Essays on welfare benefits, employment, and crime
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Preface

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Contents

List of Tables

List of Figures

1 Introduction

This PhD thesis contains four studies on *welfare benefits, employment and crime*. These four studies aim to contribute to the understanding of spillover effects on crime of welfare benefits receipt, active labor market policy, and employment. The chapters in this thesis can be read independently. This introduction provides the motivation for this thesis' topics (section 1.1), followed by the research questions underlying each of the chapters (section 1.2), and a summary of the main findings of each chapter (section 1.3).

Motivation 1.1

In the early nineteenth century, Belgian statistician Quetelet concluded that crime develops when the poor "are surrounded by subjects of temptation and find themselves irritated by the continual view of luxury and of an inequality of fortune" (Beirne 1987, p. 38). Quetelet drew this conclusion from the earliest recorded statistical account of the relationship between crime and poverty. Since then, the reduction of poverty and income inequality has become one of the core tenets upon which the redistributive policies of welfare states are founded. In recent decades, however, rising budget deficits during economic crises are increasingly met by governments of advanced welfare states with welfare state retrenchment (see Jensen et al. 2018). These cutbacks often focus on welfare benefits schemes, weakening income protection for the most vulnerable. While the effects of such reforms on directly-targeted economic outcomes are generally evaluated, this is rarely true for spillover effects on crime.

In order to gain a comprehensive overview of the societal costs and benefits of welfare policy, crime must be taken into account. The welfare and criminal justice systems can be considered as two opposite approaches to governing the poor, and are often referred to as the left and right hand of the state, or the soft and hard side of government (Wacquant 2009). From such a perspective, the trend of welfare state retrenchment equates to a shift towards a more punitive approach to crime, focusing on repression, as opposed to prevention. A substantial body of macro-level evidence suggests that welfare spending reduces crime (e.g. Chamlin et al. 2002, Grant and Martinez Jr 1997, Meloni 2014, Worrall 2009). As such, reducing welfare accessibility may reduce welfare spending, but also increase crime and its substantial societal costs.

The 2007 Great Recession's massive rise in unemployment accelerated welfare state retrenchment in many European countries (Jensen et al. 2018). As youth unemployment rates within the European Union were slow to recover (Carcillo and Königs 2015), various countries implemented youth-targeted active labor market policies (ALMPs) to reduce unemployment among young adults (OECD 2013). The aim of these reforms was labor market activation of young adults, following the success of the Job Corps (United States) and The New Deal for Young People (United Kingdom) programs (Dorsett 2006, Schochet et al. 2008). The Netherlands also saw the implementation of two consecutive welfare-related ALMPs, aimed at labor market activation of young adults below the age of 27. However, evidence thusfar suggests that both the so-called 'work-learn offer' and 'job search period' policy are more effective in reducing welfare uptake, than reducing unemployment (Bolhaar et al. 2019, Cammeraat et al. 2017). Despite the ineffectiveness in terms of labor market activation, the latter ALMP is still in effect to date. As a result, a smaller proportion of unemployed young adults have a minimum income guarantee.

Theoretically, a loss of guaranteed minimum income benefits may induce criminal behavior via several mechanisms. From a rational choice perspective, a reduction in income should increase financially motivated crime by increasing the relative financial gains of such offenses (Becker 1968, Ehrlich 1973). Insufficient income may also increase psychological stress, which in turn could increase criminal behavior as a coping mechanism (such as violent crime, see Agnew 1992). As such, income protection by welfare provisions hypothetically fulfills a vital role in crime prevention. However, employment and labor market training theoretically affect crime through additional mechanisms, such as incapacitation – (Cohen and Felson 1979), human capital – (Becker 2009), and socialization effects (Laub and Sampson 1993, Sampson and Laub 1990). Hence, the expected spillover effects of welfare reforms are dependent on its effectiveness in reducing welfare uptake, but also labor market activation.

This multidisciplinary thesis combines insights from economics and criminology, to draw causal links between welfare benefits receipt, active labor market policy, employment, and crime. While economists generally assess the effects of welfare-related policies on directly-targeted labor market outcomes, potential spillover effects on crime are often ignored. Criminologists on the other hand rarely exploit exogeneity originating from economic policy variation. The studies in this dissertation examine theories on the economics of crime, by exploiting exogenous policy variation through the use of econometric techniques. This approach is facilitated by the availability of uniquely comprehensive individual-level administrative data gathered by Statistics Netherlands. Covering the entire registered population of the Netherlands, these fine-grained data allow this thesis to assess causal effects on low-probability daily-level crime outcomes. The Netherlands also offers a valuable institutional context to examine these relationships, due to its comparatively generous social protection and lenient criminal justice system (see Aebi and Tiago 2020, Kaeble 2018, Motivans 2020, OECD 2018a). As most of the existing literature is focused on the US, this thesis sheds light on the generalizability of prior findings to a context that is more representative of Nordic and Western European countries.

Estimating causal relationships between welfare, the labor market and criminal behavior is empirically challenging due to unobserved variables simultaneously influencing these outcomes. By addressing these endogeneity problems, this thesis addresses the paucity in causal evidence on the following questions: Does welfare receipt reduce crime by providing a minimum income guarantee (RQ1)? If so, to what extent do stricter activation requirements for welfare eligibility affect criminal behavior

(RQ2)? Does welfare benefits disbursement affect criminal behavior over the payment cycle (RQ3)? And to what extent does continuity in criminal behavior materialize through adverse labor market consequences (RQ4)? In answering these questions, this thesis aims to further the understanding of the causal relationship between welfare dependency, labor market activation, employment, and crime.

1.2 Research questions

This section presents the main research questions addressed in this thesis.

Chapter 2 addresses the paucity in micro-level evidence on the welfare–crime relationship, by answering the research question *To what extent does welfare receipt affect criminal behavior among young adults?* This chapter argues that while there is a theoretical consensus that the minimum income guarantee of welfare benefits provision reduces criminal behavior (see Agnew 1992, Becker 1968, Ehrlich 1973), this hypothesis has previously not been rigorously tested using microdata on a general population.[1] Prior assessments have shown welfare spending to reduce crime at the national, state, or city level,[2] which raises the question as to what extent welfare receipt affects criminal behavior at the individual level. Research thusfar has also mostly focused on the US context, where the benefits level is comparatively low (OECD 2018a), and only households with dependent children are eligible for cash transfers.[3] Hence, this chapter aims to shed light on the causal effects of welfare receipt on crime among a general population sample, in a context with benefits levels more representative of Nordic and Western European countries.

Chapter 2 details the first investigation of the causal effects of welfare receipt on crime using microdata on a general population sample. Complementary to related work, we exploit welfare policy variation *within*, as opposed to *across*, geographical regions. Through this approach, we avoid

[1] A notable body of (quasi-)experimental evidence does show that transitional financial aid reduces recidivism among (high-risk) newly-released prisoners (e.g. Berk et al. 1980, Mallar and Thornton 1978, Rauma and Berk 1987, Yang 2017a).

[2] See Chamlin et al. (2002), Grant and Martinez Jr (1997), Meloni (2014), Worrall (2009).

[3] https://www.usa.gov/benefits.

bias from potentially endogenous welfare reform timings and unrelated region-specific developments (see Corman et al. 2014). Upon application for welfare benefits, applicants younger than 27 are subject to a four-week 'job search period' during which they are not eligible for welfare benefits. Evidence suggests that a majority of applicants refrain from applying for welfare after the job search period (Ministerie van Sociale Zaken en Werkgelegenheid 2015, Van Dodeweerd 2014), and those who do apply are left without discernible legitimate income for up to eight weeks. We exploit this age-threshold in Dutch welfare policy through an instrumental variable approach, to assess the causal effect of welfare receipt on crime as compared to nonreceipt due to the job search period policy. The availability of microdata on a large general population enables further investigation of heterogeneous effects across demographic characteristics.

Whereas Chapter 2 investigates the effects of the provision of a guaranteed minimum income on crime, *Chapter 3* expands upon this by analyzing the effects of stricter activation requirements for welfare eligibility. To address the research questions *To what extent did a recent Dutch mandatory activation program affect crime among young adults? and through which causal mechanism?*, this chapter details the policy response in the Netherlands to the rising youth unemployment rates caused by the 2007 Great Recession. Policy makers from multiple OECD countries found youth unemployment especially concerning, due to its cyclical volatility and potential to bear more negative consequences than unemployment among adults (Scarpetta et al. 2010). These not only include potentially more long-term adverse effects on labor market outcomes from labor market scarring, but also potentially larger effects on crime (e.g. Gould et al. 2002). To reduce unemployment among youths, the Dutch government implemented an active labor market policy (ALMP) aimed at labor activation of young adults below the age of 27. Introduced in October 2009, the 'work-learn offer' (WLO) policy replaced the right to welfare benefits by a right to a work-learn offer, i.e. a mandatory activation program. While multiple OECD countries have implemented comparable youth-targeted ALMPs,[4] program evaluation has thusfar mainly focused on directly-targeted labor

[4]E.g. Job Corps in the US, and The New Deal for Young People in the UK.

market outcomes. Chapter 3 complements earlier studies by empirically analyzing the spillover effects of this mandatory activation program on crime, by exploiting the policy age threshold through a regression discontinuity design.

Chapter 3 investigates the causal mechanism through which the ALMP under consideration affects crime, by differentiating between crime committed on weekdays and crime committed during weekends. Prior studies suggest that ALMPs can affect criminal behavior through incapacitation effects (Bratsberg et al. 2019), socialization effects (Fallesen et al. 2018), human capital effects (Bratsberg et al. 2019, Schochet et al. 2008), and income effects (Persson 2013, Schochet et al. 2008). As human capital, socialization, and income effects would not differ between weekdays and weekends, we analyze incapacitation effects by comparing discontinuities in weekend – and weekday crime (when time is spent in the activation program). Furthermore, the policy under consideration does not achieve labor market activation (see also Cammeraat et al. 2017), which is shown to be a mechanism through which ALMPs can reduce crime (e.g. Corman et al. 2014, Fallesen et al. 2018). This enables this study to rule out incapacitation effects from increased employment on criminal behavior. As such, this chapter is the first to analyze a direct incapacitation effect of participation in a mandatory activation program on crime.

Chapter 4 builds upon the investigation of welfare receipt as a minimum income guarantee in Chapter 2, by analyzing the relationship between welfare benefits disbursement and temporal patterns in crime over the welfare payment cycle. This chapter addresses the research question *To what extent does the time that has passed since welfare payment receipt affect crime among welfare recipients?* To this end, the chapter details that insufficient consumption smoothing among welfare recipients may affect crime through two distinct economic causal mechanisms.[5] One mechanism pertains to the possibility that welfare recipients increasingly face

[5]A vast body of evidence that suggests hyperbolic discounting among welfare recipients (Castellari et al. 2017, Damon et al. 2013, Hamrick and Andrews 2016, Hastings and Washington 2010, Shapiro 2005, Stephens Jr 2003, Wilde and Ranney 2000). These studies show consumption among welfare recipients to increase sharply after payment receipt, and decrease substantially towards the end of the month.

serious financial constraints towards the end of the welfare payment cycle. Such constraints could both push recipients towards committing crime to supplement their income from welfare benefits (Becker 1968, Ehrlich 1973), as well as increase criminal behavior in general, as a coping mechanism to psychological strain (Agnew 1992). Conversely, another mechanism pertains to the income shocks generated by the once monthly lump sum disbursement of welfare benefits. If the resulting spikes in consumption also concerns consumption that is complementary to criminal behavior (such as alcohol and illicit drugs), this may increase violent crime in particular (see Dobkin and Puller 2007, Hsu 2017). To assess the validity of both theoretical causal mechanisms, this chapter differentiates between financially motivated crime and non-financially motivated crime.

Despite the mounting body of evidence on welfare recipients facing serious financial constraints towards the end of the welfare month, little attention has gone to spillover effects on crime. Chapter 4 details the first study to address this paucity using daily-level microdata on both welfare receipt and criminal behavior among welfare recipients. These data allow us to exploit exogenous variation in welfare payment dates across Dutch municipalities. Combined with the ability to include individual fixed effects, we avoid bias from endogeneity induced by variation across individuals, municipalities, and time (e.g. from other transactions, such as wages, rents, and other benefits). These microdata also enable us to shed light on heterogenous effects across age and sex.[6] As the most closely-related prior study finds a sizable increase in US city-level financially motivated crime rates over the welfare month (Foley 2011), this chapter aims to shed light on the generalizability of these findings to welfare recipients at the individual level, and contexts with higher benefits levels.[7]

Chapter 5 complements the previous chapters on welfare benefits, by investigating the role of adverse labor market consequences as a causal pathway for continuity in criminal behavior. The research question that is dealt

[6]Both age and sex are important determinants of both criminal behavior and welfare dependency (e.g. see Corman et al. 2014, Holtfreter et al. 2004, Loeber and Farrington 2014, Steffensmeier and Allan 1996).

[7]Guaranteed minimum income benefits are much lower in the US as compared to the Netherlands (6% vs 60% of median disposable income, see OECD 2018a).

with in Chapter 5 is *To what extent does prior crime affect current criminal behavior through employment effects?* Adverse labor market consequences have long been hypothesized to form a potential pathway for crime state dependence to arise. Prior studies have shown the labor market to exert a form of secondary punishment to past criminal behavior, where employment opportunities are reduced via multiple 'scarring' mechanisms. Human capital may be adversely affected by unemployment spells resulting from investing in a criminal career and penal interventions (such as imprisonment, see Holzer et al. 2004), and the aquirement of a criminal record (Apel and Sweeten 2010, Bernburg and Krohn 2003, Dobbie et al. 2018, Pager et al. 2009, Paternoster and Iovanni 1989). This may, in turn, stimulate further criminal behavior, as a notable body of micro-level evidence indicates that stable employment substantially reduces crime.[8] However, extant evidence on the effects on prior crime on employment focuses on the US, which has a harsher penal climate than most Nordic and Western European countries (with on average longer prison terms and more accessible criminal records, see Aebi and Tiago 2020, Corda and Lageson 2020, Kaeble 2018, Motivans 2020). This chapter aims to shed light on the generalizability of these findings to the EU context.

While a substantial body of literature indicates that prior crime is a strong predictor of further criminal behavior, discussion remains as to what extent this is a spurious or causal relationship. Evidence from administrative data on high-risk (ex-offender) samples leans towards population heterogeneity as the underlying cause of continuity in criminal behavior (e.g. Nagin and Paternoster 1991, Paternoster and Brame 1997), whereas studies using survey data on general populations present evidence of causal effects (i.e., true crime state dependence, see Nagin and Farrington 1992a,b, Paternoster et al. 1997). Chapter 5 is the first to use administrative data on a large general population sample of young adults, which further allows for the analysis of potential heterogeneity across sex. As such, this chapter contributes to the ongoing discussion of whether male and female crime is influenced by the same factors and through similar mechanisms (see Kruttschnitt 2013, Steffensmeier and Allan 1996).

[8] See Apel et al. (2008), Apel and Horney (2017), Van der Geest et al. (2011), Ramakers et al. (2020), Uggen (2000).

Empirical evidence on the role of employment in crime state dependence is even more scarce, likely due to the empirical challenges posed by the reciprocal relationship between employment and crime. To address these challenges, Chapter 5 employs a joint dynamic model of crime and employment that explicitly accommodates feedback effects from past crime on current employment. Through this approach, we build upon related studies using dynamic discrete response models (e.g. Imai and Krishna 2004, Mesters et al. 2016), by avoiding the highly-restrictive exogeneity assumption, which does not allow the outcome of dependent variables to influence future outcomes of the regressors. More specifically, we apply a correlated random effects bivariate probit model with individual-specific effects in the form of individual-level correlated random effects and initial employment – and crime conditions, to control for time-invariant observed and unobserved heterogeneity. To further investigate the underlying theoretical mechanism, we differentiate between financially motivated offenses and other (non-financially motivated) offenses. To the best of our knowledge, this chapter is the first to apply this novel approach to the employment–crime state dependence relationship.

Main findings 1.3

This section provides the answers to the questions raised in section 1.2.

Chapter 2 hypothesizes that welfare receipt reduces criminal behavior through the provision of a minimum income guarantee. As prior research on this relationship is scarce, this expectation is mainly founded on the theoretical consensus between the often-cited rational choice theory (Becker 1968, Ehrlich 1973) and general strain theory (Agnew 1992). Using microdata on the entire young adult population of the Netherlands around a welfare policy age-threshold of 27, we find support for both theories. For men, we find welfare receipt to reduce financially motivated crime to a greater extent than other offenses. This is to be expected from a rational choice perspective, from which welfare receipt should reduce the relative benefits from crime aimed at financial gains. Our findings for women are more in line with general strain theory, as the reduction is equally-sized for crime in general. From this perspective, welfare receipt should reduce criminal behavior in general by reducing financial strain-induced psychological stress. Chapter 2 provides more detail on the underlying theoretical mechanisms.

Reconciling the empirical evidence in Chapter 2, we find welfare receipt to substantially reduce crime across all included samples. While our results show that the pathway through which welfare receipt reduces crime is different for men and women, we do not find evidence of heterogeneity across educational levels. Hence, a lower ability to cope with financial strain does not appear to explain the higher crime rates among low-educated samples. Prior studies into causal effects of welfare receipt on crime among a general population are scarce, but similar reductions in crime have been found among newly-released ex-offenders (see Yang 2017a). A back-of-the-envelope calculation shows that welfare provision is not cost-effective as a crime prevention strategy. Nevertheless, this chapter shows that spillover effects on crime should should be taken into account to gain a comprehensive overview of the societal costs and benefits of welfare provision.

Chapter 3 analyzes spillover effects of a welfare-related mandatory activation program on crime. The active labor market policy (ALMP) under consideration introduced stricter activation requirements for welfare egilibility among welfare applicants under the age of 27, in the form of mandatory participation in a job-training program (i.e. a 'work-learn offer'). By exploiting this age-threshold, the analysis finds evidence of incapacitation effects on criminal behavior. More specifically, crime committed during weekdays was reduced by 12% among non-native Dutch citizens. As we do not find a discontinuity in crime committed during weekends, human capital and socialization effects are ruled out as the underlying mechanisms. Chapter 3 discusses the investigation of conceivable causal effects and mechanisms in more detail, including the assessment that the ALMP under consideration did not affect income and employment among the included samples. While unsuccessful in its goal of labor activation (also see Cammeraat et al. 2017), the ALMP did reduce crime during a period of relatively low employment opportunities.

The results detailed in Chapter 3 suggest that disadvantaged groups benefit the most from the activation program under consideration. We find criminal behavior to only be affected among non-natives, who have the highest welfare dependency rate of the included samples. The sizeable reduction in crime among this sample is in line with prior studies on the effects of comparable ALMPs on disadvantaged youths (Bratsberg et al. 2019, Schochet et al. 2008). Conversely, we do not find a discontinuity in crime among men and women in general. This may be attributable to the substantively lower program participation rate among natives. An additional explanation for this heterogeneity, however, may lie in the higher likelihood that non-native participants live in more segregated, crime-prone communities (Peterson and Krivo 2005). This may amplify the incapacitation effect of the program on criminal behavior, as participants spend less time in this criminogenic environment. The identification of sizeable spillover effects on crime among this relatively vulnerable group warrants consideration in the development and evaluation of targeted welfare-related active labor market policy.

Causal effects of the time that has passed since welfare payment receipt on criminal behavior among welfare recipients are analyzed in *Chapter 4*. Prior studies suggest that welfare recipients insufficiently smooth consumption over the payment cycle, by showing spikes in consumption upon benefits receipt and serious financial constraints towards the end of the month.[9] Based on this evidence, this chapter theorizes welfare benefits disbursement to affect crime through two distinct hypothetical mechanisms: 1) reduced financial means increase financially motivated crime over the welfare month, and 2) increased consumption complementary to criminal behavior increases other offenses at the start of the welfare month. Using daily-level microdata to exploit exogenous variation in payment

[9]See Castellari et al. (2017), Damon et al. (2013), Hamrick and Andrews (2016), Hastings and Washington (2010), Shapiro (2005), Stephens Jr (2003), Wilde and Ranney (2000).

dates across 16 Dutch municipalities, we find evidence supporting both hypotheses.

