DISCUSSION PAPER SERIES

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Anna Bindler
Randi Hjalmarsson
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Andreea Mitrut

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Anna Bindler
University of Cologne, University of Go-thenburg and CEPR

Nadine Ketel
Vrije Universiteit Amsterdam, CEPR, IZA and Tinbergen Institute

Randi Hjalmarsson
University of Gothenburg and CEPR

Andreea Mitrut
University of Gothenburg

ABSTRACT

Discontinuities in the Age-Victimization Profile and the Determinants of Victimization*

Many rights are conferred on Dutch youth at ages 16 and 18. Using national register data for all reported victimizations, we find sharp and discontinuous increases in victimization rates at these ages: about 13% for both genders at 16 and 9% (15%) for males (females) at 18. These results are comparable across subsamples (based on socio-economic and neighborhood characteristics) with different baseline victimization risks. We assess potential mechanisms using data on offense location, cross-cohort variation in the minimum legal drinking age driven by a 2014 reform, and survey data of alcohol/drug consumption and mobility behaviors. We conclude that the bundle of access to weak alcohol, bars/clubs and smoking increases victimization at 16 and that age 18 rights (hard alcohol, marijuana coffee shops) exacerbate this risk; vehicle access does not play an important role. Finally, we do not find systematic spillover effects onto individuals who have not yet received these rights.

Corresponding author:
Nadine Ketel
Department of Economics
University of Gothenburg
P.O. Box 640
40530 Gothenburg
Sweden
E-mail: nadine.ketel@economics.gu.se

1. Introduction

Most crimes are not victimless. According to the U.S. National Crime Victimization Surveys, about 4% of respondents report being crime victims in just the last six months. Similar or larger victimization rates are seen worldwide and over time in the International Crime Victimization Survey (Bindler et al., 2020).[1] Lifetime victimization rates are clearly larger. Moreover, the potential costs of victimization – to both individuals and society – are large. Recent research finds that victimization significantly harms individual mental and physical health and labor market outcomes of adults (e.g., Bindler and Ketel, forthcoming; Ornstein, 2017; Cornaglia et al., 2014; Currie at al., 2018). Research on the causal effects of juvenile victimization has to date focused on rather extreme events such as mass or school shootings (e.g., Bharadwaj et al., forthcoming; Cabral et al., 2021) and on more indirect exposure (e.g. at the neighborhood level or in the school vicinity) to local crime. The findings suggest that even such indirect exposure harms juvenile education outcomes, including school attendance, test performance, and graduation rates (e.g., Monteiro and Rocha, 2017; Foureaux-Koppensteiner and Menezes, forthcoming; Chang and Padilla-Romo, 2020) and that being *exposed* to crime may lead to inequalities in opportunity for youth. Yet, the *direct* consequences of own victimization during this critical point in the lifecycle may be even more severe.

But, just a handful of papers study the *causes* of victimization. These papers largely focus on adults and highlight (i) the importance of precautionary behavior for property crimes (e.g., Ayres and Levitt, 1998; Gonzalez-Navarro, 2013; van Ours and Vollaard, 2016; Vollaard and van Ours, 2011), (ii) being in the same environment as potential offenders, whether it is sports events and related parties (Lindo et al., 2018; Rees and Schnepel, 2009), households (Card and Dahl, 2011), or schools (Anderson et al., 2013), and (iii) most recently, the role of alcohol (Chalfin et al., forthcoming). Victimization surveys do, however, point to a number of correlates of victimization, including age. Age-victimization profiles in developed countries share a stylized shape: victimization risk increases as youths approach adulthood, but starts to decrease in their early twenties. Figure 1 shows this pattern for both the U.S. and the Netherlands, the context of the current analysis. This paper contributes to our understanding of juvenile victimization by studying the particular shape of the age-victimization profile at these ages, much as studies of the age-crime profile have informed our understanding of the causes

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[1] U.S. statistics are based on the authors' analysis of NCVS files (National Crime Victimization Survey, 2005-2017 waves, Concatenated File, ICPSR 37198). International Crime Victimization Survey statistics are presented in Bindler et al. (2020); on average, about 15% report being a victim in the last year of thefts of cars, motorcycles, bicycles, theft of property, burglary, robbery, sex offenses, assaults, and/or threats.

of crime.[2] Specifically, using unique register data for all reported victimizations in the Netherlands between 2005 and 2018 linked to national population registers (including rare information about the *exact* date of birth), our paper starts with the following observation: Over and above the upward trend in victimization risk, there are sharp and discontinuous increases in victimizations at just two ages on the age-victimization profile – 16 and 18 – while no such discontinuities are observable at other ages, such as 17.

Cohen and Felson's (1979) *routine activities hypothesis* can help explain both the shape in the age profile as well as the age 16 and 18 discontinuities in victimization risk. In this framework, crime rates are a function of the extent to which potential offenders and (unsupervised) victims interact, which depends on their geographic and temporal activities.[3] As children get older, they gain more independence, leading to more unsupervised time outside of the home and during different times of the day/night. Thus, the behavior and routine activities (i.e., what one does and when, where, with whom) of teenagers change in ways that may increase their propensity to become crime victims (Hindelang et al. 1978; Miethe et al. 1987). Moreover, multiple rights are conferred upon youth in the Netherlands at 16 and 18, which can sharply change their behaviors and associated victimization risks at these age thresholds. The key rights granted relate to mobility (driving mopeds at 16 and motorcycles and cars at 18), substance use (entering coffeeshops to purchase marijuana at 18), and purchasing alcohol and tobacco.

Identifying a causal effect between behaviors, such as drinking, and victimization is challenging due to both correlated unobservables (i.e., many factors change over the life-cycle that affect both routine activities and victimization risk such as risk preferences or peer groups) and reverse causality (i.e., victimization can affect behavior; Bindler et al., 2020). We leverage exogenous shocks to youths' rights at key birthdays as well as reforms to minimum legal drinking ages to overcome these challenges and identify how changes in one's behaviors and routines affect the risk of becoming a crime victim.

This paper makes four important contributions to the small existing literature on the causal determinants of victimization. First, in contrast to previous research, we take a broader perspective on victimization than focusing on a single factor: we can study many factors (driving, environment, alcohol, marijuana) in a unified (reduced form) framework. Second, to

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[2] Researchers have used the age-crime profile, which has been remarkably steady over time (see Bindler and Hjalmarsson, 2017), to understand crime determinants. Papers link the age-crime profile to compulsory schooling ages and school dropout; see, e.g., Cook and Kang (2016), Landerso et al. (2017), and Bell et al. (2018).
[3] Similar notions are formalized in the economic model of (street) crime by Balkin and McDonald (1981).

the best of our knowledge, this is the first paper to study the causal determinants of victimization using population-wide register data, which allow us to study the extensive margin effect of 'rights' on the propensity to be victimized as well as whether there are spill-over effects of these rights and unequal effects across population groups. The latter informs us on whether the estimated effects should be interpreted as local effects for a particular group or allow for a broader interpretation as population-wide averages.[4] Third, using multiple sources of identifying variation (multiple age thresholds and reforms) and high-quality register data allows us to provide credible causal evidence that speaks to the shape of the age-victimization profile. Our design is similar to Chalfin et al.'s (forthcoming) regression discontinuity analysis of the minimum legal drinking age (21) on victimization in selected U.S. cities; the authors find effects for a subset of offences which correspond to a single point (close to the peak) on the age-victimization profile. Our analyses of age 16 and 18 rights inform us on the determinants of victimization for a younger (less studied but highly vulnerable) population as well as multiple points on the upward slope of the profile. Finally, our analyses of how the discontinuities in victimization rates change with reforms to the minimum legal drinking age shed light on the optimal timing of the granting of these rights.

Our analysis begins by collapsing the data to the date-of-birth cohort by calendar week level and using regression discontinuity (RD) designs to formally estimate the effect on victimization risk of reaching the age 16 and 18 birthday thresholds, over and above any age-specific victimization trends. The chance of victimization increases significantly by about 13% for both males and females at 16, and about 9% for males and 15% for females at 18, respectively. The results are robust to the choice of functional form (linear or split-linear trend), parametric versus non-parametric estimation and bandwidth, as well as the inclusion of birthday celebration controls. Using supplemental victimization surveys, we show that the results are not driven or confounded by changes in reporting behavior. The increases in victimization risk are largely driven by *property* offenses (the by far largest group of offenses) and *other* offenses. They are not, however, driven by vehicle-related offenses, which allows us to rule out a mechanical effect of getting a driver's license on victimization (e.g., one cannot have a car stolen if they do not own a car).

The baseline estimates represent reduced form intent-to-treat effects: all individuals gain access to a 'bundle' of rights, but not everyone will take all of them up. We next turn to

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[4] Other studies (e.g., Chalfin et al., forthcoming) focus on the timing or number of victimizations in a sample of victims.

decomposing the relative importance of the various rights in the bundle. Heterogeneity analyses across types of locations suggest that victimization risk while out increases relative to at home. This is consistent with multiple channels that change how much an individual goes 'out', including licenses and age thresholds to purchase alcohol and enter bars, clubs, or coffeeshops.

To learn more about these channels, we next make use of cross-cohort variation in the rights granted at ages 16 and 18 that arises from a 2014 minimum legal drinking age (MLDA) reform. The reform increased the MLDA for 'weak' alcohol (as well as tobacco purchases and informally bar/club access) from 16 to 18. Cohorts born between 1990 and 1995 gained access to 'weak' alcohol at age 16 (as well as to tobacco and to bars/clubs) and 'hard' alcohol at age 18, while the 1998 and 1999 cohorts gained access to all – any type of alcohol, tobacco, and bars/clubs – at 18. Applying our regression discontinuity framework separately to these two cohort groups, we find systematic increases in the chance of victimization at 16 for the 1990-1995 cohorts but not the 1998-1999 cohorts. This allows for two conclusions: Access to 'weak' alcohol, tobacco and bars/clubs at age 16 matters, while other rights granted at this age (especially vehicle-related rights) do not seem to be a prominent factor. At the age 18 cutoff, victimization risk increases in both groups but the point estimates are larger for the 1998-1999 cohorts. One takeaway is that the *additional* rights (hard alcohol, coffee shops, non-moped vehicle licenses) granted at age 18 for the pre-reform cohorts 1990-1995 are also important. Estimates from a difference-in-discontinuity specification confirm that there is a discontinuous and significant change in victimization risk at age 16 for the pre-reform relative to the post-reform cohorts, whereas the increase in risk at age 18 is not (for females) or only marginally (for males) significantly different across the pre- and post-reform cohorts. The results based on the MLDA reform suggest that access to weak and hard alcohol, tobacco, and bars/clubs might be the most important channel driving the increases in victimization risk.

Our analyses thus far only suggest that teenagers change their behavior in ways that increase their victimization risk at these age thresholds. To provide specific evidence of such behavioral changes, we use supplemental survey data (from the National Health Survey and the Mobility Survey) on drinking behavior, drug consumption, mobility behaviors and education- and work-related behaviors. Though descriptive (due to limited sample sizes), the results confirm that a major driver of the reduced-form estimates is a combination of drinking alcohol and going out. They also suggest no significant changes in school enrollment or working around these cutoffs.

The regression discontinuity design identifies the local average treatment effect for the compliers. To speak towards the generalizability of the results on the one hand and to whether

or not there are differential effects across population groups, we conduct a number of further heterogeneity analyses. We re-estimate our baseline RD design for sub-samples with different baseline risks of victimization based on socio-economic background and neighborhood characteristics: single versus dual parent households, low versus higher income households, low versus high crime neighborhoods, and urban versus rural municipalities (with and without coffee shops). Though there are pre-existing inequalities in victimization risk, reaching ages 16 and 18 significantly (and comparably) increases victimization for almost every subsample. Access to these rights does not exacerbate (nor mitigate) the pre-existing inequalities in victimization risk. Moreover, the homogeneous nature of the estimates speaks to their potential external validity and supports a broader interpretation of the results in terms of average treatment effects that represent more than just one part of the population.

We find that victimization risk significantly increases at ages 16 and 18, and provide strong evidence that a major underlying behavioral channel is going out with access to alcohol – a likely group behavior. Thus, the final part of our analysis studies potential spillover effects. As there is no information on friends' networks in the register data, we look at school cohorts and siblings as likely candidates for spillover effects. First, leveraging the cutoff date for school entry (October 1), we can compare the relatively old in the school cohort (born in October) to the relatively young (born in September). Second, we study whether the older sibling crossing the respective age threshold has an impact on the (next) younger sibling, maybe due to changes in parental behavior and supervision. Our results suggest that there are no systematic spillovers for the peer groups observed in our analysis who themselves are not eligible for these rights yet. This does not rule out other types of social multiplier effects.

The remainder of the paper is structured as follows. Section 2 describes the data and raw age-victimization-profiles and documents the rights granted at ages 16 and 18 in the Netherlands. Section 3 presents the RD design and reduced form results. Section 4 discusses the underlying mechanisms and the minimum legal drinking age reform. Section 5 presents heterogeneity analyses that support a generalized interpretation of our findings. Section 6 discusses the spillover analyses and Section 7 concludes.

2. Data and Institutional Background

2.1. Dutch Victimization Registers and Analysis Sample

Our core analysis uses individual victimization data from Dutch registers. The victimization register data are available from 2005 to 2018, and are based on files of crime incidents reported to the police. We use information on the incident date(s) and details (offence and location type)

as well as victim identifiers (anonymized social security numbers available in the vast majority of cases). Appendix Table 1 lists each of the approximately 95 detailed offence categories (sorted by number of victimizations per offense) as well as the four broad categories into which we group offenses for our main analyses: property, violent, violent property, and other. Offense location categories are listed in Appendix Table 2.

The victimization data can be linked to national registers containing demographic and socioeconomic information, which we use to investigate differences in victimization risk across subgroups. Importantly, having data for both victims and 'non-victims' allows us to study the extensive margin risk for victimization for the entire population rather than just the intensive margin in a sample of victims. Both of these features set our paper apart from the existing literature. Crucial for our research design, we have access to the restricted versions of these registers, which include the exact date of birth, for the 1985 to 2006 birth cohorts (who were between ages 12 and 20 from 2005 to 2018). Our core analyses focus on those born from 1990 to 1999, as these cohorts reach the age thresholds of 16 and 18 during the years in which victimization data are available and can be observed for at least one year before and after each birthday (i.e., ages 15 to 19). Since victimizations cannot be observed for those not living in the country, we restrict our sample to individuals registered with a valid address in the Netherlands at both 16 and 18. Appendix Table 3 shows that the number of males and females in each birth cohort fluctuates around 100,000, with slightly fewer females than males. Thus, our main analyses include about 2 million individuals born from 1990 to 1999. For computational reasons, we will collapse these data to day-of-birth cohorts and week (relative to one's birthday) of victimization.

Appendix Table 3 also presents the share of individuals who were victimized at least once between ages 15 and 19. Overall, 21.7% of females and 22.8% of males were victimized at least once in the age bracket. These shares trend down over birth cohorts and time: age 15-19 victimization rates for females (males) in the 1990 cohort are at 22.9% (25.4%), but only 15.7% (16.9%) in the 1999 cohort. The fact that this decline in victimization rates starts to kick in with the 1995/1996 birth cohorts raises the possibility that the minimum legal drinking age reform studied later plays a role even in the aggregate.[5]

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[5] To assess whether the decline in cohort level victimization rates might be due to decreasing crime trends, we also show victimization rates between ages 20 and 24 for cohorts born between 1985 and 1994. This allows us to compare victimization rates for 15-19-year-olds and 20-24-year-olds in the observation windows 2005-2009 until 2014-2018. While victimization rates at ages 20 to 24 also start to decline around the 2010-2014 observation window, this decrease is less pronounced than for the cohorts affected by the minimum legal drinking age reform.

2.2. **Descriptive Statistics and the Raw Age-Victimization Profile**

Figure 2 presents the number of victimizations per week (relative to one's birthday week) between ages 13 and 22 for males (in blue) and females (in red). Dashed vertical lines mark the 16th and 18th birthdays. Panel A presents this age-victimization profile for the 1992-1996 cohorts, for whom we fully observe victimization between 13 and 22, while Panel B presents the profile for the analysis sample (1990-1999) cohorts. Due to the availability of victimization register data, the larger sample of analysis cohorts is not balanced throughout the 13 to 22 age range. However, for these cohorts, victimization is fully observed between ages 15 and 19 (shaded in gray), i.e., the sample range on which our estimations are based.

Three patterns stand out in Figure 2. First, individuals are on the upward sloping part of the age profile until about age 19, at which point the profile flattens out and decreases slightly.[6] The slightly higher levels for males could be partially explained by the somewhat larger male cohorts. Second, there are sharp jumps in the number of victimizations at 16 and 18 for both males and females. Third, similar jumps in the number of victimizations cannot be seen at any other birthday, including age 17.

Table 1 presents victimization rates (by gender and broad offense category) in the two years surrounding the 16th (Panel A) and 18th (Panel B) birthdays after collapsing the data to the date-of-birth by week level. That is, these measures represent weekly shares of victimizations averaged over all date-of-birth cohorts in the sample (1 January 1990 until 31 December 1999). Since we look at *weekly* victimization rates, the resulting rates are low overall, with 0.09% and 0.12% of females per date-of-birth cohort victimized per week in the years around their 16th and 18th birthdays, respectively. The corresponding statistics for males are 0.10% and 0.13%.[7] This table also shows that though the male versus female victimization rate is almost identical before the 16th birthday, a 0.01 percentage point gap (with male rates larger than female rates) opens up in the year after 16 and persists until after they turn 18. Finally, these statistics also make clear that victimization rates are largest for property offenses, followed by violent, other, and then violent property offenses. Victimization of violent property offenses for this age group is almost non-existent (and therefore excluded from most of the remaining analysis or instead pooled with violent offenses), while that for other offenses is also an order of magnitude smaller than the level of property crime victimization.

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[6] The steeper decrease in Panel B is likely due to the unbalanced nature of the data after age 19.

[7] To convert these shares into victimizations per week (as shown in the age-victimization profiles), multiply with the date-of-birth cohort size (see Appendix Table 3 for year of birth cohort sizes) and by the number of day-of-birth cohorts (over birth years 1990-1999).

2.3. Background and Framework: Legal Rights Gained at Ages 16 and 18

The discontinuities in victimization risk observed in the raw data in the weeks surrounding individuals' 16th and 18th birthdays can be looked at in the context of the 'routine activities' and 'exposure' framework outlined in the introduction (Cohen and Felson, 1979; Balkin and McDonald, 1981). If routine activities sharply change at the age thresholds as rights are granted to youth, this can alter the risk of becoming a victim of crime either directly (e.g. consuming alcohol, driving a car) or indirectly, with increasing potential exposure to new environments and peers that comes with exercising these rights. Table 2 lays out the various rights granted at ages 16 and 18 in the Netherlands and emphasizes reforms to the age thresholds as well as the regime to which each birth cohort is exposed.

