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# **Age-Dependent Court Sentences and Crime Bunching: Empirical Evidence from Swedish Administrative Data**

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# Age-Dependent Court Sentences and Crime Bunching: Empirical Evidence from Swedish Administrative Data♦

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**Abstract:** According to Swedish penal code, there is a “rebate” on all prison sentences before the 21st birthday. We exploit this age discontinuity to investigate how individuals respond to harsher punishments. We use a large Swedish dataset, including dates for all crimes which led to convictions for cohorts born during the period 1973-1993. We find evidence of “bunching” in the sense that more crimes were committed during the week prior to a 21st birthday, followed by a reduction in crime during the week after this birthday. We do not, however, find that harsher punishment reduces the crime rate permanently.

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## 1. Introduction

The question of how punishment affects crime has preoccupied public policy debates as well as academic discussions in many different fields for a long time. Following Becker (1968), many economists have argued that criminals exhibit rational behavior in the sense that they weigh expected costs and benefits before committing a crime. This implies that incentives matter and that relatively harsher punishment should reduce the crime rate in society. 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 discriminating between these alternative views is ultimately an empirical question, research has been impaired by a lack of high-quality data and empirical research methods.

In this paper, we apply a research design that exploits a feature in the Swedish legal system whereby punishments increase discontinuously at the 21st birthday. In particular, below age 21, age should be considered a mitigating circumstance when the punishment is decided. Life imprisonment, for example, cannot 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 an individual’s birthday that we use as identifying variation to study the probability and timing of committing crimes.

Our research design requires day-to-day individual data on criminal behavior. To this end, we use the Swedish National Conviction Register linked with the Swedish Census which provides demographic information on the individuals included. The Swedish National Conviction Register contains detailed individual information on all convictions throughout the Swedish legal system. This implies that we know when each crime in the Register was committed and its relation to the convicted individual’s 21st birthday. This extensive database provides us with a sufficiently large sample in the age group of interest in order to obtain estimates with high precision.

We found a large sorting effect amounting to approximately a 30 percent increase in the number of crimes close to one week prior to this birthday, followed by a reduction of about the same size the week after. We did not, however, find any long-term reduction in crime following the 21st birthday.

The result is consistent with a model where individuals bunch their decisions to commit crime and the exact timing of the crimes committed depends on opportunities.<sup>4</sup> Avoiding the optimal opportunity to commit a crime comes at a cost, which may be increasing in the time distance from the optimal opportunity. If crime opportunities are limited during a certain period, and criminals take incentives into account, then criminals will front load crime around the 21th birthday. 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 theory rests on the assumption that individuals commit crimes repeatedly. We have studied which types of criminals bunch their crime decisions, and it is exactly prolific offenders who have crime as their main source of income.

Intertemporal displacement of crime has, to our knowledge, received very little attention in the literature. Jacob et al. (2007) instrument crime with weather conditions and found evidence of intertemporal displacement in crime rates. Draca et al. (2010) analyze the police intervention that occurred in London in 2005 following the terror attacks in the city. They found direct effects of the intervention, but no intertemporal displacement. Spatial displacement of crime, on the other hand, is often suggested to be an important aspect of crime control. The empirical literature has, however, found little empirical support for it (see e.g. Weisburd et al., 2006 and Braga and Bond, 2008).

Our study is also related to three earlier papers that exploit age thresholds at criminal majority which vary between age 16 and 19 across U.S. states. Levitt (1998) 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 to study potential incentive effects and found a very small (2 percent) but significant effect. 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. Our finding of the long-term effect is in line with this work, but we also find a large short-term response to a change in penalties.

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<sup>4</sup> For the theory of crime opportunity developed by criminologists, see e.g. Felson and Clark (1998).

There are two features of our study that distinguish it from the three previous studies mentioned above. First, the age of criminal majority in the U.S. tends to coincide with other changes potentially related to crime, such as laws regarding firearms, curfews, drivers' licenses, drop outs from school and gambling. In Florida, for example, at age 18, individuals are able to legally drop-out of school without parental consent. We contribute by analyzing a threshold, the 21st birthday in Sweden, when only penalties and no other factors change. Second, we study the behavior of individuals who are no longer juveniles. Moreover, we study a country where the prison population per capita is about one-tenth of the U.S. prison population, but more similar to many other countries in Western Europe.

Our study is also related to earlier literature on the effects of punishments on crime. Kessler and Levitt (1999) and Vollard (2013) exploited increases in sentence length for prolific offenders. Raphael and Ludwig (2003) and Abrams (2012) studied the effect of harsher weapon laws. Helland and Tabarrok (2007) examined the “three strikes and you're out” reform in California (see also Iyengar, 2008). Kuziemko (2013) exploit discontinuities in parole-board guidelines and Drago et al. (2009) analyzed random amnesties in Italy. Random assignment of judges as a source of exogenous variation in sentences has been used by e.g. Mueller-Smith (2015), Aizer and Doyle (2015) and Bhuller et al. (2016). The results of this literature are mixed.