Concerning the first mechanism, we find welfare recipients to commit 17% more financially motivated crime at the end of the monthly welfare payment cycle, as compared to directly after benefits disbursement. Following rational choice theory (Becker 1968, Ehrlich 1973), this is to be expected if the relative financial gains of such offenses increase towards the end of the month. Hence, this finding suggests that a reduction in financial means over the payment cycle prompts welfare recipients to commit crime to supplement their income. However, these changes in available means appear to simultaneously underlie an inversive trend in non-financially motivated crime. Confirming our second hypothesis, these offenses peak directly after benefits receipt, and decrease by 6% over the payment cycle. Based on prior studies, this is likely attributeable to a spike in consumption conducive to criminal behavior (e.g. alcohol and illicit drugs, see Dobkin and Puller 2007, Hsu 2017, Watson et al. 2019). These inversive effects somewhat smooth the trend in overall crime, which increases by 5% over the welfare month. As we do not find heterogeneous effects, differences in the ability to smooth consumption do not appear to underlie the differences in criminal activity across age and sex.

As Chapter 4 details a first investigation of the causal relationship between welfare benefits disbursement and crime at the individual level (as opposed to aggregate crime rates), direct comparison to prior studies is not without flaws. Nevertheless, the most closely-related study by Foley (2011) finds a similar increase of 14% in city-level rates of financially motivated crime in the US. Contrary to our results, however, he does not find a change in other offenses. A potential explanation for this difference may lie in the comparatively high benefits levels in the Netherlands (see OECD 2018a), as the larger spikes in the available financial means of welfare recipients upon disbursement may generate a larger 'full wallet' effect on crime. Prior research shows that reducing the size of these spikes by increasing the disbursement frequency causes spikes in domestic violence upon disbursement to disappear Hsu (2017). While further research is required, staggering benefits disbursement may ostensible reduce crime by effectively shortening time over which recipients have to smooth their consumption. As this reduces recipients' financial autonomy, however, the costs and benefits should be comprehensively considered in the formation of such welfare policy.

Chapter 5 rejects the often theorized hypothesis that adverse labor market consequences are a causal pathway for crime state dependence, by analyzing feedback effects from past crime on current employment in a joint dynamic model of crime and employment. To this end, Chapter 5 analyzes three testable hypotheses: 1) whether past criminal behavior reduces current employment probabilities, 2) whether employment contemporaneously reduces criminal behavior, 3) whether past criminal behavior increases current criminal behavior via pathways other than employment effects, when controlling for population heterogeneity.

Regarding the first hypothesis, individuals who have committed crime in the past have a lower probability of currently being employed. After controlling for population heterogeneity, however, we do not find substantive causal effects of prior crime on current employment. This suggests that the prior crime–employment correlation is likely attributable to differences in personal characteristics related to both the probability to commit crime and the probability to be employed. We do find support for the second hypothesis, as employment substantially reduces financially motivated crime, especially. To a lesser extent, other criminal behavior among men is reduced by employment as well, which suggests that employment has a behavioral impact beyond an income effect (such as an incapacitation effect). The third hypothesis suggests that criminal behavior adversely affects an individuals decision making process to repeat such behavior in the future. Our findings support this hypothesis, as we find prior criminal behavior to substantially increase current criminal behavior among both men and women.

Taken together, the findings in Chapter 5 suggest that the substantial adverse effects of criminal behavior on future criminal decision making do not appear to materialize through labor market consequences. While the substantial reductions in crime by employment are in line with prior literature (e.g. Mesters et al. 2016), we do not find evidence of substantive adverse effects of past crime on current employment probabilities. This is contrary to the expectations derived from other studies, which have found criminal behavior to substantially reduce labor market opportunities in the US.[10] This may be attributable to the comparative inaccessibility of criminal records in the Netherlands and leniency of the Dutch criminal justice system (see Aebi and Tiago 2020, Corda and Lageson 2020, Kaeble 2018, Motivans 2020). Only criminal justice actors can directly access criminal records in the Netherlands, custodial sanctions are less often imposed, and long prison terms are rare compared to the US, which may limit the adverse consequences of criminal behavior on human capital and future labor market prospects (see Dobbie et al. 2018, Pager et al. 2009, Selbin et al. 2018, Uggen et al. 2014). As such, this chapter sheds light on the generalizability of previous findings from the US to a context that is more representative of most EU countries.

Reconciling the main findings in this thesis, Chapter 2 details that welfare receipt substantially reduces crime by providing a guaranteed minimum income. However, the disbursement of welfare benefits does cause non-financially motivated offenses such as violent crime to spike among welfare recipients upon payment receipt (as analyzed in Chapter 4). Conversely, financially motivated crime increases among welfare recipients as the time since payment receipt increases over the monthly payment cycle. This suggests that recipients of guaranteed minimum income benefits face serious financial constraints towards the end of the month. Together with Chapter 2, Chapter 3 suggests that the influence of active labor market policies (ALMPs) on criminal behavior is dependent upon the presence of discouragement – versus incapacitation effects. If regular employment is not substantively affected, stricter activation requirements that reduce welfare uptake increase crime (as suggested in Chapter 2), whereas participation in a mandatory activation program reduces crime among vulnerable individuals by reducing their leisure time (as detailed in Chapter 3). Finally, Chapter 5 suggests that having a recent criminal history does not necessarily force individuals into unemployment, as we find prior criminal behavior in general to not substantively affect

[10] E.g. Apel and Sweeten (2010), Bernburg and Krohn (2003), De Li (1999), Dobbie et al. (2018), Lopes et al. (2012), Pager et al. (2009), Selbin et al. (2018), Uggen et al. (2014).

employment. Hence, adverse labor market consequences do not appear to explain the substantial continuity of criminal behavior.

2 The effects of welfare receipt on crime: A regression discontinuity and instrumental variable approach

Abstract

Popular theories state that welfare receipt reduces criminal behavior. However, estimating the causal effect of welfare receipt on crime is empirically challenging due to unobserved characteristics influencing both welfare receipt and crime. This study exploits exogenous variation in Dutch welfare policy among individuals around the age of 27, which leaves applicants below this cut-off temporarily without discernible legitimate income. Using individual-level administrative data on the entire Dutch population around the policy age threshold, we estimate an instrumental variable model with a first-stage regression discontinuity design. Results show that welfare receipt reduces monthly crime rates from 0.53% to 0.16% for men and from 0.14% to 0.03% for women. For men, we find a larger relative reduction in financially-motivated crime compared to crime in general. Our findings imply that potential effects on crime should be considered in welfare policy formation.

The chapter is co-authored by Marike Knoef and Anke Ramakers and was presented at the 29th annual conference of the European Association of Labour Economists (EALE), the 31st Annual Conference of the European Society for Population Economics (ESPE), the 2017 Workshop on Criminological Research with Register Data, the 2017 Leiden University Interaction between Legal Systems seminar and the 2017 Netherlands Economists Day (NED). We would like to thank the participants, as well as Koen Caminada and Paul Nieuwbeerta, for their helpful comments and suggestions. We thank the Gak institute for financial support, and Statistics Netherlands for providing us access to their data.

2.1 Introduction

The Great Recession caused a massive rise in unemployment rates in most Western countries (e.g. Carcillo and Königs 2015). Many countries responded by reducing welfare accessibility and increasing obligations, which often reduced welfare uptake (Bolhaar et al. 2019, Dahlberg et al. 2009, Hernæs et al. 2017). Employment rates, however, did not always increase. As a result, welfare receipt declined among unemployed individuals. This reduction in guaranteed minimum income may lead to more criminal behavior, yet such spillover effects are often ignored in research and policy. In light of this paucity, this study assesses the causal effect of welfare receipt on crime.

From a theoretical perspective, there is a large degree of consensus that welfare receipt reduces criminal behavior. Among the most-cited theories are Becker's rational choice theory (1968) and Agnew's general strain theory (1992). Rational choice theory states that an individual determines his behavior by weighing perceived costs and benefits (Becker 1968, Ehrlich 1973). Providing individuals with means of subsistence via welfare benefits would reduce the relative financial gains from financially-motivated crime (e.g. property crime). Furthermore, from a general strain perspective, insufficient income can be classified as a negative stimulus that may contribute to (anticipated) failure to achieve personal goals (Agnew 1992). This results in emotional strain, which in turn can increase criminal behavior as a coping mechanism (e.g. violent crime). Through the provision of a basic level of guaranteed income, welfare receipt may reduce strain and consequently crime in general.

While these theories agree that welfare receipt likely reduces crime, few studies have examined these causal claims using individual-level data and (quasi-)experimental research designs. Most of the existing studies offer insight into the macro-level dynamics between welfare benefits and crime. Especially in the US, a sizeable body of cross-sectional research finds evidence of an inverse relationship between welfare spending and crime on a city, county, or state level.[1] Other studies use longitudinal data

[1] E.g. Chamlin and Cochran (1997), DeFronzo (1983, 1992, 1996a,b, 1997), DeFronzo and Hannon (1998), Hannon and DeFronzo (1998a,b), Pratt and Cullen (2005), Zhang (1997).

and find causal inverse effects of welfare spending on crime at the state or national level (Chamlin et al. 2002, Grant and Martinez Jr 1997, Meloni 2014, Worrall 2009). Moreover, an innovative study in twelve large US cities by Foley (2011), shows an increase in crime over the amount of time that has passed since welfare payments were received, and ascribes this to increasing financial constraints.

Unobserved differences over time or between countries, states, cities and neighborhoods may, however, bias analyses across time and regions. In difference-in-differences analyses, a concern is that the development of crime across regions may vary due to region-specific changes in the costs and benefits of engaging in criminal activity unrelated to the reform in question (Corman et al. 2014). Furthermore, the timing of welfare reforms may be endogenous. To avoid such potential biases, this study exploits an age-based discontinuity in welfare policy to estimate average treatment effects. Dutch welfare applicants below the age of 27 are subject to a so-called 'job search period' (JSP). This means that they are required to actively search for employment or education for a period of four weeks before their application will be processed. During this period, they are not eligible for welfare benefits and therefore without discernible legitimate income. Additionally, evidence suggests that the most vulnerable youths are unable to meet the JSP policy requirements, and consequently drop off the radar of municipalities (Ministerie van Sociale Zaken en Werkgelegenheid 2015, Van Dodeweerd 2014). They are discouraged from applying for welfare benefits by the strict conditionality of the policy, and remain without discernable legitimate income even beyond the four-week job search period. This study exploits the age-based exogenous variation in welfare eligibility to instrument welfare receipt and assess the causal effects of welfare receipt on crime. We estimate an instrumental variable (IV) model with a first-stage regression discontinuity (RD) design on unique individual-level administrative data for the entire Dutch population around the age of 27. Through this approach, we assess the effects of welfare receipt compared to a lack thereof due to subjection to the job search period policy. The analyses are run for both financially-motivated crime and crime in general.

We find that welfare receipt substantially reduces general and financially-motivated crime. Moreover, for men we find that financially-motivated crime is more heavily affected than crime in general, which supports rational choice theory (Becker 1968, Ehrlich 1973). For women we find the effects on financially-motivated crime and crime in general to be more comparable, which is in line with general strain theory (Agnew 1992). Despite the higher baseline crime rates among low-educated men and women, we do not find substantial heterogeneous effects across educational levels. All of the results are robust to changes in functional form and bandwidth size.

The contribution of this study to the literature is threefold. First, complementary to most of the existing literature, we estimate the causal effect of welfare receipt on crime using variation *within* geographical regions (as opposed to variation *between* geographical regions). Consequently, we avoid potential biases caused by endogenous timing of the welfare reform in question, and of region-specific developments unrelated to the welfare reform. Second, whereas most studies are focused on welfare benefits in the US, the Netherlands offers a different context with a relatively generous welfare system. As higher benefits levels offer more income protection, the estimates in this study are likely to provide an upper bound for the potential effects in other countries. Our third contribution concerns the sample; we assess the effects of cash transfers on crime among young adults. This includes young adults without dependent children, who are not entitled to cash transfers in the US.[2] In addition, we analyze young women, who have received little attention in previous crime literature. By distinguishing between female crime and male crime, we can assess potential heterogeneous effects across gender. In this way, we aim to contribute to the ongoing discussion among scholars of whether female and male crime can be accounted for by the same factors and through similar mechanisms (see Kruttschnitt 2013, Steffensmeier and Allan 1996).

Some related studies focus on samples with specific characteristics. There is a substantial body of (quasi-)experimental evidence on the effect of transitional financial aid on recidivism among (high-risk) newly-released prisoners (e.g. Berk et al. 1980, Mallar and Thornton 1978, Rauma and

[2] https://www.usa.gov/benefits.

Berk 1987, Yang 2017a). Yang (2017a) assesses the causal effect of public assistance eligibility on recidivism among newly-released drug offenders, by exploiting the staggered reintroduction of public assistance eligibility for convicted drug offenders across 43 US states (rolling back a 1996 federal policy change). Using individual-level data on returns to prison within one year after release, she finds public assistance eligibility to reduce recidivism among this group by 13 percent. Furthermore, a related study by Tuttle (2019) exploits the absence of such a roll back in the US state of Florida for drug traffickers, and finds the lifetime ban from public assistance benefits to increase returns to prison within one year by 60 percent.[3] Another closely related study is that of Bolhaar et al. (2019), who use an experimental research design to assess the effects of a job search period in the Netherlands. For a relatively highly-employable group of welfare recipients between the ages of 27 and 64,[4] they find a reduction in welfare dependency, increased earnings, but no adverse effects on crime.

Finally, noteworthy is the substantial body of work on the effects of labor market conditions and (welfare-related) active labor market policies (ALMPs) on crime. Several studies show that advantageous labor market conditions reduce crime (e.g. Gould et al. 2002, Schnepel 2018, Yang 2017b). ALMPs, on the other hand, show mixed results. A workfare program in Denmark simultaneously reduced both welfare uptake and crime (Fallesen et al. 2018), whereas another ALMP in Sweden reduced welfare uptake, while increasing crime (Persson 2013). Fallesen et al. (2018) hypothesize that ALMPs may increase crime if they discourage welfare applications through 'threat effects', i.e. when the negative consequences of not meeting certain requirements are emphasized (see Black et al. 2003).

[3]The US state of Florida expanded the federal policy change to not only include a lifetime ban for drug felons from Supplemental Nutrition Assistance Program (SNAP) benefits, but also Temporary Assistance for Needy Families (TANF) benefits.

[4]Bolhaar et al. (2019) assess the effects of a job search period that is somewhat similar to the one exploited in this current study, on participants who (a) are older than the age group under consideration in this study, and (b) are on welfare and expected to be able to find regular employment within six months. The employability of participants is based on various individual characteristics, such as employment history, age, and education level. Our study, instead, focuses on a general sample of young individuals around the age of 27, who have to wait up to eight weeks instead of four weeks before they receive benefits, and do not receive benefits (retroactively) as from the beginning of the job search period (in case they return to the welfare agency after the job search period).

This is relevant to our study, as evidence suggests that the job search period policy discourages vulnerable individuals from welfare application, who subsequently drop out of sight of municipalities (Ministerie van Sociale Zaken en Werkgelegenheid 2015, Van Dodeweerd 2014).

Below, Section 2.2 will first discuss the age-based welfare policy that we exploit to identify the effects of welfare receipt on crime (i.e. the job search period). Section 2.3 describes the empirical model, after which we discuss the data, samples and some graphical evidence in Section 2.4. Section 2.5 contains the estimation results, including a cost-effectiveness analysis, followed by robustness checks in Section 2.6. We conclude and discuss the implications of the results in Section 2.7.

2.2 Welfare and the job search period

The Dutch welfare system guarantees a minimum income for every legally-registered inhabitant of the Netherlands, who has insufficient means of subsistence. Individuals are considered eligible for welfare benefits if they: (a) are at least 18 years old, (b) have an income lower than the welfare norm (including other household members), (c) cannot claim other benefits (e.g. unemployment benefits), (d) do not have assets exceeding a certain maximum threshold, and (e) are not imprisoned. There is no maximum time period during which individuals can receive welfare. In order to receive welfare benefits, recipients must fulfill job search requirements (e.g. a weekly job application target), and are obliged to accept all jobs. Municipalities support re-integration by offering job search assistance.

Welfare benefits are relatively high in the Netherlands. During our observation period, the welfare benefit level was around 660 euros per month for singles without children, 930 euros per month for single parents, and 1320 euros per month for couples. In addition, households may receive housing subsidies, child subsidies, and health insurance subsidies. The OECD shows that in 2018 the guaranteed minimum income benefit in the Netherlands was 60% of median disposable income (OECD 2018a). This indicator is only slightly higher in Japan (65%), Ireland (64%), and Denmark (63%), and much lower in the US (6%).

Since January 2012, all welfare applicants in the Netherlands below the age of 27 are subject by law to the so-called 'job search period' policy. This means that they are not entitled to welfare benefits in the first four weeks after notification of their intended application. It is only after this period that their right to welfare will be determined. Upon assessment, the municipality checks the actions of the applicant during the job search period. The applicants are required to have actively pursued employment during the job search period, of which they must convey tangible evidence in their application.

In addition, youths are required to hand over documents from which could be ascertained whether they are entitled to student grants.[5] Applicants below the age of 27 are only considered eligible for welfare benefits if opportunities for student grants are exhausted. According to the explanatory memorandum of the law, the official goal of the job search period policy is labor activation of youths and to emphasize their personal responsibility therein. Apart from the job search period policy, recipients on either side of the 27-year-old threshold are subject to identical rules. This also applies to the welfare benefit level, which is equal across the ages of 21 to 64.

For those who apply for welfare benefits (after the job search period), the municipality is given eight weeks to determine eligibility. Meanwhile, welfare recipients can receive an advance payment, if they provide the required information to the municipality timely and complete. Municipalities must pay this advance payment no later than four weeks after the date of the application. This means that, for individuals younger than 27 who are subject to the job search period, it can in total take eight weeks before one receives any income.

Noncooperation on part of the youth will lead to exclusion of their right to welfare benefits. Consequently, many applicants lack a guaranteed minimum income also beyond the four-week job search period. While national figures are unavailable, the municipality of Utrecht[6] reports

[5] To complete higher or continued education, Dutch citizens were entitled to student grants that partially cover tuition fees, travel costs and living expenses. The eligibility for these grants ends once a first final degree is obtained (i.e. a master's degree), or the maximum receipt period expires.

[6] Utrecht is the fourth most populous municipality in the Netherlands.

that in the first seven months after the reform, 64% of applicants refrain from applying for welfare after the job search period (Van Dodeweerd 2014). About half of them found employment instead, 5% enrolled in education, and 12% received other benefits. For the remaining one-third, it is unknown how they sustain themselves.

2.3 Empirical methodology

Estimating the effect of welfare receipt on crime is challenging due to omitted variables affecting both the probability to receive welfare benefits as well as the probability to commit crime. For example, personality traits, such as self-control, time preferences and risk aversion, influence both welfare receipt and crime.[7] To address this endogeneity problem, we estimate a bivariate probit instrumental variable (IV) model with a first-stage regression discontinuity (RD) design. This approach is facilitated by the sharp discontinuity in welfare policy, in the form of the 27-year-old threshold of the job search period. By comparing individuals just above the treatment assignment threshold to those just below that threshold, the first-stage RD design enables us to instrument welfare receipt with the job search period policy. 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 monthly level allows for a sharp regression discontinuity design. Evidence suggests that the job search period policy not only affects welfare recipients, but also discourages individuals from applying for welfare benefits (Ministerie van Sociale Zaken en Werkgelegenheid 2015, Van Dodeweerd 2014). We therefore include the full population around the age of 27, to also capture potential discouragement effects.[8] Through this approach, we fully exploit the exogenous discontinuity in welfare policy to assess the causal effects of welfare receipt on crime.

[7] E.g. Bernheim et al. (2015), Borghans et al. (2008), Coelli et al. (2007), Machin et al. (2011), Pratt and Cullen (2000).

[8] Other papers also argue that welfare policies may affect both recipients and non-recipients (e.g. DeLeire et al. 2006).

As the outcome variable crime is dichotomous and has probabilities close to zero, we use a bivariate probit (BP) model instead of a linear IV model. The BP model has been used in various areas of economics, for example, to study the effect of obesity on employment (Morris 2007), chronic diseases on labor force participation (Zhang et al. 2009), offending on the probability of being a victim of crime (Deadman and MacDonald 2004), fertility on female labor force participation (Carrasco 2001), and parental smoking habits on their children's smoking decision (Loureiro et al. 2004). Bhattacharya et al. (2006) and Chiburis et al. (2012) compare the bivariate probit model with the two-step or linear probability model estimators. Their simulation results argue in favor of using the bivariate probit model when the average probability of the dependent variable is close to 0 or 1 (which clearly applies to monthly-level crime rates).