Motor-Vehicle Related Age Thresholds

Youths may obtain a license to drive mopeds (or scooters) at 16 and motorcycles and cars at 18.[8] Vehicle licenses can increase victimization (i) directly, in that certain offenses are vehicle specific and one cannot be a victim of car theft without a car, or (ii) indirectly since a license can change one's behavior and environment in ways that impact victimization risk (e.g., by increasing the radius of movement or frequency at crowded places where alcohol is served). Though vehicle rights have not been studied with respect to victimization before, a recent paper by Huh and Reif (2021) shows large mortality rises in the US around the minimum legal driving age cutoff, which are driven by increases in motor vehicle fatalities.[9]

Substance Use: Alcohol

Carpenter and Dobkin (2011) and Bindler et al. (2020) survey the channels linking alcohol to criminal behavior and victimization, respectively. Alcohol has pharmacological effects on an individual's emotional responses and judgement and decision-making abilities; the nature of these effects depends on the level of intoxication. At relatively low levels, one may by extra happy, sociable, and disinhibited, while severe mental and physical impairments as well as aggression can appear with higher levels. Alcohol consumption can also affect victimization risk via an environmental channel, i.e. if it affects whether one is out (e.g., at a bar) or at home, at day or night, and surrounded by others consuming alcohol. Though there is evidence of a

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[8] After November 1, 2011 (affecting the 1995-1999 sample cohorts), youths could start driving lessons at 16.5 and take the exam from 17; earlier cohorts could not start lessons until 18, potentially delaying the start of driving.
[9] Our analysis conditions on the sample of individuals living in the Netherlands at their 16th and 18th birthdays. Individuals who die before the age of 18 are, because of this restriction, not included in our sample. On average around 40 juveniles (aged 15-20) are involved in a fatal traffic accident per year (Source: Statistics Netherlands).

correlation between alcohol consumption and victimization (e.g., Champion et al., 2004 and Felson and Burchfield, 2004), there is little *causal* evidence besides Chalfin et al. (forthcoming). Carpenter and Dobkin (2015) attribute this dearth to the lack of high-quality victimization data.

One relevant determinant of juvenile alcohol consumption is the minimum legal drinking age (MLDA). Compared to the U.S., but like much of Europe, the MLDA in the Netherlands is relatively low. Prior to January 1, 2014, youths aged 16 or older were allowed to purchase weak alcoholic beverages (with less than 15% alcohol), such as beer or wine, while there were no restrictions on alcohol consumption. After age 18, individuals could purchase any kind of alcohol (including hard alcohol). A 2014 reform raised the purchase age for *all* alcoholic beverages to 18. Moreover, individuals under 18 can be fined 45 Euros if found carrying alcohol on streets, stations, or in shops or bars. Enforcement is intended to be strict, as everyone under 25 as well as all members of a group (even if the purchaser is over 25) are required to show identification upon purchase. Finally, the law obligates municipalities to have a formal prevention and enforcement plan for alcohol.[10] The reform is timed such that all individuals born in 1990-1995 and 1998-1999, respectively, face MLDAs of 16 and 18.[11]

Substance Abuse: Cigarettes

The legal age to purchase cigarettes parallels alcohol; it was raised to 18 from 16 in 2014. There is, however, less evidence of a link between cigarette smoking and crime than with regards to alcohol: Cigarette smoking does not affect an individual's judgement and decision-making abilities in the same way as alcohol.[12] However, like alcohol, cigarette smoking can potentially affect victimization risk by affecting an individual's environment (changing where people are and who they are with), especially because for most cohorts in our sample (born after July 1993) smoking indoors was banned at public places, bars or clubs.[13]

Substance Use: Drugs and Coffeeshops

The Netherlands is well known for its marijuana policies, and its 'coffeeshops', which are licensed to sell cannabis in small quantities. Though there is no formal age limit on marijuana

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[10] Source: https://www.vnpf.nl/media/files/brief-wijziging-drank-en-horecawet-per-1-januari-2014.pdf

[11] The 1996 and 1997 cohorts experience both regimes, i.e. they are given the right to purchase alcohol at age 16 but have this right removed in 2014; they regain the right to purchase alcohol upon turning 18 in 2015 and 2016, respectively. In our later analyses of the reform, we drop these two cohorts from the sample.

[12] Though we are unaware of any papers that causally link cigarette smoking to one's own criminal behavior, research (Brennan et al., 1999) does link prenatal maternal smoking with higher adult crime of the children.

[13] Prior to July 1 2008, smoking was only banned in places of public access (from 2002 onwards).

consumption, one cannot enter a coffeeshop to buy and consume marijuana until age 18. Coffeeshops typically also serve food, but cannot sell hard drugs or alcohol. Policies to help control marijuana consumption have recently been implemented, including banning marijuana on school grounds (September 2013), restricting coffeeshop sales to residents (January 2013), and constraining coffeeshops to be at least 350 meters from a school (January 2014).

Like alcohol, marijuana consumption can have pharmacological effects that directly increase victimization risk: these effects can range from a typical euphoric high to, on occasion, severe anxiety and panic (especially in naïve users), and affected spatial perceptions and cognitive and psychomotor performance (Ashton, 2001). Victimization risk can also increase, however, with the change in environment and peers that comes with coffeeshop access. Though researchers have studied the effect of marijuana consumption and related laws on crime (Adda et al., 2014), education (Marie and Zölitz, 2017), alcohol consumption (Williams et al., 2004), and traffic accidents (Hansen et al., 2018), we are unaware of any studies on victimization risk.

Age Restrictions in Bar, Club, Casino Admittance

One factor highlighted in the routine activities hypothesis is *where* individuals are. At age 16, individuals are officially given the right to enter a club or bar; in practice, many clubs/bars raised this to 18 upon the introduction of the new alcohol age limit in 2014. The minimum age to enter casinos is 18 but, at the casinos' discretion, these age limits are often set higher.

Compulsory Schooling Laws

Schooling in the Netherlands is compulsory from age 5. For our oldest cohorts, a child could drop out upon completing 12 *full* years of schooling or *finishing* the school year in the year one turns 16. From August 1, 2007 (i.e., for cohorts born in 1991 or later), the drop out rule became somewhat stricter; if the amount of education does not give the individual what is called a 'starting qualification' – not all 12 years of schooling do this – then the individual must stay in school until age 18 or until receiving this qualification. These rules imply that there should not be, overall, a discontinuity in drop out behavior immediately upon turning 16. This statement is supported by our analyses of the Dutch mobility survey (discussed in more detail later); there is no observable discontinuity in school enrollment around ages 16 or 18 (see Figure 8).

Other Age Specific Thresholds

Other rights granted at 16 and 18 could affect victimization risk to different degrees. The Dutch criminal justice system treats individuals as juveniles if they are below 16 and as an adult if

they are above 23. The court has discretion between 16 and 23, but the norm is to refer individuals older than 18 to the adult system. Though Becker's (1968) economic model of crime suggests that expected harsher punishment at these age cutoffs can affect criminal behavior, it is less clear why it would affect the risk of victimization.[14] However, if one expects criminality to decrease at these cutoffs and to the extent that there is a victim-offender overlap, this would (if anything) suggest a decrease in victimization risk.[15]

Financial rights gained at 18 include opening a bank account, getting a loan, and starting a company; one must pay their own health insurance, can get rental subsidies, and can apply for some benefits. Further, one can start working full days and get a formal contract at 16; at 18, one can also work at night. Though at least some of these rights could in theory make youths more likely to be crime targets (e.g. bank/credit cards can increase the risk of being a victim of fraud online or these rights could result in changing environments), it is hard to imagine that this would happen discontinuously at 16 (or 18) or that offenders would be aware that a potential victim reached the respective birthday. However, we cannot fully dismiss that getting these rights would affect victimization risk or reporting by increasing the likelihood that a youth moves out of their parent's home; we return to this question in Section 3.1.[16]

Summary

Multiple rights are granted at ages 16 and/or 18, which may be related to victimization risk by (i) changing what one does, (ii) where one does it, (iii) the peer group, and (iv) the extent of supervision. The key rights granted upon turning 16 are driving a moped, being allowed to work, and for the cohorts born 1995 or before, being allowed to purchase weak alcohol and tobacco and enter bars and clubs. At 18, one gains the right to drive motorcycles/cars, purchase hard alcohol, enter marijuana coffeeshops, work at night, and for the later cohorts, purchase weak alcohol and tobacco and enter clubs and bars. Our empirical analysis attempts to disentangle the relative importance of these various rights around the two age thresholds.

3. Discontinuities in the Age-Victimization Profile

This section uses a sharp regression discontinuity design to formally estimate the reduced form

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[14] See Damm et al. (2017), Hjalmarsson (2009), Lee and McCrary (2017), and Loeffler and Gundwald (2015).

[15] Jennings et al. (2012) reviews the victim-offender overlap literature, which has received little attention in economics (with the exception of Entorf, 2015).

[16] At 18, individuals can get married (if both are 18) and foster care arrangements typically end. At 16, individuals can engage in sexual activity (including with a prostitute) and can get the pill without parental consent. This is potentially not 'sharp' since sex before 16 is legal if both partners are below 16 and mutually consent.

effect of turning 16 or 18 on the probability of victimization, over and above any underlying age-specific victimization trends.

3.1. Regression Discontinuity Design
3.1.1. Empirical Specification

As mentioned above, we collapse our data from the individual to the date-of-birth cohort level and from the daily to the weekly level. That is, we create four *date-of-birth cohort by calendar week* panels: one each around the 16th and 18th birthday for females and males, respectively. Besides computational reasons, collapsing the data to the date-of-birth level avoids the issues associated with clustering on a (potentially discrete) running variable (see Lee and Card, 2008; Kolesar and Rothe, 2018). Our outcome of interest is the *percentage share* (the share multiplied by 100) of individuals who are victimized in a given calendar week per date-of-birth cohort.

We begin by plotting the data: Figure 3 shows the average share victimized per week around the key birthdays for females (Panel A) and males (Panel B). The red lines are simple linear fits of the data, and the vertical blue lines mark the key birthdays. Three observations stand out: First, as in the age-victimization profiles shown earlier, there are clear discontinuities in the probability of victimization at age 16 and 18. Second, there is no such discontinuity at 17, which suggests that there are no discontinuities in unobservables related to birthdays in general. Moreover, the lack of even a temporary jump at 17 suggests that birthday effects per se will not be important in our analyses. Finally, the plots suggest a functional form with a linear trend in the running variable that we allow to differ before and after the cutoff birthdays.

The baseline specification taken to the data is presented in equation (1), where the dependent variable is the percentage share of individuals from date-of-birth cohort c who are victimized of offense category o in week t. The analysis is always conducted separately by gender g (where g=1 for females, = 0 for males).

$$(1) \quad Y_{ct}^{og} = \beta_0 + \beta_1 D\{t \geq 0\}_c + f(t) + X_c^{bday}\beta_2 + \delta_{yob} + \delta_{mob} + \varepsilon_{ct}$$

Our running variable t is measured relative to the birthday of interest; that is, t is normalized to zero in the calendar week of date-of-birth cohort c's 16th or 18th birthday, and t leading up to and following the birthday is negative and positive, respectively. Based on the observations in Figure 3, our baseline specification allows for a split-linear trend in relative time: $f(t) = \gamma_1 * t + \gamma_2 * t * D\{t \geq 0\}_c$. The coefficient of interest, β_1, is the parameter associated

with a dummy indicating that t is the week of or after the birthday. We allow for the possibility of temporary effects of birthday celebrations and/or events on behavior by including controls (X_c^{bday}) for whether week t is the birthday week and whether the birthday falls onto a weekend day. Finally, we include month of birth, δ_{mob}, and year of birth, δ_{yob}, fixed effects to control for seasonality and trends in crime over time as well as for national reforms and trends that differentially affect one cohort versus another.

Three points are important for the interpretation of the parameter of interest β_1. First, as all individuals become eligible for the respective rights at ages 16 or 18, the design is sharp. But, as not all individuals take up these rights, β_1 captures the intention-to-treat effect, i.e., the effect of simply being eligible for these rights. Second, we estimate the effect of the entire 'bundle' of rights granted at each age threshold. Thus, β_1 captures the reduced form effect of turning 16 or 18. Third, the RD design identifies the local average treatment effect parameter (LATE) in the neighborhood surrounding the age cutoffs at which the rights are gained. This raises the question of whether our findings generalize (i) to the entire population or whether they are driven by specific complier groups and (ii) to ages beyond the cutoff. We will return to the external validity of our findings beyond the cutoffs later in the paper.

Our main analysis estimates equation (1) parametrically using (unweighted) OLS while imposing a split-linear trend with a fixed bandwidth of 26 weeks on each side of the cutoff and heteroskedastic robust standard errors (see Lee and Card, 2008; Kolesar and Rothe, 2018). We will demonstrate that the results are not sensitive to: the choice of bandwidth (52, 39, 13 weeks), clustered standard errors, functional form (simple linear trend versus split-linear trend), and a non-parametric approach using local linear regression with a triangular kernel (Hahn et al., 2001) with both a 26-week bandwidth as well as an optimal bandwidth estimator as in Calonico, Cattaneo and Titiunik (2014).[17]

3.1.2. Identifying Assumptions and Potential Confounders

This section discusses three issues that are relevant to causal identification in regression discontinuity (RD) designs. First, RD designs assume that there is no manipulation in the running variable. The running variable here is based on the date of birth, and there is little reason to believe that this would be systematically misreported in the Dutch register data. Second, using age as the running variable in an RD design implies that treatment is 'inevitable'

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[17] The CCT bandwidth selector assumes a continuous running variable. Nevertheless, our understanding is that the CCT bandwidth selector still performs reasonably well when the discrete running variable has many values. Some RD papers (e.g. Gelber et al., 2016) with day of birth as a running variable use the CCT bandwidth selector.

(Lee and Lemieux, 2010). In contrast to, for example, criminal behavior (with the prospect of harsher punishment at the age of criminal majority) or saving decisions, it is intuitively hard to imagine that youth intertemporally manipulate their own victimization. Put differently, considering victimization as an externality of the behavior of others makes anticipation unlikely. Moreover, the lack of significant birthday effects also suggests that there is no large intertemporal substitution in related behaviors. Finally, our difference-in-discontinuity specification in Section 4 does not find differential trends before age 16 for cohorts with a minimum legal drinking age of 16 compared to those with one of 18. Taken together, these arguments speak against anticipation effects being important in this context.

Perhaps the most fundamental identifying assumption is that no unobservables change discontinuously at the age 16 and 18 cutoffs. We already pointed out the plausibility of this assumption when describing the flat patterns around age 17. More generally, as we estimate the very reduced form effect of a bundle of rights granted at these ages, this assumption is less strong in our context than others.

Of potentially greater concern is whether reporting behavior changes at these birthday thresholds even if (true) victimization rates do not change. One channel for such a change in reporting behavior is if the *party responsible for reporting* changes at these age cutoffs. First, there could be age thresholds related to when one is allowed to report a victimization oneself; in the Netherlands, however, it is only when the child is below 13 that the parents must be informed if the child reports a crime. Second, if schools play a role in reporting victimization, then school drop-out or graduation could impact reporting. However, as we mentioned in Section 2.3, there is no discrete change in school exit at our birthday cutoffs (see Panel C of Figure 8). A third possibility, also mentioned earlier, is that financial rights conferred on youths, especially at 18, may increase the likelihood that a youth moves out of their parents' home. This could shift the burden of reporting to the youth from the parent if they now become the household head and constitute a threat to identification.[18] Yet, by age 17, more than 99.5% of youths who lived with their parents at 16 still do. Though moving rates after the 18th birthday are somewhat higher (about 0.15% for females and 0.08% for males per week), almost 96% (92%) of males (females) still live with their parents at 19. Accordingly, our results are robust to excluding those who move out from the analysis samples (results available upon request).

Another reporting-related concern is that victims may be less likely to report a crime

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[18] Moving could also affect victimization risk, though the direction is theoretically ambiguous; e.g., it could increase if individuals move to higher crime neighborhoods or decrease if there is less of value to steal.

before reaching the threshold if they were using some rights, particularly drinking, while underage. We believe that such systematic age-related under-reporting is unlikely in the Dutch context, because unlike in the U.S., it is not illegal to *consume* alcohol underage, but rather only to *purchase* alcohol.[19] In a similar European setting, Ahammer et al. (2021) suggest that under-reporting of alcohol consumption *below* the MLDA age of 16 does not seem to be a concern among Austrian youths.[20] Moreover, even in the US context where underage drinking is illegal, Chalfin et al. (forthcoming) do not find evidence of a change in victimization reporting in the National Crime Victimization Survey (NCVS) around the minimum legal drinking age of 21.

Finally, given the above discussions, it is thus not surprising that we do not find any empirical evidence that reporting rates conditional on victimization change around birthdays. Specifically, we use the 2005 to 2016 rounds of the Dutch victimization survey, which contains questions on reporting and month of victimization. Small sample sizes at the relevant ages do not allow for a formal RD design and necessitate aggregating the data to the age (in years) level. Panel A of Figure 4 presents the raw reporting rate at each age from 15 to 30 overall and by crime type (property versus violent) and gender. About 40% of victimizations are reported at almost every age. Property crimes are reported at a higher rate than violent crimes (about 50% versus 20%) but reporting rates are similar by gender. Given potential crime trends and variation in reporting rates across crime types (which could change with age), Panel B regresses out crime type by survey wave fixed effects (as in Chalfin et al., forthcoming). As in the raw data, there are no clear differences in the likelihood to report by age.[21]

3.2. Main Results: Discontinuities in Overall Victimization

Table 3 presents the results of estimating the baseline specification separately for males and females. Depending on the specification, the first row presents the coefficients associated with reaching the age threshold of 16 or 18. The coefficients for all control variables other than the month of birth and year of birth fixed effects are included in this baseline table, but, for ease of presentation, excluded from the remaining tables presented in the paper.

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[19] Data from the 2015 European School Survey Project on Alcohol and other Drugs (ESPAD), covering the cohort of students born in 1999, shows that about 49% of the youths in the Netherlands had *consumed* alcohol during the last 30 months (even though, for this cohort, the MLDA age was 18).

[20] Compared to the survey data, the authors show similar jumps around the age 16 MLDA cutoff in administrative hospitalization records due to alcohol intoxication which, compared to the survey responses on alcohol consumption, are less prone to biases due to misreporting or false recalling.

[21] In addition, we observe that the median time between the dates of offense and reporting (conditional on victimization) is one day. This statistic does not change across the age thresholds.

At age 16, the weekly percentage share of males and females victimized significantly increases by about 0.013. Though the magnitude of these estimates appears small, relative to the mean they are quite substantial, translating into a 12.7% and 13.2% increase in the chance of victimization (of any offense) for males and females, respectively. At the 18th birthday, the chance of victimization increases by 9.4% for males and 15.0% for females. Of note with respect to the controls is that (i) the birthday celebration effects are generally insignificant and small and (ii) the linear pre-birthday trend is never significant, while the post-birthday trend is significant in the weeks after the 16th birthday. One potential explanation of the higher post-birthday slope is an increasing take-up of the granted rights as the year after the 16th birthday progresses (either with more individuals taking up the rights at the extensive margin or with individuals exercising their rights with more intensity, e.g., as more peers have the same rights). In other words, reaching age 16 both immediately increases the risk of victimization but may also put individuals on a higher path of future victimization. This in itself is suggestive that the LATE parameter identified in the RDD carries a more general interpretation than just around the cutoff. We return to this notion in terms of complier groups in our heterogeneity analysis.[22]

Table 4 presents a series of robustness checks for each of these main results (males and females at both 16 and 18); Appendix Table 6 shows the corresponding means of the dependent variable for all subsamples here and in subsequent analyses. Panel A demonstrates robustness to the inclusion/exclusion of controls and the use of a global linear versus a split-linear trend. Panel B shows that the results are robust to bandwidths other than the baseline 26-weeks on either side of the birthday. All estimates (using 13-, 39-, and 52-week bandwidths) are significant at the 1% level. The coefficients are not particularly sensitive to the choice of bandwidth when we increase the bandwidths to 39 or 52 weeks; however, when we decrease it to 13 weeks (i.e., when we cut the sample size to more than half relative to the baseline), even though our coefficients remain significant, we see an increase in the standard error and a decrease in the effect sizes for the age 16 (but not the age 18) threshold. Panel C demonstrates robustness to an alternative, non-parametric estimation approach using local linear regression, with either a 26-week or optimal bandwidth estimate (ranging from 12 to 19 weeks depending on specification). Panel D shows that the results are insensitive to clustering standard errors at the day-of-birth-cohort level and the inclusion of day-of-birth fixed effects.