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

## **2. Juvenile Punishments in the Swedish Judicial System**

According to Swedish law, the age of criminals who commit crimes has to be taken into account when sentences are decided. The Swedish Penal Code, chapter 29, § 7 states that the age “should be explicitly considered when determining the penalty if the crime is committed before the age of 21” and “no one may be sentenced to life in prison for crimes committed before one's 21st birthday”. In the case of repeated offending, “the court must not be sentenced to a higher penalty if the crime was committed before the age of 21” (The Swedish Penal Code, chapter 26, § 3). The youth date rebate at 21 is converted to the size of approximately 25 percent of the sentence (see Jareborg and Zila, 2007 and Supreme Court judge Borgeke, 2008).

There is also a sharp distinction in the Swedish law at an individual's 18<sup>th</sup> birthday because juveniles below that age can only be sentenced to prisons if there are extraordinary circumstances (The Swedish Penal Code, chapter 30, § 5). Under 15, an individual cannot be sentenced (The Swedish Penal Code, chapter 1, § 6).

Apart from these laws, there is an informal practice, which is not part of the written law, that grants smaller sentence reductions before juvenile birthdays from 15 to 21, in particular for crimes leading to prison sentences (Jareborg and Zila, 2007).

If incentives matter, we would therefore expect reductions in crime subsequent to the above-mentioned birthdays, but not afterwards. In Sweden, however, 18 is the age of majority when individuals are allowed to take their driver's license and buy alcohol in pubs and restaurants, which may affect criminal behavior. With respect to the informal rebate system at other age thresholds there are other confounders which makes an analysis intractable. At 20, alcohol may be bought in stores and consumed outside restaurants and the rebate effect can therefore not be isolated. Age 16 is also a problematic threshold to study since individuals are allowed to practice driving and obtain a license for some vehicles. Birthdays above age 21 are used as placebos.

### **3. Data**

Our dataset was obtained by matching several different national Swedish registers. The frame for obtaining the sample was consecutive years of the Swedish census. Data on criminality were 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 used is the date when a crime was committed, type of crime and the length of the potential prison sentence.

Given our research design, the exact date of birth and its relation to the date when a crime was committed are of key importance. The data from the census only contains information on month of birth, which as will become clear when studying the results, masks the sorting effect. It was

possible, however, to use information from the Swedish birth register to obtain an almost perfect prediction on the exact date of birth.<sup>5</sup>

The lowest age threshold that can be analyzed using our data is 16, since crimes committed before age 15 are not recorded in the national conviction register and our approach requires data both before and after the age threshold under study. Since our data end in 2010, we needed to ensure that we did not technically impose a jump in the density due to censoring. For example, say we analyze the number of convictions around the 20th birthday and the window around the threshold is one year. Again, since our data ends in 2010, we could only use cohorts born in 1989 or before. If we also used the cohort born in 1990, then we would have constructed a jump in the density of the number of convicted mechanically, since the last cohort could not have reached the age above the threshold. When pooling cohorts, we allowed only the cohort that had reached the specific age threshold analyzed plus one year. This means that our sample size decreased when studying thresholds for older ages. We used the cohorts from 1973 to 1993 when studying the age 16 threshold, 1973 to 1992 when studying the age 17 threshold and so on. Table 1 shows the number of observations that could potentially be used for each threshold analysis.

**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

<sup>5</sup> The information we use in the national birth register was the date of last menstruation and gestation length in days. Until the 1980s gestation length was primary calculated by using the subjective last date of menstruation, L, which is recorded by the antenatal care (Mödravårdcentralen). When the baby is born, the midwife (but not the econometrician) notes the exact date of birth, D. Gestation length G is calculated in days as being  $G = D - L$ . This mean that even though we do not observe D, we can solve for it as  $D = L + G$ . Thus, since gestation lengths were residually determined as the number of days after last menstruation and date of birth, we get an accurate prediction of the birthday. In the 1980s, ultrasound methods were increasingly used in order to determine gestation length. Our prediction of the birthday become less accurate in the later time period since we lack information on the ultrasound outcome. Moreover, there exist cases where the information is missing on either L or G as presented in Table 1 and 2.

Since our main analysis is focusing on crimes committed around the 21<sup>st</sup> birthday we explicitly discuss the attrition when matching the above discussed Swedish conviction register with the rest of necessary information.<sup>6</sup> The maximum sample size we have for analyzing the 21th year threshold is 803,832 individuals. Since we matched with the birth register, we can only use Swedish-born convicted individuals. When matching country of birth with data from Statistic Sweden, 170,578 were born abroad and 73 had an unknown country of birth. Thus, we now have 633,181 observations as documented in Table 2.

When matching this dataset with the birth register, we lost 47,918 observations, so we ended up with 585,263 convictions, i.e., our attrition is approximately 8 percent. However, since we had 75,658 empty cells with respect to date of crime, we are down to 509,605 convictions where we can determine the age at the date of crime. Since the population consists of the Swedish-born verdicts, the final attrition is approximately 20 percent. Importantly, we have no reason to believe that the attrition is systematically related to exact birth dates.

**Table 2.** Attrition when matching the different data sources for the maximum sample.

Stage in sample selection	Number of observations
All recorded convictions	803,832
Sample after matched with country of birth	633,181
Sample after matched with birth register	585,263
Sample after using only verdicts with known date of crime	509,605

Needless to say, a criminal might have committed many crimes. In Sweden, more than one crime might be handled during a court session. If so, the most severe crime in terms of length of punishment solely determines the harshness of overall punishment. We used the information solely on the most severe crime committed and the respective date. Lastly, since we will focus our attention to crimes committed close to intervals around the 21st birthday, the sample size actually used will clearly be smaller than the 509,605 since the full sample is implying using maximum bandwidth.

