Essays on welfare benefits, employment, and crime

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3 Mandatory activation of welfare recipients: Less time, less crime?

Abstract

This paper investigates the effect of a mandatory activation program on crime among young adults. While the effects of active labor market policies on labor market outcomes are often assessed, spillover effects on crime are seldom analyzed. We estimate a regression discontinuity model, using individual-level administrative data on the entire Dutch population around the 27-year-old age threshold. Overall, we do not find an effect. However, for a relatively vulnerable group (non-natives), we find a reduction in crime of 12%. Crime is reduced on weekdays, but not during weekends. This evidence points towards incapacitation as the underlying causal mechanism: due to the program, participants have less time to commit crimes.

Introduction 3.1

OECD countries on average spend 0.5% of their GDP on active labor market policies (OECD 2017), and a substantial body of literature analyzes their impact on labor market outcomes (e.g. Card et al. 2010, 2018, Kluve

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2010). ALMPs, however, may also affect other life domains, such as crime. Widening the focus to crime is relevant, as welfare recipients share key characteristics with individuals that are over represented in crime statistics. Crime is also high on the public policy agenda because it involves vast economic and social costs. There are several mechanisms through which ALMPs may affect crime.

On the one hand, ALMPs may increase crime when individuals do not fulfill program conditions and are no longer eligible for income support. Research shows that individuals may choose not to apply for welfare when it becomes harder to qualify because of mandatory ALMPs (e.g. Persson et al. 2014). The resulting lack of a guaranteed minimum income increases crime substantially (Stam et al. 2019).

On the other hand, participating in a mandatory activation program can reduce crime in various ways. First, it may contemporaneously exert a direct incapacitation effect and reduce crime, as participants are left with less leisure time to commit crime (e.g. Bratsberg et al. 2019, Fallesen et al. 2018). Routine activity theory stresses that most crimes are conditional on opportunities to engage in crime, defined as situations in which suitable targets are present and (law-abiding) supervision is absent (Cohen and Felson 1979). When an ALMP reduces leisure time, these opportunities decrease.

Second, ALMPs that include educational components may reduce crime through human capital effects (e.g. Bratsberg et al. 2019, Lochner 2004). Educational attainment may improve human capital and future labor market outcomes (Becker 2009). When individuals earn higher wages after the program, current and future crime may be reduced because the opportunity costs of crime increase (e.g. Beatton et al. 2018, Bell et al. 2016, Lochner and Moretti 2004). Bratsberg et al. (2019) found evidence of long-term human capital effects of an ALMP on crime, due to stricter activation requirements increasing school attendance.

Third, ALMPs may reduce crime through socialization effects when participants engage in new law-abiding social environments. Sampson and Laub (1990) proposed that criminal behavior is decreased by institutions of informal social control, such as the workplace. Fallesen et al. (2018)

argued that participation in an ALMP can partly yield similar socialization effects as regular work environments.

Finally, if a program is successful in activating participants, this might reduce crime through multiple work-related mechanisms, including not only the aforementioned human capital effect, incapacitation effect, and socialization effects, but also psychological stability (Jahoda 1982).

We are aware of only five studies that investigate spillover effects of ALMPs on crime. Four ALMPs in Denmark, Norway and the United States have been found to reduce crime (Bratsberg et al. 2019, Corman et al. 2014, Fallesen et al. 2018, Schochet et al. 2008), whereas an ALMP in Sweden increased crime (Persson 2013). Four of these studies exploit geographical differences in the implementation of ALMPs to study crime effects (Bratsberg et al. 2019, Corman et al. 2014, Fallesen et al. 2018, Persson 2013). Schochet et al. (2008), on the other hand, used a randomized experiment to study the effect of a nationwide training program for disadvantaged youths.

More empirical insight into *how* ALMPs affect criminal behavior remains warranted. Therefore, this study explores the incapacitation mechanism. More specifically, we investigate direct incapacitation effects of a mandatory activation program on crime. We build upon Persson (2013) and Bratsberg et al. (2019), by separating effects for crimes committed on weekdays from those committed during the weekend. If incapacitation is the causal mechanism through which a policy affects criminal behavior, crime would mainly be reduced on weekdays, when time is spent in training or labor. Persson (2013), who mainly finds an increase in crime during weekdays, concludes that this is driven by individuals who are discouraged from program participation due to stricter activation requirements. The results of Bratsberg et al. (2019), on the other hand, point towards crime prevention through the incapacitation mechanism, as the reduction in (property) crime is larger on weekdays than during weekends.

This study assesses *if* and *how* a Dutch youth-targeted 'work-learn offer' (WLO) mandatory activation program affects crime. The WLO policy replaced the right to welfare benefits by a right to a work-learn offer, consisting of labor or training aimed at labor market activation. The Netherlands does not stand alone, as other countries have also implemented comparable youth-targeted ALMPs (OECD 2013), such as Job Corps (United States) and The New Deal for Young People (United Kingdom). The WLO was aimed at labor activation of young adults below the age of 27. We exploit the age-based policy variation using a regression discontinuity model, estimated on individual-level administrative data for the entire Dutch population around the 27-year-old age threshold.

Our contribution to the literature is fourfold. First, as far as we know, we are the first to measure the incapacitation effect of participation in a mandatory activation program on crime. That is, the effect of spending time in an ALMP instead of having leisure time. We can identify this effect because the ALMP under consideration did not affect employment.[1] As employment is unaffected, we can exclude work-related income and incapacitation effects. Furthermore, by distinguishing between weekday and weekend crime, we separate the incapacitation effect from potential human capital and socialization effects of the ALMP (which would reduce both weekday *and* weekend crime). The most closely-related paper by Bratsberg et al. (2019) investigates incapacitation effects from an ALMP-induced increase in time spent in school among youths (aged 18 to 21), whereas we contribute to the literature by investigating incapacitation effects of time spent in an activation program among young adults (around the age of 27). Incapacitation effects of an activation program may differ from incapacitation effects of school, due to the differences in social environment. Participation in an activation program likely entails becoming part of a new social group, whereas school participation takes place in a familiar social environment for those who are already enrolled. The behavioral impact of an ALMP could therefore differ substantially, dependent upon the extent to which fellow ALMP participants are more or less crime prone than fellow students.

Second, we build upon prior work by using variation within a geographical region, namely an age-based policy variation. Therefore, and complementary to earlier studies in this field, our identification strategy is not vulnerable to region-specific developments (e.g. the potentially endogenous timing of reforms, time-varying spatial heterogeneity, and

[1]Cammeraat et al. (2017) finds that the ALMP under study reduced welfare uptake but did not affect employment. Our study confirms this finding.

selective migration, see Bratsberg et al. 2019, Fallesen et al. 2018, Fiva 2009).

Third, the use of a vast individual-level administrative dataset enables us to examine heterogeneous effects by gender and migrant status. Traditionally, women have received relatively little attention in criminological research. Comparative studies are warranted as both crime and welfare dependence are highly gendered (Kruttschnitt 2013). Also, members of minority groups are of interest as they are on average more economically disadvantaged (Tonry 1997). Consequently, non-natives are overrepresented in both crime as well as welfare dependency, which makes them more likely to be affected by an ALMP.

Finally, the data enable us to assess outcomes on the monthly level. Considering the high employment dynamics of young relatively crime-prone individuals (Carcillo and Königs 2015), these measurements are more appropriate to capture the effects of interest than broader units of time used in most prior studies (mostly annual data).

Our main findings show that the WLO policy reduced crime among non-natives by 12%. Evidence points towards an incapacitation effect as the causal mechanism, i.e. a reduction in the opportunity to commit crime, due to the mandatory ALMP. Among men and women in general, we do not find an effect of the WLO policy on crime. All results are robust to changes in functional form and bandwidth size.

The remainder of this paper is structured as follows. Section 3.2 will firstly discuss the welfare policy under study (i.e. the work-learn offer). Section 3.3 describes the empirical model, after which we discuss the data, samples and some graphical evidence in Section 3.4. Section 3.5 contains the estimation results, followed by a discussion of the robustness checks in Section 3.6. We conclude and discuss the implications of the results in Section 3.7.

3.2 Welfare and the work-learn offer

Every legally-registered inhabitant of the Netherlands with insufficient means of subsistence is entitled to guaranteed minimum income benefits by the Dutch welfare system. Yet, there are several exclusion criteria for welfare eligibility. Individuals are considered ineligible for welfare benefits if they: (a) are below 18 years of age, (b) have a household income above the welfare norm, (c) are eligible for other benefits (e.g. unemployment benefits), (d) have assets exceeding the specified thresholds,[2] and (e) are incarcerated. There is no limit on the time period during which individuals can receive welfare. An important condition for welfare eligibility is that recipients must meet job search requirements (such as monthly job application targets), and are required to accept all job opportunities. Reintegration is supported by municipalities through job-search assistance.