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[22] In results not shown in this paper, we vary our outcome variable to capture the intensive margin risk of victimization, i.e., the number of victimizations per week and day-of-birth cohort. The results are consistent in all respects with the extensive margin results discussed in this section. In terms of relative magnitudes, reaching age 16 (18) increases the number of victimizations by about 13% (9%) for males and 14% (15%) for females.

3.3. Discontinuities by Offense Type

Figure 5 plots the average victimization rates per week for property, violent (including violent property), and other offenses for females (Panel A) and males (Panel B). There are again visible discontinuities at the 16th and 18th birthdays for *property* offenses for males and females. For *other* offenses, they are visible at 16 and 18 for males and 18 for females. For *violent* offenses, discontinuities are harder to visibly discern. None are seen at the 17th birthday for any category.

Panel A of Table 5 presents the corresponding coefficients that result from estimating the baseline equation (1) for each broad offense category. At 16, the risk in property victimization significantly increases by 13% (18%) for males (females), while at age 18, it increases by 6% (14%) for males (females). Relative to mean victimization rates, the chance of victimization of other offenses increases by more than 30% at age 18 and for males at age 16; however, it is important to keep in mind that the mean victimization rate for other offenses is still quite low.[23] Significant increases in violent victimizations are found at the 18th birthday for males and females (the female effect is almost twice as large relative to the mean: 13% versus 7%) and for violent property offenses for males at the 16th birthday. Significant effects are also seen for males at age 16 when we combine violent property and violent offenses into one category.

Panel B presents estimates for vehicle related offenses, sex offenses and the largest single offenses in each of the *violent*, *property*, *violent property* and *other* categories. In contrast to the large age 21 effects found in the U.S. context by Chalfin et al. (forthcoming), we find no significant effects on sex offense victimization at either 16 or 18, with point estimates close to zero.[24] This does not necessarily imply diverging results, but may simply be a result of studying different points in the age-profile. The discontinuities in property victimization are not driven by a specific offense, but can rather be seen for all three largest offenses: bike theft, burglary, and pickpocketing (except for bike theft and pickpocketing for males at age 16). The largest violent property offense is robbery, for which we see an effect for males at 16 that is significant at the 10% level. The age 18 increases in violent victimization risk carry through to the two main violent offenses of assault and threat: the threat effect is seen for both males and females but the increased risk of assault is only seen for females. The other offense group is heterogeneous in nature, with the largest offenses being fraud, forgery, and leaving the scene

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[23] The two largest offences within the *other* category are (being the victim of) leaving the scene of an accident and fraud. Fraud mainly refers to online fraud, e.g., being the victim of a scam on the Dutch equivalent of eBay.
[24] One potential confounder in our context (especially at age 16) is the fact that some sex offenses are defined by the age of the victim (i.e., it is an offense if the victim is less than 16). This could mechanically decrease victimization at the age 16 threshold. Moreover, because some sex offences may require hospitalization and parental consent (e.g., to access abortion) for minors below 18, this could impact reporting of some offenses at the age 18 threshold.

of an accident. Even though these offenses make up a small share of all victimizations, we find that, for both males and females, the victimization risk for leaving the scene of an accident increases at 16, while that for fraud and forgery significantly increases at 18.[25]

The results presented in Table 5 also allow us to begin and disentangle the mechanisms underlying the victimization discontinuities. Appendix Table 1 classifies a subset of offenses as vehicle offenses: These are mainly vehicle-related thefts - which can only occur if one for instance owns a vehicle (e.g., moped at 16 or a car/motorcycle at 18). The first row of Panel B demonstrates that the victimization risk associated with these offenses significantly increases for males and females at 16 and males at 18. This can be interpreted as evidence of a first stage with respect to vehicle rights: males and females take up the right to drive at these ages. Yet, when excluding these vehicle offenses from overall or property offenses (Panel A), the coefficients become only marginally smaller. This means that our main results are *not* driven by the somewhat mechanical increase in direct vehicle related victimization, but this does not rule out an indirect vehicle effect through changes in environment and mobility.

4. Mechanisms Underlying the Discontinuities in Victimization

The previous section documented significant and robust increases in victimization risk for both males and females at ages 16 and 18. These baseline estimates represent reduced form intent-to-treat effects: all individuals gain access to a 'bundle' of rights, but not everyone will take all of them up. To learn more about the mechanisms and relative importance of rights within the bundles, this section leverages the victimization register data on the type of location, exploit the cross-cohort variation in rights that arises from a 2014 minimum legal drinking age reform, and uses supplemental survey data to learn about underlying changes in related behaviors.

4.1. Heterogeneity by Offense Location

We use the type of offense location to assess whether the *nature* of victimization changes. We categorize locations into the following main categories, as listed in Appendix Table 2: road, home, out at locations likely to sell alcohol (bar/restaurant/café and shop), out at other non-alcohol selling locations (e.g., sport area, courtyard, public facility, company site), private transport, public transport, and school. Panel A of Table 6 re-estimates the baseline

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[25] More than 70% of fraud cases concern online fraud, e.g., scams on the Dutch equivalent of eBay. About 25% of forgeries are labeled as identity fraud, while the remainder are other types of horizontal fraud (fraud between individuals and/or companies). Victims of *leaving the scene of an accident* are victims of a traffic accident where one of the involved parties deliberately left, knowing that the accident took place, without an opportunity to establish his or her identity.

specification using the percentage share victimized in a specific location as the dependent variable; thus, the counterfactual includes both no victimization and a victimization in a different location. These results indicate that overall, the risk of victimization increases in almost all location types (at least at some ages), except in schools. In Panel B, the dependent variable is the percentage share of victimizations in a particular location when conditioning on the sample of victims. This narrows the counterfactual and can inform us on whether there is a redistribution in victimization across locations following the respective birthdays. The results suggest that victimization out at both alcohol-selling and other locations increases relative to the remaining ones (especially for females). Though these results are not conclusive about which location is a definite driver of the results, the increase in victimization when 'out' is consistent with multiple underlying channels: vehicle access, age thresholds for coffee shop or bar/club admittance, or alcohol regulations on the legal age to purchase alcohol while out.

4.2. Cohort Variation in Age-Specific Rights

As highlighted before, the minimum legal drinking age in the Netherlands was reformed such that cohorts born between 1990 and 1995 gained access to 'weak' alcohol at age 16 (as well as to tobacco and to bars/clubs) and 'hard' alcohol at age 18, while the 1998 and 1999 cohorts gained access to all – any type of alcohol, tobacco, and bars/clubs – at age 18. Both groups gained access to coffee shops and marijuana at age 18. The minimum legal drinking age (MLDA) has been a heavily debated topic in many countries. While US teenagers are banned from drinking alcohol before turning 21, most European countries allow drinking for youths at the age of 16 or 18. The following analysis exploits the cross-cohort variation in the age-specific rights to better understand their relative importance in explaining victimization risk.

4.2.1.   Heterogeneity by Birth Cohorts

Figure 6 visually illustrates the differences across the cohort groups (1990-1995 and 1998-1999 with MLDA of 16 and 18, respectively) in both the level of victimization rates as well as the size of the discontinuities at the age 16 and 18 thresholds. Panels A and B correspond to females and males respectively; the black dots (gray squares) represent the 1990-1995 (1998-1999) cohorts. We exclude the 1996 and 1997 'transition' cohorts who were partly affected by both MLDA regimes. The two figures offer three insights: First, consistent with the statistics shown earlier in Appendix Table 3, victimization rates in the pre-reform cohorts (1990-1995) are higher at all ages and for both males and females. Second, the 1990-1995 cohorts (with an MLDA of 16) show sharp changes in victimization risk for males and females at both age

cutoffs. Third, for the 1998-1999 cohorts (with an MLDA of 18) such discontinuities are only seen at age 18.

We assess the size and significance of these discontinuities (overall and by crime type) by re-estimating the baseline specification separately for the 1990-1995 cohorts (Panel A of Table 7) and 1998-1999 cohorts (Panel B of Table 7). Columns (1) to (4) present the age 16 results for victimizations of any offense, property, violent (including violent property) and other offenses, respectively. A clear pattern again emerges: There are significant discontinuities in victimization risk at age 16 for both males and females in the 1990-1995 cohorts for any, property, and other offense (Panel A), but only one of these coefficients is significant (males, other) for the 1998-1999 cohorts (Panel B). Moreover, the size of the point estimate for all victimizations in column (1) for the 1998-1999 cohorts is 80% smaller than for the 1990-1995 cohorts. Relative to the mean, reaching age 16 discontinuously increased victimization rates for males and females by about 13% and 14%, respectively, for the 1990-1995 cohorts. At age 16, the 1990-1995 cohorts gained the right to drive mopeds, purchase weak alcohol and tobacco, work and be admitted to bars and/or clubs. The 1998-1999 cohorts only received the right to drive mopeds and work. Thus, these results are suggestive of two conclusions: First, the right to drive mopeds (and the change in locations that may come with this right) is not a prominent factor for the age 16 change in victimization risk, as little evidence of a change is seen for the 1998-1999 cohorts. The moped rights could explain the only significant effect for the 1998-1999 cohorts in the 'other' offense category, for which the main offense at age 16 was leaving the scene of an accident. Second, the rights to purchase alcohol and tobacco and to be admitted to bars and/or clubs appear to matter: These effects are seen at age 16 for the 1990-1995 cohorts, but disappear for the 1998-1999 cohorts for whom the age threshold was increased from 16 to 18.

Columns (5) to (8) present the corresponding results for the age 18 threshold. Victimization risk significantly increases overall as well as by offense category (except maybe for males and violent crime) and in both groups of cohorts. The magnitudes of the point estimates allow us to say more about what can be learned about the underlying mechanisms: First, for the 1990-1995 cohorts in Panel A, the additional rights granted at age 18 were to purchase hard alcohol and marijuana in coffeeshops and to drive a motorcycle/car. These additional factors appear to significantly affect victimization risk, as the point estimates are positive and significant for most offences. Relative to the mean, victimization risk for the 1990-1995 cohorts (Panel A) increases by 6% for males and 13% for females. Second, for the 1998-1999 cohorts in Panel B, the additional rights at age 18 also include the access to weak alcohol,

tobacco, and bars/clubs (rights that the 1990-1995 cohorts were already granted at age 16). The (relative) effect sizes for these cohorts are larger than for the earlier cohorts: 18% for males and 20% for females. This suggests that victimization risk increases as a result of this bundle, regardless of whether it is granted at age 16 or 18.

4.2.2. MLDA Reform: Difference-in-Discontinuity

We take the cross-cohort analysis one step further in a difference-in-discontinuity design. Intuitively, the difference-in-discontinuity design allows us to compare the discontinuity in victimization risk at the age thresholds of 16 and 18, respectively, in the 1990-1995 'pre-reform' cohorts relative to the discontinuity in the 1998-1999 'post-reform' cohorts, and thus to make firmer conclusions regarding the differences in the discontinuity effects. We emphasize here that we abstain from a strict treatment and control group wording: All cohorts are treated but to different extents at these age thresholds. While we can compare the relative effects of the respective 'bundles' of rights granted, one would need strong assumptions regarding the interaction of these rights to net out the effect of specific ones. We estimate an augmented version of equation (1):

$$
\begin{aligned}
(2) \qquad Y_{ct}^{og} = {} & \lambda_0 + \lambda_1 D\{t \geq 0\}_c + \lambda_2 D\{yob = 1990, \dots, 1995\}_c \\
& + \lambda_3 D\{yob = 1990, \dots, 1995\}_c * D\{t \geq 0\}_c + f(t) + f(t) \\
& * D\{yob = 1990, \dots, 1995\}_c + \boldsymbol{X}_c^{bday}\lambda_4 + \delta_{yob} + \delta_{mob} + \omega_{ct}
\end{aligned}
$$

Compared to the baseline RD model in equation (1), the difference-in-discontinuity model includes a dummy variable equal to one for cohorts 1990 to 1995 and zero otherwise (the λ_2 term) and the interaction of this dummy with the treatment indicator equal to one if an individual reaches the respective age cutoff (the λ_3 term). As before, $f(t)$ is a linear trend allowed to differ before and after the cutoff age. In addition, we allow for differential trends in the two groups of cohorts (1990-1995 and 1998-1999, respectively). As in the baseline RD specification, we estimate equation (2) using a fixed bandwidth of 26 weeks on each side of the cutoff and heteroskedasticity-robust standard errors. The parameter of interest is the coefficient on the interaction term, λ_3, which yields the impact of turning 16 and 18, respectively, for the pre-reform cohorts relative to the post-reform cohorts.[26]

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[26] This specification is similar to ones used by other recent papers that estimate difference-in-discontinuity models across cohorts (e.g., Malamud et al., 2021). Grembi et al. (2016) provide a more formal presentation of the standard assumptions underlying this setting.

Columns (1) and (2) of Table 8 show that the estimated coefficient $\hat{\lambda}_3$ (interaction term) is significantly different from zero, while $\hat{\lambda}_1$ (age 16) is not. That means that turning 16 only has a significant effect on the risk of victimization for the 1990-1995 cohorts, i.e., those who were granted the additional rights at age 16, including access to weak alcohol, but not on the 1998-1999 cohorts. Though this confirms what was shown before both in Figure 6 and Table 7, this allows for a firm conclusion that the sharp change in victimization risk for the pre-reform cohorts relative to the post-reform cohorts is highly statistically significant. Moving to the age 18 threshold in columns (3) and (4), we confirm our earlier observations that victimization risk sharply increases for all cohorts at 18: the estimated coefficient $\hat{\lambda}_1$ (age 18) is highly significant for both males and females. Further, the estimated coefficients on the interaction term suggest that there are no significant differential impacts for females across cohorts, and for males the increase in victimization risk for the pre-reform cohorts at 18 is only marginally smaller compared to the post-reform cohorts.

While we cannot formally decompose the separate effects of each right (to the extent that they interact with each other), these results are still suggestive of two conclusions. First, the significantly differential effect across cohorts at age 16 strongly suggests that access to alcohol and bars/clubs plays a key role in explaining the discontinuous increase in victimization risk. Second, the lack of a significantly differential effect across cohorts at age 18 suggest that even though the earlier cohorts already saw an increase in victimization risk, they still see a similar change as those cohorts that did not. This allows us to speculate that the overall effect of the relevant bundle of rights is larger for the cohorts that started to receive these rights at age 16.

4.2.3. First-Stage Evidence: Changes in Behavior

The above analyses suggest that there are underlying changes in behavior leading to an increase in victimization risk at ages 16 and 18. However, such administrative data do not contain direct information on behavioral changes surrounding the 16th and 18th birthday. We therefore supplement the analysis with a number of descriptive statistics taken from two large national (yearly) surveys. The National Health Survey ("Gezondheidsenquete") has about 5,000 respondents annually and is available from 2010 to 2019; it includes questions about alcohol, smoking and (for some years) drug use *in the past 12 months*. The Mobility Survey ("OViN") has approximately 40,000 respondents per year and is available from 2010 to 2017; in this questionnaire individuals are asked *for one specific calendar day* how often and at which times they go out, for what purpose and which mode of transport of they use. Both surveys are

designed to be nationally representative, and therefore do not oversample youth.[27] Because of the combination of small sample sizes and how these questionnaires are set up, we cannot repeat the formal discontinuity analysis from the previous sections. For instance, in the health survey, we do not know when in the past 12 months an individual consumed a certain substance. The mobility survey only asks for mobility behavior on one day, so there will be few respondents answering the questions close to or on specific birthdays. The results provided in this section will therefore be of a more descriptive nature.

In Panel A (females) and Panel B (males) of Figure 7, we zoom into the two groups of cohorts (pre- and post-reform) included in our analysis on the increase in the MLDA. We include birth cohorts up to 2004 (instead of 1999 as in the earlier analyses) to increase sample sizes for the post-reform cohorts. The figures show the fraction of individuals who report drinking alcohol or binge drinking in the past 12 months, by respondent's age at the time of the interview and separately for the pre- and post-reform cohorts.[28] One should not expect visible sharp discontinuities at the age cutoffs (even if they exist) in these figures because (i) the questions are asked retrospectively referring to the last 12 months and (ii) age is measured in 3-month bins (due to limited sample sizes). In both figures, we see that the share of males and females who drank alcohol is larger for the pre-reform than the post-reform cohorts, but that this gap closes at age 18. Moreover, the overall share of individuals reporting having drunk alcohol in the last 12 months is relatively high: about 60% and 80% around the 16th and 18th birthdays. The binge-drinking share is considerably lower.

We next look at whether individuals increase their drug use (of which the primary drug used is marijuana) at their 16th and 18th birthdays (see Panel C of Figure 7). Reported drug consumption is gradually increasing from age 14 to 20, with what appears to be a sharper increase in the months around the 18th birthday, consistent with access to coffee shops. While we cannot exclude the possibility that this is related to the sharp increases in victimization risk at 18, it is noteworthy that the incidence of drug consumption is much lower than that for alcohol (around 25% at age 18).

We use the mobility survey to assess whether individuals change the timing and destination of outings as well as their mode of transportation around the 16th and 18th

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[27] The response rate to the health survey equals roughly 60-65% while the average response rate for the mobility survey is around 55%. For the 1990-1995 and 1998-2004 cohorts we observe almost 7,000 individuals in the health survey and 26,000 individuals in the mobility survey between ages 14 and 20 at the time of the interview.
[28] We define binge drinking by combining two measures of drinking behavior from the survey: Excessive drinking and heavy drinking. For men (women), excessive drinking is defined as more than 21 (14) units of alcohol a week. Heavy drinkers drink at least once per week more than 4 (women) or 6 (men) units on a given day.

birthdays. The first pattern to note in Panel A of Figure 8, which is visible across pre-and post-reform birth cohorts, is that there is an increase in the propensity to go out after 8pm, but there are no sharp changes around the 16th or 18th birthday.[29] Once again, we highlight that measuring age in quarters can potentially mask sharper changes. Individuals also begin to increase the distance they travel: distance travelled from home increases quite sharply after their 16th birthday and continues to trend up but does not appear to discretely change at their 18th birthday. A similar pattern (not shown) is also seen in the amount of time spent travelling. Consistent with these longer distances, Panel B of Figure 8 shows a shift in the mode of transportation, from using a bike to public transport and, around the 18th birthday, a car. There is also an increase in moped ownership (not shown) around the 16th birthday. One possibility is that some of the changes from the figures above are related to shifts in working related behavior or school, and that this translates into changes in victimization risk. The mobility survey allows us to rule out this channel: Panel C of Figure 8 shows that while the share enrolled in school decreases and the share with any paid work increases with age, these changes are so smooth that they are unlikely to mask discontinuities at ages 16 or 18.

In summary, though just descriptive, this section helps to narrow down the underlying mechanisms. The victimization results are not driven by changes in education- or work-related behaviors. In line with our earlier analyses with respect to specific offenses and locations of victimization, individuals are more likely to be out and further away from home, and during high crime (evening) periods. These observations are consistent with going out and drinking alcohol playing an important role as mechanisms. As the share of youth drinking alcohol (at least at some level) is already comparatively high by age 16,[30] these results suggest that it is not so much the consumption of any alcohol that matters, but rather the consumption of alcohol out – in the evening and further from home (e.g., in bars and/or clubs) – that matters most.