The model is specified as follows:

$$y_{it}^* = \beta_0 + \beta_1 w_{it} + \beta_2 A_{it} + \beta_3 1(A_{it} < 27) A_{it} + \beta_4 X_i + \beta_5 T_t + v_{it} \tag{2.1}$$

$$w_{it}^* = \gamma_0 + \gamma_1 RD_{it} + \gamma_2 A_{it} + \gamma_3 1(A_{it} < 27) A_{it} + \gamma_4 X_i + \gamma_5 T_t + \varepsilon_{it} \tag{2.2}$$

$$y_{it} = \begin{cases} 1 & \text{if } y_{it}^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

$$w_{it} = \begin{cases} 1 & \text{if } w_{it}^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

$$\begin{bmatrix} v_{it} \\ \varepsilon_{it} \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right)$$

For $i = 1 \ldots n$ and $t = 1 \ldots T$

Where y_{it}^* in equation (2.1) is a latent variable that indicates whether individual i is suspected of (financially-motivated) crime in month t, w_{it} is a dummy variable indicating the welfare receipt status of individual i in month 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_i indicates whether individual i is a native-born Dutch citizen, T_t represents a linear time trend (months) and v_{it} the error term.[9] As we expect welfare receipt status to be endogenous, we instrument welfare receipt with the job search period, as shown in equation (2.2). In this equation, w^*_{it} is the latent variable indicating welfare receipt, RD_{it} is the treatment dummy that captures the job search period policy below the age of 27 (a value of one indicates an age below the policy threshold for individual i at time t), and ε_{it} is the error term. The error terms v_{it} and ε_{it} are assumed to follow a bivariate normal distribution with mean zero, variance one and covariance ρ. We are interested in the coefficient β_1, which captures the effect of welfare receipt on crime.

Although the BP model is identified by functional form (relying on the assumption of normality), we follow common practice by imposing an exclusion restriction to improve identification. Using simulations, Li et al. (2019) show that the inclusion of valid instruments can significantly improve the precision of the estimation, and that biases decrease with sample size. They conclude that the BP model is a readily implementable and reasonably resilient empirical tool for estimating the effect of an endogenous binary regressor on a binary outcome variable. For comparison, we include two-stage least squares estimates in Appendix 2.A.

As suggested by Chiburis et al. (2012), we recover standard errors through bootstrapping. Furthermore, following the work of Lee and Card (2008), we cluster the standard errors on the assignment variable age (in months).

In light of the findings by Gelman and Imbens (2018), we limit the analyses to local linear and local quadratic polynomials.[10] For the baseline estimates, we determine the optimal bandwidth size per sample via the commonly used mean-squared error (MSE) bandwidth selection method presented by Calonico et al. (2014). Through this approach, we find optimal bandwidths of 12 months for men, 11 months for women, and 10 and 11 months for low-educated men and women, respectively. These

[9] Additional analyses were performed with quadratic and cubic time trends, which do not change the conclusions.

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

bandwidths are considered optimal in terms of the "bias-variance trade-off" (see Cattaneo et al. 2020). Using larger bandwidths than the MSE-optimal bandwidth size will produce more biased point estimators (if the unknown function differs from the specified functional form), whereas smaller bandwidths increase the variance due to the reduced number of observations. As robustness checks, we compare the estimates across functional forms and multiple bandwidths.

Finally, to increase the interpretability of the estimates, we compute the average treatment effects (ATEs) of welfare receipt on crime as follows,

$$\begin{aligned} ATE = \frac{1}{N}\sum_i\sum_t &\Phi(\beta_0 + \beta_1 + \beta_2 A_{it} + \beta_3 1(A_{it} < 27)A_{it} + \beta_4 X_i + \beta_5 T_t) \\ &-\Phi(\beta_0 + \beta_2 A_{it} + \beta_3 1(A_{it} < 27)A_{it} + \beta_4 X_i + \beta_5 T_t) \end{aligned} \tag{2.3}$$

While the combination of an instrumental variable approach with a regression discontinuity design allows us to exploit the full potential of the available data and to adequately account for endogeneity, it also brings along a fair amount of model assumptions. For the first-stage RD approach, the main underlying assumption 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. This assumption realistically holds, as we use age (in months) as the assignment variable, which is centrally registered and cannot be manipulated.

For the IV approach, there are two main model assumptions: instrument relevance and instrument exogeneity. We will check for instrument relevance, i.e. whether the instrument causes a sufficient amount of variation in the first-stage outcome variable. The assumption of instrument exogeneity, also known as the exclusion restriction, states that the instrument may not be correlated to the second-stage error terms and must only affect the second-stage outcome through the instrumented variable (welfare receipt). Since the instrument consists of a nationwide age-based discontinuity in welfare policy, there are no conceivable mechanisms through which this welfare policy might affect criminal behavior in other ways than through its effect on welfare receipt.[11]

2.4 Data and graphical evidence

To estimate the models, we use longitudinal individual-level data from Statistics Netherlands on all registered Dutch inhabitants around the 27-year-old policy threshold.[12] As the job search period was introduced in January 2012 and the data are available until December 2014, we have a three-year observation window.

Administrative data on welfare are derived from municipal monthly payment registrations. The crime data are derived from crime reports of the Dutch law enforcement 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, there is no reason to expect the unmeasured crime distribution to be correlated with the policy discontinuity at the 27-year-old threshold. The available daily crime measures are aggregated to dichotomous monthly values.

2.4.1 Sample and descriptive statistics

In line with our research design, we select all registered inhabitants of the Netherlands who reached the age of 27 in the years 2012 to 2014. This results in a full sample of 635,179 individuals, aged 24 to 29 years, and a total of 21,433,664 monthly observations between January 2012 and

[11] As will be discussed in Section 2.4, we do not find the job search period policy to affect employment rates.

[12] 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 bijstanduitkeringtab, gbaadresobjectbus, gbapersoontab, hdiplomaregtab, integraal huishoudens inkomen, integraal persoonlijk inkomen, polisbus, spolisbus, verdtab and vslgwbtab.

December 2014. The large sample size facilitates our exploitation of the welfare policy discontinuity at this age cut-off.[13]

To investigate potential heterogeneous effects, we run the analyses over four subsamples: men, women, low-educated men, and low-educated women. Men and women are considered separately, as men are more likely to commit offenses compared to women (e.g. Steffensmeier and Allan 1996). Previous literature emphasizes the importance of analyzing the effects of welfare on crime among women, due to their higher poverty and welfare dependency rates (see Corman et al. 2014, Holtfreter et al. 2004). With regard to education, Lochner and Moretti (2004) and Machin et al. (2011) find that education reduces criminal activity. This may be caused by higher opportunity costs of crime and/or by differences in cognitive and personality traits, such as self-control and patience.[14] We classify individuals as being low educated if their educational attainment is below the Dutch classification of higher education (i.e. a higher vocational or university degree).[15]

Table 2.1 gives an overview of the most relevant characteristics in the selected samples. In the full sample, 3.75% of the individuals receive welfare benefits in any given month within the observation window. The employment rate is 73.42% and the monthly general and financially-motivated crime rates are 0.25% and 0.09%, respectively. Within our observation window (2012-2014), 5.30% of the full sample committed crime in general and 2.16% committed financially-motivated crime.

A comparison of the subsamples shows that men have the lowest welfare dependency rate (3.35%). This rate is higher among women

[13]We take into account that all welfare applicants residing in the city of Rotterdam are subject to the job search period, irrespective of age.

[14]Patient people are more likely to finish education, but education may also increase one's patience (Becker and Mulligan 1997) Borghans et al. (2008) discuss the relation between education and personality traits. Hjalmarsson (2008) notes that individuals with a low ability to make considered decisions may be more likely to commit crimes and be arrested, as well as to drop out of school. In studying the relationship between education and crime, Lochner (2004) distinguishes between unskilled crimes and white collar crimes. More educated adults should commit fewer unskilled crimes, but white collar crimes decline less (or even increase) with education.

[15]The educational attainment data have limited coverage among first-generation immigrants, which may have resulted in unmeasured highly-educated individuals among the low-educated subsamples. Due to their limited population size, we consider any potential influence on the estimates to be negligible.

(4.16%), low-educated men (4.23%), and low-educated women (6.02%). Conversely, the employment rate is highest among men (73.49%), and lowest among low-educated women (66.47%). Low-educated women show the lowest annual incomes, with an average personal primary income of €17,988 and a standardized household income of €20,109. These are the highest among men (€29,252 and €22,829 respectively). Low-educated men score the highest on all crime measures, with a monthly crime rate of 0.52%, a financially-motivated crime rate of 0.19% and on average 1.98 offenses per offender over the three-year observation window. We find the lowest crime rates among women (monthly general and financially-motivated crime rates of 0.08% and 0.04%, and on average 1.49 offenses per offender).

Table 2.1: Descriptive statistics, 2012-2014

	Full sample	Men	Women	Low-educated men	Low-educated women
Native	68.96%	69.50%	68.41%	65.15%	61.31%
Crime (monthly)	0.25%	0.41%	0.08%	0.52%	0.12%
if welfare receipt = 1	1.19%	2.05%	0.47%	2.14%	0.49%
if welfare receipt = 0	0.21%	0.35%	0.07%	0.45%	0.10%
Financially-motivated crime (monthly)	0.09%	0.15%	0.04%	0.19%	0.06%
if welfare receipt = 1	0.56%	0.91%	0.28%	0.95%	0.29%
if welfare receipt = 0	0.08%	0.12%	0.03%	0.15%	0.05%
Offenses per offender	1.85	1.94	1.49	1.98	1.51
Welfare dependency rate (monthly)	3.75%	3.35%	4.16%	4.23%	6.02%
Employment rate (monthly)	73.42%	73.49%	73.34%	70.44%	66.47%
Annual personal primary income	26,450	29,252	23,596	26,415	17,988
Annual standardized household income	22,641	22,829	22,450	21,604	20,109
Number of individuals	635,179	321,466	313,713	245,223	208,632
Number of observations	21,433,664	10,815,461	10,618,203	8,172,030	6,956,063

2.4.2 Graphical evidence

Before turning to the estimation results, we present some exploratory graphs on the evolution of various outcomes around the 27-year-old threshold. Figures 2.1 to 2.3 present local polynomial smooth plots, along with 95% confidence intervals. As the identification in the first-stage regression discontinuity design comes from the welfare policy discontinuity at the age of 27, these graphs offer insight into the feasibility of this approach.

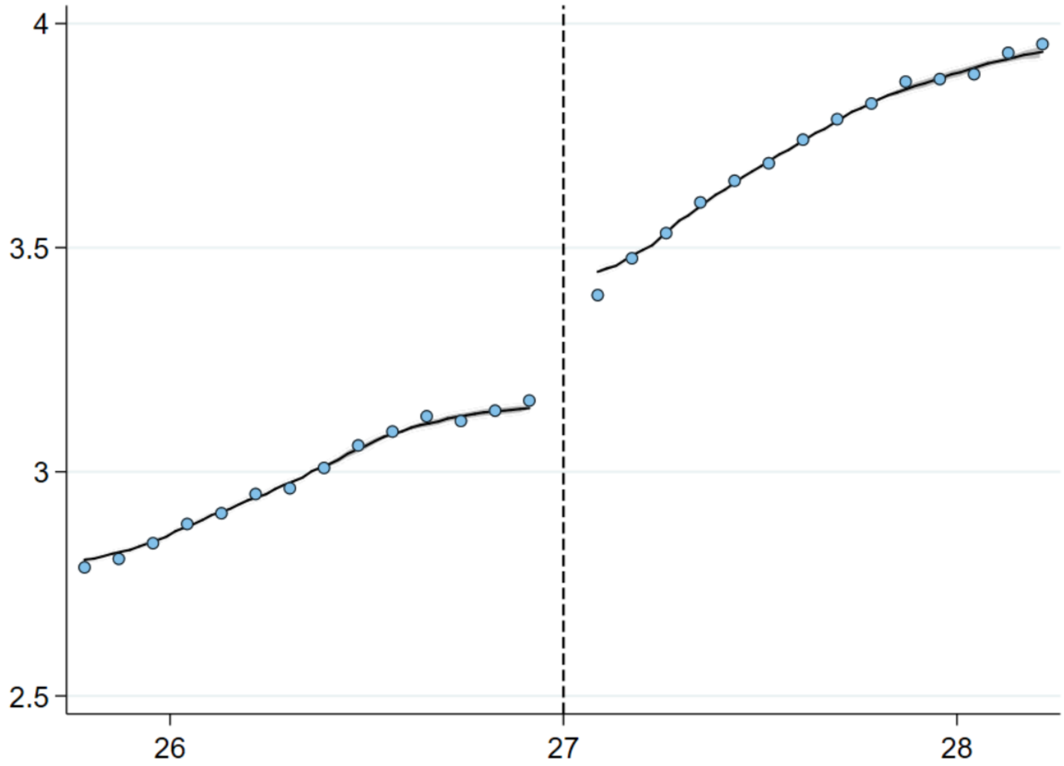
Figure 2.1 shows the evolution of the welfare dependency rates across age among men and women. In line with the descriptives shown in Table 2.1, the monthly welfare dependency rates are about 3-4%. The jumps upward at the cut-off value indicate reductions in welfare receipt due to the job search period, which only applies to those on the left-hand side of the cut-off. The discontinuity is larger for men than for women.

Figure 2.2 presents crime rates for men and women. Monthly crime rates are about 0.40% for men and 0.08% for women, in line with the descriptives shown in Table 2.1. Compared to men, women show a larger drop at the cut-off, relative to their average crime rate (note the different scales of the vertical axes). This may indicate heterogeneous effects of welfare receipt on crime between the sexes. As the frequency of crime is comparatively low, we also see that the confidence intervals are larger for crime than for welfare dependency rates.

This study investigates the effect of welfare receipt on crime. The main goal of the job search period policy, however, is to increase employment. If successful, crime would likely be reduced among the activated individuals (see Lageson and Uggen 2013). In that case, our estimates of the effect of welfare receipt on crime would be biased towards zero (and thus be a lower bound of the true effect). Figure 2.3 does not show discontinuities in the employment rate around the age of 27 (note the scales of the vertical axes). This is further supported by the estimates in Table 2.15 of the appendix, which show statistically significant, but insubstantial discontinuities in the employment rate at the 27-year-old threshold.

Figure 2.1: Welfare dependency rates across age among men (a) and women (b)

(a) Welfare dependency rate men



(b) Welfare dependency rate women

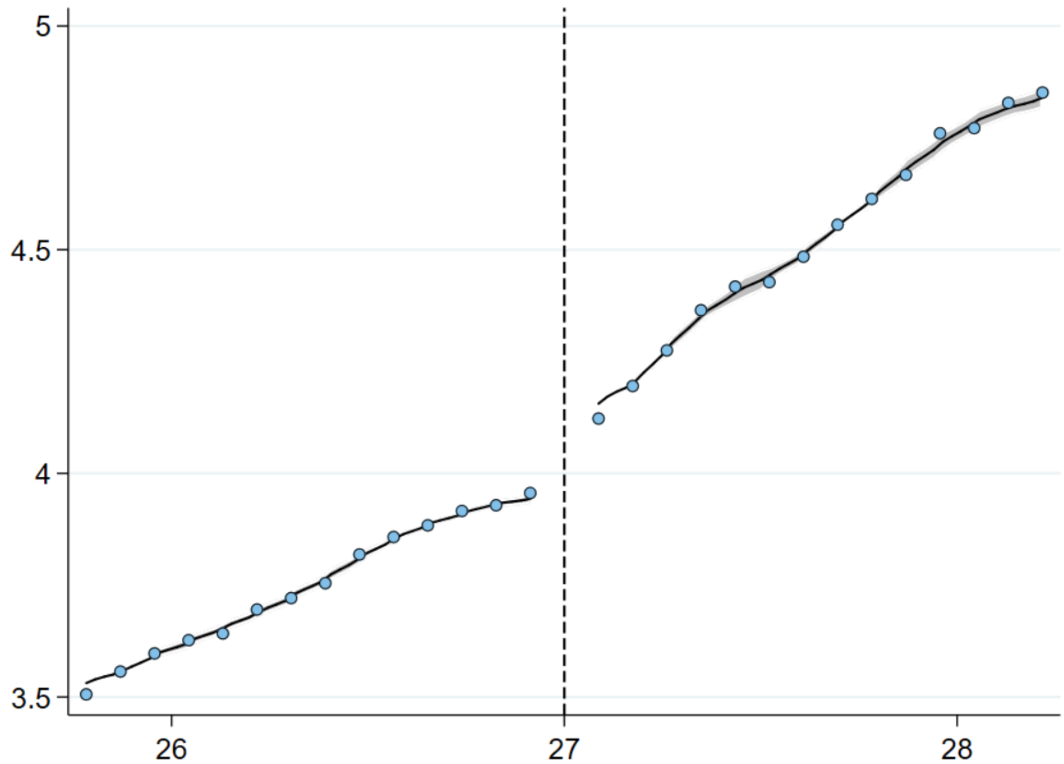
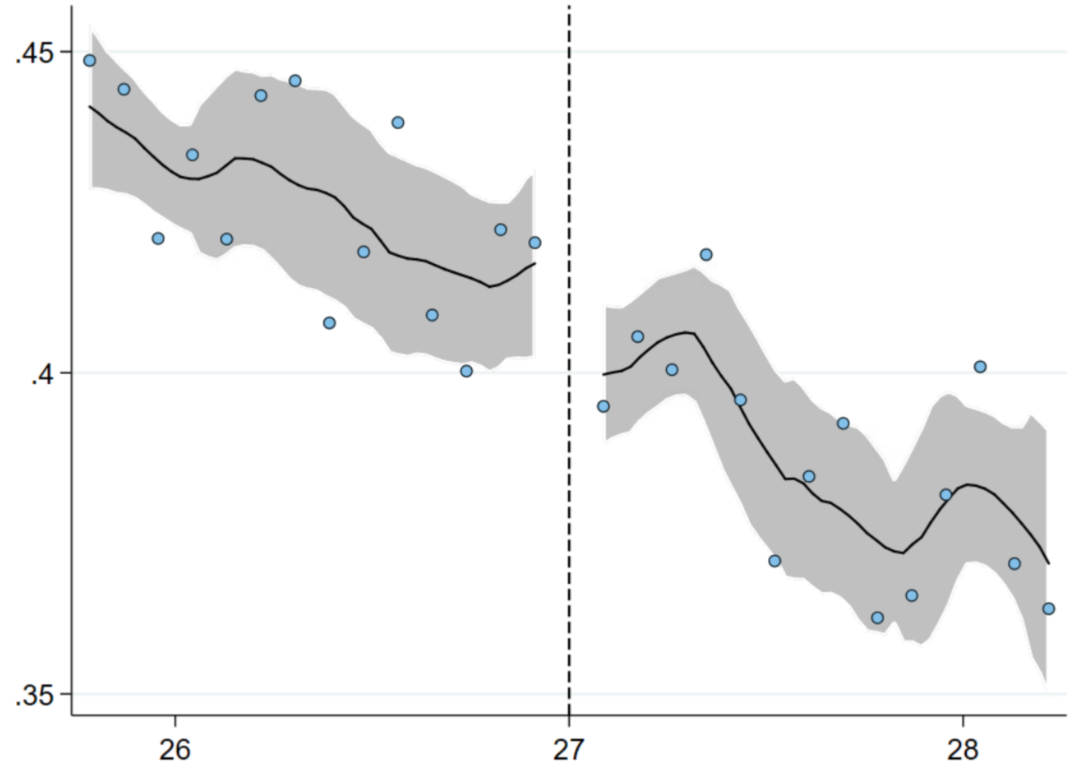


Figure 2.2: Crime rates across age among men (a) and women (b)