<sup>6</sup> The pattern of attrition is similar for all thresholds. Please contact the authors for full information.



#### 4. Empirical Strategy

In order to estimate a jump in the density of number of verdicts, we apply standard regression discontinuity methods, following Lee and Lemieux (2010), where the outcome *Verdict* is the number of verdicts for a crime that was committed at a certain age  $j$ , measured in days relative to the age threshold. Thus, if we are studying the 21 age threshold we count number of verdicts that committed a crime one day before turning 21, at the same day and one day after, etc. For ease of interpretation we use the natural log.

We estimated the following model:

$$\log(\textit{Verdict}_j) = \alpha + \beta \textit{Above}_j + f(W_j) + \varepsilon_j, \quad (1)$$

where *Above* is an indicator variable taking the value of one if the crime was committed at the same age or above the age threshold studied and zero otherwise. The parameter of interest is  $\beta$ , which measures the difference in the number of verdicts at the threshold in percentages.  $W$  is the forcing variable, age at the time of the crime measured in days, although normalized to be zero at the threshold, positive above and negative below. In other words, if the age threshold of 21 is studied, then  $W_j = \text{Age (at crime)} - 21$ .

Equation (1) was 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 an optimal bandwidth (see for example Imbens and Kalyanaraman, 2012 and Calonico et al., 2014) Thus, we are agnostic and report many bandwidths. We also included estimation based on higher order of the polynomial function,  $f(W_j)$ .

Since the forcing variable is discrete (age in days at date of crime), we clustered the standard errors at the forcing variable following Card and Lee (2008). Lastly, the smallest bandwidth we present is only 0.02, which means that we compare the number of crimes carried out by criminals roughly one week before their birthday with crimes committed one week after since the bandwidth is measured as shares of a year in days (365,25 days). This gives us only 15 clusters;

hence the standard error in these regressions should be interpreted cautiously. However, following the logic of a standard regression discontinuity design, as discussed in Lee and Lemieux (2010), the point estimate is still informative and should be unbiased.

In order for the estimates to be causally valid, we need to assume that no other criminal determinant, other than the severity of punishments, changes discontinuously at the birthday thresholds. This implies assuming that factors of criminal behavior evolve continuously as the potential criminal approaches age 21, except for the severity of punishment, which increases discontinuously at 21. In order to detect discontinuities in the density, high frequency data are required. As argued in Lee and McCrary (2016), “all other factors” are likely to be only constant when examining offense rates in relatively short intervals. When pooling large intervals (such as annual comparisons across thresholds), many factors affecting criminal activity change in ways that could influence underlying criminal propensities. Compared to previous studies, the 21 year-old threshold in Sweden seems to be better suited for causal analysis, in the sense that 21 is simply reminiscent of voting eligibility in the old days.

## **5. Results**

### **5.1 Main results**

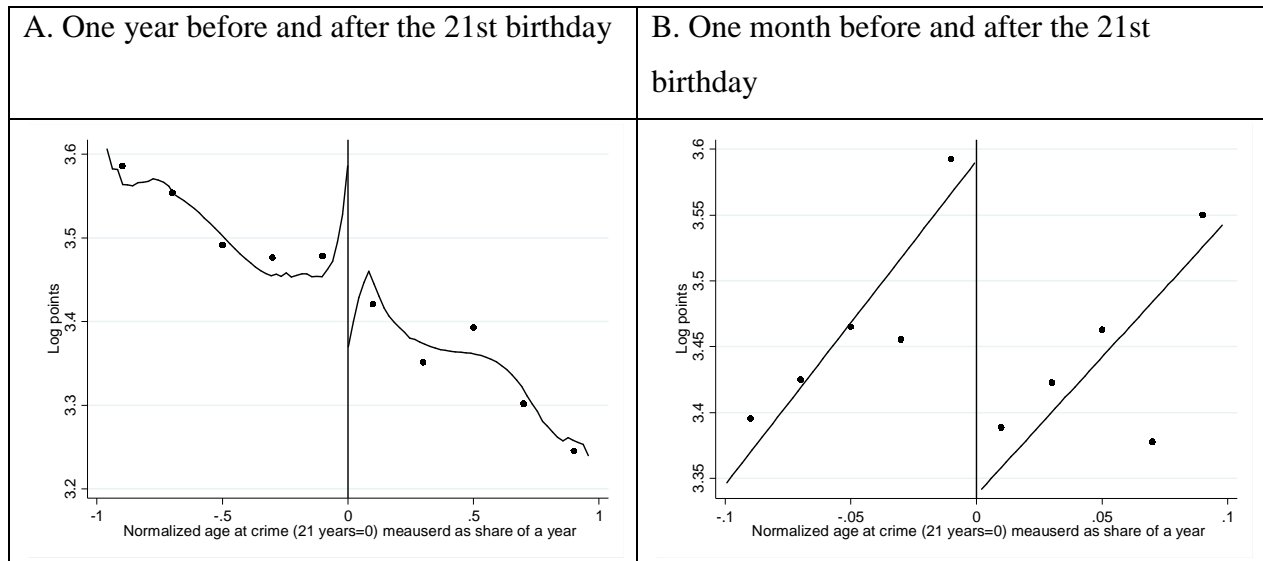
Consider first the effects of the 21-year threshold on all index crimes (murder and non-negligent manslaughter, forcible rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft and arson).<sup>7</sup> Figure 1A shows the crime pattern one year prior to and one year subsequent to the 21st birthday. Since the outcome is the natural log of the number of crimes, the effect is approximated as a percentage difference around the threshold. There is a clear local specific deterrence driven by bunching behavior close to the 21st birthday. The week before the 21st birthday, there is a large increase in number of crimes, which is followed by a sharp reduction the week following this birthday. Crime then reverts to the original trend. In order to prove that the results are not functional form specific and overshooting at the threshold due to no support, we also zoom in using a sample of roughly one month on each side. Figure 1B shows a strong local