The welfare benefit level is relatively high in the Netherlands. The mean welfare benefits in our observation period were 1,315 euros per month for couples, 920 euros per month for single parents, and 655 euros per month for single-person households. Welfare recipients can additionally receive health insurance subsidies, child subsidies, and housing subsidies. The OECD corroborates that the minimum income benefit level is comparatively high in the Netherlands, with 60% of median disposable income in 2018 (OECD 2018a). This indicator is substantially lower in the US (6%), and only slightly exceeded by Denmark (63%), Ireland (64%), and Japan (65%).

From 1996 until 2008, under the "General Welfare Act" (1996-2003) and the "Work and Social Assistance Act" or WSAA (2004-2008), welfare eligibility conditions were the same for all Dutch citizens between 18 and 64 years. In October 2009, the "Investment in the Young Act" (IYA) was introduced. From then on, welfare applicants below the age of 27 were no longer subject to the WSAA. The official goal of this reform was labor activation of inactive youths. The reform aimed to achieve this by replacing the right to welfare benefits with a right to a 'work-learn offer'.

[2]Welfare benefits are means-tested in the Netherlands. To be considered eligible for welfare benefits in 2011, the maximum net worth for single-person households was €5,555, and €11,110 for couples and single parents. An additional maximum amount of 46,900 euros of housing wealth was allowed for homeowners.

Youths were only entitled to benefits in the form of an income supplement if their income from the WLO was lower than the social assistance norm. Municipalities had the obligation to offer a work-learn offer, meaning that they had to facilitate either (1) generally accepted labor, or (2) provisions aimed at labor integration in the form of education, assistance in acquiring employment, or social activation. The latter of which was defined as "unrewarded, societally useful activities aimed at labor integration". While the IYA was implemented nation-wide as of the first of October 2009, this was only the case for new applicants. Youths who were already recipients of benefits under the WSAA were not immediately transferred to the IYA, but were entitled to general welfare under the WSAA until June 2010. This transitional period was prolonged for single parents, who were transferred to the IYA as of the first of January 2011. In the analysis we use data as from July 2010. However, when we exclude single parent until January 2011 the results are highly similar. Apart from the WLO, recipients on either side of the 27-year-old threshold were subject to identical rules. This also applied to the welfare benefit level, which is equal across the ages of 21 to 64.

Despite apparent satisfaction with the WLO policy, a main critique was that it allowed for passivity of the youths themselves. They were considered to be left with too little obligations as the municipality provided their WLOs. This was the officially-stated motivation for the abolishment of the IYA and work-learn offer program, as of January 2012.

Empirical strategy 3.3

The sharp discontinuity in welfare policy, in the form of the 27-year-old threshold, facilitates the application of a regression discontinuity (RD) design. By comparing individuals just above the treatment assignment threshold to those just below that threshold, the RD design enables us to assess the causal effects of the WLO policy on crime, being incapacitated by the ALMP, and income. Since the WLO policy may not only affect welfare recipients, but may also discourage individuals from applying for welfare benefits, we include the full population around the age of 27 in our analysis. In this way we capture potential discouragement effects.

Theoretically, by taking a narrow enough bandwidth to measure the effect on the threshold itself, the RD approach isolates treatment variation that is "as good as randomized" (Lee 2008). The availability of data on a (sub)monthly level allows for a sharp regression discontinuity design. As crime is a dichotomous variables, we estimate a probit model with the following specification:

$$y_{it}^{*} = \beta_0 + \beta_1 RD_{it} + \beta_2 A_{it} + \beta_3 1(A_{it} < 27) A_{it} + \beta_4 X_{it} + \beta_5 T_t + \epsilon_{it} \tag{3.1}$$

where y_{it}^{*} is a latent variable. Individual i is suspected of having committed crime if the continuous latent variable y_{it}^{*} is positive (then $y_{it} = 1$, otherwise $y_{it} = 0$). RD_{it} is the treatment dummy that captures being subject to the work-learn offer policy (a value of one indicates an age below the 27-year-old threshold for individual i at time t), A_{it} is age (in months), $1(A_{it} < 27)A_{it}$ is an interaction term that allows for different slopes on both sides of the discontinuity, X_{it} is a vector of individual characteristics, T_t represents a linear time (month) trend, and ϵ_{it} denotes the error term with a standard normal distribution. β_1 is the coefficient of interest and shows us the extent to which the WLO policy affects crime. We estimate a similar probit model using incapacitation by the ALMP as dependent variable.

To assess the change in income from wages and welfare benefits, we run an OLS model with log-transformed income as the dependent variable. The OLS model is specified as follows:

$$\ln(I_{it}) = \gamma_0 + \gamma_1 RD_{it} + \gamma_2 A_{it} + \gamma_3 1(A_{it} < 27) A_{it} + \gamma_4 X_{it} + \gamma_5 T_t + u_{it} \tag{3.2}$$

where $\ln(I_{it})$ denotes the log-transformed income of individual i in month t from wages and welfare benefits. γ_1 is the coefficient of interest and shows us the extent to which the WLO policy affects income.

To obtain a better understanding of the mechanisms behind the causal effect of the WLO policy on crime, we also investigate employment and we simultaneously model weekday and weekend crime in a bivariate probit model. In this way we test the effects of the WLO policy on employment, weekday crime, and weekend crime.

Following the work of Lee and Card (2008), we cluster the errors on the assignment variable age (in months). As our assignment variable is discrete, this clustering approach accounts for the group structure induced by potential specification errors and prevents overstatement of the significance of the estimated effects. Additionally, to increase the interpretability of the obtained estimates, we calculate the average treatment effects (ATEs) for the probit models.

As a robustness checks, we compare both linear and quadratic model specifications, as well as multiple bandwidths. Following Gelman and Imbens (2018), we limit the analyses to local linear and local quadratic polynomials.[3] The choice of bandwidth involves a "bias-variance trade-off" (Cattaneo et al. 2020). A (too) large bandwidth will result in more bias if the unknown function differs considerably from the linear or quadratic model used for approximation. On the other hand, a (too) small bandwidth increases the variance because the number of observations in the interval will be smaller. For the baseline analyses, we specify a bandwidth of seven months on each side of the 27th-birthday-month cut-off. Furthermore, in the robustness checks we compute several data driven bandwidths as presented by Calonico et al. (2014), and we show the results of several bandwidths.

The main underlying assumption of the RD approach is the continuity assumption. The characteristics of the participants are required to evolve smoothly over the assignment variable. The distribution of characteristics just above the threshold should not differ from the distribution just below the threshold. If there is a discontinuity, this would indicate that the participants are able to manipulate the assignment variable (Lee and Lemieux 2010). One could then no longer state that a discontinuity in the outcome variable on the treatment threshold is a treatment effect. This

[3]The quadratic model specification additionally includes quadratic terms for the assignment variable (A_{it}^2 and $1(A_{it} < 27)A_{it}^2$). Gelman and Imbens (2018) find that using global higher-order polynomials in regression discontinuity designs result in noisy estimates, poor coverage of confidence intervals, and sensitivity to the degree of the polynomial.

assumption realistically holds, as we use age (in months) as the assignment variable, which is centrally registered and cannot be manipulated.

3.4 Data

To estimate the models, we use unique longitudinal individual-level data from Statistics Netherlands on all registered Dutch inhabitants around the welfare policy age threshold.[4] In this way we take into account that the policy does not only affect welfare recipients, but may also discourage people to apply for welfare. We link administrative data on welfare benefits, employment and criminal offenses, as well as various socio-demographic variables. As the work-learn offer was fully implemented in July 2010 and abolished as of January 2012, we have an 18-month post-reform observation window.

Data on welfare benefits are derived from municipal monthly payment registrations. These data cover welfare receipt and WLO participation status, and the amount of welfare benefits received. The employment data are collected by the Employee Insurance Agency (i.e. 'UWV'), which is the central Dutch administrative authority that registers all employee insurances. The available daily crime measures are also aggregated to dichotomous monthly values. The crime data are derived from crime reports of the Dutch law enforcements agencies, which have been submitted to the public prosecutor. These reports contain information concerning crimes of which individuals are officially suspected and are strong indicators of committed offenses. When brought to trial, approximately 90 percent of cases result in a conviction (Statistics Netherlands et al. 2013). Although we only observe registered crime, we do not expect the unmeasured crime distribution to be correlated with the policy discontinuity at the 27-year-old threshold.

[4] Under certain conditions, these microdata are accessible to all researchers for statistical and scientific research. For further information, contact microdata@cbs.nl. Included datasets are bijstanduitkeringint, gbaadresobjectbus, gbapersoontab, integraal huishoudens inkomen, integraal persoonlijk inkomen, polisbus, spolisbus, verdtab and vslgwbtab.