(a) Crime rate men



(b) Crime rate women

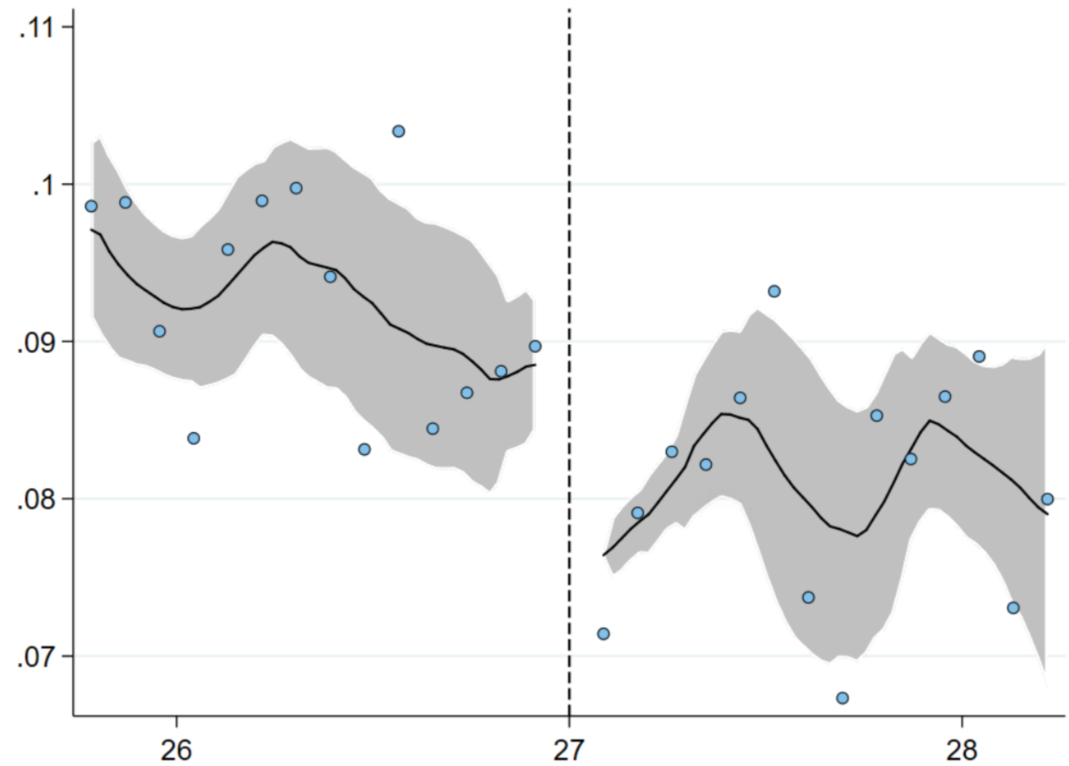
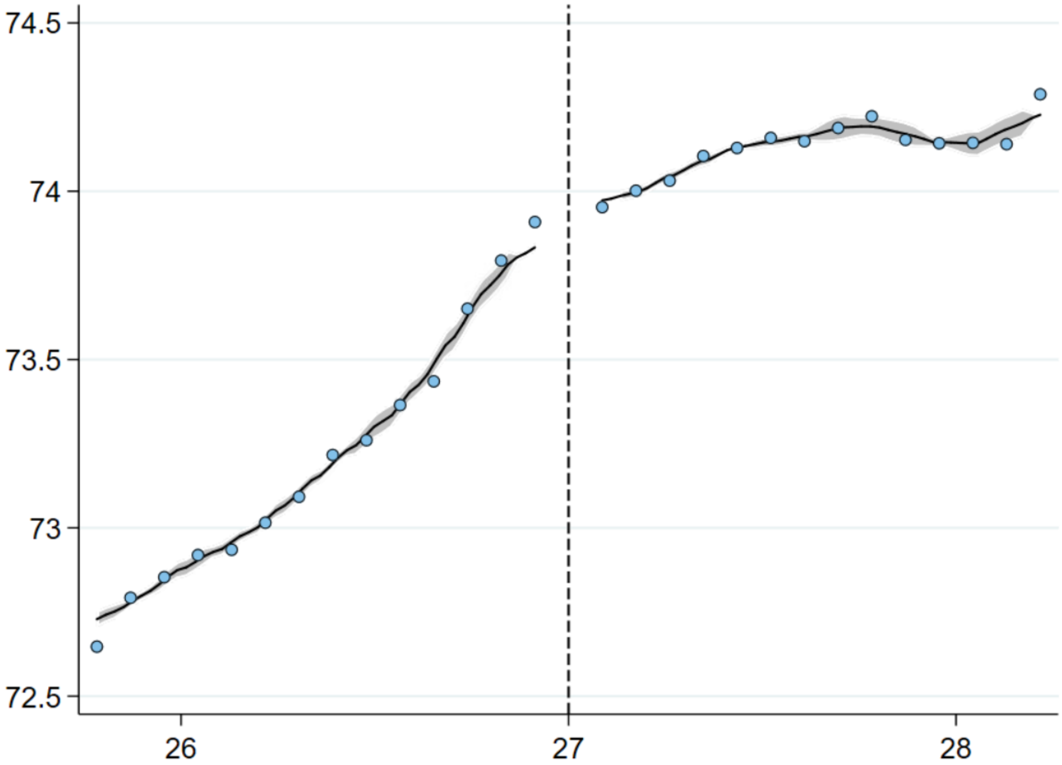
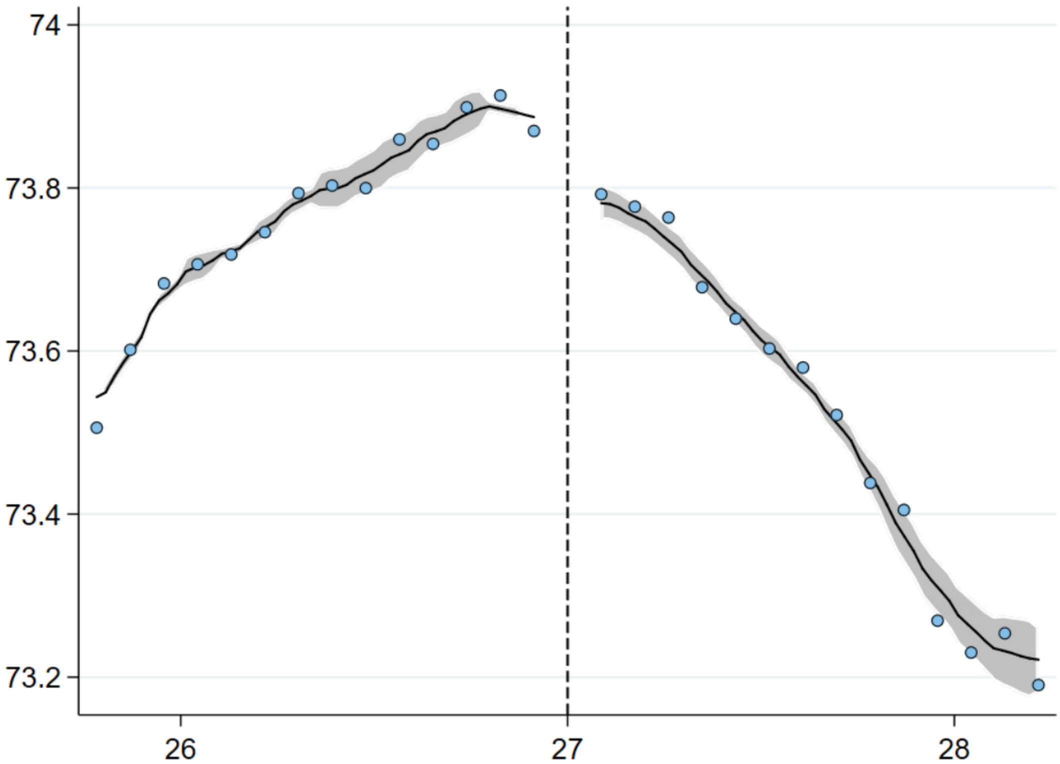


Figure 2.3: Employment rates across age among men (a) and women (b)

(a) Employment rate men



(b) Employment rate women



2.5 Results

2.5.1 Estimation results

Tables 2.2 to 2.5 present the baseline estimation results for crime and financially-motivated crime, for men, women and the low-educated subgroups. The standard probit models show substantial positive correlations between welfare receipt and (financially-motivated) crime. Controlling for endogeneity, however, the IV estimates reveal that welfare receipt reduces crime. The first-stage coefficients show that the instrument ('RD') is statistically highly significant across all subsamples in the baseline analyses (p<.001), which suggests a sufficiently strong instrument. We find the job search period policy to reduce welfare receipt by approximately 6% for men and 2.5% for women. This reduction is likely attributable to both delayed eligibility and discouragement effects.[16]

Table 2.2 shows the estimation results for crime in general. The IV estimates show statistically significant negative coefficients of welfare receipt on crime for men (-0.4016) and women (-0.4230). To enhance the interpretability of the estimation results, we compute the average treatment effects (ATEs). The ATEs indicate how much the conditional probability of committing crime changes due to welfare receipt. The ATEs show that welfare receipt significantly reduces the average monthly probability of committing crime among men by 0.37 percentage points (from 0.53% to 0.16%).[17] For women, we find a statistically significant reduction of 0.11 percentage points (from 0.14% to 0.03%). The lack of a basic level of guaranteed income thus appears to be a major risk factor for crime. In absolute terms, we find that this effect is substantially larger

[16] Evidence suggests that the JSP policy discourages individuals from applying for welfare benefits (Ministerie van Sociale Zaken en Werkgelegenheid 2015, Van Dodeweerd 2014). In the fourth-largest city of the Netherlands, it was found that 64% of the applicants refrained from applying for welfare after the job search period. For one-third of them it is unknown how they sustain themselves, as they did not transition to formal employment or education, and did not receive other benefits.

[17] As expected, the crime probabilities are larger when we weigh the observations by the treatment compliance propensity (i.e. the predicted individual-level first-stage marginal effect). For example, for men we find crime per month to decline from 0.67% to 0.21%, which amounts to a weighted ATE of -0.46 percentage points (compared to -0.37 percentage points, non-weighted).

for men than for women, which may partially explain the gender gap in crime. In relative terms, however, the reduction is quite similar for women (-77%) and men (-71%).[18] Note that the relative effects may seem exceptionally large, however, as recognized in the medical literature, this can be misleading because of the low absolute numbers.[19] Furthermore, a related study by Tuttle (2019) finds a similar relative effect for newly-released drug traffickers in the US.[20]

With regard to financially-motivated crime, Table 2.3 presents statistically significant negative coefficients of welfare receipt on financially-motivated crime for men and women. The ATEs show that welfare receipt reduces financially-motivated crime by 0.20 percentage points among men (from 0.25% to 0.05%), and 0.05 percentage points among women (from 0.07% to 0.02%). For men we find a slightly larger relative reduction in financially-motivated crime (-82%) compared to crime in general (-71%), which is in line with Becker's rational choice theory. For women however, the relative reduction in financially-motivated crime (-72%) is slightly lower than the relative reduction in crime in general (-77%). This is more in line with Agnew's general strain theory, which argues that, by alleviating financial stress, welfare receipt reduces emotional strain and consequently criminal behavior in general.

Low-educated individuals may have lower opportunity costs than highly-educated individuals, and may be less well equipped to cope with strain, due to (on average) lower self-control, patience and risk aversion (Becker and Mulligan 1997, Borghans et al. 2008, Pratt and Cullen 2000). Tables 2.4 and 2.5 show the estimation results for low-educated men and women, with regard to crime and financially-motivated crime, respectively. For low-educated individuals we find larger ATEs than for the general population, but the relative effects are quite similar: -71% versus -66%

[18]-0.11/0.14 and -0.37/0.53 for women and men, respectively.

[19]As an example, Gigerenzer et al. (2010) mentions the third generation oral contraceptive pills, which increased the risk of potentially life threatening thrombosis twofold. The news provoked great anxiety, and many women stopped taking the pill, which led to unwanted pregnancies and abortions. Yet, it was only that for every 7000 women who took the earlier pills one had a thrombosis, and this number increased to two for women who took third generation pills.

[20]More specifically, Tuttle (2019) finds a 60 percent increase in recidivism among convicted drug traffickers, by exploiting a welfare reform which introduced a lifetime ban from food stamp eligibility for this group in the US state of Florida.

for men and low-educated men, and -77% versus -75% for women and low-educated women, respectively. The relative reductions in financially-motivated crime are also very similar for low-educated individuals and the general population (-82% for men and -81% low-educated men, and -72% versus -71% for women and low-educated women, respectively). We thus do not find evidence that the absence of a basic minimum income causes larger relative increases in crime among low-educated individuals, compared to individuals with a higher education level.

For comparison, Tables 2.11 to 2.14 in Appendix 2.A present ordinary least squares (OLS) and two-stage least squares (2SLS) estimates. First, the OLS estimates show the positive correlation between welfare receipt and crime (as was also shown by the probit estimates). Second, the sign of the coefficients reverse when we take into account endogeneity in the 2SLS estimates. For example, we find statistically non-significant but very large reductions ranging from 1.58 percentage points for men to 10.41 percentage points among women (greatly exceeding their monthly crime rates). So, although the signs are similar to the nonlinear models, these estimates show the disadvantage of linear estimation if the average probability of the outcome variable is near 0 or 1, as is detailed by Bhattacharya et al. (2006) and Chiburis et al. (2012).

To summarize, the estimation results show that welfare receipt reduces crime. For men we find a comparatively large reduction in financially-motivated crime, while for women the effects are similar for financially-motivated crime and crime in general. The relative reduction in crime as a result of a guaranteed minimum income is similar for low and highly-educated individuals.

Table 2.2: Probit and bivariate probit (IV) estimates for crime among men and women

	PROBIT MEN 12 MONTHS	BIPROBIT MEN 12 MONTHS	PROBIT WOMEN 11 MONTHS	BIPROBIT WOMEN 11 MONTHS
Crime				
Welfare receipt	0.6017***	-0.4016**	0.5825***	-0.4230***
	(0.0079)	(0.1554)	(0.0138)	(0.0134)
Age	-0.0025**	-0.0010	-0.0009	0.0009
	(0.0009)	(0.0010)	(0.0019)	(0.0017)
Age x 1(<27)	0.0011	0.0005	-0.0033	-0.0038
	(0.0014)	(0.0013)	(0.0034)	(0.0031)
Native	-0.2320***	-0.3434***	-0.1198***	-0.2483***
	(0.0046)	(0.0259)	(0.0081)	(0.0072)
Time (month)	-0.0022***	-0.0016***	-0.0014***	-0.0010**
	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt				
RD		-0.0262***		-0.0101***
		(0.0036)		(0.0021)
Age		0.0042***		0.0056***
		(0.0005)		(0.0002)
Age x 1(<27)		-0.0016**		-0.0024***
		(0.0005)		(0.0003)
Native		-0.5908***		-0.5160***
		(0.0027)		(0.0045)
Time (month)		0.0026***		0.0012***
		(0.0001)		(0.0001)
ρ		0.5136***		0.5351***
		(0.0907)		(0.0043)
Probabilities (per month)				
If welfare receipt = 1 (%)		0.16		0.03
If welfare receipt = 0 (%)		0.53		0.14
ATE (%point)		-0.37**		-0.11***
		(0.13)		(0.00)
Observations	5,910,778	5,910,778	5,411,291	5,411,291
Individuals	314,691	314,691	306,249	306,249

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

Table 2.3: Probit and bivariate probit (IV) estimates for financially-motivated crime among men and women

	PROBIT MEN 12 MONTHS	BIPROBIT MEN 12 MONTHS	PROBIT WOMEN 11 MONTHS	BIPROBIT WOMEN 11 MONTHS
Financially-motivated crime				
Welfare receipt	0.6145***	-0.5137***	0.6013***	-0.3503***
	(0.0086)	(0.0213)	(0.0199)	(0.0179)
Age	-0.0017	0.0003	0.0009	0.0025†
	(0.0013)	(0.0010)	(0.0016)	(0.0015)
Age x 1(<27)	0.0001	-0.0005	-0.0067*	-0.0070**
	(0.0021)	(0.0017)	(0.0028)	(0.0026)
Native	-0.2414***	-0.3872***	-0.1428***	-0.2650***
	(0.0091)	(0.0093)	(0.0109)	(0.0103)
Time (month)	-0.0011**	-0.0002	-0.0012†	-0.0008
	(0.0004)	(0.0003)	(0.0007)	(0.0006)
Welfare receipt				
RD		-0.0264***		-0.0096***
		(0.0036)		(0.0022)
Age		0.0042***		0.0057***
		(0.0005)		(0.0002)
Age x 1(<27)		-0.0016**		-0.0024***
		(0.0005)		(0.0003)
Native		-0.5909***		-0.5159***
		(0.0028)		(0.0045)
Time (month)		0.0026***		0.0012***
		(0.0001)		(0.0001)
ρ		0.5956***		0.5070***
		(0.0121)		(0.0048)
Probabilities (per month)				
If welfare receipt = 1 (%)		0.05		0.02
If welfare receipt = 0 (%)		0.25		0.07
ATE (%point)		-0.20***		-0.05***
		(0.01)		(0.00)
Observations	5,910,778	5,910,778	5,411,291	5,411,291
Individuals	314,691	314,691	306,249	306,249

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

Table 2.4: Probit and bivariate probit (IV) estimates for crime among low-educated men and low-educated women

	PROBIT LE MEN 10 MONTHS	BIPROBIT LE MEN 10 MONTHS	PROBIT LE WOMEN 11 MONTHS	BIPROBIT LE WOMEN 11 MONTHS
Crime				
Welfare receipt	0.5532***	-0.3643*	0.5088***	-0.4167***
	(0.0075)	(0.1623)	(0.0136)	(0.0148)
Age	-0.0033***	-0.0015	-0.0007	0.0014
	(0.0008)	(0.0011)	(0.0019)	(0.0018)
Age x 1(<27)	0.0028†	0.0020	-0.0031	-0.0037
	(0.0016)	(0.0016)	(0.0033)	(0.0030)
Native	-0.1859***	-0.2864***	-0.0515***	-0.1610***
	(0.0050)	(0.0259)	(0.0088)	(0.0083)
Time (month)	-0.0028***	-0.0023***	-0.0017***	-0.0015***
	(0.0002)	(0.0003)	(0.0004)	(0.0003)
Welfare receipt				
RD		-0.0266***		-0.0117***
		(0.0027)		(0.0020)
Age		0.0059***		0.0068***
		(0.0004)		(0.0002)
Age x 1(<27)		-0.0028***		-0.0028***
		(0.0004)		(0.0003)
Native		-0.5524***		-0.4304***
		(0.0018)		(0.0034)
Time (month)		0.0020***		0.0004***
		(0.0001)		(0.0001)
ρ		0.4758***		0.5059***
		(0.0947)		(0.0054)
Probabilities (per month)				
If welfare receipt = 1 (%)		0.22		0.05
If welfare receipt = 0 (%)		0.65		0.20
ATE (%point)		-0.43*		-0.15***
		(0.17)		(0.01)
Observations	3,846,170	3,846,170	3,541,347	3,541,347
Individuals	237,519	237,519	202,597	202,597

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

Table 2.5: Probit and bivariate probit (IV) estimates for financially-motivated crime among low-educated men and low-educated women

	PROBIT LE MEN 10 MONTHS	BIPROBIT LE MEN 10 MONTHS	PROBIT LE WOMEN 11 MONTHS	BIPROBIT LE WOMEN 11 MONTHS
Financially-motivated crime				
Welfare receipt	0.5654***	-0.5085***	0.5299***	-0.3407***
	(0.0097)	(0.0203)	(0.0199)	(0.0190)
Age	-0.0029†	-0.0003	0.0011	0.0030†
	(0.0016)	(0.0013)	(0.0018)	(0.0017)
Age x 1(<27)	0.0020	0.0009	-0.0062*	-0.0067*
	(0.0029)	(0.0025)	(0.0030)	(0.0028)
Native	-0.1996***	-0.3369***	-0.0799***	-0.1823***
	(0.0102)	(0.0100)	(0.0111)	(0.0107)
Time (month)	-0.0018***	-0.0011**	-0.0015*	-0.0013*
	(0.0004)	(0.0003)	(0.0007)	(0.0006)
Welfare receipt				
RD		-0.0269***		-0.0112***
		(0.0027)		(0.0020)
Age		0.0059***		0.0068***
		(0.0004)		(0.0002)
Age x 1(<27)		-0.0027***		-0.0028***
		(0.0004)		(0.0003)
Native		-0.5526***		-0.4304***
		(0.0018)		(0.0034)
Time (month)		0.0020***		0.0004***
		(0.0001)		(0.0001)
ρ		0.5757***		0.4761***
		(0.0120)		(0.0055)
Probabilities (per month)				
If welfare receipt = 1 (%)		0.06		0.03
If welfare receipt = 0 (%)		0.32		0.10
ATE (%point)		-0.26***		-0.07***
		(0.01)		(0.00)
Observations	3,846,170	3,846,170	3,541,347	3,541,347
Individuals	237,519	237,519	202,597	202,597

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

Cost-effectiveness 2.5.2

Crime reduction is high on the public policy agenda, due to its vast social and economic costs. As we find welfare receipt to substantially reduce crime, the question arises how cost-effective the provision of a basic level of guaranteed income is as a crime prevention strategy. To answer this question, we include a back-of-the-envelope cost-effectiveness calculation. First, we compute the number of individual-month observations that switch to nonreceipt due to the job search period policy. By multiplying this number with the average contemporary single-person monthly benefit level (€ 667), we approximate the total decline in welfare spending caused by the job search period policy. Next, to determine the total absolute change in crime, we also multiply the number of treatment compliers with the ATEs and the average number of offenses. Finally, the costs per prevented offense are approximated by dividing the change in welfare spending by the total absolute change in crime.