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<sup>7</sup> There are no direct translations from index crimes to the Swedish criminal codes. See Table A1 in Appendix for the corresponding Swedish codes and our definitions.

deterrence effect of around 25 percent and a stable patten when using only data very close to the threshold.

**Figure 1.** Number of index crimes (log scale) close to the 21st birthday.



Note: Zero denotes 21 years old at the date of the crime. The dependent variable is the logarithm of the number of sentenced individuals who committed the crime on a certain date. In (A) plus/minus 1.0 year around the threshold is used and in (B), plus/minus 0.1 years around the threshold (roughly one month on both sides) is used. In (A) the plotted points are conditional means for a bin where the bandwidth is 0.2 of a year. In (B) the bandwidth is 0.02 of a year, roughly a week in width. The solid line in (A) is the predicted value of a fifth-degree smoother with a rectangular kernel and a bandwidth of 0.7, and in (B) the solid line is the predicted value of a local linear smoother with a rectangular kernel.

The regression results reported in Table 3 (without control variables) show a very large, significant and robust negative effect of sorting at the 21st birthday. For example, for the second bandwidth 0.06, where the standard errors can also be trusted, the effect is around 30 percent. Thus, 30 percent more crimes are committed the week before turning 21 compared to the week after. When using a wider bandwidth, the effects decrease, thereby 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. Thus, the regression results do line up well with the graphical evidence in Figure 1.<sup>8</sup> In Table A2 in appendix we add the control variables birth month and year fixed effects and previous criminal history and the result do not vary significantly compared to the results without controls.

<sup>8</sup> We ran the STATA RD-package for optimal bandwidth, which gives a bandwidth of 0.117 cct. The search was made plus/minus 0.9 of the 21st birthday in order to exclude the 20th birthday.

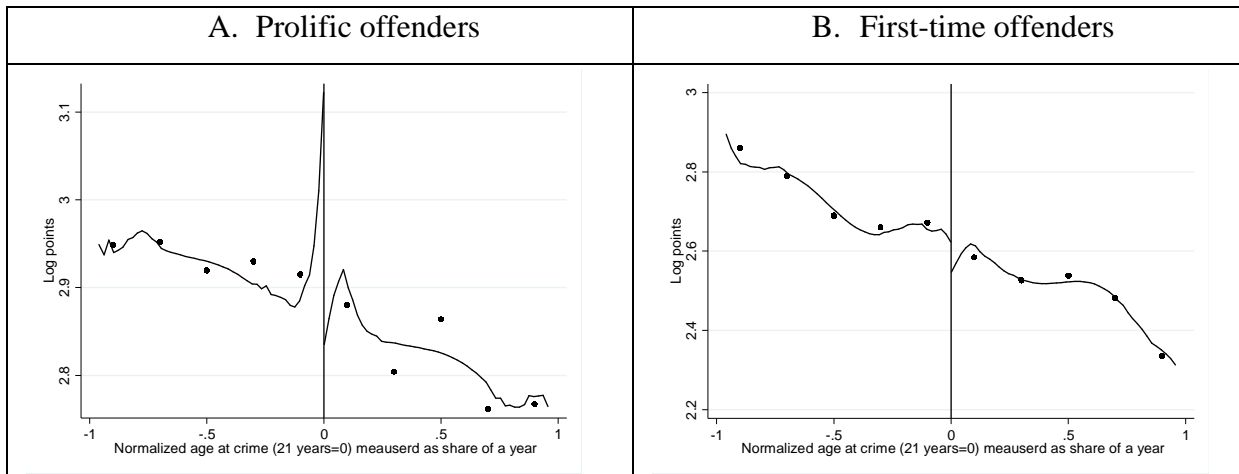
**Table 3.** The 21st birthday rebate effect on the number of index crimes in percentages.

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

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

## 5.2 Characterizing the sorting by type of criminal and types of crimes

We now characterize the type of criminals and the types of crimes generating the bunching pattern. We start by splitting the sample into prolific offenders and first-time offenders. Figure 2 shows that the main effect is driven by prolific offenders while there is no bunching by first-time offenders.

**Figure 2.** Number of index crimes (log scale) close to the 21st birthday for prolific and first-time offenders.

Note: Zero denotes 21 years old at the date of the crime. The dependent variable is the logarithm of the number of sentenced individuals who committed the crime on a certain date. 1.0 years around the threshold is used and the plotted points are conditional means for a bin where the bandwidth is 0.2 of a year. The solid line is the predicted value of a fifth-degree smoother with a rectangular kernel and a bandwidth of 0.7.