5. Heterogeneity by Victimization Risk and External Validity

The regression discontinuity design identifies the local average treatment effect on the risk of victimization for the compliers. The results presented in the previous section suggest that the combination of going out and drinking alcohol plays an important role; the survey evidence suggests that considerable shares of those granted these rights also take them up. This already

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[29] Similar patterns are seen when we look at males and females separately.
[30] In the Netherlands, from birth cohort 1985 onwards, more than 50% of respondents report having drunk alcohol before their 16th birthday. This is an important illustration that the minimum legal drinking age in the Netherlands, as in the US, refers to the legal age to *purchase* alcohol or consume alcohol *in public spaces*, while private consumption is not illegal.

speaks towards a possible interpretation of our results beyond the LATE parameter. We conduct a number of further heterogeneity analyses to further understand the generalizability of our findings and whether there are differential effects across population groups.

For this purpose, we re-estimate our baseline RD specification for sub-samples with different baseline risks of victimization based on socio-economic background and neighborhood characteristics. The choice of sub-samples is motivated by factors that are likely to be strong predictors of victimization risk at the household and neighborhood level: household structure (single versus dual parent households), household income (above and below poverty thresholds and quartiles of the national income distribution), rural versus urban municipalities, and neighborhood victimization rate quartiles.[31]

For each sub-sample, weekly victimization shares are plotted in Appendix Figures 1 and 2. The first take-away from these figures is that victimization risk is indeed higher for youths growing up in disadvantaged households or neighborhoods. Victimization rates are nearly twice as large in single versus dual parent households. Victimization rates are higher in high-crime neighborhoods, urban municipalities and those with coffeeshops, and for financially worse-off households (except for property crime, see Appendix Figure 2). The second take-away is that discontinuities at the age thresholds are readily visible in almost all subsamples. The third take-away is that the size of the victimization gaps across subsamples does not substantively change at the age thresholds.

To assess the magnitudes and statistical precision of these discontinuities, we graphically present the results of estimating equation (1) for each subsample in Panels A and B of Figure 9 for males and females at ages 16 (left) and 18 (right), respectively. We present the point estimates and 95% confidence interval, including the baseline for ease of comparison, plus the mean of the dependent variable for each subsample. We find significant discontinuities for males and females at both ages and almost every sample. The only exception is for those below the poverty threshold at 16 (which could be due to lower precision from small samples) and females in the third income quartile at 18.[32] Finally, these figures again highlight differences

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[31] We create neighborhood victimization rates by dividing the number of annual victimizations (i.e., incidents) by neighborhood population. We smooth the neighborhood victimization rate by taking the average over all observation years (2005 to 2018) and exclude the smallest 5% neighborhoods as the measures are too noisy.

[32] Depending on the age/gender subsample, 75-78% live in dual parent households; 18-19% in single parent households; 17-19%, 22-25%, 30%, 26-30% in households within the first, second, third and fourth quartile of the national income distribution; 6-7% in households below the poverty threshold. The average neighborhood victimization rate is 0.05 with a standard deviation of 0.014. 67-69% of youths live in urban municipalities; urban municipalities have a population of on average above 171,000 whereas rural municipalities have a population of on average 31,000. About 75% of the urban municipalities have at least one coffeeshop compared to 15% of rural municipalities.

in baseline victimization risk by illustrating variation in the sample means. While the baseline differences are larger than the discontinuities, the latter are not small in relative terms.

In summary, regardless of the inequalities in baseline level of risk between different population groups, reaching the key age thresholds of 16 and 18 significantly increases the risk of victimization by comparable magnitudes for almost every subsample. These findings have two important implications. First, access to these rights does not exacerbate (nor mitigate) existing inequalities in victimization risk across subsamples.[33] Second, individuals who differ in multiple dimensions related to their 'pre-treatment' victimization risk are all impacted by reaching these age thresholds in the same way. This homogeneous nature of our findings speaks to their potential external validity and supports a broader interpretation of the results in terms of average treatment effects that represent more than just one part of the population. We thus argue that our results bear population-wide implications.

6. Spillover Effects and Peer Effects

The previous sections documented significant and meaningful increases in the risk of victimization risk at ages 16 and 18 with underlying changes in behavior, for which behaviors related to going out and access to alcohol appear to play a major role. While we have so far focused on the individual who is granted these rights at their birthday, we note that such behaviors are likely group behaviors. This opens the question of whether there are spillover effects on individuals who have not yet received these rights or peer effects in a more general sense. As there is no information on friends or social networks in administrative registers, this final part of our paper studies (i) school cohorts and (ii) siblings as observable 'groups'. This exploration of possible spillover and/or peer effects remains more speculative because of data limitations, but is still informative on potential group effects.

6.1. Heterogeneity by Relative Age in School Cohort

In the absence of school or class level data, we use the fact that the cutoff date to start school in the Netherlands is October 1 of every year. Leveraging the resulting variation in (intended) assignment to school years by month of birth, we simply compare individuals born in October to those born in September: Those born in October are the oldest in their school cohort with a potentially younger peer group, while those born in September are the youngest in their school

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[33] Note that while we document significant increases in the risk of victimization by comparable magnitudes for the different population groups, we cannot say anything about potential differential costs of victimization for youth from different backgrounds.

cohort with a potentially older peer group (see for example Oosterbeek et al., 2021). To examine this question, we re-estimate equation (1) separately for each month of birth (and by gender and age threshold). Panels A and B of Appendix Table 4 show the results for the September and October born individuals, respectively, around the age 16 cutoff.[34] The results suggest that there are significant increases for both groups and males and females at age 16, with no significant differences in magnitude (Panel C). These results are also depicted in Appendix Figure 3, which shows the regression results at both age 16 and 18 cutoffs for each month of birth (January through December). Though we do not find evidence of spillover effects at 16, we cannot rule out they exist when defining peer groups in other ways or at other ages. Moreover, as depicted in Appendix Figure 3, month of birth can also be related to seasonality effects (e.g., youths may be less likely to go out at night in the colder fall/winter months), suggesting caution in the interpretation of this peer group analysis.

6.2. Spillover Effects onto Younger Siblings

Finally, the register data allow us to study potential spillover effects within the household. These could arise if, correlated with the rights granted to one child, there are changes in parental behavior and supervision. An example would be the younger sibling being allowed to go out with the older sibling, but not on their own.

To investigate the potential for such spillovers, we restrict our sample to individuals who, at the time of their respective birthday, have a younger sibling living in the same household. We focus on the next younger sibling only and on cases in which the younger sibling is at least 13 years old.[35,36] We re-define the outcome variable as the percentage share of younger siblings victimized by day-of-birth cohort (of the older siblings) and calendar week, and re-estimate equation (1) with this new outcome variable. As there may be heterogeneities by same-sex and opposite-sex pairs of siblings, we split the analysis sample into eight subsamples by gender of the older and younger sibling, respectively.

The results are shown in Appendix Table 5. They suggest that there are no spillover effects onto younger siblings: The point estimates are close to and not significantly different

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[34] We focus in this analysis on the age 16 cutoff since education is compulsory until the year individuals turn 16, and thus at 18, the relative age of peer groups is not as cleanly defined.

[35] A number of papers find sibling spill-overs from only the older to younger sibling with respect to risky behavior, such as crime. See for instance Bhuller et al. (2018), Averett et al. (2011) and Breining et al., (2020).

[36] We restrict on age 13 or older for several reasons. First, with younger children, one might not expect spillover effects at all if the mechanism is going out and drinking; even 13 might be young in this respect and we tighten our restriction in specification tests. Second, up to age 13, children need parental supervision to report crimes to the police which would introduce reporting concerns.

from zero across sub-samples. Empirically, we cannot distinguish whether this is due to strict compliance with the laws (i.e., no change in within-household behavior except for the child who gets the rights) or strict (public) enforcement of the laws. We can, however, conclude that the estimated discontinuity effects are largely concentrated on the individuals who receive the rights without meaningful (observed) spillover effects.[37]

7. Conclusion

As youths become more independent and transition into young adulthood, many rights and responsibilities are conferred on them at the key ages of 16 and 18. This paper provides clear evidence that this bundle of rights significantly increases an individual's risk of criminal victimization, regardless of their baseline risk. We demonstrate that these jumps represent real changes in victimization risk and do not reflect changes in reporting rates. Capitalizing on detailed data on offense type and location as well as cross-cohort variation in the minimum legal drinking age help illuminate the relative importance of each right. We conclude that the bundle of access to weak alcohol, bars/clubs and smoking clearly increases victimization risk at age 16, and that remaining and related rights granted at age 18 (hard alcohol, marijuana coffee shops) exacerbate this risk. The discontinuities are not driven by either access to vehicles (mopeds at 16 and motorcycles/cars at 18) or changes in schooling or employment statuses.

We return to the idea from the introduction that understanding the impact of this bundle of rights on victimization risks at the two age cutoffs can help explain the age-victimization profile. As emphasized throughout the paper, our analyses can only retrieve the intent-to-treat effect: not all individuals will immediately exercise the rights gained at ages 16 and 18. To the extent that some of these rights are gradually taken up as individuals age or taken up with increasing intensity, causal mechanisms that drive the cutoffs can plausibly explain the upward sloping age-victimization profile from ages 15 to 19. Indeed, as we argue throughout, the high degree in homogeneity across different population groups supports external validity and an interpretation beyond the LATE parameter, both with respect to complier groups around as well as further away from the cutoffs.

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[37] There are two further dimensions of peer effects in the broader sense that one could consider. First, we observe in the data whether there was more than one victim in a given incident. The share of incidents with multiple victims is low, between 7.2 and 11.3%. The median incident with more than one victim has one co-victim and the average age of the co-victims is around 35 years, i.e., well above the age thresholds. Second, one could wonder about the role of a victim-offender overlap. We observe offenders only for 25% of incidents in our data (10.6% for property offenses, 27.8% for other offenses and 64.6% for violent offenses). Conditional on observing the offender, the average age varies between 19.6 years for violent property offenses and 25.6 years for other offenses. This implies that the average (observed) offender is older than the cutoff ages of 16 and 18.

Finally, we consider the potential policy implications of our results. Taking away these rights completely is obviously not up for debate. Rather, the relevant policy questions are perhaps related to the optimal timing of granting the rights and whether they should in a way be conditional. With respect to the latter, one possibility is an increased effort to provide information and education regarding the various risks associated with exercising these rights. Chalfin et al. (forthcoming) also highlight the potential for an information-related policy intervention, which "have low marginal costs and, as such, are easier to scale". With regards to the timing, is there an optimal age at which to grant youths rights? Should these rights be given all at the same time or spread over different age cutoffs? Our difference-in-discontinuity analyses of the MLDA reform allow us to speculate that the overall effect of the relevant bundle of rights on victimization risk is larger for the cohorts that received a subset of rights at age 16 compared to those that received all rights at 18. In other words, spreading out the rights across ages does not appear to reduce their overall impact on victimization risk.

We emphasize, however, that this is only a partial answer to the question of optimal ages for granting such rights: We shed light on the effects on victimization – one potential cost– at ages of 16 and 18. There are other benefits and costs associated with the receiving these rights. Access to alcohol, for instance, yields utility to (at least a subset of) individuals. Is this utility greater for those who receive the right at 16 versus 18? Does granting all of the alcohol-related rights at 18 (i.e., when also being allowed to obtain drivers licenses) have other harmful effects, such as on drunk driving related behaviors? Are there externalities associated with the granting (or removal) of these rights: if 16-year-olds are not out drinking and in bars/clubs, then does this increase the victimization risk of other age groups? Though this paper cannot answer these questions, it does provide insight into the differential effects of granting these rights on one important and understudied piece of the puzzle – victimization risk. This, indeed, can be understood as one component of the price of becoming an adult.

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Figure 1. Age-Victimization Profile in the Netherlands and the US
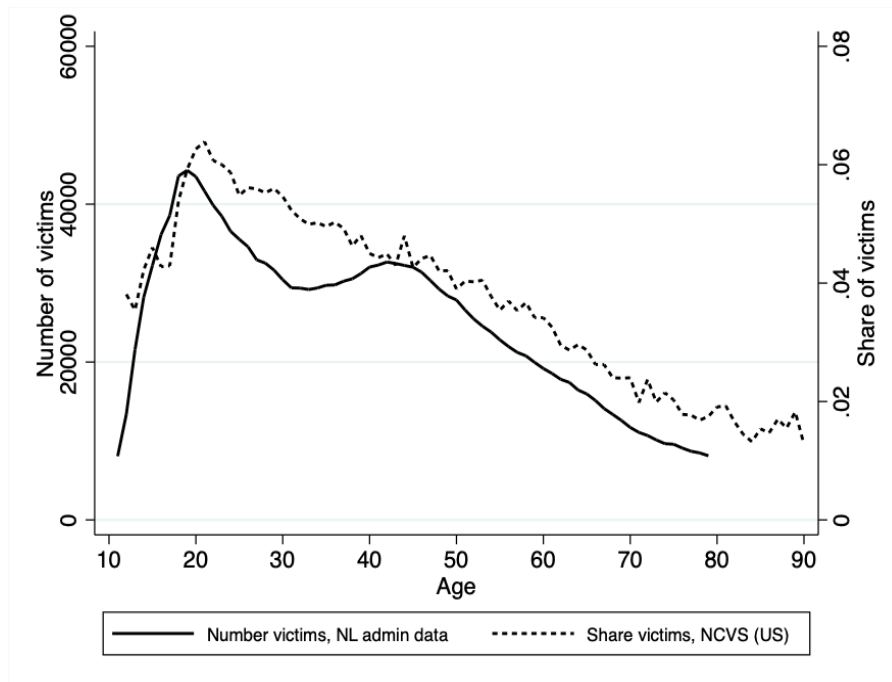
NOTE – The solid line shows the number of victims by year of age in the Netherlands for six offences (assault, threat, sex offences, burglary, robbery and pickpocketing) for the years 2005 to 2016. These are based on calculations by the authors using administrative microdata from Statistics Netherlands. The dashed line shows the share of respondents at a given age in the National Crime Victimization Survey (U.S.) who report to have been a victim of any crime in the last six months. SOURCE - These are based on calculations by the authors using the 2005-2017 survey waves of the NCVS (Concatenated File, ICPSR 37198).

Figure 2. Age-Victimization Profiles in the Netherlands

Panel A. Any offense (balanced)
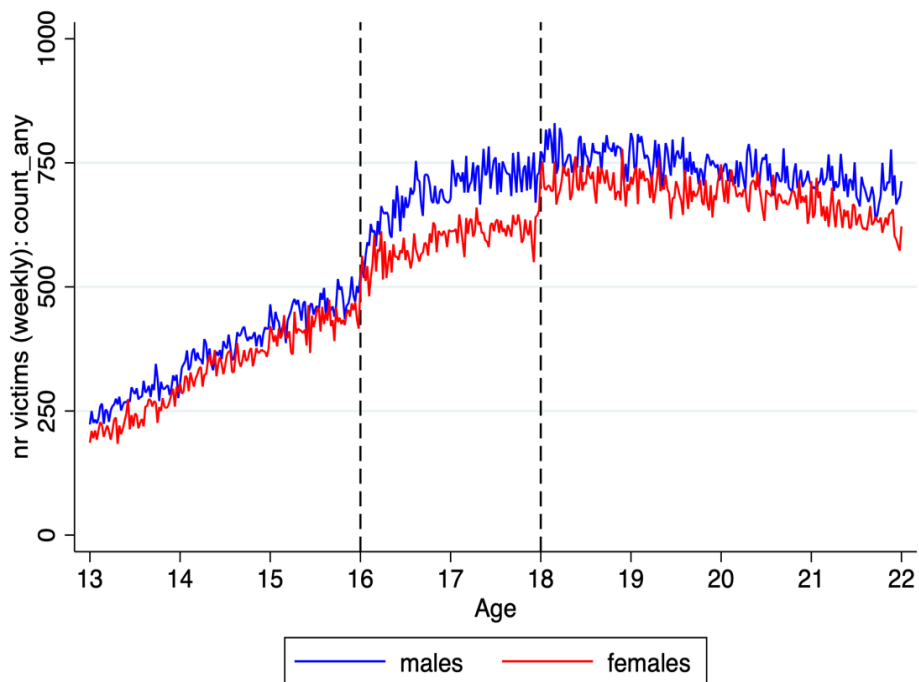
Birth cohorts 1992-1996.

Panel B. Any offense (unbalanced)
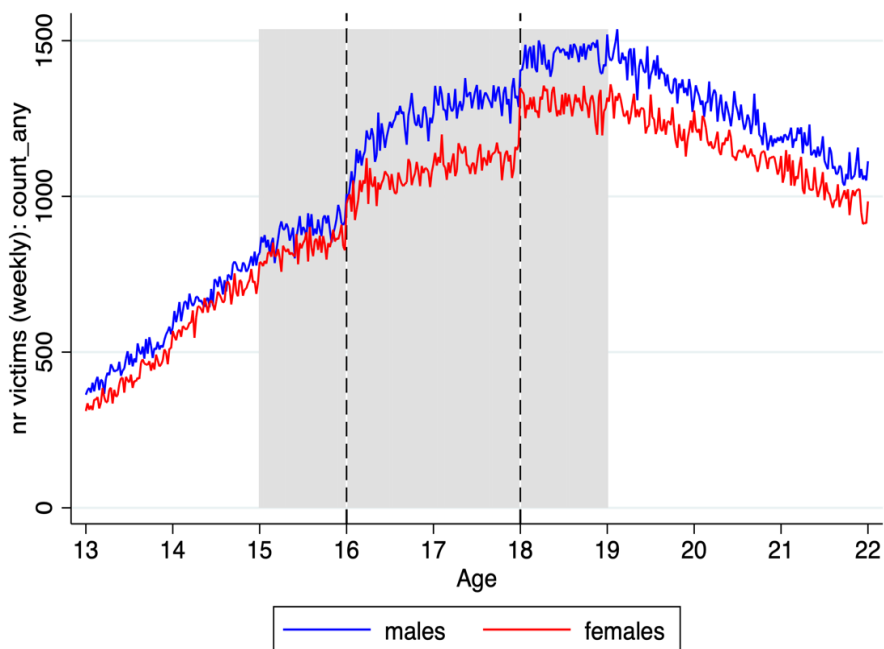
Birth cohorts 1990-1999.