Table 2.6 presents the average amount of welfare spending in euros required per prevented (financially-motivated) offense for all subgroups. In line with the estimation results, we find the cost effectiveness to differ substantially across subgroups and crime outcomes. Welfare spending is approximately three times as cost effective in preventing crime among men compared to women, with € 144,974 and € 466,871 per offense respectively. For low-educated men and women, with higher absolute treatment effects, we find that the amount of welfare spending needed to prevent an offense is lower (€ 130,128 for men and € 360,688 for women). Since financially-motivated crime is only part of all crime prevented by welfare receipt, the amount of welfare spend per prevented financially-motivated offense are higher than for crime in general.

Table 2.6: Welfare spending per prevented offense

€ /offense	MEN	WOMEN	LE MEN	LE WOMEN
Crime	144,974	466,871	130,128	360,688
Financially-motivated crime	204,596	948,367	165,895	730,881

Notes. The shown values are derived from the baseline IV estimates shown in Tables 2.2 to 2.5

To assess the cost-effectiveness of welfare spending as a crime prevention strategy, we need to compare welfare spending (shown in Table 2.6) with a comprehensive approximation of the costs of crime. The direct costs of crime are easily measureable (e.g. criminal justice costs and financial damages). However, it is notoriously difficult to quantify the indirect costs of crime, such as reduced labor market opportunities for the perpetrators, reduced productivity of victims and nonfinancial damages. Consequently, only few studies have attempted to comprehensively estimate all costs per offense. Among these studies there is substantial variation in estimates, due to differences in methodologies and included costs.[21] A seminal study in the US by Cohen et al. (2004) uses a contingent valuation method to estimate all costs per offense for several types of crime. Compared to more traditional methods, this approach aims to generate a more comprehensive cost approximation. Converted to 2013 euros,[22] they find costs per offense of €25,448 for household burglary, €71,253 for serious assault, €236,154 for armed robbery, €241,244 for rape/sexual assault and €9,873,682 for murder. While the costs per murder greatly exceed the amount of welfare spending required to prevent an offense, this is generally not the case for the more common crime categories. Therefore, based on our estimates we conclude that although welfare spending can significantly reduce crime, it does not seem to be a cost-effective crime prevention strategy.

2.6 Robustness checks

This section presents multiple robustness checks over (a) two functional forms: a linear and a quadratic model, and (b) four additional bandwidth specifications, ranging from 14 to 35 months on each side of the 27th-birthday-month cut-off.[23] The latter is the upper bandwidth limit due to

[21] E.g. Cohen (1988), McCollister et al. (2010), Rajkumar and French (1997).

[22] Based on US Bureau of Labor Statistics data on the consumer price index (US Bureau of Labor Statistics 2018), an inflation rate of 1.3518 was used to convert (July) 2000 dollars to (July) 2013 dollars. The resulting figures were subsequently converted to euros using a dollar/euro conversion rate of 0.753, as reported by the OECD for the year 2013 (OECD 2018b).

[23] We limit the sensitivity analyses to first and second-order polynomials, in line with Gelman and Imbens (2018).

the three-year observation window (2012-2014). Tables 2.7 to 2.10 present the coefficients for the instrument ('RD') and the variable of interest ('Welfare receipt') per subsample. Extended estimation results can be found in Appendix 2.C.

We find highly robust estimates for both crime and financially-motivated crime, across all subsamples. Starting with men, Table 2.7 shows that both coefficients change only slightly when we increase the bandwidth. Furthermore, the coefficients hardly differ between a linear and a quadratic model, and all coefficients remain statistically significant across the board. Table 2.8 shows that the welfare receipt estimates for women are the least sensitive to changes in functional form and bandwidth. Tables 2.9 and 2.10 present the robustness checks for low-educated men and women, which show similar results. In summary, we can be confident in interpreting the estimation results, as all estimates are highly robust.

Table 2.7: IV estimates with different bandwidths and functional forms, men

Bandwidth		12 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime						
Welfare receipt	Linear	-0.4016**	-0.3314**	-0.3364**	-0.3163**	-0.3049**
		(0.1554)	(0.1108)	(0.1122)	(0.1010)	(0.0900)
	Quadratic	-0.3932**	-0.3358**	-0.3648**	-0.3247**	-0.3169**
		(0.1417)	(0.1143)	(0.1348)	(0.1071)	(0.0968)
RD	Linear	-0.0262***	-0.0279***	-0.0292***	-0.0298***	-0.0310***
		(0.0036)	(0.0037)	(0.0042)	(0.0044)	(0.0042)
	Quadratic	-0.0201***	-0.0205***	-0.0266***	-0.0263***	-0.0267***
		(0.0013)	(0.0015)	(0.0026)	(0.0029)	(0.0038)
Financially-motivated crime						
Welfare receipt	Linear	-0.5137***	-0.5072***	-0.4980***	-0.5035***	-0.4942***
		(0.0213)	(0.0220)	(0.0222)	(0.0225)	(0.0226)
	Quadratic	-0.5057***	-0.5193***	-0.5120***	-0.5099***	-0.5059***
		(0.0158)	(0.0181)	(0.0192)	(0.0181)	(0.0182)
RD	Linear	-0.0264***	-0.0278***	-0.0293***	-0.0298***	-0.0310***
		(0.0036)	(0.0037)	(0.0041)	(0.0043)	(0.0042)
	Quadratic	-0.0211***	-0.0213***	-0.0267***	-0.0265***	-0.0268***
		(0.0013)	(0.0017)	(0.0026)	(0.0029)	(0.0038)

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.
Extended estimation results can be found in Appendix Tables 2.16 to 2.19.

Table 2.8: IV estimates with different bandwidths and functional forms, women

Bandwidth		11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime						
Welfare receipt	Linear	-0.4230***	-0.4235***	-0.4124***	-0.4123***	-0.4126***
		(0.0134)	(0.0125)	(0.0130)	(0.0133)	(0.0137)
	Quadratic	-0.4200***	-0.4205***	-0.4086***	-0.4069***	-0.4079***
		(0.0127)	(0.0124)	(0.0126)	(0.0126)	(0.0126)
RD	Linear	-0.0101***	-0.0115***	-0.0143***	-0.0171***	-0.0195***
		(0.0021)	(0.0023)	(0.0027)	(0.0029)	(0.0031)
	Quadratic	-0.0107***	-0.0090***	-0.0097***	-0.0092***	-0.0095***
		(0.0021)	(0.0017)	(0.0016)	(0.0022)	(0.0026)
Financially-motivated crime						
Welfare receipt	Linear	-0.3503***	-0.3563***	-0.3423***	-0.3407***	-0.3390***
		(0.0179)	(0.0170)	(0.0152)	(0.0149)	(0.0146)
	Quadratic	-0.3500***	-0.3581***	-0.3410***	-0.3371***	-0.3360***
		(0.0177)	(0.0167)	(0.0149)	(0.0143)	(0.0139)
RD	Linear	-0.0096***	-0.0111***	-0.0140***	-0.0168***	-0.0193***
		(0.0022)	(0.0023)	(0.0028)	(0.0029)	(0.0032)
	Quadratic	-0.0102***	-0.0084***	-0.0091***	-0.0087***	-0.0090**
		(0.0022)	(0.0017)	(0.0016)	(0.0022)	(0.0026)

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.
Extended estimation results can be found in Appendix Tables 2.20 to 2.23.

Table 2.9: IV estimates with different bandwidths and functional forms, low-educated men

Bandwidth		10 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime						
Welfare receipt	Linear	-0.3643*	-0.3048**	-0.3229*	-0.3316**	-0.3210**
		(0.1623)	(0.1174)	(0.1258)	(0.1218)	(0.1067)
	Quadratic	-0.3937†	-0.3067*	-0.3610*	-0.3419*	-0.3388**
		(0.2067)	(0.1189)	(0.1633)	(0.1319)	(0.1200)
RD	Linear	-0.0266***	-0.0307***	-0.0322***	-0.0328***	-0.0336***
		(0.0027)	(0.0035)	(0.0040)	(0.0042)	(0.0041)
	Quadratic	-0.0252***	-0.0235***	-0.0290***	-0.0291***	-0.0299***
		(0.0017)	(0.0016)	(0.0026)	(0.0028)	(0.0035)
Financially-motivated crime						
Welfare receipt	Linear	-0.5085***	-0.5167***	-0.5119***	-0.5194***	-0.5132***
		(0.0203)	(0.0283)	(0.0276)	(0.0273)	(0.0266)
	Quadratic	-0.5167***	-0.5278***	-0.5270***	-0.5267***	-0.5240***
		(0.0201)	(0.0221)	(0.0239)	(0.0220)	(0.0218)
RD	Linear	-0.0269***	-0.0306***	-0.0323***	-0.0329***	-0.0336***
		(0.0027)	(0.0035)	(0.0039)	(0.0042)	(0.0041)
	Quadratic	-0.0259***	-0.0242***	-0.0290***	-0.0294***	-0.0301***
		(0.0015)	(0.0017)	(0.0026)	(0.0028)	(0.0035)

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.
Extended estimation results can be found in Appendix Tables 2.24 to 2.27.

Table 2.10: IV estimates with different bandwidths and functional forms, low-educated women

Bandwidth		11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime						
Welfare receipt	Linear	-0.4167***	-0.4171***	-0.4058***	-0.4080***	-0.4053***
		(0.0148)	(0.0144)	(0.0149)	(0.0159)	(0.0167)
	Quadratic	-0.4119***	-0.4144***	-0.4008***	-0.4016***	-0.4012***
		(0.0133)	(0.0136)	(0.0141)	(0.0146)	(0.0146)
RD	Linear	-0.0117***	-0.0127***	-0.0152***	-0.0181***	-0.0205***
		(0.0020)	(0.0023)	(0.0029)	(0.0030)	(0.0033)
	Quadratic	-0.0104***	-0.0103***	-0.0107***	-0.0100***	-0.0105***
		(0.0018)	(0.0013)	(0.0014)	(0.0022)	(0.0026)
Financially-motivated crime						
Welfare receipt	Linear	-0.3407***	-0.3483***	-0.3338***	-0.3347***	-0.3314***
		(0.0190)	(0.0189)	(0.0170)	(0.0171)	(0.0168)
	Quadratic	-0.3407***	-0.3519***	-0.3331***	-0.3302***	-0.3287***
		(0.0185)	(0.0184)	(0.0165)	(0.0158)	(0.0154)
RD	Linear	-0.0112***	-0.0123***	-0.0149***	-0.0178***	-0.0203***
		(0.0020)	(0.0023)	(0.0029)	(0.0031)	(0.0034)
	Quadratic	-0.0100***	-0.0097***	-0.0102***	-0.0095***	-0.0100***
		(0.0019)	(0.0014)	(0.0014)	(0.0022)	(0.0026)

Notes. *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.
Extended estimation results can be found in Appendix Tables 2.28 to 2.31.

2.7 Conclusion

This study examines the causal effect of welfare receipt on crime among young adults. This is empirically challenging due to omitted variables affecting both welfare receipt and crime. Confounding factors, such as self-control, time preferences and risk aversion, lead to positive correlations between welfare receipt and crime. In this study, we control for endogeneity by exploiting an age-based discontinuity in welfare policy in the Netherlands. Upon application for welfare, applicants below the age of 27 are subject to a four-week 'job search period', during which they are not eligible for welfare benefits and therefore without discernible legitimate income. Furthermore, especially the most vulnerable youths are discouraged from applying for welfare benefits by the strict conditionality of the policy, and remain without discernable legitimate income even beyond the four-week job search period (Ministerie van Sociale Zaken en Werkgelegenheid 2015, Van Dodeweerd 2014). The access to a unique individual-level administrative dataset on the entire Dutch population around the age of 27, allows us to exploit this exogenous variation. We estimate an instrumental variable (IV) bivariate probit model with a first-stage regression discontinuity (RD) design. The nature of the welfare-crime relation is investigated by examining both crime in general and financially-motivated crime.

We find that welfare receipt substantially reduces crime, compared to nonreceipt of welfare benefits due to the job search period policy. Welfare receipt reduces the monthly crime rate of men by 0.37 percentage points (from 0.53% to 0.16%). For women, we find a reduction of 0.11 percentage points (from 0.14% to 0.03%). In absolute terms, financial hardship thus has a larger effect on crime among men, which partially explains the absolute gender gap in crime. In relative terms, however, the reduction is quite similar for men (-71%) and women (-77%). Welfare receipt reduces financially-motivated crime by 0.20 percentage points among men (from 0.25% to 0.05%), and 0.05 percentage points among women (from 0.07% to 0.02%). Not only in absolute terms, but also in relative terms, welfare receipt has a larger effect on male financially-motivated crime (-82%) than female financially-motivated crime (-72%). Overall, a basic level of guaranteed income appears to prevent crime. While the estimated relative effect sizes may appear exceptionally large, they are similar to the 60 percent increase in returns to prison found by a related study on a US public assistance eligibility ban for drug felons (Tuttle 2019). All of the results are also robust to changes in functional form and bandwidth size.

The estimation results support both Becker's rational choice theory (1968) as well as Agnew's general strain theory (1992). From a rational choice perspective, we expect welfare receipt to mainly reduce financially-motivated crime by reducing the relative financial gains from such crimes through the provision of legitimate income, whereas other types of crime would be less affected. This holds true for men, for whom we find larger relative effects on financially-motivated crime compared to crime in general. For women, however, the relative effect size is slightly smaller for financially-motivated crime than for crime in general, which is more in line with Agnew's general strain theory. This theory argues that, by alleviating financial stress, welfare receipt reduces emotional strain and consequently criminal behavior in general (Agnew 1992). Reconciling our empirical evidence, we find that the pathway through which welfare receipt reduces crime is different for men and women. For men, welfare receipt appears to mainly reduce crime by addressing financial needs, while for women, a basic level of guaranteed income appears to reduce both financial needs and emotional strain that could otherwise lead to crime.

Women have received little attention in academic research on crime. By distinguishing between men and women in the analysis, the results of this study contribute to the ongoing discussion of whether female and male crime can be accounted for by the same factors and through similar mechanisms. Our findings add to the existing evidence that although most causes of crime are gender invariant, the effect sizes and mechanisms are heterogeneous across gender (see Kruttschnitt 2013, Steffensmeier and Allan 1996).

Finally, our findings suggest that the effect of financial hardship on crime is not heterogeneous across educational levels. The relative effect sizes that we find for low-educated samples are highly comparable to those for the general population. An explanation for the higher crime rates among the low-educated samples may therefore not lie in a lower ability to cope with financial strain, but in lower opportunity costs (Lochner 2004, Lochner and Moretti 2004), and a higher prevalence of financial hardship and other criminogenic factors (e.g. lower self-control, patience and risk aversion, see Becker and Mulligan 1997, Borghans et al. 2008, Pratt and Cullen 2000).

Our identification strategy enables us to assess the causal effects of welfare receipt on crime among a general population of young adults around the age threshold of 27. An inherent limitation of the RD approach is that it produces estimates that only pertain to observations around the age threshold. 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). In order to examine the generalizability of our results to other age groups, further research into the welfare-crime relationship is therefore warranted.

Although a sizeable body of research has assessed the effects of welfare receipt on economic outcomes (such as poverty and unemployment), potential spillover effects on crime are often ignored. Even though political discourse surrounding welfare is often rife with mentions of crime (e.g. Beckett and Sasson 2003), we find micro-level research on the welfare-crime relationship to be comparatively scarce. We do not find the provision of a basic level of guaranteed income to be a cost-effective crime prevention strategy. In addition to the provision of welfare benefits being costly, this can be attributed to the limited reduction in the absolute number of committed offenses. Nevertheless, this study shows that potential effects on crime should be considered in welfare policy formation, as the relative effects of welfare receipt on criminal behavior are substantial. In addition to the direct costs of crime, long-term effects should be taken into account, such as reduced labor market opportunities for the perpetrators and reduced productivity of the victims. In several Western countries the current trend is to reduce welfare accessibility (e.g. Dahlberg et al. 2009, Hernæs et al. 2017). This study is relevant to increase our understanding of the consequences of this trend on crime. In order to gain a comprehensive overview of the societal costs and benefits of welfare, spillover effects on crime should be taken into account.

Linear models 2.A

Table 2.11: Ordinary least squares estimates for crime

	MEN 12 MONTHS	LE MEN 10 MONTHS	WOMEN 11 MONTHS	LE WOMEN 11 MONTHS
Crime				
Welfare receipt	0.0166***	0.0167***	0.0040***	0.0040***
	(0.0004)	(0.0004)	(0.0002)	(0.0002)
Age	-0.0000*	-0.0000***	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	0.0000	0.0000†	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.0031***	-0.0029***	-0.0004***	-0.0002***
	(0.0001)	(0.0001)	(0.0000)	(0.0000)
Time (month)	-0.0000***	-0.0000***	-0.0000***	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0228***	0.0334***	0.0035***	0.0056***
	(0.0017)	(0.0024)	(0.0006)	(0.0009)
Observations	5,910,778	3,846,170	5,411,291	3,541,347
Individuals	314,691	237,519	306,249	202,597
Clusters	24	20	22	22

Notes. Linear model specification, standard errors clustered by age (in months), 'Time (month)' captures the number of calendar months that have passed since January 1960 and has a mean value of 642, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.12: Two-stage least squares estimates for crime

	MEN 12 MONTHS	LE MEN 10 MONTHS	WOMEN 11 MONTHS	LE WOMEN 11 MONTHS
Crime				
Welfare receipt	-0.0373	-0.0158	-0.1300†	-0.1041†
	(0.0509)	(0.0613)	(0.0716)	(0.0621)
Age	-0.0000	-0.0000	0.0001†	0.0001†
	(0.0000)	(0.0000)	(0.0000)	(0.0001)
Age x 1(<27)	0.0000	0.0000	-0.0000*	-0.0001†
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.0058*	-0.0047	-0.0073*	-0.0061†
	(0.0026)	(0.0034)	(0.0037)	(0.0034)
Time (month)	-0.0000†	-0.0000***	0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0208***	0.0328***	0.0062**	0.0133**
	(0.0026)	(0.0025)	(0.0019)	(0.0050)
Observations	5,910,778	3,846,170	5,411,291	3,541,347
Individuals	314,691	237,519	306,249	202,597
Clusters	24	20	22	22

Notes. Linear model specification, standard errors clustered by age (in months), 'Time (month)' captures the number of calendar months that have passed since January 1960 and has a mean value of 642, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.13: Ordinary least squares estimates for financially-motivated crime

	MEN 12 MONTHS	LE MEN 10 MONTHS	WOMEN 11 MONTHS	LE WOMEN 11 MONTHS
Financially-motivated crime				
Welfare receipt	0.0077***	0.0078***	0.0025***	0.0025***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age	-0.0000	-0.0000†	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	0.0000	0.0000	-0.0000*	-0.0000*
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.0013***	-0.0013***	-0.0002***	-0.0002***
	(0.0001)	(0.0001)	(0.0000)	(0.0000)
Time (month)	-0.0000**	-0.0000***	-0.0000†	-0.0000*
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0055***	0.0094***	0.0017*	0.0028**
	(0.0011)	(0.0015)	(0.0007)	(0.0009)
Observations	5,910,778	3,846,170	5,411,291	3,541,347
Individuals	314,691	237,519	306,249	202,597
Clusters	24	20	22	22

Notes. Linear model specification, standard errors clustered by age (in months), 'Time (month)' captures the number of calendar months that have passed since January 1960 and has a mean value of 642, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.14: Two-stage least squares estimates for financially-motivated crime

	MEN 12 MONTHS	LE MEN 10 MONTHS	WOMEN 11 MONTHS	LE WOMEN 11 MONTHS
Financially-motivated crime				
Welfare receipt	-0.0345	-0.0251	-0.0059	-0.0095
	(0.0337)	(0.0426)	(0.0338)	(0.0329)
Age	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0000	0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.0034*	-0.0031	-0.0007	-0.0008
	(0.0017)	(0.0024)	(0.0017)	(0.0018)
Time (month)	0.0000	-0.0000	-0.0000	-0.0000†
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0039*	0.0088***	0.0019*	0.0036
	(0.0017)	(0.0017)	(0.0009)	(0.0025)
Observations	5,910,778	3,846,170	5,411,291	3,541,347
Individuals	314,691	237,519	306,249	202,597
Clusters	24	20	22	22

Notes. Linear model specification, standard errors clustered by age (in months), 'Time (month)' captures the number of calendar months that have passed since January 1960 and has a mean value of 642, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Employment 2.B

Table 2.15: Testing for a discontinuity in employment at the policy threshold

	LINEAR MEN 12 MONTHS	QUADRATIC MEN 12 MONTHS	LINEAR WOMEN 11 MONTHS	QUADRATIC WOMEN 11 MONTHS
Employment				
RD	-0.0018	0.0064***	0.0018†	0.0026*
	(0.0016)	(0.0008)	(0.0011)	(0.0013)
Age	0.0021***	0.0036***	0.0002†	0.0011***
	(0.0002)	(0.0002)	(0.0001)	(0.0002)
Age squared		-0.0001***		-0.0001***
		(0.0000)		(0.0000)
Age x 1(<27)	0.0027***	0.0033***	0.0025***	0.0011*
	(0.0002)	(0.0003)	(0.0002)	(0.0005)
Age x 1(<27) squared		0.0003***		0.0000
		(0.0000)		(0.0000)
Native	0.5914***	0.5914***	0.7615***	0.7615***
	(0.0013)	(0.0013)	(0.0017)	(0.0017)
Time (month)	-0.0023***	-0.0023***	-0.0031***	-0.0031***
	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Probabilities (per month)				
If RD = 1 (%)	73.69	73.82	73.74	73.75
If RD = 0 (%)	73.74	73.61	73.69	73.67
ATE (%point)	-0.06	0.20***	0.06†	0.08*
	(0.05)	(0.03)	(0.03)	(0.04)
Observations	5,910,778	5,910,778	5,411,291	5,411,291
Respondents	314,691	314,691	306,249	306,249
Clusters	24	24	22	22

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10. The ATEs show a significant, but insubstantial discontinuity in the employment rate around the age of 27.