The regression results are presented in Table 4 (prolific offenders) and in Table A3 (first-time offenders). It is again apparent that the main effect is driven by prolific offenders.

**Table 4.** The 21st birthday rebate effect for prolific offenders on the number of index crimes in percentages.

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

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

We next analyze subgroups based on variables that are correlated with the individuals' attachment to the society. In Figure A1 in appendix, we observe that the bunching behavior is explained by the subgroups that is not earning any legal taxable income, not receiving student aid and not receiving conditional unemployment benefits but, however, do receive unconditional welfare. We can conclude that the bunching behavior is caused by prolific offenders with a weak attachment to the society.

We would further expect incentives to matter more for planned crimes than other crimes. In fact, the index crimes reported in Table 1 tend to be planned. Moreover, as shown in Tables A4 and A5, the results are mostly driven by aggravated assaults, burglary and larceny, which are all planned crimes. On the other hand, we would not expect to find any deterrent effects when considering crimes that are less planned. Table A6 and A7 show that there is no bunching for traffic- and drug-related crimes, which are typically not planned, Table A8 shows that there are no bunching effects for non-index crimes, which also tend to be less planned.

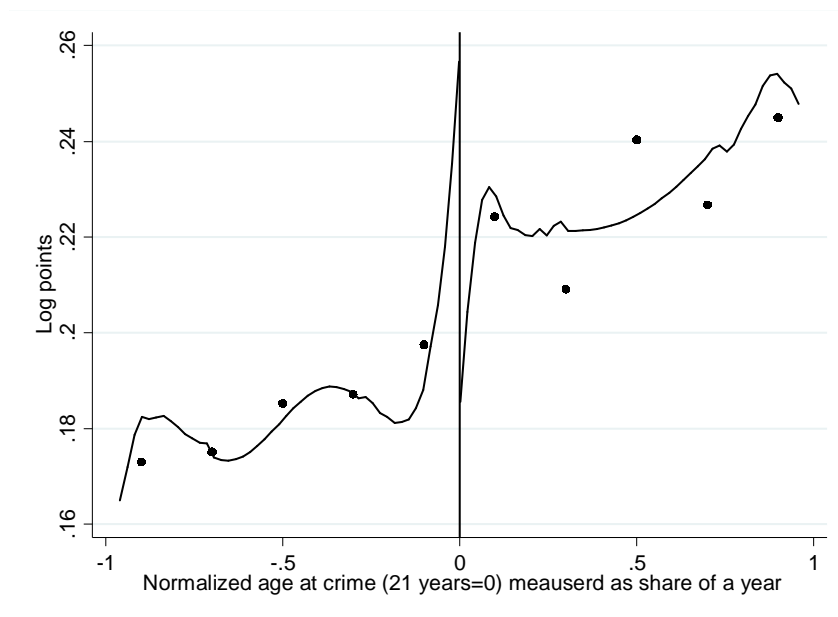
For crimes which lead to a maximum of six months in prison, individuals are usually fined. For such crimes, the jump in penalties is less clear at age 21 than for crimes leading to more than six months in prison (Jareborg and Zila 2007). We therefore do not expect significant results. Table A9 confirms this hypothesis. Table A10 and A11 finally show the effects for men and women separately. The results are entirely driven by men.

### 5.3 Prison sentences

Studying the probability of prison sentences allows us to study the effect of sorting in relation to the rebate system in more detail. We expect two counteracting effects. First, a lower probability of prison sentences as a result of the rebate system before the 21<sup>st</sup> birthday. Second, provided that there is a behavioral response, a reallocation of committed crimes to before the 21<sup>st</sup> birthday, which would increase the number of prison sentences.

Figure 3 shows the two counteracting effects described above. Immediately around the cutoff point there is a reallocation of crimes working towards an increase in the number of prison sentences before and a decrease immediately after the cutoff. However, moving out from the surroundings of the cutoff we see a marked increase in the number of prison sentences. Indeed, the size of the long-term increase in prison sentences is around 25 percent, precisely the level that is proposed in the legal literature (Jareborg and Zila, 2007).<sup>9</sup>

**Figure 3.** Probability of being sent to prison close to the 21st birthday.



Note: Zero denotes 21 years old at the date of the crime. The dependent variable is the logarithm of the number of sentenced individuals who committed the crime on a certain date. 1.0 years around the threshold is used and the plotted points are conditional means for a bin where the bandwidth is 0.2 of a year. The solid line is the predicted value of a fifth-degree smoother with a rectangular kernel and a bandwidth of 0.7.

<sup>9</sup>This result suggests that a regression discontinuity design could be suitable using the 21st birthday as an instrumental variable for the severity of punishment as discussed in Almond and Doyle (2011) and Barreca et al. (2011).

## 5.4 Placebo tests

As mentioned above, there is a rebate in the Swedish law at 18, and in official recommendations, there are smaller rebates at 16, 17, 19 and 20. The thresholds 18 and 20 cannot be isolated due to strong confounding factors. Nevertheless, for these thresholds, we present results in Tables A12 and A13. In general, there is no clear pattern but we note that there is an *increase* in crime subsequent to the 20th birthday, which may be attributed to the legal age for freely buying alcohol in Sweden.