Descriptives 3.4.1

Following the research design, we select Dutch inhabitants that have reached the age of 27 between July 2010 and December 2011. This results in a total sample size of 309,093 individuals, aged 25 to 28 years, and a total of 5,415,540 monthly observations from July 2010 to December 2011. To account for potential heterogeneous effects, we run the analyses for three subsamples. Men and women are considered separately, as men are more likely to commit offenses compared to women (e.g. Statistics Netherlands 2018a, Steffensmeier and Allan 1996). Previous literature has emphasized the importance of analyzing the effects of welfare on crime among women, due to their higher poverty and welfare dependency rates (e.g. Corman et al. 2014, Holtfreter et al. 2004). Additionally, we analyze the relatively vulnerable group of non-natives.[5] Minority groups more oftenly live in socially and ethnically segregated low-income communities characterized by social disorganization and impeded cooperation, where oppositional identities and crime flourishes (Peterson and Krivo 2005). As such, by reducing the exposure time to such an environment, program participation may have a greater behavioral impact on non-natives, as compared to the full population. Their disadvantaged position is supported by the descriptives discussed below.

Table 3.1 provides an overview of the most relevant characteristics in the selected samples. Below the policy age threshold of 27, we find an average work-learn offer (WLO) participation rate of 1.33% for the full sample. The rate is only 0.02% above the cut-off, averaging to 0.64% over the full observation window. 3.75% of the individuals receive welfare benefits in any given month. The employment rate is 75.57%, and the monthly crime rate is 0.27%. For the full sample period, 3.51% of the sample committed crime at least once.

Men have a comparatively low WLO participation rate of 1.17% (age < 27). This rate is higher among women (1.50%), and highest among non-natives (2.60%). This is in line with the welfare dependency rates, which are the lowest among men (3.27%), and the highest among non-

[5] An individual is considered a native resident if the individual is born in the Netherlands, as well as both of the parents.

natives (7.76%). Conversely, the employment rate is the highest among men and the lowest among non-natives. Non-natives also show the lowest average annual incomes, with a personal primary income of €19,070 and a standardized household income of €18,016. These are highest among men, with €30,088 and €22,459, respectively. Men score highest on monthly and total crime rates, with 0.45% and 5.66%, respectively. A monthly crime rate of 0.45% means that in an average month, 0.45% of the males commit at least one offense. During the whole sample period 5.66% of the males commit at least one offense. Non-natives show the highest average number of offenses per offender (1.40). The lowest crime rates are found among women, which show monthly and total crime rates of 0.09% and 1.30%, and on average 1.19 offenses per offender. The weekday and weekend crime rates do not sum to the total crime rate, as some individuals simultaneously commit crime during both week and weekend days within a month.

Table 3.1: Descriptive statistics, July 2010-December 2011

	Full sample	Men	Women	Non-natives
WLO participation rate (%)	0.64	0.57	0.71	1.23
if $Age < 27$	1.33	1.17	1.50	2.60
if $Age \geq 27$	0.02	0.03	0.01	0.04
Welfare dependency rate (%)	3.75	3.27	4.24	7.76
Employment rate (%)	75.57	75.95	75.17	59.26
Crime rate (total, %)	3.51	5.66	1.30	5.20
Crime rate (%)	0.27	0.45	0.09	0.44
Weekday crime rate (total, %)	2.30	3.64	0.93	3.64
Weekday crime rate (%)	0.17	0.28	0.06	0.29
Weekend crime rate (total, %)	1.65	2.81	0.46	2.30
Weekend crime rate (%)	0.11	0.19	0.03	0.16
Offenses per offender	1.36	1.40	1.19	1.40
Annual personal primary income	27,123	30,088	24,097	19,070
Annual standardized HH income	22,267	22,459	22,070	18,016
Number of individuals	309,093	156,596	152,497	97,060
Number of observations	5,415,540	2,735,946	2,679,594	1,625,124

Notes. The shown standard crime rates indicate the average percentage of individuals committing at least one crime in any given month, whereas the total crime rates represent the percentage of individuals committing at least one offense during the full (18-month) observation window.

Graphical evidence 3.4.2

Before turning to the estimation results, we discuss some exploratory graphs on crime rates, work-learn offer (WLO) participation rates, and income around the 27-year-old threshold. Figures 3.1a to 3.1c show the evolution of crime rates across age for men, women and non-natives. The lines present local polynomial smooth plots, along with 95% confidence intervals. Every dot represents a monthly crime rate and the order of magnitudes in the figures are in line with the monthly crime rates in Table 3.1. For men and women, we do not find discernible discontinuities in the crime rates (Figures 3.1a and 3.1b). For non-natives, we find a sizeable discontinuity at the age cut-off (Figure 3.1c). The jump upwards indicates a reduction in crime due to the WLO policy, which only applies to those on the left-hand side of the threshold.

We assume that a jump in the crime rate around the age of 27 is due to a difference in the WLO participation rate around this threshold. To check for this, Figures 3.2a to 3.2c present the evolution of the WLO participation rates across age. Compared to the crime rates, the standard errors are substantially smaller. Note that the graphs for men and women have smaller scales on the vertical axes than the graph for non-natives. For non-natives, we find the largest discontinuity at the policy threshold of about 3 percentage points. Discontinuities are smaller among men and women in general, which is to be expected from their lower welfare dependency and higher employment rates.

Discontinuities in the crime rate around the age of 27 may not only be due to differences in WLO participation. If the work-learn offer affects income or employment, this may also affect crime rates. Figures 3.3a to 3.3c therefore show the evolution of the average log-transformed income from wages and welfare benefits across age. For all of the investigated samples, we do not find any substantial discontinuities in income at the age threshold. This suggests that the apparent discontinuity in crime among non-natives is not due to income effects. We also do not find any notable discontinuity in employment (Appendix 3.A).

To further explore whether the WLO policy affects crime through incapacitation, we separately graph the evolution of weekday and weekend crime rates. Figures 3.4a to 3.5c present the weekday and weekend crime rates across age. In line with Figures 3.1a to 3.1c, we do not find any distinguishable discontinuity at the policy threshold for men and women. For non-natives, we find a jump upwards at the policy threshold in weekday crime (Figure 3.4c), while a discontinuity in crime committed on weekends appears absent (Figure 3.5c). These findings corroborate the hypothesis that the WLO reduces crime through incapacitation, as such reductions would take place during the time spent in training or labor (i.e. workdays). If crime would be reduced through other mechanisms (such as people having improved expectations about their future income, or socialization effects), crime rates would also decline during the weekend.

Figure 3.1: Crime rates across age among men (a), women (b) and non-natives (c)