2.C Extended estimation results

Table 2.16: Extended instrumental variable estimation results for the effect of welfare receipt on crime among men

BANDWIDTH	12 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime					
Welfare receipt	-0.4016**	-0.3314**	-0.3364**	-0.3163**	-0.3049**
	(0.1554)	(0.1108)	(0.1122)	(0.1010)	(0.0900)
Age	-0.0010	-0.0012†	-0.0015***	-0.0014***	-0.0016***
	(0.0010)	(0.0006)	(0.0004)	(0.0003)	(0.0003)
Age x 1(<27)	0.0005	0.0004	0.0006	0.0004	0.0005
	(0.0013)	(0.0009)	(0.0005)	(0.0004)	(0.0003)
Native	-0.3434***	-0.3319***	-0.3319***	-0.3286***	-0.3274***
	(0.0259)	(0.0177)	(0.0177)	(0.0160)	(0.0140)
Time (month)	-0.0016***	-0.0017***	-0.0016***	-0.0016***	-0.0016***
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Welfare receipt					
RD	-0.0262***	-0.0279***	-0.0292***	-0.0298***	-0.0310***
	(0.0036)	(0.0037)	(0.0042)	(0.0044)	(0.0042)
Age	0.0042***	0.0037***	0.0030***	0.0026***	0.0024***
	(0.0005)	(0.0004)	(0.0002)	(0.0002)	(0.0002)
Age x 1(<27)	-0.0016**	-0.0010*	0.0005	0.0014***	0.0015***
	(0.0005)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Native	-0.5908***	-0.5923***	-0.5944***	-0.5949***	-0.5949***
	(0.0027)	(0.0029)	(0.0031)	(0.0030)	(0.0031)
Time (month)	0.0026***	0.0025***	0.0024***	0.0023***	0.0022***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5136***	0.4705***	0.4733***	0.4621***	0.4550***
	(0.0907)	(0.0642)	(0.0640)	(0.0574)	(0.0511)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.16	0.19	0.19	0.20	0.21
If welfare receipt = 0 (%)	0.53	0.51	0.52	0.52	0.52
ATE (%point)	-0.37**	-0.32***	-0.33***	-0.32***	-0.31***
	(0.13)	(0.09)	(0.09)	(0.08)	(0.08)
Observations	5,910,778	6,663,749	8,766,366	10,053,511	10,515,037
Respondents	314,691	315,773	319,595	321,335	321,457
Clusters	24	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.17: Extended instrumental variable estimation results for the effect of welfare receipt on crime among men, including quadratic age terms

BANDWIDTH	12 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime					
Welfare receipt	-0.3932**	-0.3358**	-0.3648**	-0.3247**	-0.3169**
	(0.1417)	(0.1143)	(0.1348)	(0.1071)	(0.0968)
Age	-0.0022	-0.0018	-0.0003	-0.0014	-0.0009
	(0.0031)	(0.0023)	(0.0014)	(0.0009)	(0.0008)
Age squared	0.0001	0.0000	-0.0001	-0.0000	-0.0000
	(0.0003)	(0.0002)	(0.0001)	(0.0000)	(0.0000)
Age x 1(<27)	0.0015	0.0018	-0.0005	0.0006	-0.0001
	(0.0050)	(0.0038)	(0.0020)	(0.0015)	(0.0011)
Age x 1(<27) squared	-0.0002	0.0000	0.0001	0.0000	0.0000
	(0.0003)	(0.0002)	(0.0001)	(0.0000)	(0.0000)
Native	-0.3420***	-0.3326***	-0.3365***	-0.3300***	-0.3292***
	(0.0235)	(0.0184)	(0.0219)	(0.0170)	(0.0152)
Time (month)	-0.0016***	-0.0017***	-0.0015***	-0.0016***	-0.0016***
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Welfare receipt					
RD	-0.0201***	-0.0205***	-0.0266***	-0.0263***	-0.0267***
	(0.0013)	(0.0015)	(0.0026)	(0.0029)	(0.0038)
Age	0.0093***	0.0085***	0.0062***	0.0055***	0.0047***
	(0.0003)	(0.0003)	(0.0006)	(0.0004)	(0.0005)
Age squared	-0.0004***	-0.0003***	-0.0002***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0091***	-0.0076***	-0.0053***	-0.0037***	-0.0022***
	(0.0006)	(0.0006)	(0.0007)	(0.0005)	(0.0006)
Age x 1(<27) squared	0.0002***	0.0002***	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.5908***	-0.5923***	-0.5943***	-0.5949***	-0.5949***
	(0.0028)	(0.0029)	(0.0031)	(0.0030)	(0.0031)
Time (month)	0.0026***	0.0025***	0.0024***	0.0023***	0.0022***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5088***	0.4729***	0.4894***	0.4669***	0.4617***
	(0.0827)	(0.0663)	(0.0773)	(0.0610)	(0.0551)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.16	0.19	0.18	0.20	0.20
If welfare receipt = 0 (%)	0.52	0.51	0.53	0.52	0.52
ATE (%point)	-0.36**	-0.33***	-0.35**	-0.32***	-0.32***
	(0.12)	(0.09)	(0.11)	(0.09)	(0.08)
Observations	5,910,778	6,663,749	8,766,366	10,053,511	10,515,037
Respondents	314,691	315,773	319,595	321,335	321,457
Clusters	24	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.18: Extended instrumental variable estimation results for the effect of welfare receipt on financially-motivated crime among men

BANDWIDTH	12 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Financially-motivated crime					
Welfare receipt	-0.5137***	-0.5072***	-0.4980***	-0.5035***	-0.4942***
	(0.0213)	(0.0220)	(0.0222)	(0.0225)	(0.0226)
Age	0.0003	-0.0009	-0.0015**	-0.0014***	-0.0017***
	(0.0010)	(0.0010)	(0.0005)	(0.0004)	(0.0003)
Age x 1(<27)	-0.0005	0.0008	0.0016†	0.0015*	0.0018**
	(0.0017)	(0.0016)	(0.0009)	(0.0006)	(0.0006)
Native	-0.3872***	-0.3874***	-0.3842***	-0.3843***	-0.3821***
	(0.0093)	(0.0086)	(0.0078)	(0.0076)	(0.0074)
Time (month)	-0.0002	-0.0003	-0.0001	-0.0001	-0.0002
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt					
RD	-0.0264***	-0.0278***	-0.0293***	-0.0298***	-0.0310***
	(0.0036)	(0.0037)	(0.0041)	(0.0043)	(0.0042)
Age	0.0042***	0.0037***	0.0030***	0.0026***	0.0024***
	(0.0005)	(0.0004)	(0.0002)	(0.0002)	(0.0002)
Age x 1(<27)	-0.0016**	-0.0009*	0.0005	0.0014***	0.0015***
	(0.0005)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Native	-0.5909***	-0.5924***	-0.5945***	-0.5950***	-0.5950***
	(0.0028)	(0.0029)	(0.0031)	(0.0030)	(0.0031)
Time (month)	0.0026***	0.0025***	0.0024***	0.0023***	0.0022***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5956***	0.5912***	0.5877***	0.5900***	0.5832***
	(0.0121)	(0.0129)	(0.0128)	(0.0127)	(0.0130)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.05	0.05	0.05	0.05	0.05
If welfare receipt = 0 (%)	0.25	0.25	0.25	0.25	0.24
ATE (%point)	-0.20***	-0.20***	-0.20***	-0.20***	-0.19***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	5,910,778	6,663,749	8,766,366	10,053,511	10,515,037
Respondents	314,691	315,773	319,595	321,335	321,457
Clusters	24	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.19: Extended instrumental variable estimation results for the effect of welfare receipt on financially-motivated crime among men, including quadratic age terms

BANDWIDTH	12 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Financially-motivated crime					
Welfare receipt	-0.5057***	-0.5193***	-0.5120***	-0.5099***	-0.5059***
	(0.0158)	(0.0181)	(0.0192)	(0.0181)	(0.0182)
Age	-0.0037	0.0017	0.0014	-0.0002	0.0000
	(0.0051)	(0.0042)	(0.0022)	(0.0014)	(0.0012)
Age squared	0.0003	-0.0002	-0.0002	-0.0000	-0.0001
	(0.0004)	(0.0003)	(0.0001)	(0.0001)	(0.0000)
Age x 1(<27)	0.0056	-0.0017	-0.0025	-0.0003	-0.0006
	(0.0090)	(0.0073)	(0.0035)	(0.0023)	(0.0019)
Age x 1(<27) squared	-0.0002	0.0003	0.0001	0.0000	0.0000
	(0.0004)	(0.0003)	(0.0001)	(0.0001)	(0.0000)
Native	-0.3856***	-0.3898***	-0.3869***	-0.3855***	-0.3844***
	(0.0080)	(0.0079)	(0.0073)	(0.0069)	(0.0067)
Time (month)	-0.0003	-0.0003	-0.0001	-0.0001	-0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt					
RD	-0.0211***	-0.0213***	-0.0267***	-0.0265***	-0.0268***
	(0.0013)	(0.0017)	(0.0026)	(0.0029)	(0.0038)
Age	0.0091***	0.0084***	0.0062***	0.0054***	0.0047***
	(0.0003)	(0.0003)	(0.0005)	(0.0004)	(0.0004)
Age squared	-0.0004***	-0.0003***	-0.0002***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0091***	-0.0076***	-0.0052***	-0.0037***	-0.0021***
	(0.0006)	(0.0006)	(0.0007)	(0.0005)	(0.0006)
Age x 1(<27) squared	0.0002***	0.0002***	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.5910***	-0.5924***	-0.5945***	-0.5950***	-0.5950***
	(0.0028)	(0.0029)	(0.0031)	(0.0030)	(0.0031)
Time (month)	0.0026***	0.0025***	0.0024***	0.0023***	0.0022***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5910***	0.5982***	0.5959***	0.5937***	0.5901***
	(0.0085)	(0.0095)	(0.0100)	(0.0099)	(0.0099)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.05	0.05	0.05	0.05	0.05
If welfare receipt = 0 (%)	0.25	0.25	0.25	0.25	0.25
ATE (%point)	-0.20***	-0.21***	-0.20***	-0.20***	-0.20***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	5,910,778	6,663,749	8,766,366	10,053,511	10,515,037
Respondents	314,691	315,773	319,595	321,335	321,457
Clusters	24	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.20: Extended instrumental variable estimation results for the effect of welfare receipt on crime among women

BANDWIDTH	11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime					
Welfare receipt	-0.4230***	-0.4235***	-0.4124***	-0.4123***	-0.4126***
	(0.0134)	(0.0125)	(0.0130)	(0.0133)	(0.0137)
Age	0.0009	0.0005	-0.0008	-0.0014**	-0.0015**
	(0.0017)	(0.0012)	(0.0007)	(0.0005)	(0.0005)
Age x 1(<27)	-0.0038	-0.0028	0.0003	0.0017*	0.0019*
	(0.0031)	(0.0019)	(0.0011)	(0.0008)	(0.0008)
Native	-0.2483***	-0.2553***	-0.2491***	-0.2504***	-0.2496***
	(0.0072)	(0.0072)	(0.0066)	(0.0065)	(0.0065)
Time (month)	-0.0010**	-0.0010**	-0.0009**	-0.0010**	-0.0010**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt					
RD	-0.0101***	-0.0115***	-0.0143***	-0.0171***	-0.0195***
	(0.0021)	(0.0023)	(0.0027)	(0.0029)	(0.0031)
Age	0.0056***	0.0052***	0.0044***	0.0040***	0.0038***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age x 1(<27)	-0.0024***	-0.0019***	-0.0008**	-0.0002	-0.0000
	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Native	-0.5160***	-0.5155***	-0.5181***	-0.5207***	-0.5218***
	(0.0045)	(0.0046)	(0.0047)	(0.0048)	(0.0047)
Time (month)	0.0012***	0.0011***	0.0012***	0.0011***	0.0011***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5351***	0.5361***	0.5317***	0.5314***	0.5301***
	(0.0043)	(0.0043)	(0.0050)	(0.0056)	(0.0059)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.03	0.03	0.03	0.03	0.03
If welfare receipt = 0 (%)	0.14	0.14	0.14	0.14	0.14
ATE (%point)	-0.11***	-0.11***	-0.11***	-0.11***	-0.11***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	5,411,291	6,546,177	8,611,854	9,871,600	10,323,410
Respondents	306,249	308,298	311,818	313,550	313,708
Clusters	22	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.21: Extended instrumental variable estimation results for the effect of welfare receipt on crime among women, including quadratic age terms

BANDWIDTH	11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime					
Welfare receipt	-0.4200***	-0.4205***	-0.4086***	-0.4069***	-0.4079***
	(0.0127)	(0.0124)	(0.0126)	(0.0126)	(0.0126)
Age	0.0039	0.0033	0.0035	0.0019	0.0012
	(0.0078)	(0.0055)	(0.0028)	(0.0018)	(0.0015)
Age squared	-0.0001	-0.0002	-0.0002	-0.0001†	-0.0001†
	(0.0006)	(0.0004)	(0.0001)	(0.0001)	(0.0001)
Age x 1(<27)	-0.0167	-0.0114	-0.0104*	-0.0062*	-0.0039
	(0.0146)	(0.0096)	(0.0045)	(0.0031)	(0.0026)
Age x 1(<27) squared	-0.0008	-0.0003	-0.0001	-0.0001	-0.0000
	(0.0005)	(0.0003)	(0.0001)	(0.0001)	(0.0001)
Native	-0.2477***	-0.2547***	-0.2484***	-0.2494***	-0.2487***
	(0.0070)	(0.0070)	(0.0065)	(0.0063)	(0.0062)
Time (month)	-0.0010**	-0.0010**	-0.0009**	-0.0010**	-0.0010**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt					
RD	-0.0107***	-0.0090***	-0.0097***	-0.0092***	-0.0095***
	(0.0021)	(0.0017)	(0.0016)	(0.0022)	(0.0026)
Age	0.0069***	0.0073***	0.0070***	0.0064***	0.0061***
	(0.0009)	(0.0005)	(0.0003)	(0.0003)	(0.0004)
Age squared	-0.0001	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0054***	-0.0051***	-0.0047***	-0.0033***	-0.0027***
	(0.0011)	(0.0006)	(0.0004)	(0.0004)	(0.0005)
Age x 1(<27) squared	-0.0000	0.0001	0.0001**	0.0001***	0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.5160***	-0.5155***	-0.5181***	-0.5207***	-0.5218***
	(0.0045)	(0.0046)	(0.0047)	(0.0048)	(0.0047)
Time (month)	0.0012***	0.0011***	0.0012***	0.0011***	0.0011***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5333***	0.5343***	0.5295***	0.5282***	0.5273***
	(0.0040)	(0.0040)	(0.0046)	(0.0049)	(0.0050)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.03	0.03	0.03	0.03	0.03
If welfare receipt = 0 (%)	0.14	0.14	0.14	0.14	0.14
ATE (%point)	-0.11***	-0.11***	-0.11***	-0.10***	-0.10***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	5,411,291	6,546,177	8,611,854	9,871,600	10,323,410
Respondents	306,249	308,298	311,818	313,550	313,708
Clusters	22	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.22: Extended instrumental variable estimation results for the effect of welfare receipt on financially-motivated crime among women

BANDWIDTH	11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Financially-motivated crime					
Welfare receipt	-0.3503***	-0.3563***	-0.3423***	-0.3407***	-0.3390***
	(0.0179)	(0.0170)	(0.0152)	(0.0149)	(0.0146)
Age	0.0025†	0.0007	-0.0014	-0.0018**	-0.0018**
	(0.0015)	(0.0013)	(0.0009)	(0.0007)	(0.0006)
Age x 1(<27)	-0.0070**	-0.0037*	0.0006	0.0019†	0.0020*
	(0.0026)	(0.0018)	(0.0014)	(0.0011)	(0.0009)
Native	-0.2650***	-0.2638***	-0.2582***	-0.2602***	-0.2601***
	(0.0103)	(0.0090)	(0.0081)	(0.0079)	(0.0077)
Time (month)	-0.0008	-0.0008	-0.0007	-0.0008	-0.0008
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Welfare receipt					
RD	-0.0096***	-0.0111***	-0.0140***	-0.0168***	-0.0193***
	(0.0022)	(0.0023)	(0.0028)	(0.0029)	(0.0032)
Age	0.0057***	0.0052***	0.0044***	0.0040***	0.0038***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age x 1(<27)	-0.0024***	-0.0019***	-0.0008**	-0.0002	-0.0000
	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Native	-0.5159***	-0.5154***	-0.5180***	-0.5206***	-0.5217***
	(0.0045)	(0.0046)	(0.0047)	(0.0048)	(0.0047)
Time (month)	0.0012***	0.0011***	0.0012***	0.0011***	0.0011***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5070***	0.5080***	0.5033***	0.5033***	0.5014***
	(0.0048)	(0.0051)	(0.0055)	(0.0060)	(0.0060)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.02	0.02	0.02	0.02	0.02
If welfare receipt = 0 (%)	0.07	0.07	0.07	0.07	0.07
ATE (%point)	-0.05***	-0.05***	-0.05***	-0.05***	-0.05***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	5,411,291	6,546,177	8,611,854	9,871,600	10,323,410
Respondents	306,249	308,298	311,818	313,550	313,708
Clusters	22	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.23: Extended instrumental variable estimation results for the effect of welfare receipt on financially-motivated crime among women, including quadratic age terms