Lastly, in Table A14 and A15 we present the results the placebo thresholds 22 - 31. In general, there is little evidence of any robust and significant sorting, except at age 30, where there is weak evidence of *positive* sorting. Again, this fits with the tradition of celebrating decennial birthdays. We conclude that this placebo analysis strongly supports the findings at age 21.

## 6. Discussion

Our analysis shows that prolific offenders likely to be outsiders to formal society bunch their criminal behavior around their 21st birthday. We believe that these results are consistent with a model with rational criminals who take advantage of the best opportunity to commit crime. Consider the examples of car theft or burglary. During a certain time period, opportunities or costs of committing crime, vary across days depending on, for example, the day of the week. More people might be at home or on the streets at certain times. The total number of opportunities for crime during a period may be limited (there are only so many cars and houses in an area). Committing a crime in the current period may come at an increased cost to commit a similar crime later on, either because the opportunity has already been exploited, or because of increased watchfulness on the part of car and home owners. Crime will then be displaced due to harsher punishment if the benefits dominate the cost in terms of a foregone opportunity.

We find that prolific criminals do in fact find it attractive to displace crime, but only from the week after their 21st birthday to the week before. Why is this effect so temporary? The cost of varying the date of a crime may be increasing in the time span from the optimal opportunity. Front loading the date of crime by a week may not be so costly, but committing many car thefts

or burglaries which should have taken place weeks or months later in accordance with optimal opportunities may be very costly.<sup>10</sup>

## 7. Conclusions

The results from earlier empirical studies addressing the problem of causality between punishments and crime are very mixed. The previous studies most similar to ours, Hjalmarsson (2009) and Lee and McCrary (2016), find no or only a very small deterrent effect of punishment.

Using Swedish data based on a legal system that generates a sharp increase in penalties at an individual's 21st birthday, we find a very large deterrent effect from harsher punishment one week after the 21st birthday. We also find a corresponding increase in crime the week before one's 21st birthday. This indicates that individuals take incentives into account and bunch their decisions to commit crime within a certain time period. In other words, they reallocate, or displace, crime in time.

We do not observe any long-term effects. A possible background to the large but short-lived effects is that it is costly to reallocate crime in time. Our results reveal that for offenders, it is not worthwhile, or even possible, to frontload crime from a more distant future. An implication of our analysis is that the reductions in punishment analyzed here do not seem to have negative welfare consequences in terms of higher costs of crime.

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<sup>10</sup> Jacob et al. (2007), who also show evidence of intertemporal substitution, instead argue that property crime is displaced through an income effect, and violent crime is displaced due to the diminishing marginal utility of violence (i.e., an offender may "settle a score" one week, which is then followed by a week where less utility is derived from using violence).



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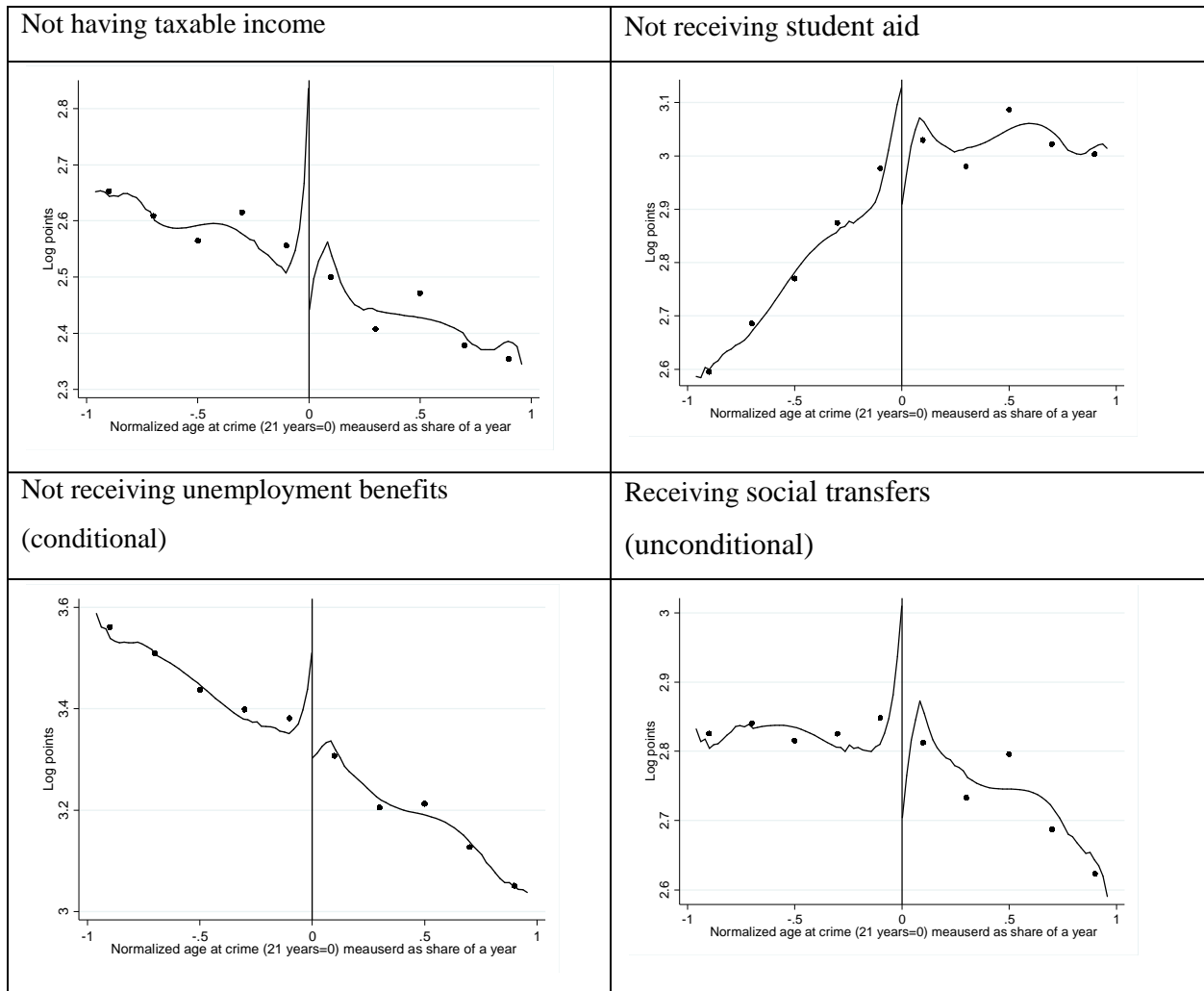
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## Appendix