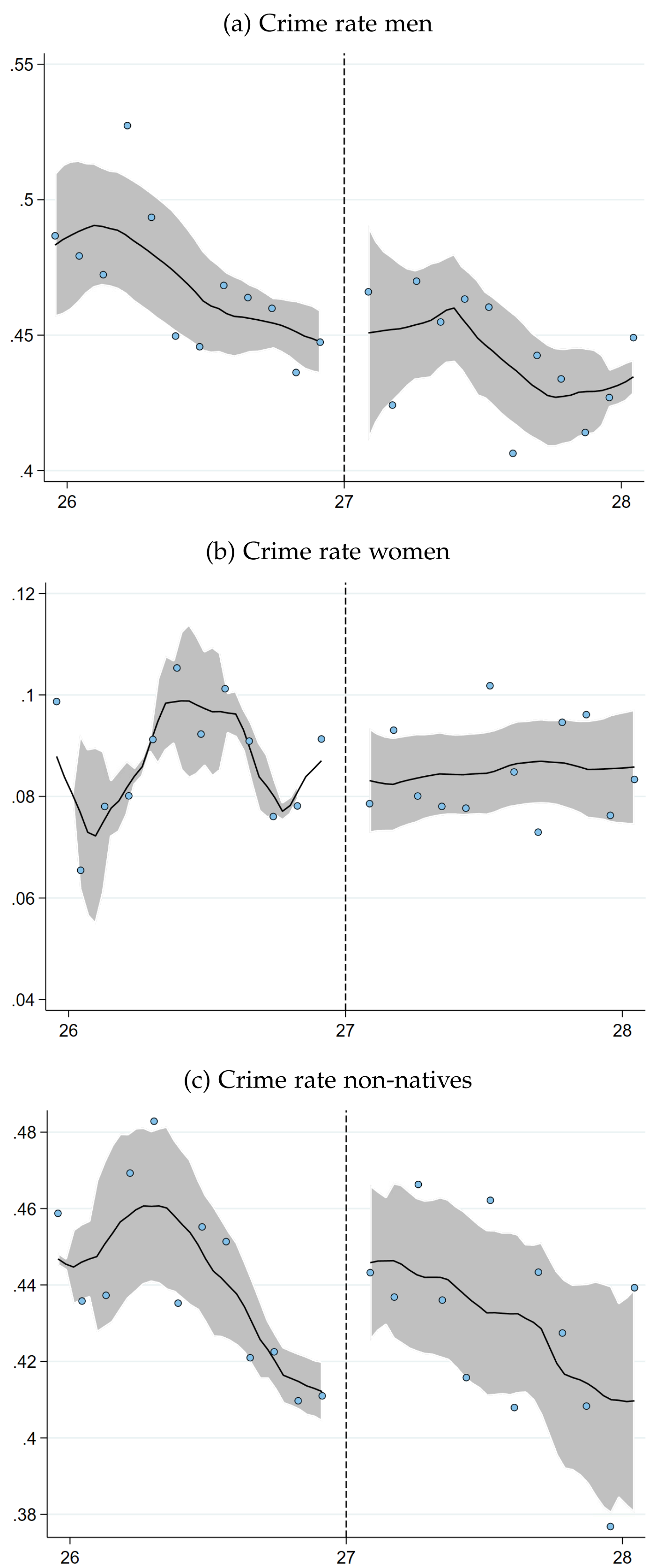
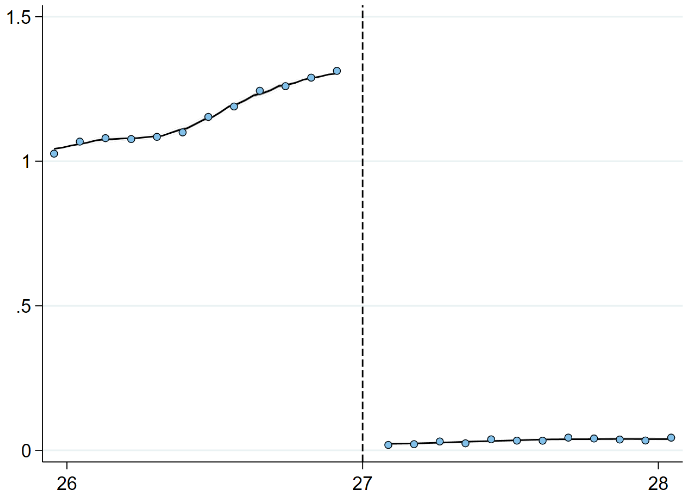
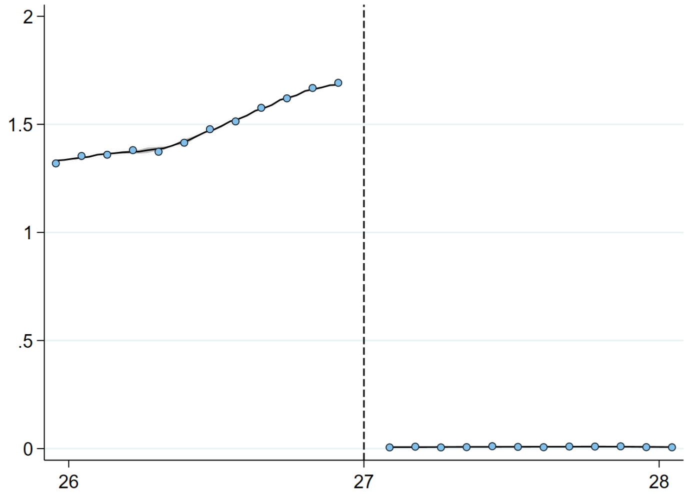


Figure 3.2: Work-learn offer rates across age among men (a), women (b) and non-natives (c)

(a) Work-learn offer rate men



(b) Work-learn offer rate women



(c) Work-learn offer rate non-natives

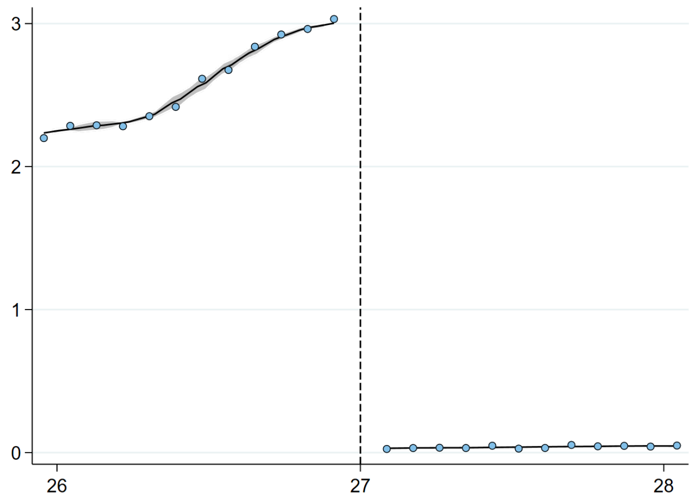
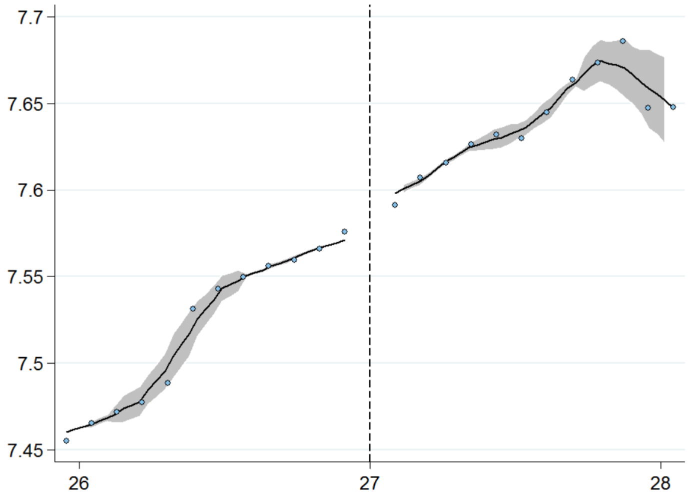
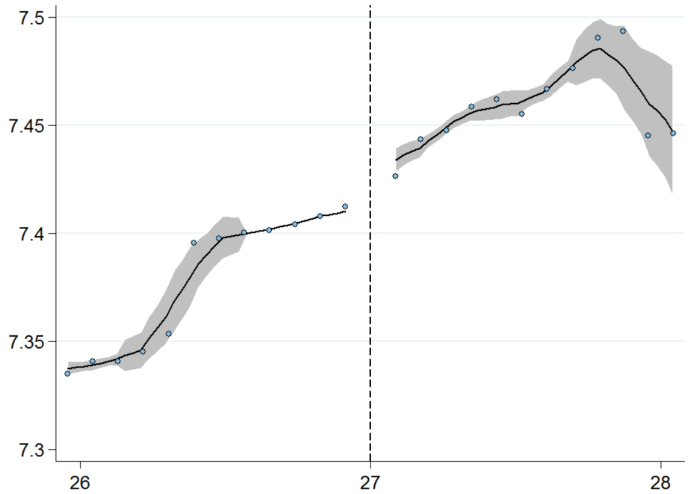


Figure 3.3: Log-transformed income across age among men (a), women (b) and non-natives (c)

(a) Log-transformed income men



(b) Log-transformed income women



(c) Log-transformed income non-natives

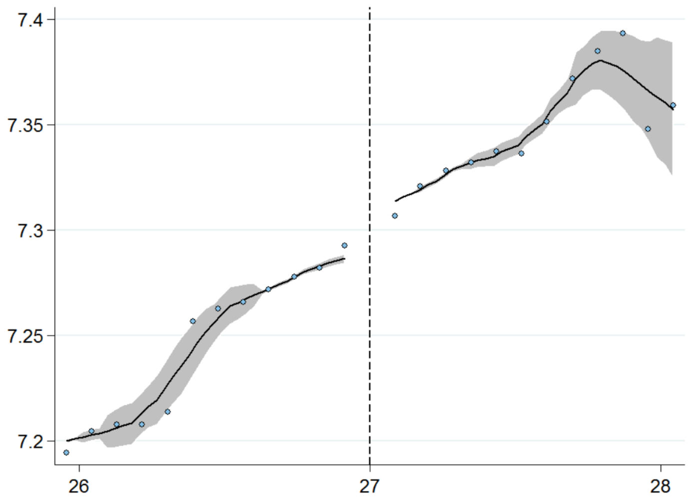
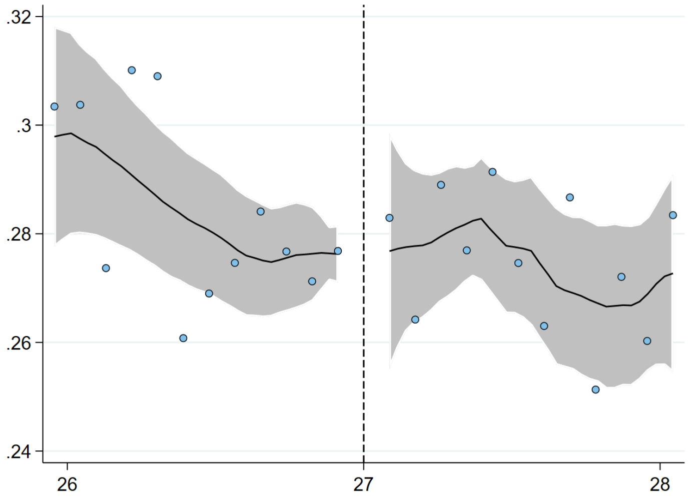
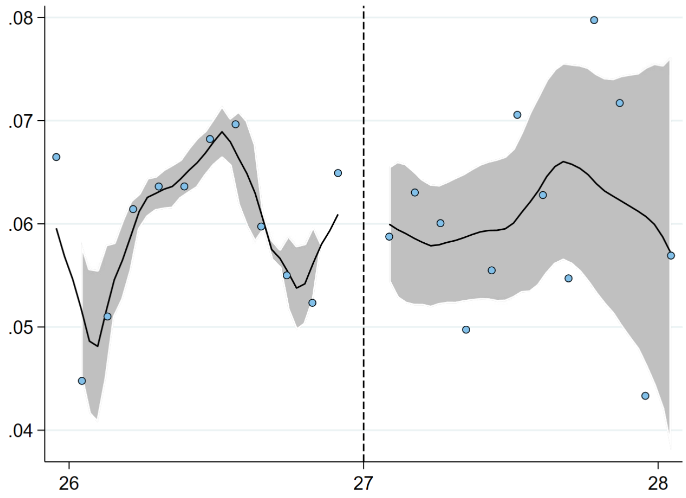