BANDWIDTH	11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Financially-motivated crime					
Welfare receipt	-0.3500***	-0.3581***	-0.3410***	-0.3371***	-0.3360***
	(0.0177)	(0.0167)	(0.0149)	(0.0143)	(0.0139)
Age	0.0122†	0.0109*	0.0049†	0.0020	0.0008
	(0.0062)	(0.0043)	(0.0026)	(0.0018)	(0.0018)
Age squared	-0.0008	-0.0007*	-0.0003*	-0.0001*	-0.0001
	(0.0005)	(0.0003)	(0.0001)	(0.0001)	(0.0001)
Age x 1(<27)	-0.0270†	-0.0222*	-0.0130**	-0.0077*	-0.0043
	(0.0144)	(0.0090)	(0.0043)	(0.0030)	(0.0028)
Age x 1(<27) squared	-0.0001	0.0002	-0.0001	-0.0001	-0.0001
	(0.0007)	(0.0004)	(0.0002)	(0.0001)	(0.0001)
Native	-0.2650***	-0.2641***	-0.2580***	-0.2595***	-0.2596***
	(0.0101)	(0.0089)	(0.0080)	(0.0077)	(0.0076)
Time (month)	-0.0008	-0.0008	-0.0007	-0.0008	-0.0008
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Welfare receipt					
RD	-0.0102***	-0.0084***	-0.0091***	-0.0087***	-0.0090**
	(0.0022)	(0.0017)	(0.0016)	(0.0022)	(0.0026)
Age	0.0070***	0.0073***	0.0070***	0.0065***	0.0062***
	(0.0009)	(0.0005)	(0.0003)	(0.0003)	(0.0004)
Age squared	-0.0001	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0054***	-0.0050***	-0.0047***	-0.0033***	-0.0027***
	(0.0011)	(0.0006)	(0.0004)	(0.0004)	(0.0005)
Age x 1(<27) squared	-0.0000	0.0001†	0.0001**	0.0001***	0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.5159***	-0.5154***	-0.5180***	-0.5206***	-0.5217***
	(0.0045)	(0.0046)	(0.0047)	(0.0048)	(0.0047)
Time (month)	0.0012***	0.0011***	0.0012***	0.0011***	0.0011***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5068***	0.5090***	0.5025***	0.5012***	0.4996***
	(0.0042)	(0.0045)	(0.0048)	(0.0051)	(0.0050)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.02	0.02	0.02	0.02	0.02
If welfare receipt = 0 (%)	0.07	0.07	0.07	0.07	0.07
ATE (%point)	-0.05***	-0.05***	-0.05***	-0.05***	-0.05***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	5,411,291	6,546,177	8,611,854	9,871,600	10,323,410
Respondents	306,249	308,298	311,818	313,550	313,708
Clusters	22	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.24: Extended instrumental variable estimation results for the effect of welfare receipt on crime among low-educated men

BANDWIDTH	10 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime					
Welfare receipt	-0.3643*	-0.3048**	-0.3229*	-0.3316**	-0.3210**
	(0.1623)	(0.1174)	(0.1258)	(0.1218)	(0.1067)
Age	-0.0015	-0.0007	-0.0011*	-0.0009*	-0.0010**
	(0.0011)	(0.0007)	(0.0004)	(0.0004)	(0.0003)
Age x 1(<27)	0.0020	0.0006	0.0007	0.0006	0.0007†
	(0.0016)	(0.0010)	(0.0005)	(0.0005)	(0.0004)
Native	-0.2864***	-0.2804***	-0.2822***	-0.2834***	-0.2821***
	(0.0259)	(0.0178)	(0.0192)	(0.0188)	(0.0163)
Time (month)	-0.0023***	-0.0024***	-0.0023***	-0.0023***	-0.0023***
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Welfare receipt					
RD	-0.0266***	-0.0307***	-0.0322***	-0.0328***	-0.0336***
	(0.0027)	(0.0035)	(0.0040)	(0.0042)	(0.0041)
Age	0.0059***	0.0046***	0.0039***	0.0035***	0.0033***
	(0.0004)	(0.0004)	(0.0002)	(0.0002)	(0.0002)
Age x 1(<27)	-0.0028***	-0.0011**	0.0003	0.0011***	0.0014***
	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0002)
Native	-0.5524***	-0.5562***	-0.5578***	-0.5579***	-0.5575***
	(0.0018)	(0.0022)	(0.0022)	(0.0021)	(0.0022)
Time (month)	0.0020***	0.0019***	0.0018***	0.0016***	0.0016***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.4758***	0.4379***	0.4483***	0.4535***	0.4467***
	(0.0947)	(0.0683)	(0.0722)	(0.0700)	(0.0612)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.22	0.26	0.26	0.25	0.26
If welfare receipt = 0 (%)	0.65	0.64	0.66	0.66	0.66
ATE (%point)	-0.43*	-0.38**	-0.40**	-0.41**	-0.40***
	(0.17)	(0.12)	(0.13)	(0.13)	(0.11)
Observations	3,846,170	5,032,325	6,620,974	7,594,865	7,945,022
Respondents	237,519	240,308	243,628	245,131	245,215
Clusters	20	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.25: Extended instrumental variable estimation results for the effect of welfare receipt on crime among low-educated men, including quadratic age terms

BANDWIDTH	10 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime					
Welfare receipt	-0.3937†	-0.3067*	-0.3610*	-0.3419*	-0.3388**
	(0.2067)	(0.1189)	(0.1633)	(0.1319)	(0.1200)
Age	0.0027	-0.0015	0.0003	-0.0009	-0.0004
	(0.0043)	(0.0024)	(0.0015)	(0.0011)	(0.0008)
Age squared	-0.0004	0.0001	-0.0001	-0.0000	-0.0000
	(0.0004)	(0.0002)	(0.0001)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0039	0.0022	-0.0003	0.0007	0.0001
	(0.0075)	(0.0042)	(0.0022)	(0.0016)	(0.0013)
Age x 1(<27) squared	0.0003	0.0000	0.0001	0.0000	0.0000
	(0.0004)	(0.0002)	(0.0001)	(0.0000)	(0.0000)
Native	-0.2912***	-0.2806***	-0.2882***	-0.2850***	-0.2849***
	(0.0337)	(0.0181)	(0.0258)	(0.0206)	(0.0185)
Time (month)	-0.0023***	-0.0024***	-0.0023***	-0.0023***	-0.0023***
	(0.0004)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Welfare receipt					
RD	-0.0252***	-0.0235***	-0.0290***	-0.0291***	-0.0299***
	(0.0017)	(0.0016)	(0.0026)	(0.0028)	(0.0035)
Age	0.0093***	0.0091***	0.0071***	0.0064***	0.0056***
	(0.0005)	(0.0004)	(0.0006)	(0.0004)	(0.0004)
Age squared	-0.0003***	-0.0003***	-0.0002***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0089***	-0.0073***	-0.0052***	-0.0038***	-0.0025***
	(0.0009)	(0.0006)	(0.0007)	(0.0005)	(0.0005)
Age x 1(<27) squared	0.0001	0.0002***	0.0000	0.0000	0.0000
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.5524***	-0.5562***	-0.5578***	-0.5579***	-0.5575***
	(0.0018)	(0.0022)	(0.0022)	(0.0021)	(0.0022)
Time (month)	0.0020***	0.0019***	0.0018***	0.0016***	0.0016***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.4927***	0.4390***	0.4701***	0.4593***	0.4568***
	(0.1210)	(0.0693)	(0.0943)	(0.0759)	(0.0691)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.21	0.26	0.23	0.25	0.25
If welfare receipt = 0 (%)	0.67	0.64	0.67	0.67	0.67
ATE (%point)	-0.46*	-0.38**	-0.44**	-0.42**	-0.42**
	(0.21)	(0.12)	(0.17)	(0.14)	(0.12)
Observations	3,846,170	5,032,325	6,620,974	7,594,865	7,945,022
Respondents	237,519	240,308	243,628	245,131	245,215
Clusters	20	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.26: Extended instrumental variable estimation results for the effect of welfare receipt on financially-motivated crime among low-educated men

BANDWIDTH	10 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Financially-motivated crime					
Welfare receipt	-0.5085***	-0.5167***	-0.5119***	-0.5194***	-0.5132***
	(0.0203)	(0.0283)	(0.0276)	(0.0273)	(0.0266)
Age	-0.0003	-0.0002	-0.0008	-0.0008†	-0.0010**
	(0.0013)	(0.0011)	(0.0006)	(0.0004)	(0.0004)
Age x 1(<27)	0.0009	0.0006	0.0015	0.0016*	0.0019**
	(0.0025)	(0.0017)	(0.0009)	(0.0007)	(0.0006)
Native	-0.3369***	-0.3416***	-0.3389***	-0.3393***	-0.3377***
	(0.0100)	(0.0096)	(0.0088)	(0.0086)	(0.0083)
Time (month)	-0.0011**	-0.0010**	-0.0008**	-0.0008**	-0.0009**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt					
RD	-0.0269***	-0.0306***	-0.0323***	-0.0329***	-0.0336***
	(0.0027)	(0.0035)	(0.0039)	(0.0042)	(0.0041)
Age	0.0059***	0.0046***	0.0039***	0.0035***	0.0033***
	(0.0004)	(0.0004)	(0.0002)	(0.0002)	(0.0002)
Age x 1(<27)	-0.0027***	-0.0011*	0.0003	0.0012***	0.0014***
	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0002)
Native	-0.5526***	-0.5564***	-0.5579***	-0.5580***	-0.5576***
	(0.0018)	(0.0022)	(0.0022)	(0.0020)	(0.0022)
Time (month)	0.0020***	0.0019***	0.0018***	0.0016***	0.0016***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5757***	0.5799***	0.5790***	0.5826***	0.5776***
	(0.0120)	(0.0168)	(0.0161)	(0.0157)	(0.0154)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.06	0.06	0.06	0.06	0.06
If welfare receipt = 0 (%)	0.32	0.32	0.32	0.32	0.32
ATE (%point)	-0.26***	-0.26***	-0.26***	-0.26***	-0.26***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	3,846,170	5,032,325	6,620,974	7,594,865	7,945,022
Respondents	237,519	240,308	243,628	245,131	245,215
Clusters	20	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.27: Extended instrumental variable estimation results for the effect of welfare receipt on financially-motivated crime among low-educated men, including quadratic age terms

BANDWIDTH	10 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Financially-motivated crime					
Welfare receipt	-0.5167***	-0.5278***	-0.5270***	-0.5267***	-0.5240***
	(0.0201)	(0.0221)	(0.0239)	(0.0220)	(0.0218)
Age	0.0002	0.0019	0.0021	0.0008	0.0009
	(0.0074)	(0.0043)	(0.0023)	(0.0015)	(0.0013)
Age squared	-0.0001	-0.0002	-0.0002	-0.0001	-0.0001†
	(0.0007)	(0.0003)	(0.0001)	(0.0001)	(0.0000)
Age x 1(<27)	0.0022	-0.0013	-0.0026	-0.0010	-0.0011
	(0.0141)	(0.0077)	(0.0038)	(0.0025)	(0.0021)
Age x 1(<27) squared	0.0003	0.0002	0.0001	0.0000	0.0000
	(0.0006)	(0.0003)	(0.0001)	(0.0001)	(0.0000)
Native	-0.3385***	-0.3438***	-0.3418***	-0.3407***	-0.3398***
	(0.0095)	(0.0086)	(0.0082)	(0.0077)	(0.0075)
Time (month)	-0.0011**	-0.0010**	-0.0008**	-0.0008**	-0.0008**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt					
RD	-0.0259***	-0.0242***	-0.0290***	-0.0294***	-0.0301***
	(0.0015)	(0.0017)	(0.0026)	(0.0028)	(0.0035)
Age	0.0093***	0.0091***	0.0070***	0.0063***	0.0056***
	(0.0005)	(0.0004)	(0.0005)	(0.0004)	(0.0004)
Age squared	-0.0003***	-0.0003***	-0.0002***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0090***	-0.0074***	-0.0051***	-0.0038***	-0.0024***
	(0.0009)	(0.0006)	(0.0006)	(0.0005)	(0.0005)
Age x 1(<27) squared	0.0001	0.0002***	0.0000	0.0000	0.0000
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.5526***	-0.5564***	-0.5579***	-0.5580***	-0.5576***
	(0.0018)	(0.0022)	(0.0022)	(0.0020)	(0.0022)
Time (month)	0.0020***	0.0019***	0.0018***	0.0016***	0.0016***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5805***	0.5864***	0.5879***	0.5868***	0.5839***
	(0.0112)	(0.0121)	(0.0129)	(0.0122)	(0.0121)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.06	0.06	0.06	0.06	0.06
If welfare receipt = 0 (%)	0.32	0.33	0.32	0.32	0.32
ATE (%point)	-0.26***	-0.27***	-0.27***	-0.27***	-0.26***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	3,846,170	5,032,325	6,620,974	7,594,865	7,945,022
Respondents	237,519	240,308	243,628	245,131	245,215
Clusters	20	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.28: Extended instrumental variable estimation results for the effect of welfare receipt on crime among low-educated women

BANDWIDTH	11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime					
Welfare receipt	-0.4167***	-0.4171***	-0.4058***	-0.4080***	-0.4053***
	(0.0148)	(0.0144)	(0.0149)	(0.0159)	(0.0167)
Age	0.0014	0.0009	-0.0004	-0.0010†	-0.0013*
	(0.0018)	(0.0013)	(0.0007)	(0.0006)	(0.0005)
Age x 1(<27)	-0.0037	-0.0027	0.0004	0.0018*	0.0021**
	(0.0030)	(0.0020)	(0.0011)	(0.0009)	(0.0008)
Native	-0.1610***	-0.1690***	-0.1631***	-0.1651***	-0.1641***
	(0.0083)	(0.0081)	(0.0075)	(0.0073)	(0.0072)
Time (month)	-0.0015***	-0.0015***	-0.0014***	-0.0015***	-0.0015***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt					
RD	-0.0117***	-0.0127***	-0.0152***	-0.0181***	-0.0205***
	(0.0020)	(0.0023)	(0.0029)	(0.0030)	(0.0033)
Age	0.0068***	0.0063***	0.0055***	0.0051***	0.0048***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age x 1(<27)	-0.0028***	-0.0021***	-0.0009***	-0.0004†	-0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Native	-0.4304***	-0.4298***	-0.4320***	-0.4344***	-0.4355***
	(0.0034)	(0.0036)	(0.0036)	(0.0036)	(0.0036)
Time (month)	0.0004***	0.0004***	0.0004***	0.0004***	0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5059***	0.5071***	0.5024***	0.5034***	0.5005***
	(0.0054)	(0.0057)	(0.0065)	(0.0075)	(0.0081)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.05	0.05	0.05	0.05	0.05
If welfare receipt = 0 (%)	0.20	0.20	0.20	0.20	0.20
ATE (%point)	-0.15***	-0.15***	-0.15***	-0.15***	-0.15***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	3,541,347	4,283,703	5,636,719	6,464,560	6,762,853
Respondents	202,597	204,278	207,150	208,531	208,627
Clusters	22	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.29: Extended instrumental variable estimation results for the effect of welfare receipt on crime among low-educated women, including quadratic age terms

BANDWIDTH	11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Crime					
Welfare receipt	-0.4119***	-0.4144***	-0.4008***	-0.4016***	-0.4012***
	(0.0133)	(0.0136)	(0.0141)	(0.0146)	(0.0146)
Age	0.0063	0.0052	0.0042	0.0024	0.0021
	(0.0081)	(0.0057)	(0.0029)	(0.0020)	(0.0016)
Age squared	-0.0003	-0.0003	-0.0002	-0.0001†	-0.0001*
	(0.0006)	(0.0004)	(0.0001)	(0.0001)	(0.0001)
Age x 1(<27)	-0.0192	-0.0127	-0.0107*	-0.0061†	-0.0043
	(0.0150)	(0.0097)	(0.0046)	(0.0033)	(0.0026)
Age x 1(<27) squared	-0.0007	-0.0001	-0.0001	-0.0001	0.0000
	(0.0006)	(0.0004)	(0.0001)	(0.0001)	(0.0001)
Native	-0.1601***	-0.1685***	-0.1623***	-0.1641***	-0.1635***
	(0.0080)	(0.0078)	(0.0073)	(0.0070)	(0.0068)
Time (month)	-0.0015***	-0.0015***	-0.0014***	-0.0015***	-0.0015***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Welfare receipt					
RD	-0.0104***	-0.0103***	-0.0107***	-0.0100***	-0.0105***
	(0.0018)	(0.0013)	(0.0014)	(0.0022)	(0.0026)
Age	0.0084***	0.0086***	0.0083***	0.0076***	0.0073***
	(0.0008)	(0.0004)	(0.0002)	(0.0003)	(0.0004)
Age squared	-0.0001†	-0.0002***	-0.0001***	-0.0001***	-0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0056***	-0.0057***	-0.0052***	-0.0037***	-0.0032***
	(0.0010)	(0.0005)	(0.0003)	(0.0004)	(0.0005)
Age x 1(<27) squared	0.0000	0.0001†	0.0001**	0.0001***	0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.4304***	-0.4298***	-0.4320***	-0.4344***	-0.4356***
	(0.0034)	(0.0036)	(0.0036)	(0.0036)	(0.0036)
Time (month)	0.0004***	0.0004***	0.0004***	0.0004***	0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.5030***	0.5056***	0.4994***	0.4996***	0.4981***
	(0.0045)	(0.0048)	(0.0057)	(0.0063)	(0.0065)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.05	0.05	0.05	0.05	0.05
If welfare receipt = 0 (%)	0.20	0.20	0.20	0.20	0.20
ATE (%point)	-0.15***	-0.15***	-0.15***	-0.15***	-0.14***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	3,541,347	4,283,703	5,636,719	6,464,560	6,762,853
Respondents	202,597	204,278	207,150	208,531	208,627
Clusters	22	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.30: Extended instrumental variable estimation results for the effect of welfare receipt on financially-motivated crime among low-educated women

BANDWIDTH	11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Financially-motivated crime					
Welfare receipt	-0.3407***	-0.3483***	-0.3338***	-0.3347***	-0.3314***
	(0.0190)	(0.0189)	(0.0170)	(0.0171)	(0.0168)
Age	0.0030†	0.0010	-0.0009	-0.0014*	-0.0016*
	(0.0017)	(0.0014)	(0.0009)	(0.0007)	(0.0007)
Age x 1(<27)	-0.0067*	-0.0036†	0.0003	0.0020†	0.0022*
	(0.0028)	(0.0020)	(0.0015)	(0.0011)	(0.0010)
Native	-0.1823***	-0.1836***	-0.1777***	-0.1805***	-0.1802***
	(0.0107)	(0.0094)	(0.0087)	(0.0082)	(0.0080)
Time (month)	-0.0013*	-0.0013*	-0.0012*	-0.0013*	-0.0013*
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Welfare receipt					
RD	-0.0112***	-0.0123***	-0.0149***	-0.0178***	-0.0203***
	(0.0020)	(0.0023)	(0.0029)	(0.0031)	(0.0034)
Age	0.0068***	0.0064***	0.0055***	0.0051***	0.0048***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age x 1(<27)	-0.0028***	-0.0021***	-0.0009**	-0.0004†	-0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Native	-0.4304***	-0.4298***	-0.4319***	-0.4343***	-0.4355***
	(0.0034)	(0.0036)	(0.0036)	(0.0036)	(0.0036)
Time (month)	0.0004***	0.0004***	0.0004***	0.0004***	0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.4761***	0.4781***	0.4724***	0.4738***	0.4711***
	(0.0055)	(0.0064)	(0.0069)	(0.0075)	(0.0076)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.03	0.03	0.03	0.03	0.03
If welfare receipt = 0 (%)	0.10	0.10	0.10	0.10	0.10
ATE (%point)	-0.07***	-0.07***	-0.07***	-0.07***	-0.07***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	3,541,347	4,283,703	5,636,719	6,464,560	6,762,853
Respondents	202,597	204,278	207,150	208,531	208,627
Clusters	22	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 2.31: Extended instrumental variable estimation results for the effect of welfare receipt on financially-motivated crime among low-educated women, including quadratic age terms

BANDWIDTH	11 MONTHS	14 MONTHS	21 MONTHS	28 MONTHS	35 MONTHS
Financially-motivated crime					
Welfare receipt	-0.3407***	-0.3519***	-0.3331***	-0.3302***	-0.3287***
	(0.0185)	(0.0184)	(0.0165)	(0.0158)	(0.0154)
Age	0.0146*	0.0125**	0.0054†	0.0028	0.0017
	(0.0068)	(0.0046)	(0.0028)	(0.0020)	(0.0019)
Age squared	-0.0010	-0.0008*	-0.0003*	-0.0002*	-0.0001
	(0.0006)	(0.0003)	(0.0001)	(0.0001)	(0.0001)
Age x 1(<27)	-0.0304*	-0.0233*	-0.0125**	-0.0082*	-0.0049
	(0.0147)	(0.0092)	(0.0046)	(0.0033)	(0.0030)
Age x 1(<27) squared	-0.0001	0.0003	-0.0000	-0.0001	-0.0000
	(0.0007)	(0.0004)	(0.0002)	(0.0001)	(0.0001)
Native	-0.1823***	-0.1842***	-0.1776***	-0.1799***	-0.1798***
	(0.0105)	(0.0093)	(0.0085)	(0.0080)	(0.0078)
Time (month)	-0.0013*	-0.0013*	-0.0012*	-0.0013*	-0.0013*
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Welfare receipt					
RD	-0.0100***	-0.0097***	-0.0102***	-0.0095***	-0.0100***
	(0.0019)	(0.0014)	(0.0014)	(0.0022)	(0.0026)
Age	0.0085***	0.0086***	0.0083***	0.0077***	0.0074***
	(0.0008)	(0.0004)	(0.0002)	(0.0003)	(0.0004)
Age squared	-0.0001*	-0.0002***	-0.0001***	-0.0001***	-0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Age x 1(<27)	-0.0056***	-0.0057***	-0.0052***	-0.0037***	-0.0032***
	(0.0010)	(0.0005)	(0.0003)	(0.0004)	(0.0005)
Age x 1(<27) squared	0.0000	0.0001*	0.0001***	0.0001***	0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native	-0.4304***	-0.4298***	-0.4319***	-0.4343***	-0.4355***
	(0.0034)	(0.0036)	(0.0036)	(0.0036)	(0.0036)
Time (month)	0.0004***	0.0004***	0.0004***	0.0004***	0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ρ	0.4761***	0.4801***	0.4719***	0.4712***	0.4695***
	(0.0045)	(0.0055)	(0.0059)	(0.0061)	(0.0061)
Probabilities (per month)					
If welfare receipt = 1 (%)	0.03	0.03	0.03	0.03	0.03
If welfare receipt = 0 (%)	0.10	0.10	0.10	0.10	0.10
ATE (%point)	-0.07***	-0.07***	-0.07***	-0.07***	-0.07***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	3,541,347	4,283,703	5,636,719	6,464,560	6,762,853
Respondents	202,597	204,278	207,150	208,531	208,627
Clusters	22	28	42	56	70

Notes. Standard errors clustered by age (in months), *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

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.

