0.232	-0.012	-0.027	-0.136	-0.116	-0.114
		(0.303)	(0.151)	(0.121)	(0.109)	(0.100)	(0.088)
	2	0.035	0.227	-0.035	-0.022	-0.109	-0.120
		(0.438)	(0.260)	(0.183)	(0.152)	(0.137)	(0.130)
	3	0.408	0.155	0.343	0.153	0.082	-0.007
		(0.531)	(0.336)	(0.278)	(0.211)	(0.182)	(0.162)
Obs.		139	437	705	978	1,268	1,534
No. days		15	44	73	102	131	161
28	1	0.501*	0.073	0.258*	0.190	0.117	0.153
		(0.253)	(0.170)	(0.134)	(0.122)	(0.114)	(0.104)
	2	0.820**	0.423*	0.154	0.245	0.239	0.153
		(0.317)	(0.240)	(0.209)	(0.168)	(0.152)	(0.141)
	3	0.459	0.529*	0.156	0.168	0.298	0.315*
		(0.431)	(0.289)	(0.282)	(0.234)	(0.206)	(0.182)
Obs.		131	351	613	872	1,100	1,350
No. days		15	43	73	103	131	161
29	1	0.269	-0.041	0.120	0.123	0.237*	0.228*
		(0.288)	(0.227)	(0.168)	(0.146)	(0.142)	(0.119)
	2	0.152	0.047	-0.038	0.079	0.002	0.092
		(0.273)	(0.278)	(0.244)	(0.201)	(0.188)	(0.170)
	3	-0.884**	0.251	0.016	-0.093	0.086	0.026
		(0.382)	(0.282)	(0.272)	(0.258)	(0.235)	(0.216)
Obs.		112	310	506	702	881	1,064
No. days		15	44	73	102	131	161
30	1	0.800***	0.242	0.231	0.183	0.241*	0.159
		(0.259)	(0.218)	(0.160)	(0.138)	(0.125)	(0.114)
	2	1.363***	0.799***	0.315	0.314	0.142	0.238
		(0.311)	(0.247)	(0.241)	(0.202)	(0.188)	(0.164)
	3	1.305**	1.247***	0.817***	0.417	0.507**	0.289
		(0.463)	(0.234)	(0.258)	(0.267)	(0.223)	(0.230)
Obs.		79	258	421	576	744	894
No. days		14	44	74	102	132	160

Table 15. Prolific offenders, index crime

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.537***	-0.377***	-0.286***	-0.093	-0.127	-0.117
		(0.163)	(0.121)	(0.089)	(0.090)	(0.078)	(0.076)
	2	-0.615**	-0.400**	-0.310**	-0.422***	-0.227**	-0.209**
		(0.250)	(0.165)	(0.140)	(0.110)	(0.105)	(0.099)
	3	-1.455***	-0.495**	-0.551***	-0.356**	-0.467***	-0.301**
		(0.156)	(0.212)	(0.168)	(0.159)	(0.129)	(0.126)
Obs.		287	824	1,317	1,802	2,274	2,776
No. days		15	44	73	102	131	161

Table 16. First-time offenders, index crime.

Age threshold	Order of polynomial	Bandwidths (share of a year=365.25 days)					
		0.02	**0.06**	**0.1**	**0.14**	**0.18**	**0.22**
21	1	-0.403	-0.159	-0.169	-0.044	-0.005	-0.049
		(0.284)	(0.163)	(0.112)	(0.103)	(0.084)	(0.080)
	2	-0.242	-0.307	-0.136	-0.157	-0.148	-0.038
		(0.411)	(0.244)	(0.183)	(0.151)	(0.139)	(0.124)
	3	-0.492	-0.572*	-0.372	-0.326	-0.192	-0.232
		(0.566)	(0.307)	(0.240)	(0.204)	(0.180)	(0.161)
Obs.		201	568	958	1,368	1,727	2,123
No. days		15	44	73	102	131	161