Figure 3.4: Weekday crime rates across age among men (a), women (b) and non-natives (c)

(a) Weekday crime rate men



(b) Weekday crime rate women



(c) Weekday crime rate non-natives

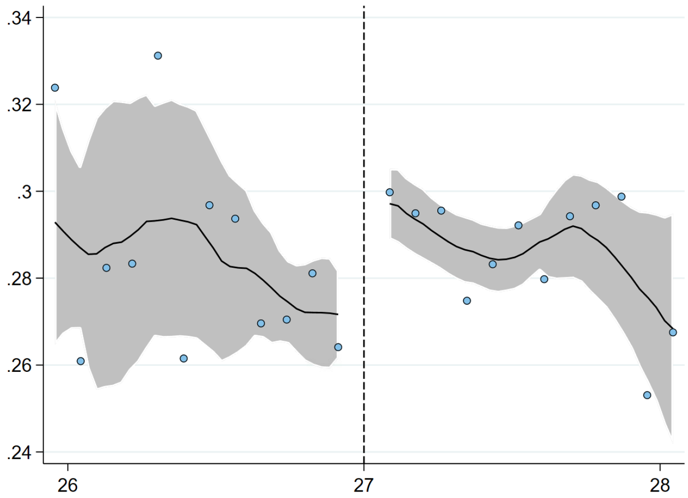
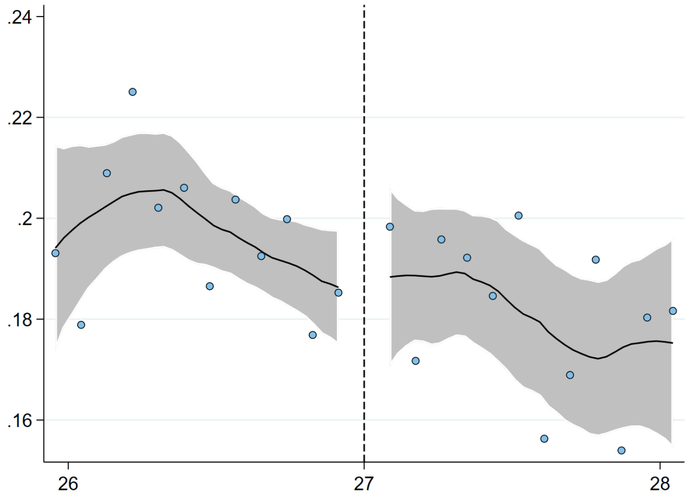
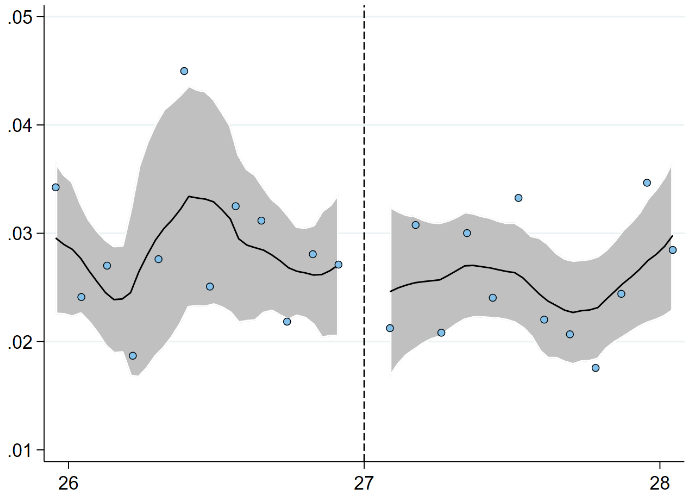


Figure 3.5: Weekend crime rates across age among men (a), women (b) and non-natives (c)

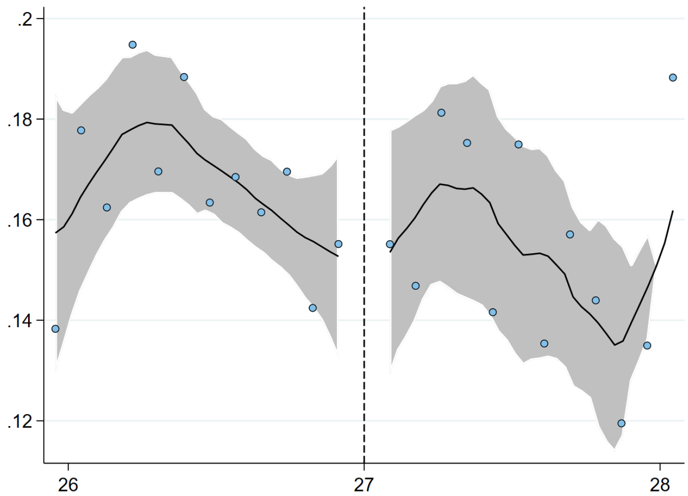
(a) Weekend crime rate men



(b) Weekend crime rate women



(c) Weekend crime rate non-natives



3.5 Results

Table 3.2 presents the baseline regression discontinuity results for crime among men, women and non-natives. In line with Figures 3.1a and 3.1b, for men and women we do not find a significant effect of the work-learn offer on crime. When focussing on non-natives, we do find a statistically significant negative coefficient of -0.0421. To enhance the interpretability of the probit coefficients, we compute the average treatment effects (ATEs). In line with Figure 3.1c, the ATEs show that among non-natives, the WLO policy reduced the average monthly probability of committing a crime with 0.05 percentage points (from 0.46% to 0.41%). In relative terms, this amounts to a reduction of 12% ((0.41-0.46)/0.46).

Table 3.3 shows the estimation results for participation in the work-learn offer program. We find statistically significant positive coefficients across all samples (1.3991 for men, 1.7718 for women, and 1.5964 for non-natives). The ATEs show that the discontinuity is the largest among non-natives, with 3.12 percentage points. This discontinuity is in line with Figure 3.2c, and is about twice the size of the result found for men (1.49 percentage points) and also substantially higher than the ATE for women (1.75 percentage points). In line with the descriptives and graphical evidence (Sections 3.4.1 and 3.4.2), the estimates suggest very high treatment compliance around the policy age threshold.

Table 3.4 presents the estimation results for log-transformed income from wages and welfare benefits. We find statistically significant reductions among women and non-natives, but the effects are small (-1.45% and -0.97% for women and non-natives, respectively). For men, we find only a weakly significant reduction of 0.74% ($p<.10$). In Section 3.6, we find that none of the income estimates are robust against changes in functional form and bandwidth specification. Thus, we find no evidence that the WLO increases income. If any, we only find a small non-robust lower income level before the threshold, which is unlikely to explain the reduction in crime. For employment, we also do not find any substantive or robust discontinuity (Appendix 3.A).

The discontinuity in the WLO participation rate, together with the reduction in crime, point towards incapacitation or human capital effects as causal mechanisms through which the work-learn offer policy affects crime. Table 3.5 corroborates the incapacitation hypothesis, by presenting the estimates obtained from a simultaneous modelling of both weekday and weekend crime outcomes. Although the estimates for week and weekend crime do not differ significantly from each other, we find the weekday crime coefficient (-0.0393) to be statistically different from zero ($p<.001$), whereas this does not hold for weekend crime. This is in line with Figures 3.4c and 3.5c, where we see a much clearer discontinuity for weekday crime compared to weekend crime. The ATEs show that the WLO policy reduced weekday crime with 0.03 percentage points, from 0.27% to 0.30%. This is a relative decline of 11.6%.