NOTE - The figure plots the number of victimizations (of any offense) per week (to and from birthdays) over ages 13-22 for the birth cohorts 1992-1996 (balanced, Panel A) and 10-30 for the birth cohorts 1990-1999 (unbalanced, Panels B-F). Blue lines represent males, red lines females. The two dashed vertical lines mark birthdays 16 and 18, respectively. The gray shaded areas mark the parts of the age-profile that are based on a balanced sample. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Figure 3. Discontinuities in Victimization Risk: Any Offense

Panel A. Females
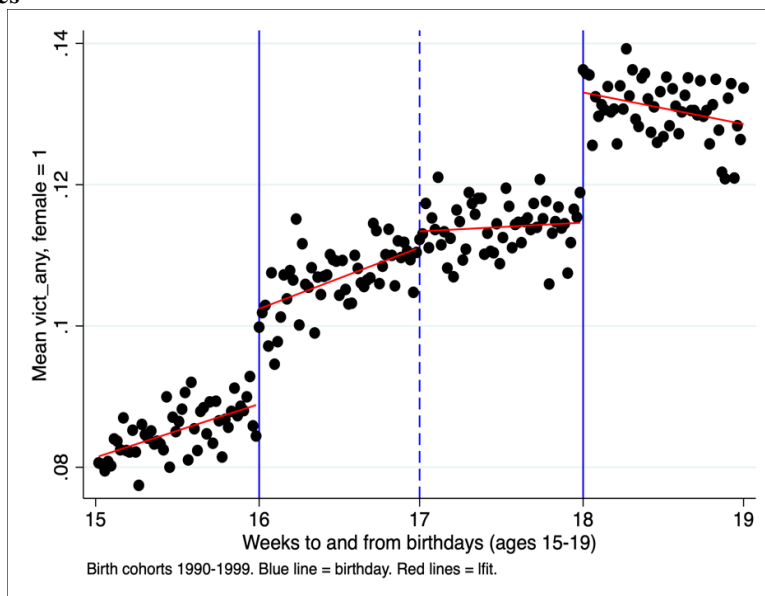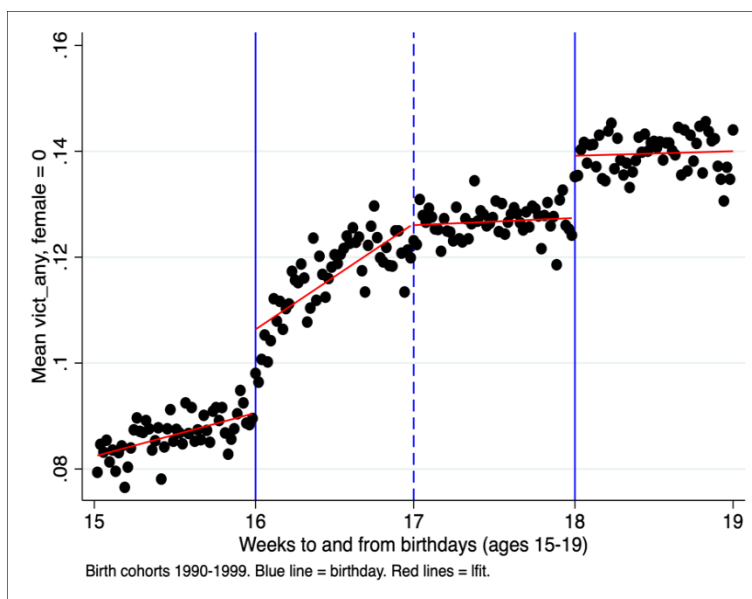
Birth cohorts 1990-1999. Blue line = birthday. Red lines = lfit.

Panel B. Males

Birth cohorts 1990-1999. Blue line = birthday. Red lines = lfit.

NOTE - The figures show the average victimization rates (in percent) per week around the key birthdays (15-19) for any offense and for females in Panel A and males in Panel B, respectively. The blue lines mark the key birthdays (solid lines for 16 and 18, dashed line for 17); the red lines represent simple linear fits. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Figure 4. Dutch Victimization Survey - Reporting Rates around Birthdays

Panel A. Raw data
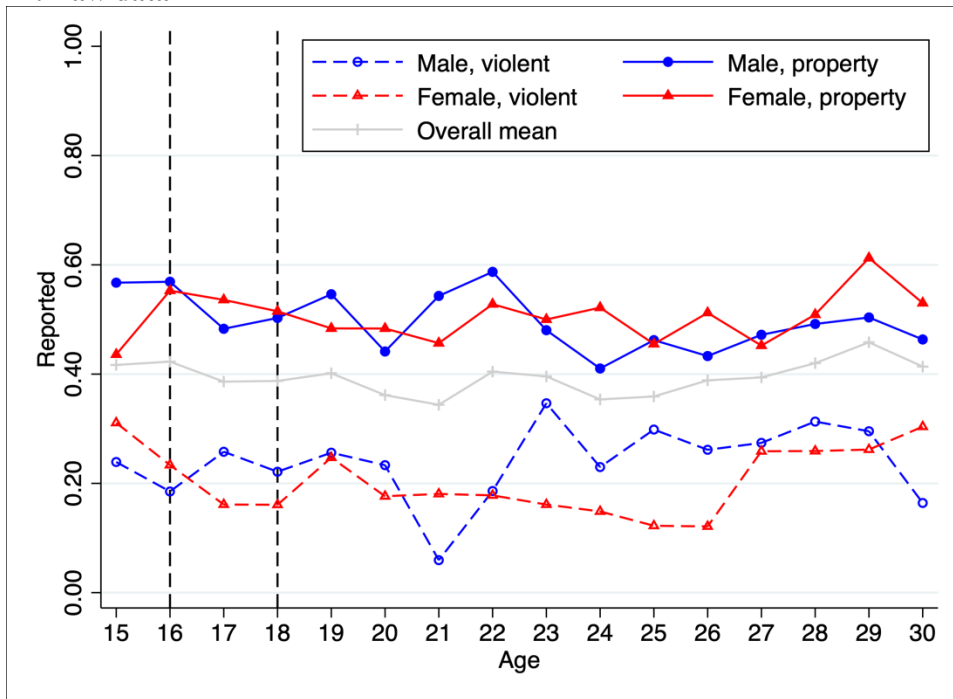
Panel B. Regression estimates
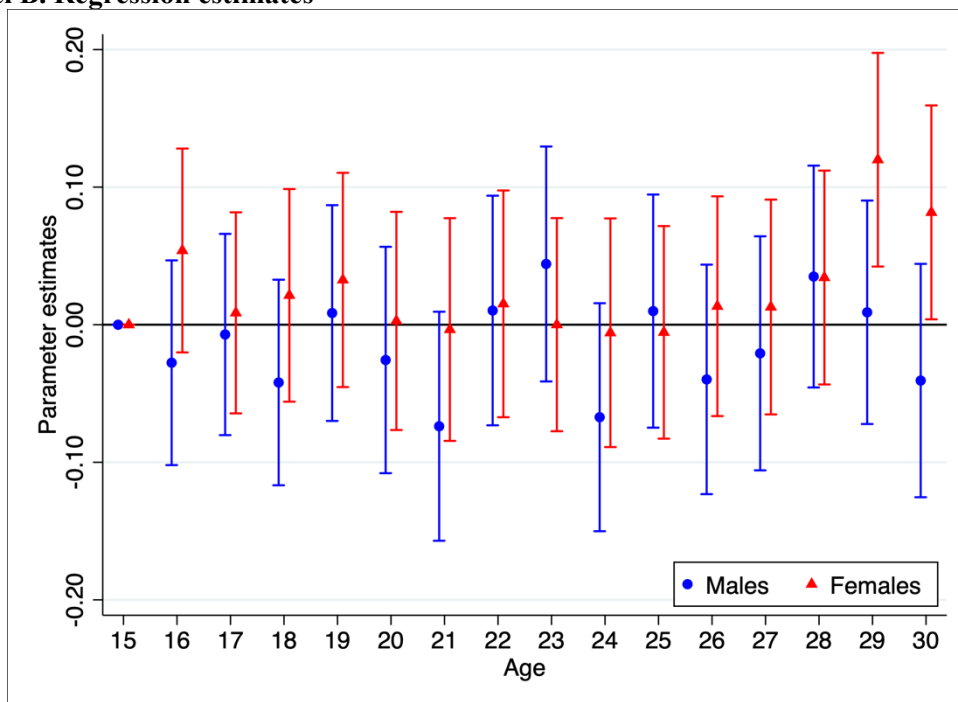
NOTE – These figures are based on author calculations from the Dutch victimization surveys from 2005 to 2016. Panel A presents the share of victimizations by age that the respondent reported to the police. These are shown overall and by gender and broad crime category (property versus violent). Panel B shows regression estimates, where whether a respondent report the victimization is regressed on age dummies as well as the interaction of offense by survey wave fixed effects. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Figure 5. Discontinuities in Victimization Risk: By Offense Type

Panel A. Females
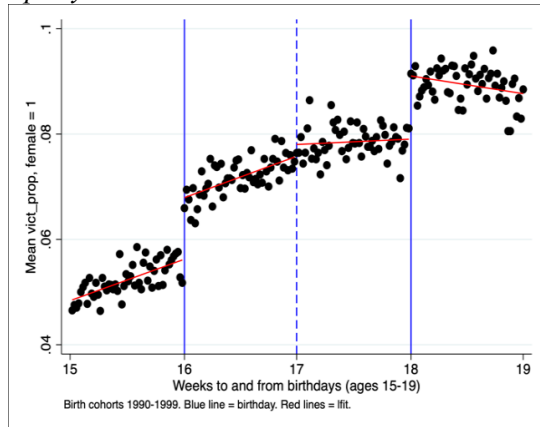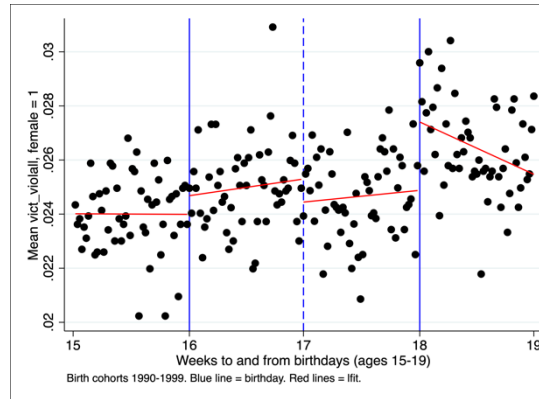
Property   *Violent (all)*   *Other*
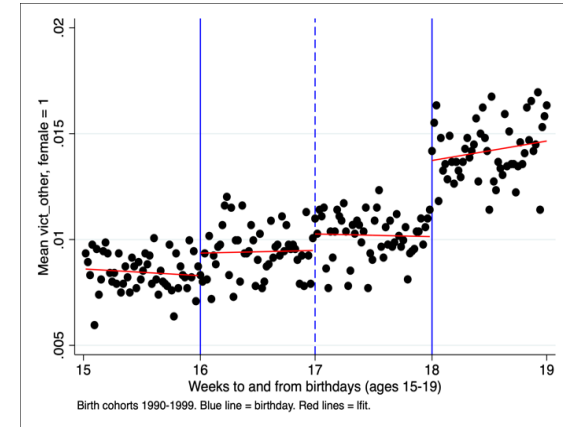
Panel B. Males
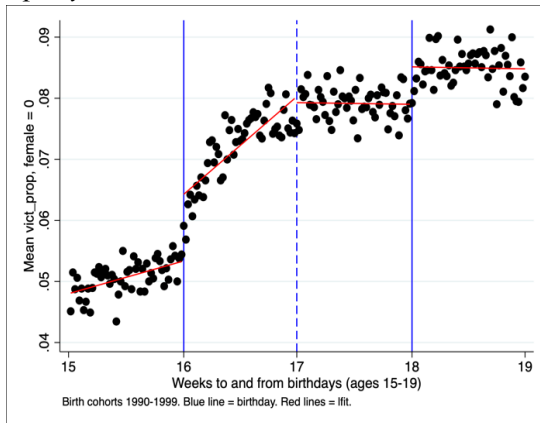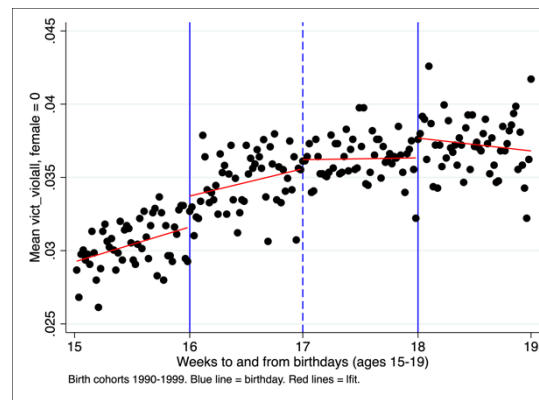
Property   *Violent (all)*   *Other*
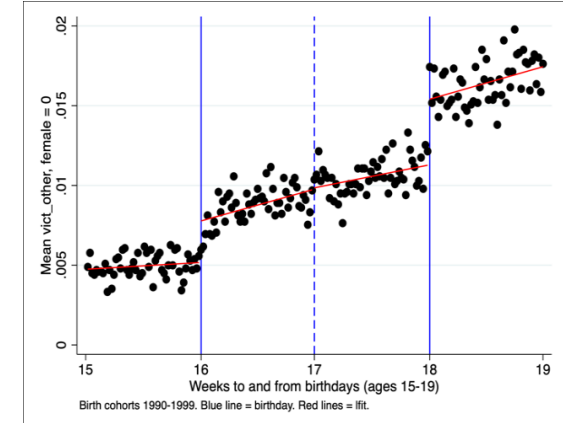
NOTE - The figures show the average victimization rates per week (in percent) around the key birthdays (15-19) for four offense groups as indicated and for females in Panel A and males in Panel B, respectively. The blue lines mark the key birthdays (solid lines for 16 and 18, dashed line for 17); the red lines represent simple linear fits. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Figure 6. Discontinuities in the Victimization Risk: By MLDA Cohorts

Panel A. Females
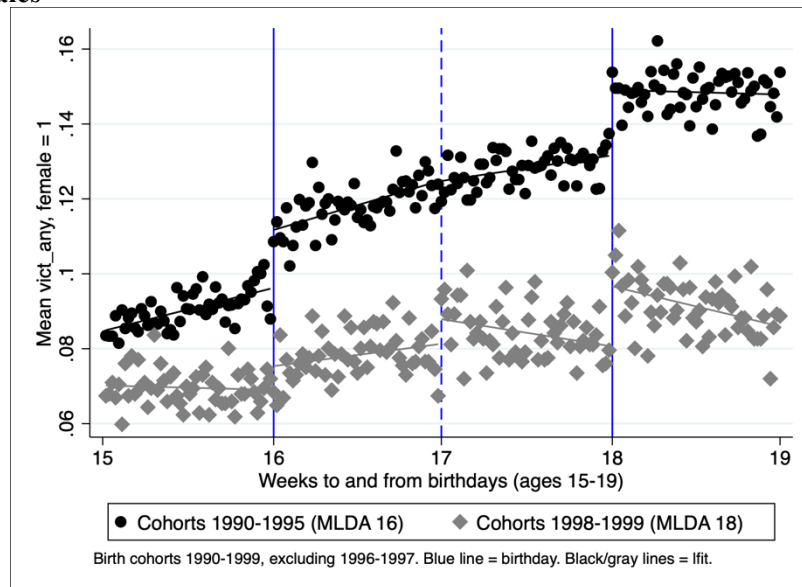
Panel B. Males
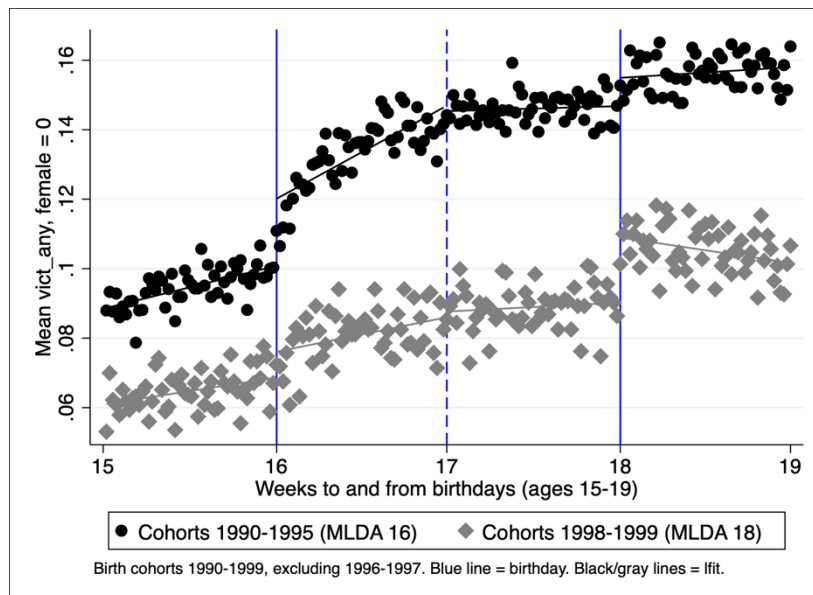
NOTE - The figures show the average victimization rates (in percent) per week around the key birthdays (15-19) for any offense for females in Panel A and males in Panel B, respectively. Black markers represent cohorts born between 1990 and 1995, gray markers those born between 1998 and 1999. The blue lines mark the key birthdays (solid lines for 16 and 18, dashed line for 17); the black/gray lines represent simple linear fits. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Figure 7. Alcohol and Drug Consumption Changes

Panel A. Females, drinking in the last 12 months
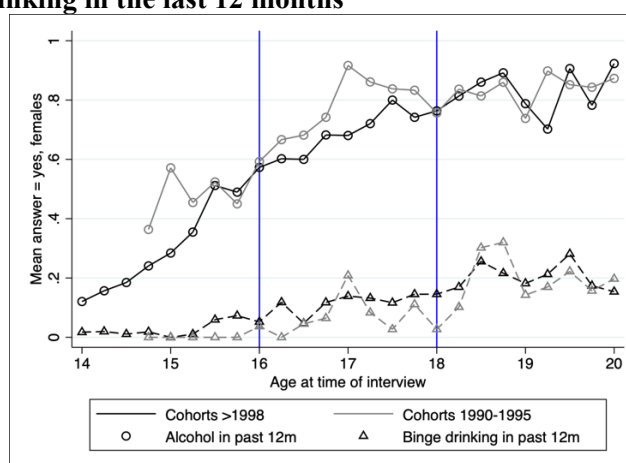
Panel B. Males, drinking in the last 12 months
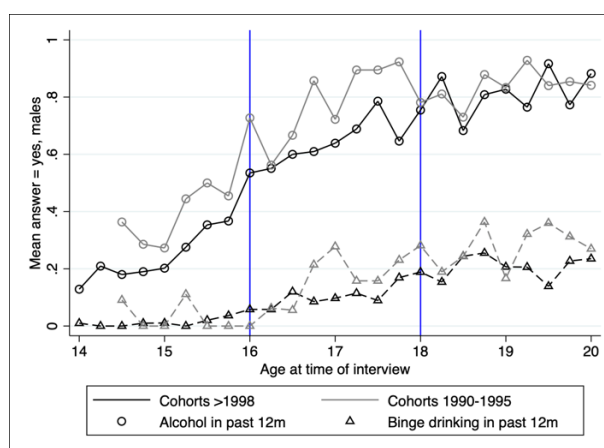
Panel C. Drug consumption in the last 12 months
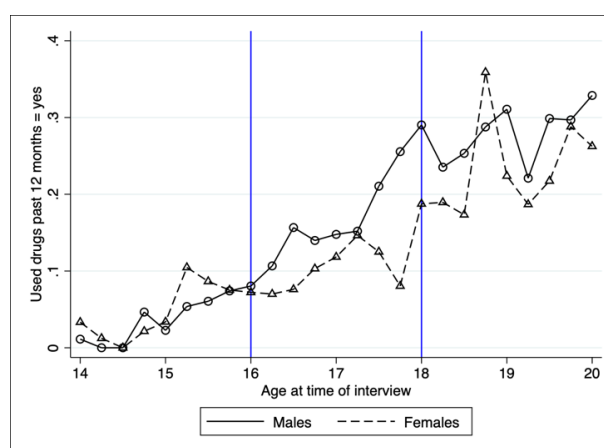
NOTE – The figures show drinking behavior of females (Panel A), males (Panel B) and drug consumption of males and females (Panel C) by age at time of the interview (coded in quarters). Both alcohol and drug consumption refer to the past 12 months. The blue lines mark the key birthdays (16 and 18). Panel A and Panel B present separate numbers for birth cohorts 1990-1995 (N=3,261 males and females) and birth cohorts 1998-2004 (N=3,658 males and females). Panel C provides aggregate information for birth cohorts 1998-2004 (N=2,124 for females and N=2,091 for males); no data are available for earlier cohorts. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Figure 8. Changes in Time and Nature of Being Away from Home