**Figure A1.** Number of index crimes (log scale) close to the 21st birthday for sub groups.



Note: Zero denotes 21 years old at the date of the crime. The dependent variable is the logarithm of the number of sentenced individuals who committed the crime on a certain date. 1.0 years around the threshold is used and the plotted points are conditional means for a bin where the bandwidth is 0.2 of a year. The solid line is the predicted value of a fifth-degree smoother with a rectangular kernel and a bandwidth of 0.7. All outcomes are constructed as of the year before the crime.

**Table A1.** Index crimes and corresponding Swedish crimes.

Index crime type US	Swedish law	Chapter	Paragraph	No. of obs. +-0.22 of the 21st birthday	No. of obs. +-0.22 of the 21st birthday
<b>Index crimes</b>				4920	100
Aggravated assault	BRB	3	5-6	1639	33,31
Burglary and Larceny	BRB	8	1-4	2639	53,64
Forcible rape	BRB	6	1-4	44	0,89
Murder	BRB	3	1-4	30	0,61
Robbery	BRB	8	5-6	178	3,62
Arson	BRB	13	1-2	21	0,43
Motor vehicle theft	BRB	8	7	369	7,5

**Table A2.** Index crimes with control variables.

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

Note: the control variables are birth month, year fixed effects and previous criminal history.

**Table A3.** Index crimes using only first time verdicts.

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

**Table A4.** Violence, aggravated assault.

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

**Table A5.** Burglary and larceny.

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

**Table A6.** Traffic (including drunk driving and driving under the influence of drugs).

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

**Table A7.** Drug-related crime.

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

**Table A8.** Non-index crimes.

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

**Table A9.** All crimes that yield less than 6 months in prison.

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

**Table A10.** Women Index crimes.

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

**Table A11.** Men Index crimes.

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



**Table A12.** Percentage change, all index crime age 16 to 18.

Age threshold	Order of polynom	Bandwidths in share of a year measured as 365.25 days					
		<b>0.02</b>	<b>0.06</b>	<b>0.1</b>	<b>0.14</b>	<b>0.18</b>	<b>0.22</b>
16	1	-0.038 (0.076)	0.015 (0.065)	0.007 (0.050)	-0.008 (0.042)	-0.016 (0.037)	-0.005 (0.033)
	2	-0.152* (0.081)	0.031 (0.082)	0.044 (0.070)	0.047 (0.063)	0.023 (0.056)	-0.001 (0.051)
	3	0.003 (0.138)	-0.049 (0.101)	0.008 (0.078)	0.013 (0.072)	0.048 (0.069)	0.039 (0.065)
<i>Obs.</i>		1,650	4,754	8,209	11,752	15,020	18,468
<i>No. days</i>		15	44	73	102	131	161
17	1	0.053 (0.059)	0.008 (0.044)	-0.022 (0.036)	-0.021 (0.032)	-0.016 (0.030)	-0.008 (0.027)
	2	0.156*** (0.046)	0.015 (0.056)	0.025 (0.045)	-0.001 (0.043)	-0.009 (0.041)	-0.029 (0.037)
	3	0.022 (0.099)	0.054 (0.063)	0.018 (0.060)	0.017 (0.049)	-0.003 (0.049)	0.020 (0.045)
<i>Obs.</i>		1,294	3,814	6,240	8,775	11,388	14,034
<i>No. days</i>		15	44	73	102	131	161
18	1	0.219 (0.155)	0.022 (0.086)	-0.035 (0.064)	-0.015 (0.054)	-0.059 (0.047)	-0.080* (0.041)
	2	0.322 (0.236)	0.150 (0.133)	-0.016 (0.105)	-0.012 (0.084)	0.033 (0.072)	0.001 (0.066)
	3	-0.057 (0.159)	0.235 (0.189)	0.236* (0.132)	0.000 (0.123)	-0.039 (0.106)	0.035 (0.089)
<i>Obs.</i>		844	2,588	4,433	6,254	8,216	10,052
<i>No. days</i>		14	44	74	102	132	160