To sum up, the WLO policy reduced crime among non-natives by almost 12%. This is likely to be the result of increased incapacitation by the work-learn offer.

Table 3.2: Baseline probit estimates for crime

	MEN	WOMEN	NON-NATIVES
Crime			
RD	-0.0110	-0.0046	-0.0421***
	(0.0146)	(0.0336)	(0.0095)
Age	-0.0032	0.0033	-0.0027
	(0.0035)	(0.0054)	(0.0030)
Age $* 1(< 27)$	0.0013	-0.0140†	-0.0037
	(0.0036)	(0.0073)	(0.0031)
Male			0.5405***
			(0.0135)
Native	-0.2697***	-0.2475***	
	(0.0060)	(0.0157)	
Time (month)	0.0013	0.0010	0.0004
	(0.0008)	(0.0012)	(0.0013)
Average treatment effects			
Monthly probabilities			
If treatment = 1 (%)	0.44	0.09	0.41
If treatment = 0 (%)	0.46	0.09	0.46
ATE (%point)	-0.01	-0.00	-0.05***
	(0.02)	(0.01)	(0.01)
Observations	1,658,432	1,623,579	983,372
Individuals	155,449	151,517	95,381

Notes. Linear model specification, 7-month bandwidth (14 months total), standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 3.3: Baseline probit estimates for work-learn offer status

	MEN	WOMEN	NON-NATIVES
Work-learn offer			
RD	1.3991***	1.7718***	1.5964***
	(0.0274)	(0.0476)	(0.0283)
Age	0.0226***	0.0018	0.0018
	(0.0057)	(0.0106)	(0.0085)
Age $* 1(< 27)$	-0.0204***	-0.0013***	0.0020
	(0.0058)	(0.0106)	(0.0087)
Male			-0.0403***
			(0.0062)
Native	-0.5339***	-0.4512***	
	(0.0090)	(0.0074)	
Time (month)	0.0154***	0.0201	0.0202***
	(0.0005)	(0.0009)	(0.0013)
Average treatment effects			
Monthly probabilities			
If treatment = 1 (%)	1.51	1.76	3.15
If treatment = 0 (%)	0.02	0.01	0.03
ATE (%point)	1.49***	1.75***	3.12***
	(0.05)	(0.09)	(0.13)
Observations	1,658,432	1,623,579	983,372
Respondents	155,449	151,517	95,381

Notes. Linear model specification, 7-month bandwidth (14 months total), standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 3.4: Baseline OLS estimates for log-transformed income

	MEN	WOMEN	NON-NATIVES
Log income			
RD	-0.0074†	-0.0145*	-0.0097*
	(0.0042)	(0.0055)	(0.0043)
Age	0.0070***	0.0041**	0.0046***
	(0.0010)	(0.0013)	(0.0010)
Age $* 1(< 27)$	-0.0015	-0.0034*	-0.0006
	(0.0010)	(0.0012)	(0.0009)
Male			0.1340***
			(0.0010)
Native	0.2940***	0.2573***	
	(0.0022)	(0.0019)	
Time (month)	0.0026*	0.0040**	0.0035**
	(0.0009)	(0.0010)	(0.0009)
Observations	1,302,483	1,275,377	645,147
Individuals	132,420	128,987	71,197

Notes. Linear model specification, 7-month bandwidth (14 months total), standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 3.5: Baseline biprobit estimates for weekday and weekend crime

	MEN	WOMEN	NON-NATIVES
Weekday crime			
RD	0.0000	-0.0018	-0.0393***
	(0.0105)	(0.0316)	(0.0101)
Age	-0.0010	0.0022	-0.0031†
	(0.0023)	(0.0042)	(0.0016)
Age $*$ $1(<27)$	0.0031	-0.0095	0.0002
	(0.0024)	(0.0067)	(0.0035)
Male			0.4812***
			(0.0138)
Native	-0.2870***	-0.2630***	
	(0.0070)	(0.0179)	
Time (month)	-0.0004	0.0020	-0.0001
	(0.0009)	(0.0020)	(0.0017)
Weekend crime			
RD	-0.0246	-0.0103	-0.0367
	(0.0206)	(0.0539)	(0.0267)
Age	-0.0058	0.0050	-0.0019
	(0.0053)	(0.0099)	(0.0064)
Age $*$ $1(<27)$	-0.0011	-0.0216	-0.0089
	(0.0056)	(0.0137)	(0.0068)
Male			0.5761***
			(0.0212)
Native	-0.2045***	-0.1811***	
	(0.0099)	(0.0188)	
Time (month)	0.0032*	-0.0004	0.0012
	(0.0015)	(0.0026)	(0.0025)
ρ	0.4541***	0.4580	0.3984***
	(0.0072)	(0.0245)	(0.0192)
Average treatment effects			
Monthly probabilities			
Weekday crime			
If treatment = 1 (%)	0.28	0.06	0.27
If treatment = 0 (%)	0.28	0.06	0.30
ATE (%point)	0.00	-0.00	-0.03***
	(0.01)	(0.01)	(0.01)
Weekend crime			
If treatment = 1 (%)	0.18	0.03	0.15
If treatment = 0 (%)	0.20	0.03	0.17
ATE (%point)	-0.01	-0.00	-0.02
	(0.01)	(0.01)	(0.01)
Test weekday vs weekend			
χ^2	3.55†	0.03	0.01
p-value	0.0596	0.8725	0.9376
Observations	1,658,432	1,623,579	983,372
Individuals	155,449	151,517	95,381

Notes. Linear model specification, 7-month bandwidth (14 months total), standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

3.6 Robustness checks

We conduct robustness checks over (a) two functional forms: a linear and a quadratic model, and (b) eight bandwidth specifications, ranging from 3 to 17 months on each side of the 27th-birthday-month cut-off. Following Gelman and Imbens (2018), we limit the sensitivity analyses to first and second-order polynomials. 17 months is the upper bandwidth limit due to the 18-month policy observation window, during which the work-learn offer policy was applied to all welfare recipients below the age of 27.

Using the popular mean-squared error (MSE) bandwidth selection method over the full sample, we find optimal bandwidths of 4 for crime, 4 for work-learn offer participation, 5 for log-transfered income, 4 for weekday crime, and 4 for weekend crime. Bandwidths much larger than the MSE-optimal bandwidth will lead to point estimators that have too much bias, and bandwidths much smaller than the MSE-optimal choice will lead to estimators with too much variance. Conclusions do not change when looking at the MSE-optimal bandwidths, compared to the baseline bandwidth of 7 months shown in the previous section. The Coverage Error Rate (CER) optimal bandwidth is 3 for crime, 3 for work-learn offer status, 4 for log-transfered income, 3 for weekday crime, and 3 for weekend crime. We find some divergent results for the small bandwidth of 3 months, which is attributable to the number of observations becoming too small for the low crime prevalence.

For men and women, the robustness checks confirm null effects of the work-learn offer policy on crime, as almost none of the models produce statistically significant estimates (Table 3.6). For non-natives, the conclusions are robust to changes in functional form and bandwidth size. A decrease in bandwidth size introduces more noise, which results in a nonsignificant estimate in one model. When increasing the bandwidth, however, the coefficients are similar and significantly different from zero across all model specifications.

Table 3.7 shows that the estimates for the work-learn offer are the least sensitive to changes in functional form and bandwidth. Across all samples, we find minimal variation in coefficient size and all estimates are statistically significant ($p<.001$).

Table 3.8 presents the estimates for income from wages and benefits. None of the estimates are robust to changes in functional form, nor statistically significant beyond a 9-month bandwidth specification. We also find the coefficients to be very small, across all samples. These findings confirm that it is not very likely that income effects act as a causal mechanism through which the WLO policy affects crime.

Table 3.9 supports the hypothesis that the WLO policy affects crime through incapacitation. In line with the baseline estimates, most of the weekday and weekend crime estimates are not significantly different from each other. Nonetheless, we find statistically significant estimates for weekday crime across all specifications, in contrast to weekend crime estimates, which are not significantly different from zero across almost all model specifications.