Panel A. Out after 8pm and distance away from home
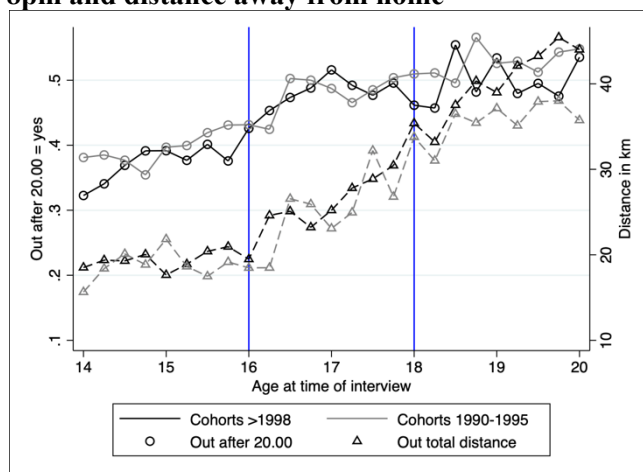
Panel B. Mode of transportation
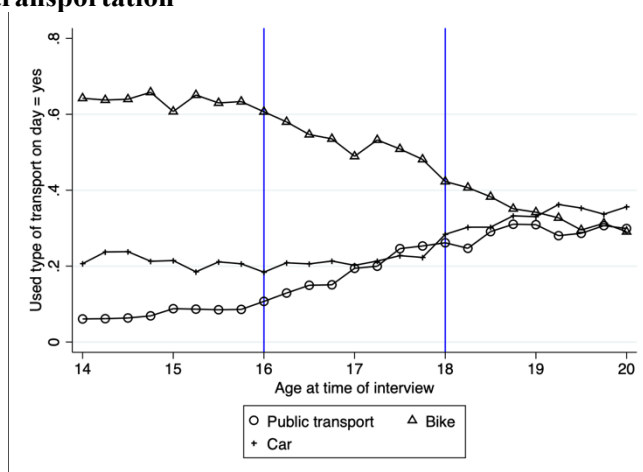
Panel C. School enrollment and employment
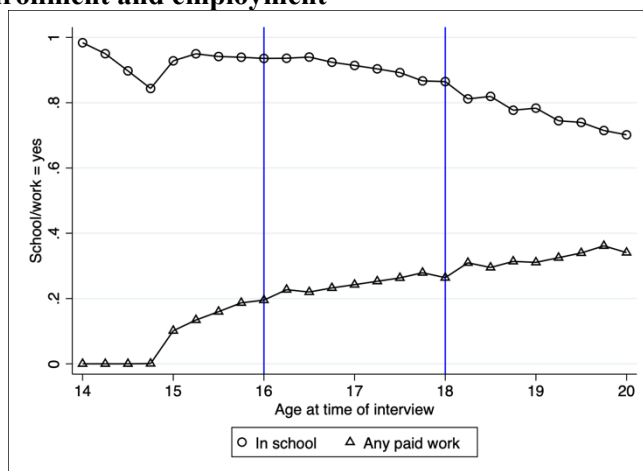
NOTE – The figures show average responses with respect to mobility behavior (Panel A), mode of transportation (Panel B) and school enrollment/employment at the time of the interview (Panel C) by age at time of the interview (coded in quarters). The blue lines mark the quarters of the key birthdays (16 and 18). Panel A presents separate results for birth cohorts 1990-1995 and birth cohorts 1998-2004; Panels B and C provide aggregate information for birth cohorts 1990-1995 and 1998-2004. Each figure is based on a sample of 25,926 individuals. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Figure 9. Heterogeneity across Household and Neighborhood Characteristics

Panel A. Females

Age 16   *Age 18*

Panel B. Males

Age 16   *Age 18*
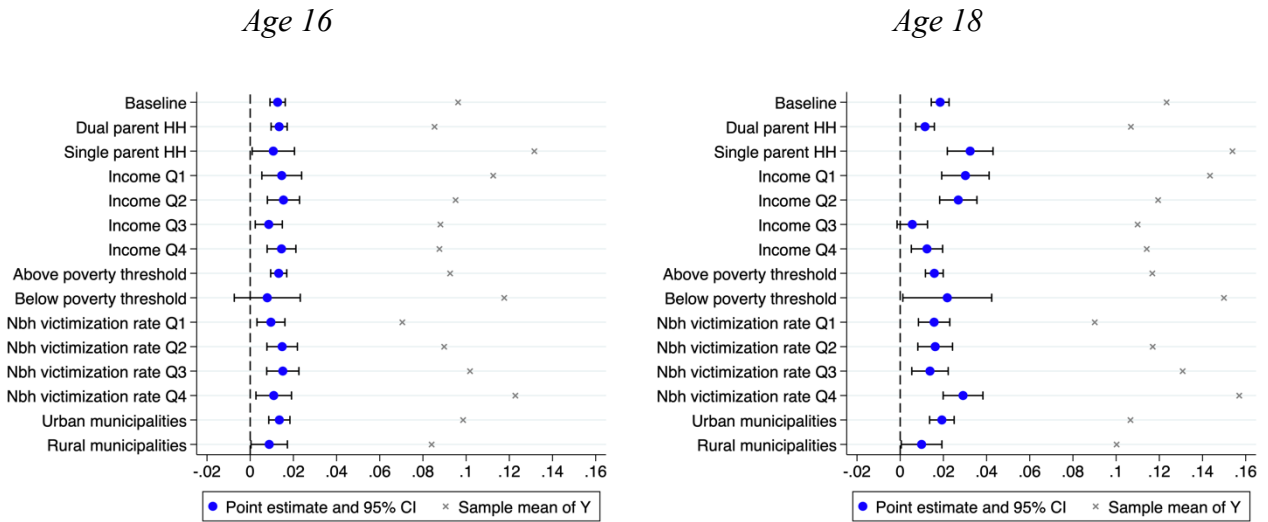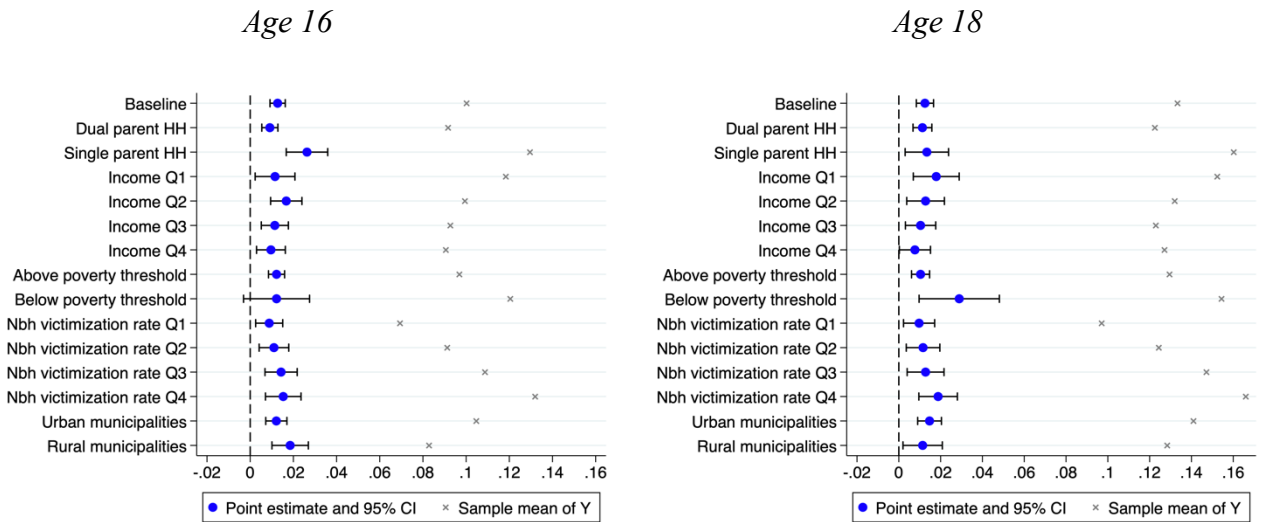
NOTE - The figures show the point estimates (blue dots) and corresponding 95% confidence intervals from heterogeneity analyses, estimating the RD design as in equation (1) for subsamples according to different household and neighborhood characteristics. The gray crosses indicate the sample means of the outcome variable (percentage share victimized per day-of-birth-cohort and calendar week) for each subsample, respectively. See notes of Table 3 for further details on the underlying regression specification. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Table 1. Summary Statistics: Victimization Rates Around Key Birthdays

	All	Before BD	After BD	All	Before BD	After BD
	Victimization rates +/-52 weeks around 16th/18th birthdays					
	Female			**Male**		
Panel A. Age 16						
Average share victimized (in %):						
Any offense	0.0961	0.0852	0.1068	0.1016	0.0865	0.1164
Property	0.0622	0.0524	0.0719	0.0616	0.0507	0.0723
Violent Property	0.0017	0.0014	0.0020	0.0058	0.0055	0.0061
Violent	0.0227	0.0226	0.0229	0.0268	0.0250	0.0286
All violent	0.0245	0.0240	0.0249	0.0326	0.0304	0.0347
Other	0.0090	0.0084	0.0095	0.0069	0.0050	0.0088
Number of obs. (DOB by week level)	383,460	189,904	193,556	383,460	189,904	193,556
Panel B. Age 18						
Average share victimized (in %):						
Any offense	0.1226	0.1141	0.1309	0.1332	0.1268	0.1395
Property	0.0841	0.0786	0.0894	0.0821	0.0792	0.0850
Violent Property	0.0023	0.0021	0.0025	0.0059	0.0059	0.0060
Violent	0.0233	0.0225	0.0240	0.0308	0.0303	0.0312
All violent	0.0256	0.0247	0.0264	0.0367	0.0362	0.0372
Other	0.0122	0.0102	0.0142	0.0135	0.0106	0.0164
Number of obs. (DOB by week level)	383,250	189,800	193,450	383,250	189,800	193,450

NOTE - This table provides summary statistics for the analysis sample (day-of-birth cohort by week panel). It shows the average share of females and males victimized per day-of-birth cohort and calendar week for the 104 weeks surrounding the key birthday (16 and 18, respectively), as well as separately for the 52 weeks before and 52 weeks after the birthday. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Table 2. Rights at Ages 16 and 18

Cohort	Year Age 16	Year Age 18	Moped	Motorcycle/ Car	Purchase Weak Alcohol	Purchase Hard Alcohol	Consume marijuana	Coffeeshop	Tobacco	Bar/club admittance	Minimum school drop-out age	Work	Work at night
1990	2006	2008	16	18	16	18	no age	18	16	16	year 16	16	18
1991	2007	2009	16	18	16	18	no age	18	16	16	year 16	16	18
1992	2008	2010	16	18	16	18	no age	18	16	16	year 16	16	18
1993	2009	2011	16	18	16	18	no age	18	16	16	year 16	16	18
1994	2010	2012	16	18	16	18	no age	18	16	16	year 16	16	18
1995	2011	2013	16	18	16	18	no age	18	16	16	year 16	16	18
1996	2012	2014	16	18	16/18	18	no age	18	16/18	16/18	year 16	16	18
1997	2013	2015	16	18	16/18	18	no age	18	16/18	16/18	year 16	16	18
1998	2014	2016	16	18	18	18	no age	18	18	18	year 16	16	18
1999	2015	2017	16	18	18	18	no age	18	18	18	year 16	16	18

NOTE - The table summarizes the age thresholds at which the listed rights are granted to youth. Whenever there are reforms in such minimum ages, the age thresholds change across birth cohorts. SOURCE - https://www.kinderrechten.nl/jeugd/leeftijdsladder.

Table 3. Reduced Form: Age Thresholds and Victimization Risk

Sample:	Males	Females	Males	Females
		Y: Share victimized		
Age threshold:	16	16	18	18
	(1)	(2)	(3)	(4)
Age 16/18 (=1/0)	0.0127***	0.0127***	0.0125***	0.0185***
	(0.0018)	(0.0018)	(0.0021)	(0.0021)
Trend: pre BD	0.0001	0.0001	-0.0000	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Trend: post BD	0.0007***	0.0003***	0.0000	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Birthday week (=1/0)	-0.0044	-0.0001	-0.0031	0.0034
	(0.0034)	(0.0035)	(0.0040)	(0.0041)
Weekend birthday (=1/0)	-0.0007	-0.0010	0.0000	-0.0021**
	(0.0009)	(0.0009)	(0.0010)	(0.0010)
Mean of Y	0.1002	0.0962	0.1333	0.1233
Bandwidth	+/-26 weeks	+/-26 weeks	+/-26 weeks	+/-26 weeks
N	193,556	193,556	193,450	193,450
Month of birth f.e.	yes	yes	yes	yes
Year of birth f.e.	yes	yes	yes	yes
Standard errors	robust	robust	robust	robust

NOTE - This table shows the results from estimating equation (1). The outcome variable is the percentage share victimized per day-of-birth-cohort and calendar week. The main variable of interest is Age 16 and Age 18, respectively, which is a dummy equal to 1 on and after the relevant birthday and equal to 0 before. This specification is our baseline specification including a separate linear trend for before and after the birthday, a dummy variable indicating whether the week is the birthday week, a dummy variable indicating whether the birthday is on a weekend, as well as month of birth and year of birth fixed effects. The regressions are estimated using a parametric specification (linear regression), and use a bandwidth of 26 weeks on each side of the cutoff (birthday). Robust standard errors are shown in parentheses below the coefficient. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Table 4. Reduced Form: Robustness Tests

Sample:		Males	Females	Males	Females
Age threshold:		16	16	18	18
		(1)	(2)	(3)	(4)
Panel A. Baseline with/without controls					
No controls, simple linear trend		0.0114***	0.0124***	0.0120***	0.0190***
		(0.0017)	(0.0017)	(0.0020)	(0.0020)
No controls, separate linear trend		0.0120***	0.0127***	0.0121***	0.0190***
		(0.0017)	(0.0017)	(0.0020)	(0.0020)
Mob and yob f.e., separate linear trend		0.0120***	0.0127***	0.0121***	0.0190***
		(0.0017)	(0.0017)	(0.0020)	(0.0020)
All controls, separate linear trend,		0.0127***	0.0127***	0.0125***	0.0185***
robust standard errors (baseline)		(0.0018)	(0.0018)	(0.0021)	(0.0021)
	N	193,556	193,556	193,450	193,450
Panel B. Baseline with different bandwidth choices					
+/- 52 weeks		0.0166***	0.0134***	0.0121***	0.0184***
		(0.0013)	(0.0013)	(0.0015)	(0.0014)
	N	383,460	383,460	383,250	383,250
+/- 39 weeks		0.0143***	0.0133***	0.0120***	0.0175***
		(0.0015)	(0.0015)	(0.0017)	(0.0017)
	N	288,508	288,508	288,350	288,350
+/- 13 weeks		0.0076***	0.0091***	0.0133***	0.0180***
		(0.0026)	(0.0026)	(0.0030)	(0.0030)
	N	98,604	98,604	98,550	98,550
Panel C. Estimation					
Local linear regeression, +/- 26 weeks		0.0103***	0.0113***	0.0133***	0.0176***
		(0.0019)	(0.0019)	(0.0022)	(0.0022)
Local linear regression, optimal BW		0.0060**	0.0103***	0.0133***	0.0178***
		(0.0028)	(0.0023)	(0.0028)	(0.0029)
	Optimal BW	11.77	18.80	16.49	15.05
Panel D. Standard errors					
Baseline with clustered standard errors		0.0127***	0.0127***	0.0125***	0.0185***
		(0.0018)	(0.0018)	(0.0021)	(0.0021)
Baseline with clustered standard errors		0.0127***	0.0127***	0.0125***	0.0185***
and day-of-birth f.e.		(0.0018)	(0.0018)	(0.0021)	(0.0021)

The header spans: **Y: Share victimized** over columns (1)–(4).

NOTE - This table shows the robustness tests for the results shown in Table 3. Panel A builds the specification up to the baseline specification as indicated in the first column. Panel B estimates the baseline specification using different bandwidth choices (baseline: +/- 26 weeks). Panel C uses local linear regression instead of the parametric linear regression (Stata command: rdrobust) with a manually chosen bandwidth of +/- 26 weeks and an optimal bandwidth selection as an alternative. Panel D shows the results when clustering standard errors at the day-of-birth-cohort level and when in addition including day-of-birth fixed effects. Every cell of the table represents the coefficient on the Age 16 (Age 18) variable from a separate regression. For further details regarding the baseline specification, please see table notes of Table 3. Standard errors are shown in parentheses below the coefficient (when not otherwise indicated, robust standard errors are shown). *** $p<0.01$, ** $p<0.05$, * $p<0.1$. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Table 5. Heterogeneity by Offense

	Y: Share victimized			
Sample:	Males	Females	Males	Females
Age threshold:	16	16	18	18
	(1)	(2)	(3)	(4)
Panel A. Broad offense categories				
Any offense except vehicle offenses	0.0112***	0.0122***	0.0122***	0.0185***
	(0.0018)	(0.0018)	(0.0021)	(0.0021)
Property offenses	0.0078***	0.0111***	0.0051***	0.0115***
	(0.0014)	(0.0015)	(0.0016)	(0.0017)
Property offenses except vehicle offenses	0.0065***	0.0105***	0.0048***	0.0115***
	(0.0014)	(0.0014)	(0.0016)	(0.0017)
Violent property offenses	0.0011**	0.0000	0.0008*	-0.0002
	(0.0004)	(0.0002)	(0.0004)	(0.0003)
Violent offenses	0.0012	0.0008	0.0021**	0.0031***
	(0.0009)	(0.0009)	(0.0010)	(0.0009)
All violent offenses combined	0.0023**	0.0008	0.0028**	0.0029***
	(0.0010)	(0.0009)	(0.0011)	(0.0009)
Other offenses	0.0024***	0.0010*	0.0042***	0.0039***
	(0.0005)	(0.0005)	(0.0007)	(0.0006)
Panel B. Selected specific offense categories				
Vehicle offenses	0.0015***	0.0005***	0.0003*	-0.0000
	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Sex offenses	-0.0001	-0.0000	0.0000	-0.0004
	(0.0002)	(0.0005)	(0.0001)	(0.0004)
Property: Bike theft	0.0013	0.0034***	0.0016*	0.0039***
	(0.0009)	(0.0010)	(0.0010)	(0.0011)
Property: Burglary	0.0018**	0.0036***	0.0021**	0.0044***
	(0.0008)	(0.0008)	(0.0008)	(0.0009)
Property: Pickpocketing	0.0003	0.0018***	0.0009**	0.0016***
	(0.0003)	(0.0005)	(0.0004)	(0.0006)
Violent property: Robbery	0.0008*	0.0001	0.0006	-0.0002
	(0.0004)	(0.0002)	(0.0004)	(0.0002)
Violent: Assault	0.001	0.0003	0.0007	0.0016**
	(0.0008)	(0.0006)	(0.0008)	(0.0007)
Violent: Threat	0.0001	0.0004	0.0008**	0.0012***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Other: Fraud	0.0005**	0.0002	0.0013***	0.0016***
	(0.0002)	(0.0002)	(0.0004)	(0.0004)
Other: Leaving scene of accident	0.0012***	0.0010***	0.0003	-0.0001
	(0.0003)	(0.0002)	(0.0003)	(0.0002)
Other: Forgery of documents	0.0000	-0.0000	0.0009***	0.0011***
	(0.0001)	(0.0001)	(0.0002)	(0.0002)

NOTE - This table shows heterogeneity analyses by offence type. The underlying specification is the baseline specification from Table 3; All specifications use a +/-26 week bandwidth, and include month and year of birth fixed effects, birthday controls, and a separately pre and post birthday linear trend. Every cell represents the coefficient on the Age 16 (Age 18) variable from a separate regression. Panels A and B show the results for broad offense and selected more specific offense categories, respectively. The outcome variable is the percentage share victimized per day-of-birth-cohort and calendar week for the indicated offense category. Robust standard errors are shown in parentheses. N = 193,556 in columns (1) and (2) and 193,450 in (3) and (4). *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Table 6. Heterogeneity by location