**Table A13.** Percentage change, all index crime age 19 to 20

Age threshold	Order of polynom	Bandwidths in share of a year measured as 365.25 days					
		<b>0.02</b>	<b>0.06</b>	<b>0.1</b>	<b>0.14</b>	<b>0.18</b>	<b>0.22</b>
19	1	-0.169 (0.171)	-0.061 (0.093)	-0.088 (0.069)	-0.062 (0.055)	-0.029 (0.051)	-0.032 (0.046)
	2	-0.160 (0.249)	0.002 (0.144)	-0.021 (0.112)	-0.072 (0.087)	-0.097 (0.075)	-0.065 (0.067)
	3	0.277 (0.177)	-0.171 (0.197)	-0.081 (0.151)	-0.045 (0.126)	-0.048 (0.109)	-0.089 (0.094)
<i>Obs.</i>		723	2,196	3,663	5,142	6,570	8,049
<i>No. days</i>		15	44	73	102	131	161
20	1	0.197* (0.096)	0.253** (0.097)	0.158** (0.072)	0.132** (0.062)	0.128** (0.057)	0.099* (0.052)
	2	0.243** (0.106)	0.101 (0.112)	0.300*** (0.104)	0.197** (0.086)	0.166** (0.079)	0.163** (0.073)
	3	0.218 (0.172)	0.209 (0.141)	0.123 (0.108)	0.305*** (0.109)	0.251** (0.099)	0.230** (0.091)
<i>Obs.</i>		565	1,598	2,725	3,862	4,910	5,998
<i>No. days</i>		15	43	73	103	131	161

**Table A14.** Placebo thresholds 22-25.

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

**Table A15.** Placebo thresholds 26-30.

Age threshold	Order of polynom	Bandwidths in share of a year measured as 365.25 days					
		<b>0.02</b>	<b>0.06</b>	<b>0.1</b>	<b>0.14</b>	<b>0.18</b>	<b>0.22</b>
26	1	0.337 (0.229)	0.057 (0.164)	0.010 (0.133)	-0.023 (0.113)	0.004 (0.095)	-0.023 (0.088)
	2	0.038 (0.224)	0.279 (0.212)	0.187 (0.181)	0.053 (0.170)	0.016 (0.160)	0.059 (0.137)
	3	0.072 (0.333)	0.151 (0.220)	0.196 (0.206)	0.303 (0.194)	0.117 (0.182)	-0.005 (0.181)
<i>Obs.</i>		176	488	828	1,177	1,483	1,803
<i>No. days</i>		0.072	0.151	0.196	0.303	0.117	-0.005
27	1	0.232 (0.303)	-0.012 (0.151)	-0.027 (0.121)	-0.136 (0.109)	-0.116 (0.100)	-0.114 (0.088)
	2	0.035 (0.438)	0.227 (0.260)	-0.035 (0.183)	-0.022 (0.152)	-0.109 (0.137)	-0.120 (0.130)
	3	0.408 (0.531)	0.155 (0.336)	0.343 (0.278)	0.153 (0.211)	0.082 (0.182)	-0.007 (0.162)
<i>Obs.</i>		139	437	705	978	1,268	1,534
<i>No. days</i>		15	44	73	102	131	161
28	1	0.501* (0.253)	0.073 (0.170)	0.258* (0.134)	0.190 (0.122)	0.117 (0.114)	0.153 (0.104)
	2	0.820** (0.317)	0.423* (0.240)	0.154 (0.209)	0.245 (0.168)	0.239 (0.152)	0.153 (0.141)
	3	0.459 (0.431)	0.529* (0.289)	0.156 (0.282)	0.168 (0.234)	0.298 (0.206)	0.315* (0.182)
<i>Obs.</i>		131	351	613	872	1,100	1,350
<i>No. days</i>		15	43	73	103	131	161
29	1	0.269 (0.288)	-0.041 (0.227)	0.120 (0.168)	0.123 (0.146)	0.237* (0.142)	0.228* (0.119)
	2	0.152 (0.273)	0.047 (0.278)	-0.038 (0.244)	0.079 (0.201)	0.002 (0.188)	0.092 (0.170)
	3	-0.884** (0.382)	0.251 (0.282)	0.016 (0.272)	-0.093 (0.258)	0.086 (0.235)	0.026 (0.216)
<i>Obs.</i>		112	310	506	702	881	1,064
<i>No. days</i>		15	44	73	102	131	161
30	1	0.800*** (0.259)	0.242 (0.218)	0.231 (0.160)	0.183 (0.138)	0.241* (0.125)	0.159 (0.114)
	2	1.363*** (0.311)	0.799*** (0.247)	0.315 (0.241)	0.314 (0.202)	0.142 (0.188)	0.238 (0.164)
	3	1.305** (0.463)	1.247*** (0.234)	0.817*** (0.258)	0.417 (0.267)	0.507** (0.223)	0.289 (0.230)
<i>Obs.</i>		79	258	421	576	744	894
<i>No. days</i>		14	44	74	102	132	160