Table 3.6: Crime estimates with different bandwidths and functional forms

BANDWIDTH		3 MONTHS	5 MONTHS	7 MONTHS	9 MONTHS	11 MONTHS	13 MONTHS	15 MONTHS	17 MONTHS
Men									
	Linear	-0.0108	-0.0099	-0.0110	-0.0214†	-0.0184†	-0.0139	-0.0113	-0.0124
		(0.0206)	(0.0168)	(0.0146)	(0.0126)	(0.0108)	(0.0100)	(0.0094)	(0.0093)
	Quadratic	-0.0767***	-0.0216	-0.0045	0.0049	-0.0112	-0.0197	-0.0225	-0.0178
		(0.0001)	(0.0204)	(0.0246)	(0.0187)	(0.0175)	(0.0165)	(0.0152)	(0.0148)
Women									
	Linear	0.0568†	-0.0221	-0.0046	0.0102	0.0267	0.0353	0.0196	0.0120
		(0.0293)	(0.0422)	(0.0336)	(0.0274)	(0.0251)	(0.0251)	(0.0237)	(0.0233)
	Quadratic	0.2739***	0.1168***	0.0234	-0.0184	-0.0251	-0.0232	0.0171	0.0327
		(0.0005)	(0.0322)	(0.0433)	(0.0517)	(0.0494)	(0.0424)	(0.0332)	(0.0319)
Non-natives									
	Linear	-0.0193**	-0.0497***	-0.0421***	-0.0412***	-0.0377***	-0.0286**	-0.0284**	-0.0280**
		(0.0074)	(0.0106)	(0.0095)	(0.0071)	(0.0092)	(0.0094)	(0.0086)	(0.0090)
	Quadratic	-0.0377***	0.0080	-0.0352*	-0.0408**	-0.0443**	-0.0549***	-0.0476***	-0.0432***
		(0.0006)	(0.0125)	(0.0146)	(0.0121)	(0.0131)	(0.0123)	(0.0103)	(0.0118)

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 3.7: Work-learn offer status estimates with different bandwidths and functional forms

BANDWIDTH		3 MONTHS	5 MONTHS	7 MONTHS	9 MONTHS	11 MONTHS	13 MONTHS	15 MONTHS	17 MONTHS
Men									
	Linear	1.4744***	1.4324***	1.3991***	1.3899***	1.3536***	1.3441***	1.3280***	1.3243***
		(0.0239)	(0.0185)	(0.0274)	(0.0262)	(0.0316)	(0.0288)	(0.0296)	(0.0286)
	Quadratic	1.3460***	1.4218***	1.4590***	1.4370***	1.4611***	1.4311***	1.4283***	1.4111***
		(0.0012)	(0.0612)	(0.0362)	(0.0216)	(0.0209)	(0.0226)	(0.0228)	(0.0275)
Women									
	Linear	1.7652***	1.8171***	1.7718***	1.7735***	1.7581***	1.7430***	1.7559***	1.7642***
		(0.0737)	(0.0546)	(0.0476)	(0.0396)	(0.0379)	(0.0357)	(0.0338)	(0.0324)
	Quadratic	2.1535***	1.7261***	1.8363***	1.7935***	1.8055***	1.8016***	1.7631***	1.7484***
		(0.0008)	(0.0891)	(0.0609)	(0.0603)	(0.0540)	(0.0497)	(0.0493)	(0.0484)
Non-natives									
	Linear	1.6471***	1.6526***	1.5964***	1.6171***	1.6020***	1.6058***	1.6054***	1.6095***
		(0.0128)	(0.0177)	(0.0283)	(0.0209)	(0.0213)	(0.0210)	(0.0204)	(0.0203)
	Quadratic	1.7228***	1.6005***	1.6750***	1.6146***	1.6356***	1.6145***	1.6139***	1.6056***
		(0.0006)	(0.0436)	(0.0398)	(0.0442)	(0.0350)	(0.0320)	(0.0267)	(0.0264)

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 3.8: Log-transformed income estimates with different bandwidths and functional forms

BANDWIDTH		3 MONTHS	5 MONTHS	7 MONTHS	9 MONTHS	11 MONTHS	13 MONTHS	15 MONTHS	17 MONTHS
Men									
	Linear	0.0029	-0.0038	-0.0074†	0.0078	0.0063	0.0013	0.0017	0.0036
		(0.0022)	(0.0035)	(0.0042)	(0.0064)	(0.0072)	(0.0077)	(0.0073)	(0.0069)
	Quadratic	0.0193***	0.0096**	0.0010	-0.0198*	-0.0028	0.0066	0.0043	0.0002
		(0.0000)	(0.0020)	(0.0040)	(0.0090)	(0.0078)	(0.0063)	(0.0061)	(0.0072)
Women									
	Linear	-0.0011	-0.0068	-0.0145*	-0.0009	-0.0042	-0.0083	-0.0055	-0.0001
		(0.0036)	(0.0043)	(0.0055)	(0.0067)	(0.0084)	(0.0091)	(0.0087)	(0.0085)
	Quadratic	0.0215***	0.0075*	0.0031	-0.0219*	-0.0054	-0.0004	-0.0073	-0.0170
		(0.0000)	(0.0030)	(0.0036)	(0.0097)	(0.0084)	(0.0063)	(0.0071)	(0.0104)
Non-natives									
	Linear	0.0022	-0.0058	-0.0097*	0.0067	0.0029	-0.0011	-0.0026	-0.0015
		(0.0026)	(0.0043)	(0.0043)	(0.0055)	(0.0063)	(0.0055)	(0.0049)	(0.0047)
	Quadratic	0.0237***	0.0091**	0.0001	-0.0227*	-0.0017	0.0047	0.0051	0.0014
		(0.0000)	(0.0024)	(0.0047)	(0.0099)	(0.0090)	(0.0067)	(0.0065)	(0.0065)

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 3.9: Weekday versus weekend crime estimates with different bandwidths and functional forms for non-natives

BANDWIDTH		3 MONTHS	5 MONTHS	7 MONTHS	9 MONTHS	11 MONTHS	13 MONTHS	15 MONTHS	17 MONTHS
Linear									
	Weekday	-0.0412***	-0.0556***	-0.0393***	-0.0374***	-0.0312**	-0.0331***	-0.0392***	-0.0398***
		(0.0086)	(0.0083)	(0.0101)	(0.0101)	(0.0103)	(0.0090)	(0.0084)	(0.0089)
	Weekend	0.0182	-0.0267	-0.0367	-0.0377	-0.0375	-0.0112	-0.0016	-0.0007
		(0.0308)	(0.0281)	(0.0267)	(0.0229)	(0.0230)	(0.0230)	(0.0210)	(0.0201)
	Difference(χ^2)	2.49	0.79	0.01	0.00	0.05	0.68	2.37	2.77†
	p-value	0.1145	0.3735	0.9376	0.9927	0.8221	0.4112	0.1239	0.0962
Quadratic									
	Weekday	-0.0902***	-0.0359*	-0.0701***	-0.0606***	-0.0564***	-0.0465***	-0.0345**	-0.0331*
		(0.0010)	(0.0170)	(0.0149)	(0.0121)	(0.0149)	(0.0126)	(0.0126)	(0.0141)
	Weekend	0.0278***	0.0758†	0.0348	0.0026	-0.0156	-0.0578†	-0.0572†	-0.0467
		(0.0012)	(0.0400)	(0.0299)	(0.0275)	(0.0280)	(0.0320)	(0.0302)	(0.0285)
	Difference(χ^2)	40,918.21	4.87	8.59**	3.59†	1.33	0.08	0.35	0.14
	p-value	0.0000	0.0273	0.0034	0.0580	0.2484	0.7729	0.5566	0.7069

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

3.7 Conclusion

This study examines the effect of an active labor market policy on crime and provides insight into underlying mechanisms. The policy under consideration replaced the right to welfare benefits by the right to a work-learn offer for individuals below the age of 27. We exploit the exogenous variation caused by this policy age threshold through a regression discontinuity approach using unique administrative data on the entire Dutch population around the age cut-off.

The results show that the WLO policy reduced crime. While we do not find a discontinuity in criminal behavior among men and women in general, we do find a reduction of 12% in crime among non-natives. The estimates for non-natives are robust to changes in functional form and bandwidth size.

Evidence points towards a direct incapacitation effect as the causal mechanism, i.e. a reduction in time and opportunities to commit crime, due to the time spent in the mandatory activation program. Crime among non-natives is reduced by 12% on weekdays (i.e. days spent in the program), while we do not find a discontinuity in crime committed during weekends. This rules out human capital and socialization effects as underlying mechanisms, as such effects would not differ between weekdays and weekends. Furthermore, we find employment and income to be unaffected across all included samples.[6] As the ALMP was unsuccesful in its goal of labor market activation, we can also exclude incapacitation and income effects from increased employment as underlying mechanisms.