	Y: Share victimized			
Sample:	Males	Females	Males	Females
Age threshold:	16	16	18	18
	(1)	(2)	(3)	(4)
Panel A. Share victimized at location X of all individuals				
Road	0.0050***	0.0046***	0.0040***	0.0055***
	(0.0012)	(0.0011)	(0.0014)	(0.0013)
Home	0.0016***	0.0003	0.0025***	0.0034***
	(0.0005)	(0.0007)	(0.0007)	(0.0008)
Out: Likely alcohol selling locations	0.0005	0.0028***	0.0025***	0.0029***
	(0.0004)	(0.0005)	(0.0007)	(0.0008)
Out: Other locations	0.0033***	0.0018***	0.0006	0.0035***
	(0.0007)	(0.0006)	(0.0007)	(0.0006)
Private transport	0.0006**	0.0009***	0.0005	0.0001
	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Public transport	0.0000	0.0007**	0.0011**	0.0008
	(0.0003)	(0.0003)	(0.0005)	(0.0006)
School	0.0003	0.0007	-0.0001	0.0003
	(0.0006)	(0.0006)	(0.0004)	(0.0004)
Panel B. Share victimized at location X of all victimized individuals				
Road	-0.9653	-0.5377	-1.4240*	-1.4658*
	(0.9353)	(0.9439)	(0.8079)	(0.8335)
Home	0.7213	-1.4131**	0.5911	0.0895
	(0.5190)	(0.6810)	(0.4968)	(0.6360)
Out: Likely alcohol selling locations	-0.2085	1.6986***	1.0295**	0.068
	(0.4148)	(0.5570)	(0.4971)	(0.6066)
Out: Other locations	1.5625**	0.0207	-0.7414	1.8715***
	(0.6823)	(0.6486)	(0.5274)	(0.5022)
Private transport	0.3864	0.6802**	0.0401	-0.3660
	(0.2638)	(0.2826)	(0.3054)	(0.3156)
Public transport	-0.1060	0.4647	0.3779	-0.6532
	(0.2990)	(0.3480)	(0.3681)	(0.4674)
School	-0.9763*	-0.6844	-0.3824	-0.2489
	(0.5562)	(0.5970)	(0.2899)	(0.3403)
Bandwidth	+/-26 weeks	+/-26 weeks	+/-26 weeks	+/-26 weeks
N (Panel A)	193,556	193,556	193,450	193,450
N (Panel B)	46,818	43,450	59,534	53,580
Month of birth f.e.	yes	yes	yes	yes
Year of birth f.e.	yes	yes	yes	yes
Birthday controls	yes	yes	yes	yes
Trend	sep. linear	sep. linear	sep. linear	sep. linear
Standard errors	robust	robust	robust	robust

NOTE - This table shows heterogeneity analyses by offence location. The underlying specification is the baseline specification from Table 3. Every cell represents the coefficient on the Age 16 (Age 18) variable from a separate regression. In Panel A, the outcome variable is the percentage share victimized per day-of-birth-cohort and calendar week who were victimized at the indicated location of all individuals. In Panel B, the outcome variable is the percentage share victimized per day-of-birth-cohort and calendar week who were victimized at the indicated location of all victimized individuals. Robust standard errors are shown in parentheses below the coefficient. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Table 7. Heterogeneity by Cohorts: MLDA Reform

Sample:	Y: Share victimized					Y: Share victimized			
	Any	Property	Violent (all)	Other		Any	Property	Violent (all)	Other
Age threshold:	16	16	16	16		18	18	18	18
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)
Panel A. 1990-1995 Cohorts (MLDA = 16)									
Males	0.0151***	0.0113***	0.0016	0.0021***		0.0092***	0.0043*	0.0029*	0.0016**
	(0.0024)	(0.0019)	(0.0014)	(0.0006)		(0.0029)	(0.0023)	(0.0015)	(0.0008)
Females	0.0155***	0.0132***	0.0008	0.0015**		0.0183***	0.0132***	0.0023*	0.0028***
	(0.0024)	(0.0020)	(0.0012)	(0.0007)		(0.0028)	(0.0024)	(0.0013)	(0.0008)
N	116,123	116,123	116,123	116,123		116,070	116,070	116,070	116,070
Panel B. 1998-1999 Cohorts (MLDA = 18)									
Males	0.0022	-0.0016	0.0015	0.0020**		0.0182***	0.0075**	0.0027	0.0077***
	(0.0034)	(0.0026)	(0.0019)	(0.0010)		(0.0040)	(0.0030)	(0.0021)	(0.0017)
Females	0.0031	0.0029	-0.0001	0.0004		0.0178***	0.0054*	0.0055***	0.0064***
	(0.0035)	(0.0027)	(0.0018)	(0.0013)		(0.0039)	(0.0030)	(0.0018)	(0.0017)
N	38,690	38,690	38,690	38,690		38,690	38,690	38,690	38,690
Bandwidth	+/-26 weeks	+/-26 weeks	+/-26 weeks	+/-26 weeks		+/-26 weeks	+/-26 weeks	+/-26 weeks	+/-26 weeks
Month of birth f.e.	yes	yes	yes	yes		yes	yes	yes	yes
Year of birth f.e.	yes	yes	yes	yes		yes	yes	yes	yes
Birthday controls	yes	yes	yes	yes		yes	yes	yes	yes
Trend	sep. linear	sep. linear	sep. linear	sep. linear		sep. linear	sep. linear	sep. linear	sep. linear
Standard errors	robust	robust	robust	robust		robust	robust	robust	robust

NOTE - This table shows heterogeneity analyses by birth cohorts that are subject to different rights with respect to alcohol and tobacco (see Table 2). The underlying specification is the baseline specification from Table 3. Every cell represents the coefficient on the Age 16 (Age 18) variable from a separate regression. The outcome variable is the percentage share victimized per day-of-birth-cohort and calendar week who were victimized of the indicated offence. PanelA shows the results for birth cohorts 1990 to 1995 (pre-reform), Panel B for birth cohorts 1998 to 1999 (post-reform). Robust standard errors are shown in parentheses below the coefficient. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Table 8. MLDA Reform: Difference-in-Discontinuity

	Y: Share victimized			
Sample:	Males	Females	Males	Females
Age threshold:	16	16	18	18
	(1)	(2)	(3)	(4)
Age 16/18 * 1990-1995 cohorts	0.0126***	0.0127***	-0.0083*	0.0007
	(0.0040)	(0.0041)	(0.0048)	(0.0046)
Age 16/18 (=1/0)	0.0024	0.0029	0.0176***	0.0176***
	(0.0033)	(0.0034)	(0.0039)	(0.0038)
1990-1995 cohorts	0.0409***	0.0321***	0.0576***	0.0488***
	(0.0033)	(0.0034)	(0.0038)	(0.0036)
Mean of Y – 1990-1995 cohorts	0.1125	0.1047	0.1510	0.1396
Mean of Y – 1998-1999 cohorts	0.0729	0.0730	0.0987	0.0887
Bandwidth	+/-26 weeks	+/-26 weeks	+/-26 weeks	+/-26 weeks
N	154,813	154,813	154,760	154,760
Month of birth f.e.	yes	yes	yes	yes
Year of birth f.e.	yes	yes	yes	yes
Trend	sep. linear	sep. linear	sep. linear	sep. linear
Standard errors	robust	robust	robust	robust

NOTE - This table shows the results for the difference-in-discontinuity estimation. The underlying specification is the specification from equation (2). The outcome variable is the percentage share victimized per day-of-birth-cohort and calendar week who were victimized. The estimation sample includes the birth cohorts 1990 to 1995 (pre-reform) and 1998 to 1999 (post-reform). We omit the transition cohorts 1996 and 1997. Robust standard errors are shown in parentheses below the coefficient. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.
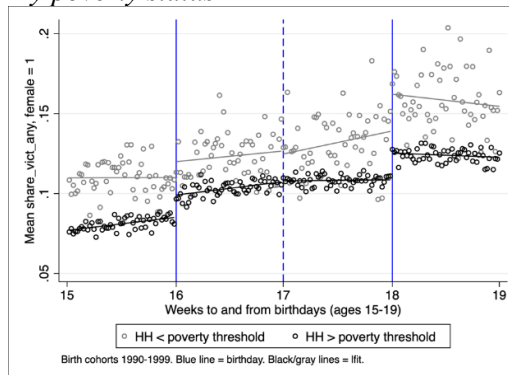
Appendix Figure 1. Discontinuities in Victimization Risk: By Household Composition, Poverty Status, and Neighborhood Victimization

Panel A. Females, any offense
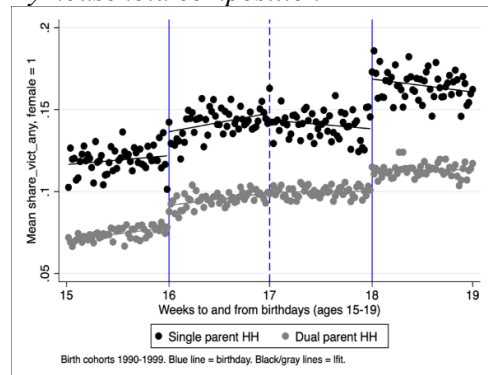
By household composition

By poverty status

By neighborhood victimization rate
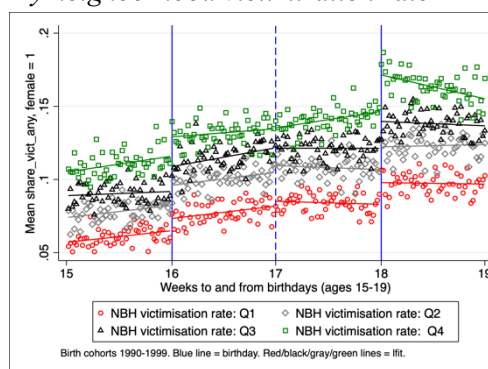
Urban vs rural (with/without coffeeshop)
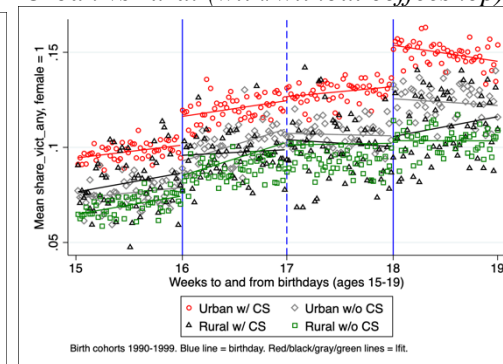
Panel B. Males, any offense
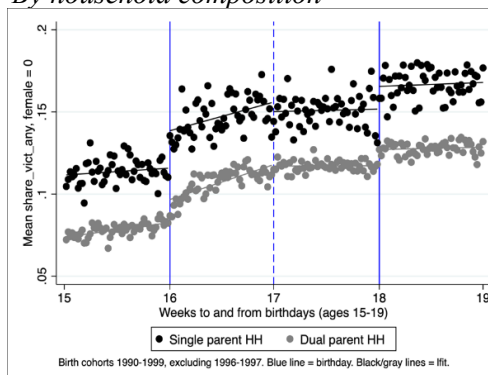
By household composition

By poverty status
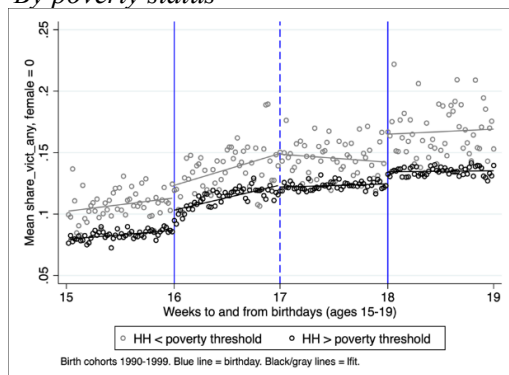
By neighborhood victimization rate
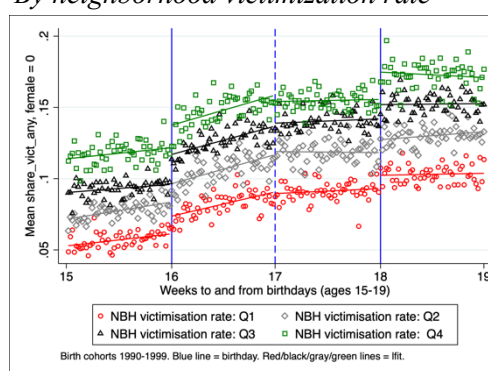
Urban vs rural (with/without coffeeshop)
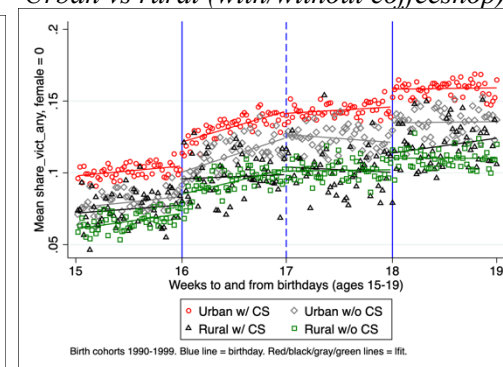
NOTE - The figures show the average victimization rates (in percent) per week around the key birthdays (15-19) for any offense and by household/neighborhood characteristics for females in Panel A and males in Panel B, respectively. From left to right: (i) Black markers represent single parent households, gray markers dual parent households; (ii) black dots represent households above the poverty thresholds while gray dots represent those below; (iii) red markers represent individuals in the lowest quartile of the neighborhood victimization rate distribution, black markers in the second quartile, gray markers those in the third quartile and green markers those in the highest quartile (neighborhood victimization rates are averaged over the sample period (2005-2018) and include all offences, and we exclude the 5% smallest neighborhoods); (iv) red markers represent individuals in urban municipalities with coffeeshops, gray markers in those without, black markers in rural municipalities with coffeeshops, green markers in those without. The blue lines in all figures mark the key birthdays (solid lines for 16 and 18, dashed line for 17); the red/green/black/gray lines represent simple linear fits. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Appendix Figure 2. Discontinuities in Victimization Risk: By Household Income Quartile

Panel A. Females
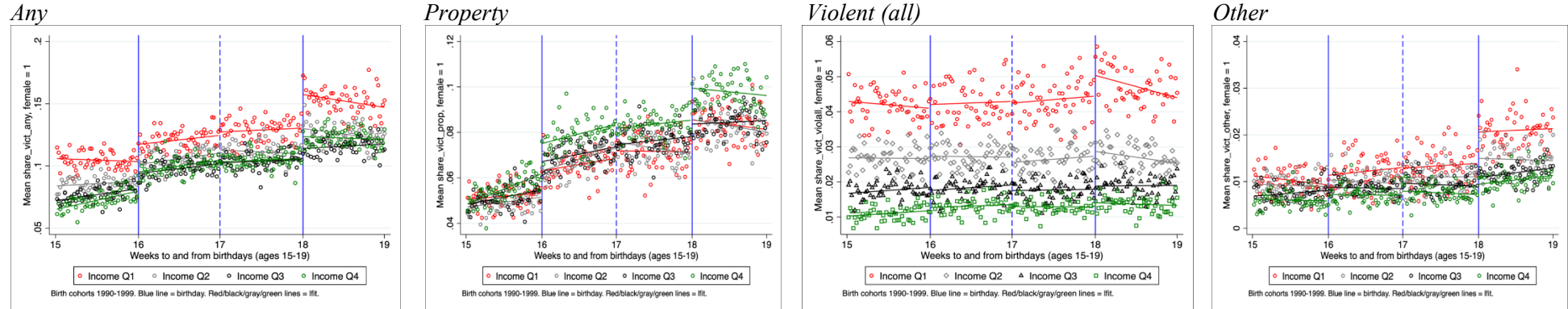
Panel B. Males
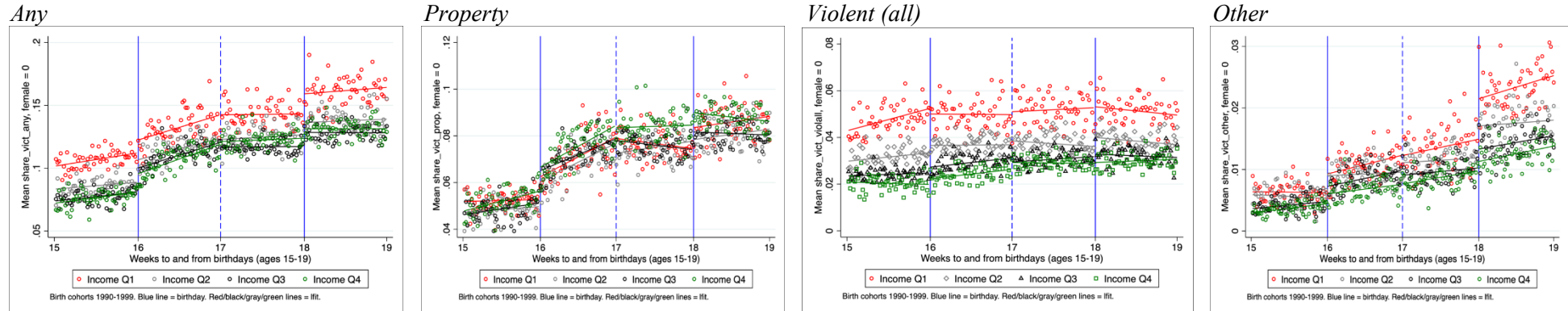
NOTE - The figures show the average victimization rates (in percent) per week around the key birthdays (15-19) for any offense and by type of offense and for females in Panel A and males in Panel B, respectively. Red markers represent individuals in the lowest quartile of the income distribution, black markers in the second quartile, gray markers those in the third quartile and green markers those in the highest quartile. The blue lines mark the key birthdays (solid lines for 16 and 18, dashed line for 17); the red/black/gray/green lines represent simple linear fits. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Appendix Figure 3. Heterogeneity by Month of Birth

Panel A. Females

Any offense, age 16
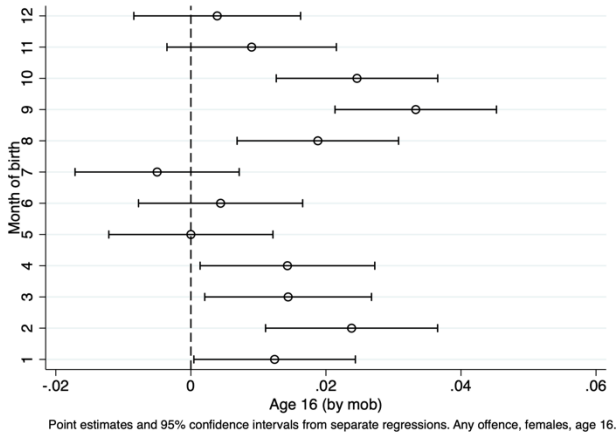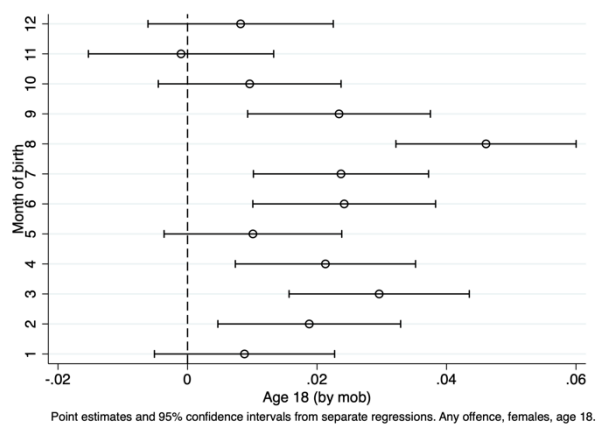
Point estimates and 95% confidence intervals from separate regressions. Any offence, females, age 16.

Any offense, age 18

Point estimates and 95% confidence intervals from separate regressions. Any offence, females, age 18.

Panel B. Males

Any offense, age 16

Point estimates and 95% confidence intervals from separate regressions. Any offence, males, age 16.

Any offense, age 18

Point estimates and 95% confidence intervals from separate regressions. Any offence, males, age 18.
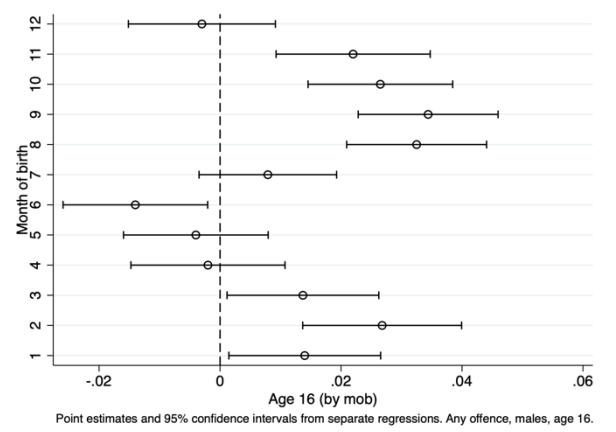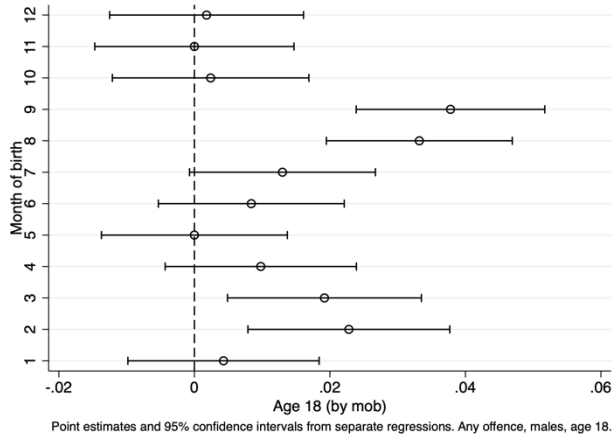
NOTE - The figures show the point estimates and corresponding 95% confidence intervals from heterogeneity analyses, estimating the RD design as in equation (1) for subsamples by month of birth. Each dot represents the results from a separate regression. See notes of Table 3 for further details on the underlying regression specification (month of birth fixed effects are omitted from the specification here). SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Appendix Table 1. Offense Categories

Offense	N	Broad category	Detailed category
theft bike	250,663	property	theft bike
theft/burglary (other)	198,937	property	other theft
pickpocketing	70,123	property	pickpocketing
theft scooter	53,417	property	theft vehicle
theft from car	51,216	property	other theft
theft from dwelling	42,686	property	other theft
shoplifting	39,275	property	other theft
vandalism and damages (other)	32,826	property	vandalism
vandalism of car	26,447	property	vandalism
theft from other vehicle	19,427	property	other theft
theft from sports complex	14,833	property	other theft
theft from school	12,881	property	other theft
theft from shop/office	11,736	property	other theft
theft from garage	9,793	property	other theft
theft car	6,912	property	theft vehicle
theft from building (other)	4,711	property	other theft
arson/explosion	3,508	property	other property
theft motorbike	2,384	property	theft vehicle
theft other vehicle	1,615	property	theft vehicle
theft from boat	633	property	other theft
theft boat	476	property	theft vehicle
theft from hotel	445	property	other theft
vandalism of public building	379	property	vandalism
theft of animal	118	property	other theft
vandalism of public transport	96	property	n/a
other property crime	18	property	other property
(street) robbery with violence	25,952	violent property	robbery
theft/burglary (other) w violence	3,399	violent property	other theft
robbery w violence	3,252	violent property	robbery
shoplifting with violence	1,241	violent property	other theft
theft bike w violence	642	violent property	theft bike
theft from shop/office w violence	505	violent property	other theft
theft from dwelling w violence	458	violent property	other theft
theft scooter w violence	356	violent property	theft vehicle
theft from car w violence	236	violent property	other theft
theft car w violence	159	violent property	theft vehicle
theft from other vehicle w violence	153	violent property	other theft
theft from garage w violence	94	violent property	other theft
theft from building (other) w violence	67	violent property	other theft
theft other vehicle w violence	38	violent property	theft vehicle
theft motorbike w violence	28	violent property	theft vehicle
theft from sports complex w violence	26	violent property	other theft
theft from school w violence	21	violent property	other theft
theft from hotel w violence	<10	violent property	other theft
theft from boat w violence	<10	violent property	other theft
theft boat w violence	<10	violent property	theft vehicle
assault	166,176	violent	assault
threat	52,971	violent	threat
public violence against a person	25,209	violent	other violent

Appendix Table 1. Offense Categories (continued)

Offense	N	Broad category	Detailed category
sexual assault	11,702	violent	violent sex
rape	7,328	violent	violent sex
crime against life	4,813	violent	other violent
stalking	4,734	violent	other violent
kidnapping/deprivation of liberty	1,361	violent	other violent
other violent crime	1,250	violent	other violent
public violence against a good	986	violent	other violent
human trafficking	953	violent	other violent
violence against civil servant	145	violent	other violent
leaving scene of accident	50,542	other	other
fraud	47,554	other	other
forgery of documents	15,423	other	other
other crime (in penal code)	9,986	other	other
other traffic crime	8,552	other	other
blatant offence to modesty	6,873	other	other sex
other sexual offences	5,439	other	other sex
extortion	5,256	other	other
(acts of) sex(ual) nature with minors	5,087	other	other sex
trespassing of a computer (hacking)	2,539	other	other
receiving stolen goods	2,498	other	other
sexual act with abuse of authority	2,062	other	other sex
trespassing of a dwelling	2,006	other	other
counterfeit of money	1,778	other	other
discrimination	1103	other	other
deception (other)	973	other	other
pornography	843	other	other sex
other	815	other	other
other crime against public order	702	other	other
false police report	312	other	other
illegal possesion of a weapon/gun	297	other	other
joyriding	295	other	other
trespassing of a building	193	other	other
repeated deception/fraud	137	other	other
soft drugs	136	other	other
DUI (driving under influence)	131	other	other
hard drugs	131	other	other
driving with invalid license plate	113	other	other
other crime against public authority	82	other	other
refusal to follow public order	78	other	other
forgery of seals/brands	65	other	other
intentional bankruptcy	47	other	other
money laundering	42	other	other
animal abuse	<10	other	other
driving without a license	<10	other	other
stay of unwanted stranger	<10	other	other
refusal to take breath/blood test	<10	other	other

NOTE – The table lists the offenses underlying the offense categories used in the empirical analysis. The first column lists the original offense, the second column the number of incidents in our sample, the third column the broad offense category and the last column a more detailed category. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Appendix Table 2. Location Categories

Location (with >10 incidents)	N	Broad category
public road/waterway	544,942	road
dwelling	211,111	home
bar/restaurant/cafe	84,588	Out: Likely alcohol selling locations
shop	73,931	Out: Likely alcohol selling locations
parking/garage/stable	72,769	private transport
train/tram/bus stop/station	57,034	public transport
school/education facility	56,134	school
sport area/playground/sport building	33,957	Out: Other locations
courtyard/balcony/garden	33,184	Out: Other locations
public facility	31,611	Out: Other locations
company site	23,384	Out: Other locations
n/a	20,126	n/a
special site/building	19,737	Out: Other locations
campsite	12,824	not classified
vehicle (e.g. car, public transport	8,718	not classified
unknown	7,836	not classified
park	6,601	not classified
shed/garage at home	6,423	not classified
party/fair ground	5,612	not classified
club house	4,703	not classified
forest/heath/meadow/beach	4,266	not classified
hospital/nursing/retirement home	4,192	not classified
porch/elevator/stairwell	3,967	not classified
cellar	3,268	not classified
sauna/fitness	2,636	not classified
petrol station	2,260	not classified
market	1,947	not classified
office	1,917	not classified
builders' site/cabin	1,219	not classified
holiday home/mobil home	1,114	not classified
railways	1,090	not classified
bank/post office/currency exchange	1,083	not classified
cloak room	1,052	not classified
farm/greenhouse	843	not classified
church	815	not classified
storage facility	791	not classified
pharmacy	770	not classified
port/marina	552	not classified
practice (e.g. doctor)	385	not classified
casino	189	not classified
vessel	146	not classified
sex club/brothel	89	not classified
stall/kiosk	42	not classified
trailer/container	30	not classified

NOTE – The table lists the locations reported in the victimization data underlying our empirical analysis. The first column lists the original location (translated from Dutch), the second column the number of incidents in our sample, the third column the broad location category (own categorization) that we use for the heterogeneity analysis in Table 6. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Appendix Table 3. Cohort Statistics

Year of birth	Years at ages 15-19	Years at ages 20-24	Cohort size	% victimized (ages 15-19)	% victimized (ages 20-24)	Cohort size	% victimized (ages 15-19)	% victimized (ages 20-24)
				Females			Males	
1985		2005-2009	92062		26.28	96177		29.41
1986		2006-2010	95285		26.98	99084		29.96
1987		2007-2011	95276		27.84	100508		30.73
1988		2008-2012	95098		29.07	99603		30.81
1989		2009-2013	96319		29.67	100254		31.07
1990	2005-2009	2010-2014	99540	22.87	29.30	104702	25.41	30.10
1991	2006-2010	2011-2015	99601	23.69	28.55	104065	25.43	28.82
1992	2007-2011	2012-2016	97919	23.95	27.33	102938	26.11	27.30
1993	2008-2012	2013-2017	97228	24.54	25.52	101703	26.35	25.66
1994	2009-2013	2014-2018	97433	24.23	23.66	102205	24.80	24.02
1995	2010-2014		94898	23.01		99405	23.60	
1996	2011-2015		94132	21.09		99903	21.48	
1997	2012-2016		95658	19.68		99626	20.13	
1998	2013-2017		98245	17.75		103157	18.08	
1999	2014-2018		98984	15.70		104105	16.85	
All (1990-99)			973638	21.65		1021809	22.83	

NOTE - The table shows the cohort size and share ever victimized at ages 15-19 and 20-24 per year of birth cohort for females and males separately. Years of victimization include 2005-2018. Cohorts included in our analysis sample are those born in 1990-1999. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Appendix Table 4. Heterogeneity by Relative Age: September versus October Born

Sample:	Y: Share victimized	
	Males	Females
Offence:	Any	Any
Age threshold:	16	16
	(1)	(2)

Panel A. Born in September (young in cohort)

Age 16 (=1/0)	0.0344***	0.0333***
	(0.0059)	(0.0061)

Panel B. Born in October (old in cohort)

Age 16 (=1/0)	0.0265***	0.0246***
	(0.0061)	(0.0061)

Panel C. Difference-in-discontinuity (old versus young in cohort)

Age 16 * Born in October	-0.0077	-0.0064
	(0.0081)	(0.0084)
Age 16 (=1/0)	0.0343***	0.0321***
	(0.0058)	(0.0060)
Born in October	0.005	0.0085
	(0.0056)	(0.0057)
Bandwidth	+/-26 weeks	+/-26 weeks
N - Panel A	15900	15900
N - Panel B	16430	16430
N - Panel C	32330	32330
Mean of Y - September born	0.0976	0.0962
Mean of Y - October born	0.0988	0.0938
Year of birth f.e.	yes	yes
Birthday controls	yes	yes
Trend	sep. linear	sep. linear
Standard errors	robust	robust

NOTE - This table shows heterogeneity analyses by month of birth around the cutoff day for school entry (September and October). Cohorts born in September are on average relatively young per cohort, those born in October relatively old. Panel A shows the results for September birth cohorts, Panel B for October birth cohorts, and Panel C the differential effects. The underlying specification for Panels A and B is the baseline specification from Table 3, for Panel C the difference-in-discontinuity specification from Table 8. The outcome variable is the percentage share victimized per day-of-birth-cohort and calendar week who were victimized of the indicated offence. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Appendix Table 5. Siblings Spillovers

Sibling:	Older	Younger	Younger	Younger	Younger
		Y: Share victimized (any offence)			
Sample restriction:		>13 years	>13 years	< 2 yrs spacing	< 2 yrs spacing
Cohorts (older sibling):	All	All	All	All	1990-1995
Specification:	Baseline	Baseline	Age diff ctrl	Age diff ctrl	Age diff ctrl
	(1)	(2)	(3)	(4)	(5)
Panel A. Older: Males, Younger: Males					
Age 16 (=1/0)	0.0144***	0.0009	0.0009	0.0015	-0.0009
	(0.0044)	(0.0032)	(0.0032)	(0.0051)	(0.0066)
Mean of Y	0.0901	0.0480	0.0480	0.0553	0.0619
Age 18 (=1/0)	0.0101**	-0.0038	-0.0038	-0.0035	-0.0003
	(0.0044)	(0.0033)	(0.0033)	(0.0070)	(0.0099)
Mean of Y	0.1255	0.0696	0.0696	0.0945	0.1150
Panel B. Older: Males, Younger: Females					
Age 16 (=1/0)	0.0092**	-0.0009	-0.0009	-0.0028	-0.0063
	(0.0045)	(0.0033)	(0.0033)	(0.0054)	(0.0074)
Mean of Y	0.0895	0.0467	0.0467	0.0548	0.0615
Age 18 (=1/0)	0.0165***	-0.0022	-0.0022	0.0064	0.001
	(0.0045)	(0.0033)	(0.0033)	(0.0067)	(0.0096)
Mean of Y	0.1248	0.0674	0.0674	0.0844	0.1010
Panel C. Older: Females, Younger: Males					
Age 16 (=1/0)	0.007	0.0019	0.0019	0.0026	0.0156**
	(0.0043)	(0.0034)	(0.0034)	(0.0055)	(0.0075)
Mean of Y	0.0839	0.0494	0.0494	0.0584	0.0651
Age 18 (=1/0)	0.0124***	-0.0009	-0.0009	0.0076	-0.0030
	(0.0042)	(0.0034)	(0.0034)	(0.0070)	(0.0097)
Mean of Y	0.1089	0.0709	0.0709	0.0930	0.1105
Panel D. Older: Females, Younger: Females					
Age 16 (=1/0)	0.0087*	0.0043	0.0043	0.0054	0.012
	(0.0045)	(0.0033)	(0.0033)	(0.0055)	(0.0074)
Mean of Y	0.0828	0.0452	0.0452	0.0557	0.0619
Age 18 (=1/0)	0.0139***	0.0036	0.0036	0.006	0.0022
	(0.0043)	(0.0034)	(0.0034)	(0.0071)	(0.0100)
Mean of Y	0.1074	0.0662	0.0662	0.0846	0.1014

NOTE - This table shows spillover analyses onto younger siblings. The underlying specification is the baseline specification from Table 3, but using the next younger sibling's victimisation as the outcome. Every cell represents the coefficient on the Age 16 (Age 18) variable from a separate regression. The outcome variable is the percentage share of older/next younger siblings victimized per day-of-birth-cohort (of the older sibling turning 16/18) and calendar week. Column (1) shows the results when repeating the baseline for individuals in the sample who have a younger sibling of at least 13 years. Columns (2) to (5) show the results using the younger siblings' victimisation as the outcome, adding a control for the age difference in (3), restricting to no more than two years spacing in (4) and to pre-reform cohorts in (5). Panel A refers to males with next-younger male siblings; Panel B to males with next-younger female siblings; Panel C to females with next-younger male siblings; and Panel D to females with next-younger female siblings. Robust standard errors are shown in parentheses below the coefficient. *** p<0,01, ** p<0,05, * p<0,1. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.

Appendix Table 6. Regression Sample Means

	Y: Share victimized			
Sample:	Males	Females	Males	Females
Age threshold:	16	16	18	18
Table 4. Robustness tests				
Panels A and D	0.1002	0.0962	0.1333	0.1233
Panel B: +/-52 weeks	0.1016	0.0961	0.1332	0.1226
Panel B: +/-39 weeks	0.1015	0.0961	0.1334	0.1231
Panel B: +/-13 weeks	0.0985	0.0956	0.1333	0.1232
Panel C: +/-26 weeks	0.1002	0.0962	0.1333	0.1233
Panel C: Optimal Bandwidth	0.0977	0.0958	0.1333	0.1236
Table 5. Heterogeneity by offence				
Panel A. Any offense (baseline)	0.1002	0.0962	0.1333	0.1233
Panel A: Any offense except vehicle offenses	0.0973	0.0948	0.1323	0.1230
Panel A: Property offenses	0.0604	0.0625	0.0820	0.0844
Panel A: Property offenses except vehicle offenses	0.0576	0.0611	0.0811	0.0840
Panel A: Violent property offenses	0.0058	0.0017	0.0059	0.0023
Panel A: Violent offenses	0.0267	0.0226	0.0309	0.0238
Panel A: All violent offenses combined	0.0325	0.0244	0.0368	0.0261
Panel A: Other offenses	0.0067	0.0089	0.0136	0.0122
Panel B: Vehicle offenses	0.0028	0.0014	0.0009	0.0003
Panel B: Sex offenses	0.0006	0.0077	0.0004	0.0052
Panel B: Property: Bike theft	0.0235	0.0254	0.0277	0.0337
Panel B: Property: Burglary	0.0183	0.0199	0.0201	0.0222
Panel B: Property: Pickpocketing	0.0034	0.0063	0.0054	0.0104
Panel B: Violent property: Robbery	0.0049	0.0013	0.0044	0.0016
Panel B: Violent: Assault	0.0179	0.0116	0.0203	0.0130
Panel B: Violent: Threat	0.0043	0.0042	0.0047	0.0046
Panel B: Other: Fraud	0.0018	0.0009	0.0049	0.0038
Panel B: Other: Leaving scene of accident	0.0022	0.0017	0.0031	0.0018
Panel B: Other: Forgery of documents	0.0003	0.0003	0.0013	0.0012
Table 6. Heterogeneity by location				
Panel A: Road	0.0451	0.0366	0.0621	0.0466
Panel A: Home	0.0083	0.0135	0.0143	0.0198
Panel A: Out	0.0051	0.0091	0.0137	0.0178
Panel A: Other out	0.0162	0.0123	0.0159	0.0117
Panel A: Private transport	0.0022	0.0022	0.0051	0.0044
Panel A: Public transport	0.0026	0.0034	0.0071	0.0099
Panel A: School	0.0097	0.0100	0.0045	0.0049
Panel B: Road	45.010	37.970	46.483	37.874
Panel B: Home	8.327	14.189	10.846	16.132
Panel B: Out	5.166	9.422	10.297	14.370
Panel B: Other out	16.117	12.808	11.944	9.412
Panel B: Private transport	2.182	2.288	3.823	3.542
Panel B: Public transport	2.597	3.493	5.282	7.978
Panel B: School	9.637	10.301	3.413	4.035
Table 7. Heterogeneity by cohorts: MLDA reform				
Panel A: Any	0.1125	0.1047	0.1510	0.1396
Panel A: Property	0.0688	0.0695	0.0958	0.0988
Panel A: Violent	0.0306	0.0250	0.0360	0.0268
Panel A: Other	0.0065	0.0080	0.0112	0.0104
Panel B: Any	0.0729	0.0730	0.0987	0.0887
Panel B: Property	0.0425	0.0438	0.0525	0.0527
Panel B: Violent	0.0185	0.0170	0.0222	0.0183
Panel B: Other	0.0073	0.0106	0.0191	0.0158

NOTE - The table shows the relevant means for the different regressions and estimation samples. SOURCE - Results are based on calculations by the authors using microdata from Statistics Netherlands.