Similar to prior studies on stricter activation requirements, we find evidence for a crime reducing effect (Bratsberg et al. 2019, Corman et al. 2014, Fallesen et al. 2018, Schochet et al. 2008). The current study contributes to the literature by showing that findings hold using fine-grained monthly data and a method that is not vulnerable to region-specific developments. Our findings show an incapacitation effect to be the causal mechanism behind the crime reducing effect of the ALMP under consideration. Whereas Bratsberg et al. (2019) identified incapacitation effects induced by more

[6]These results are in line with Cammeraat et al. (2017), who find null effects for various income and labor market outcomes.

time spent in school between the ages of 18 and 21, we contribute to the literature by identifying a direct incapacitation effect of participation in a mandatory activation program among young adults around the age of 27. Moreover, our vast dataset allows for an examination of heterogeneous effects across a general population sample. Schochet et al. (2008) did not uncover differential effects across gender and race within a sample of disadvantaged youth (aged 16 to 24). Our findings indicate that heterogeneity exists when studying a more general population. The finding that only non-natives, i.e. the most disadvantaged group, are affected by the policy is in line with studies that find strong effects among disadvantaged youths (Bratsberg et al. 2019, Schochet et al. 2008). For comparatively non-disadvantaged groups (men and women in general), we do not find an effect. This may (partly) be explained by the lower WLO participation among natives. The results, however, suggest that there is more to the story. Another relevant factor may be that disadvantaged groups are more likely to live in segregated, crime-prone communities (Peterson and Krivo 2005). Following routine activities theory, most crimes are conditional on opportunities; situations in which both offender and victim are present and capable guardians are absent (Cohen and Felson 1979). WLO participation meant that individuals spent less time in their communities, reducing opportunities for criminal behavior and exposure to potentially criminogenic environments.

The application of a regression discontinuity approach on data of the entire registered Dutch population around the policy age threshold of 27, allows us to assess the causal effects of a mandatory activation program on crime among a general population of young adults. This approach enables us to take into account potential discouragement effects; welfare-related ALMPs may not only affect welfare recipients, but may also discourage individuals from applying for welfare benefits (see Persson 2013). An inherent limitation of this approach is that it produces estimates that only pertain to the observations around the threshold. As a key notion in developmental and life-course criminology is that determinants of criminal behavior vary by age and across developmental stages (e.g. Blokland and Nieuwbeerta 2010b, Elder 1998, Uggen 2000), further research is warranted to examine whether findings hold across age groups.

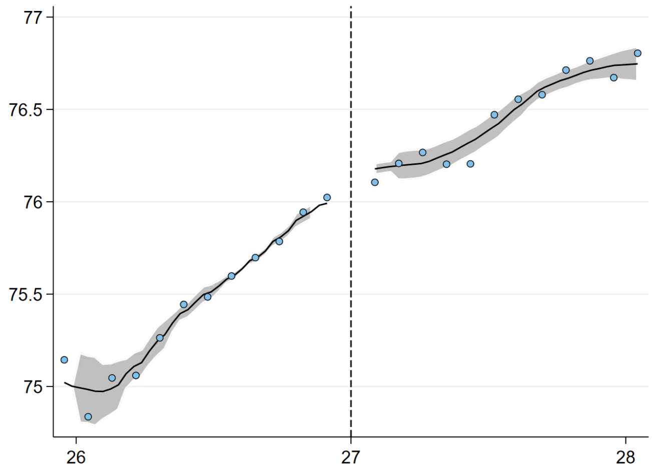
For future research, it would be interesting to investigate long-term effects. For instance, human capital effects are more likely to appear after program completion. Furthermore, the low crime rates among the sample under consideration restricted the analysis of separate crime categories. Differentiating between different types of crime may help further our understanding of how ALMPs affect crime.

In terms of crime reduction, the cautious conclusion seems to be that disadvantaged groups benefit most from activation provisions. The results of this specific study show that a mandatory activation program did not impact labor market outcomes in a period with relatively few employment opportunities. However, it did substantially reduce crime among non-natives through an incapacitation effect. These findings emphasize that spillover effects on criminal behavior warrant consideration in the evaluation and development of ALMPs for welfare recipients.

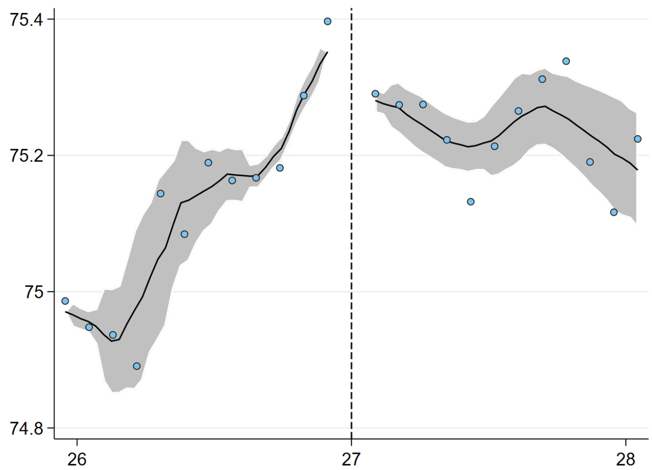
3.A Employment

Figure 3.6: Employment rates across age among men (a), women (b) and non-natives (c)

(a) Employment rate men



(b) Employment rate women



(c) Employment rate non-natives

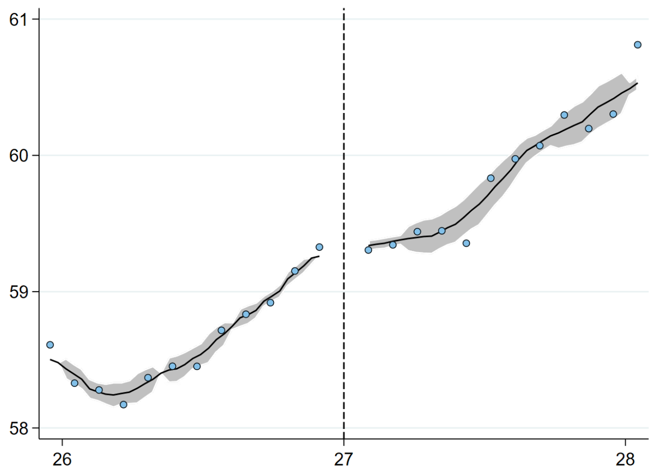


Table 3.10: Baseline probit estimates for employment status

	MEN	WOMEN	NON-NATIVES
Employment			
RD	0.0013	0.0005	0.0080***
	(0.0011)	(0.0017)	(0.0019)
Age	0.0021***	-0.0000	0.0023***
	(0.0003)	(0.0002)	(0.0005)
$\text{Age} * 1(< 27)$	0.0012***	0.0018***	0.0014*
	(0.0003)	(0.0004)	(0.0007)
Male			0.1261***
			(0.0027)
Native	0.6154***	0.8001***	
	(0.0017)	(0.0016)	
Time (month)	0.0004	-0.0004*	0.0004
	(0.0003)	(0.0002)	(0.0004)
Average treatment effects			
Monthly probabilities			
If treatment = 1 (%)	76.02	75.24	59.35
If treatment = 0 (%)	75.99	75.23	59.04
ATE_{abs} (%point)	0.04	0.01	0.31***
	(0.03)	(0.05)	(0.07)
ATE_{rel} (%)	0.05	0.02	0.53
Observations	1,658,432	1,623,579	983,372
Respondents	155,449	151,517	95,381

Notes. Linear model specification, 7-month bandwidth (14 months total), standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 3.11: Employment status estimates with different bandwidths and functional forms

BANDWIDTH		3 MONTHS	5 MONTHS	7 MONTHS	9 MONTHS	11 MONTHS	13 MONTHS	15 MONTHS	17 MONTHS
Men									
	Linear	0.0032**	-0.0003	0.0013	0.0024*	0.0026*	0.0006	0.0007	0.0011
		(0.0009)	(0.0016)	(0.0011)	(0.0011)	(0.0011)	(0.0016)	(0.0014)	(0.0017)
	Quadratic	0.0013***	0.0047**	-0.0000	-0.0009	0.0005	0.0040*	0.0027†	0.0018
		(0.0000)	(0.0014)	(0.0025)	(0.0018)	(0.0015)	(0.0018)	(0.0016)	(0.0022)
Women									
	Linear	0.0056***	0.0013	0.0005	0.0013	0.0007	0.0014	0.0041*	0.0039†
		(0.0004)	(0.0016)	(0.0017)	(0.0017)	(0.0014)	(0.0016)	(0.0019)	(0.0020)
	Quadratic	0.0041***	0.0094***	0.0036*	0.0004	0.0016	0.0003	-0.0034	-0.0015
		(0.0000)	(0.0008)	(0.0016)	(0.0023)	(0.0020)	(0.0020)	(0.0030)	(0.0037)
Non-natives									
	Linear	0.0080***	0.0037**	0.0080***	0.0088***	0.0062*	0.0039	0.0033	0.0010
		(0.0006)	(0.0013)	(0.0019)	(0.0021)	(0.0024)	(0.0038)	(0.0034)	(0.0035)
	Quadratic	0.0029***	0.0112***	0.0029	0.0045*	0.0098***	0.0119***	0.0104***	0.0133**
		(0.0000)	(0.0013)	(0.0027)	(0.0021)	(0.0022)	(0.0023)	(0.0025)	(0.0044)

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.