3.1 Introduction

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. Re-integration 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.

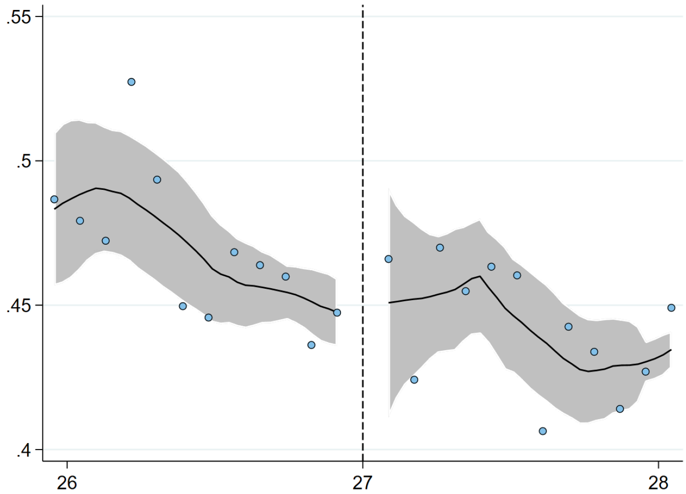
We assume that a jump in the crime rate around the age of 27 is due to a difference in the WLO participation rate around this threshold. To check for this, Figures 3.2a to 3.2c present the evolution of the WLO participation rates across age. Compared to the crime rates, the standard errors are substantially 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).

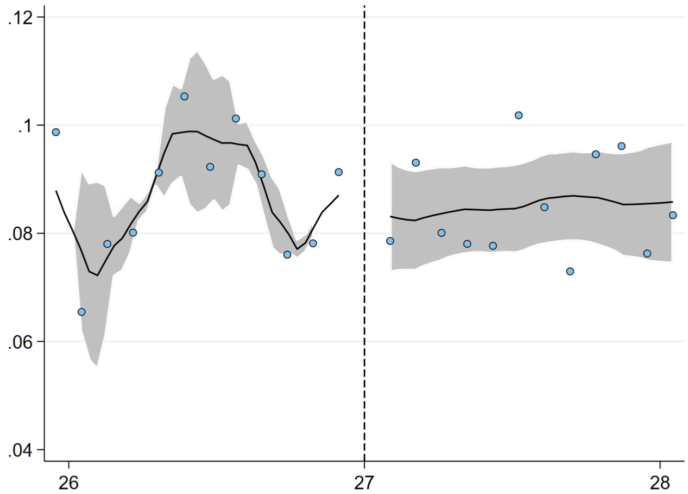
To further explore whether the WLO policy affects crime through incapacitation, we separately graph the evolution of weekday and weekend crime rates. Figures 3.4a to 3.5c present the weekday and weekend crime rates across age. In line with Figures 3.1a to 3.1c, we do not find any distinguishable discontinuity 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)

(a) Crime rate men



(b) Crime rate women



(c) Crime rate non-natives

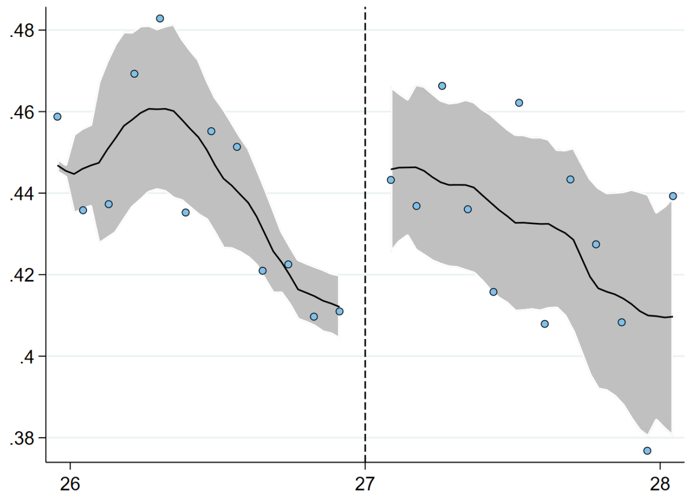
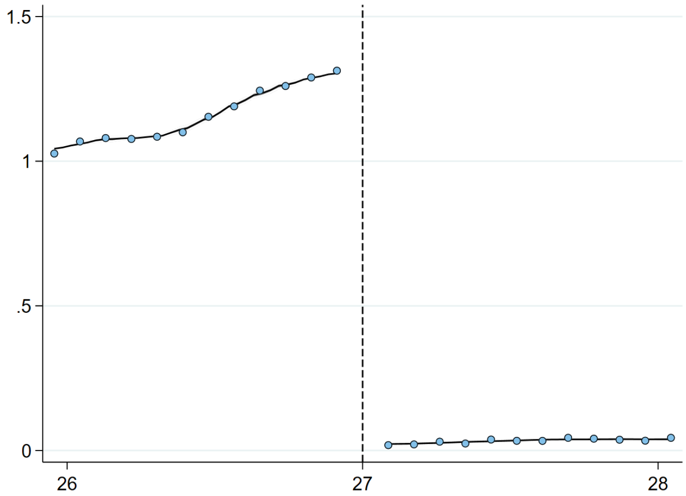
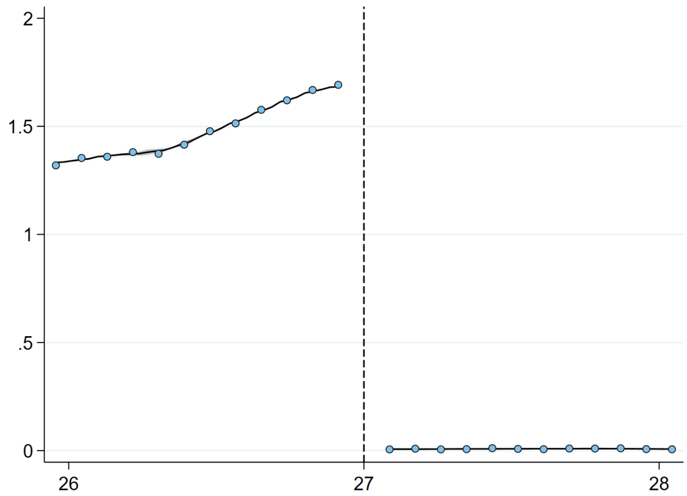


Figure 3.2: Work-learn offer rates across age among men (a), women (b) and non-natives (c)

(a) Work-learn offer rate men



(b) Work-learn offer rate women



(c) Work-learn offer rate non-natives

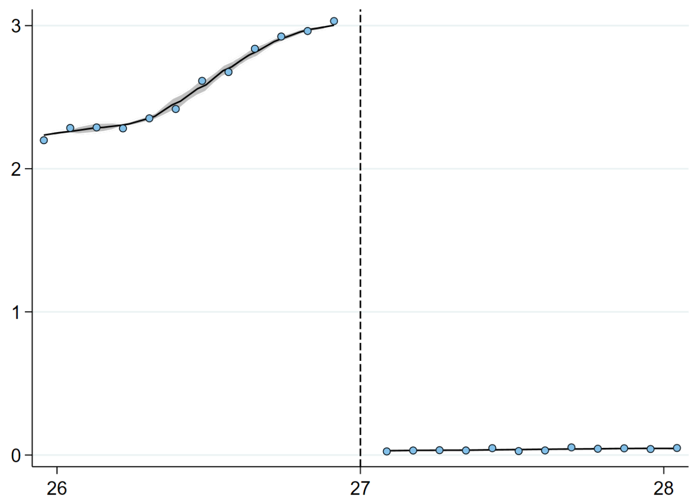
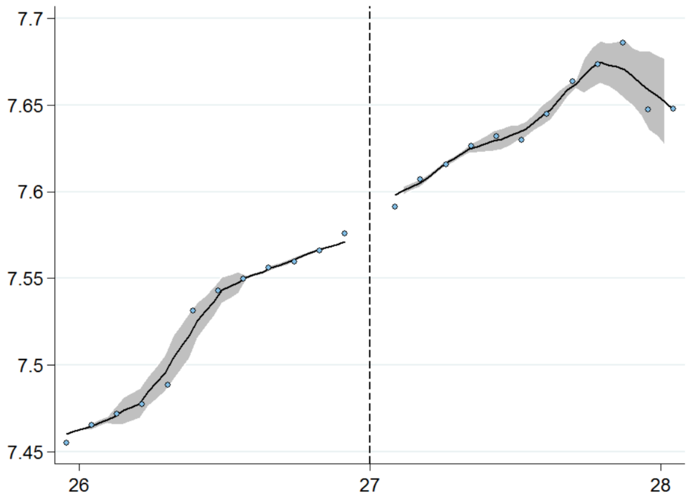
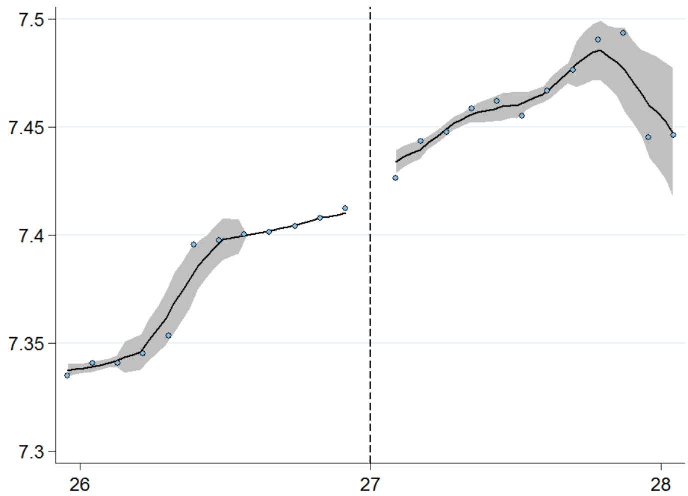


Figure 3.3: Log-transformed income across age among men (a), women (b) and non-natives (c)

(a) Log-transformed income men



(b) Log-transformed income women



(c) Log-transformed income non-natives

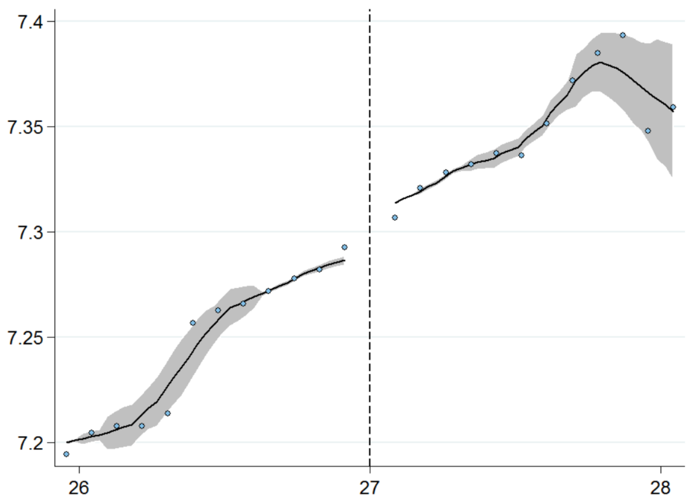
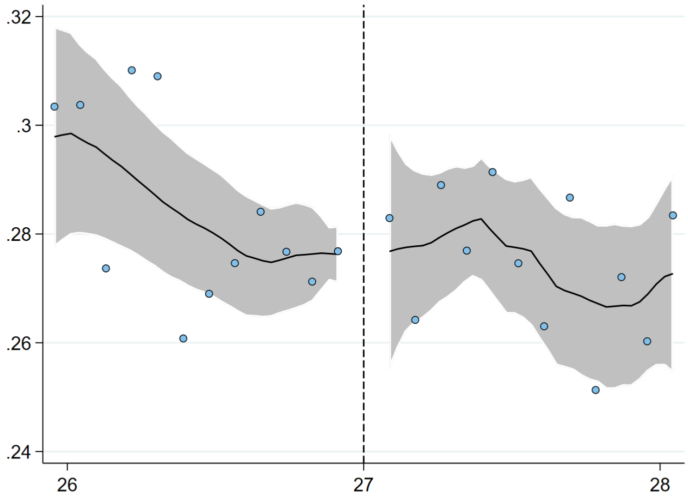
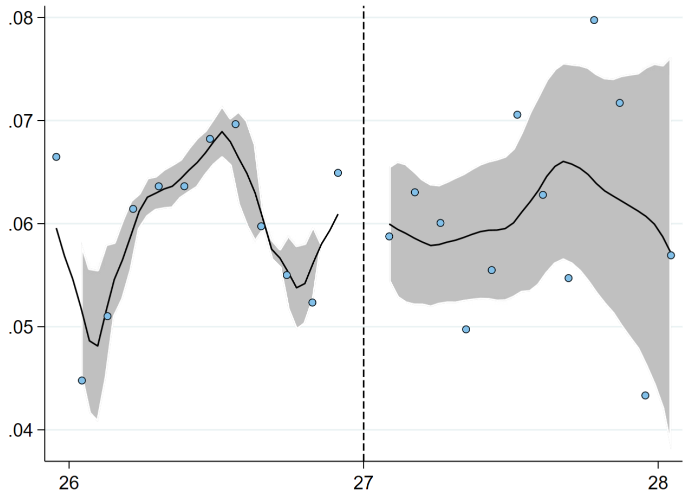


Figure 3.4: Weekday crime rates across age among men (a), women (b) and non-natives (c)

(a) Weekday crime rate men



(b) Weekday crime rate women



(c) Weekday crime rate non-natives

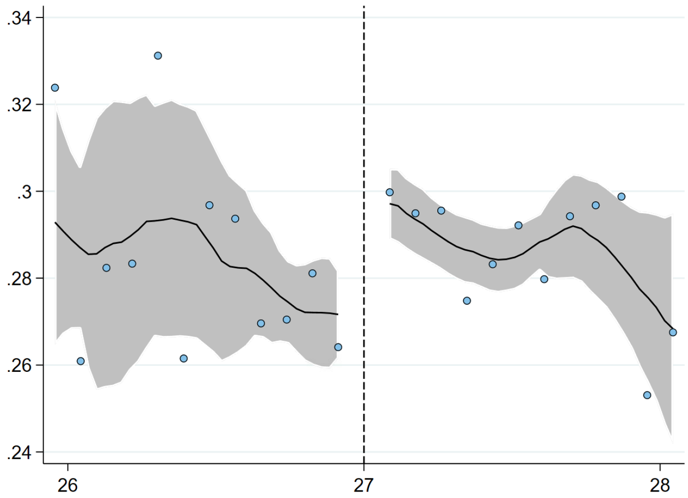
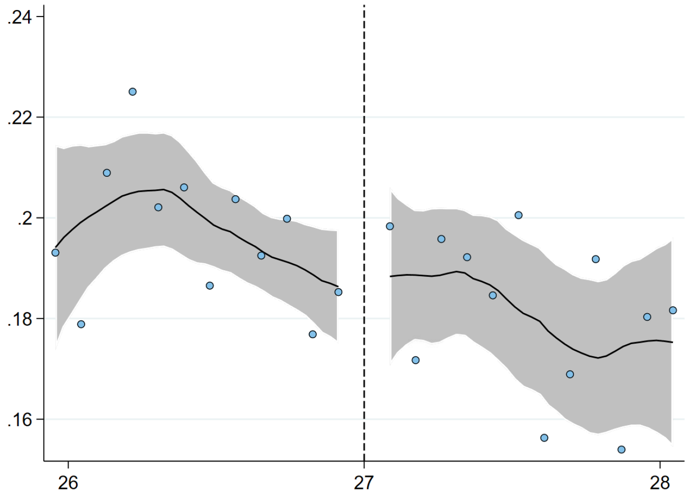
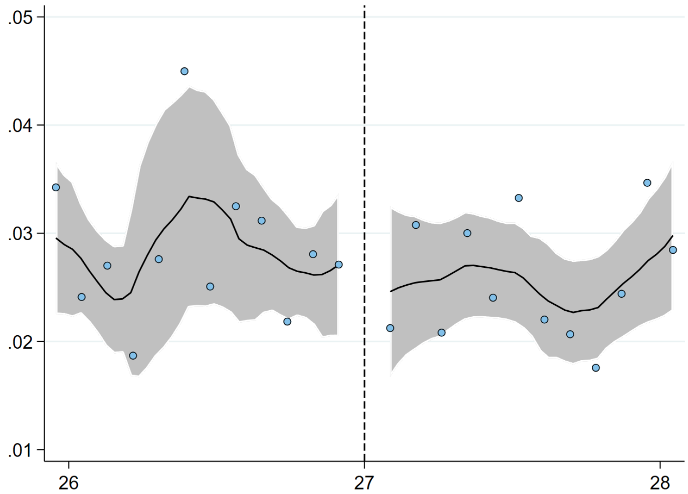


Figure 3.5: Weekend crime rates across age among men (a), women (b) and non-natives (c)

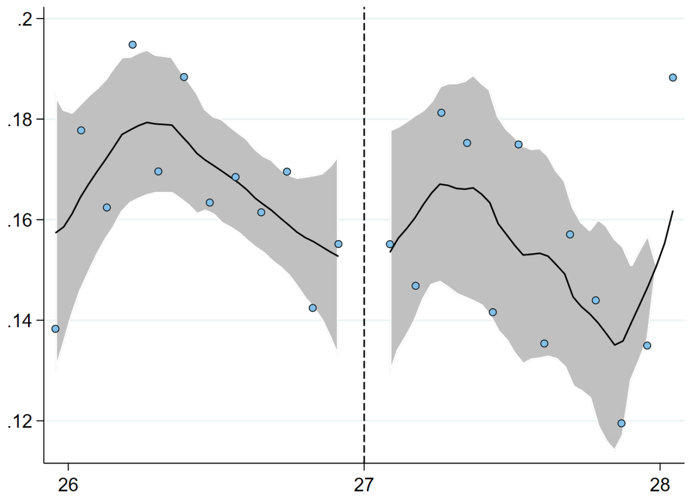
(a) Weekend crime rate men



(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.

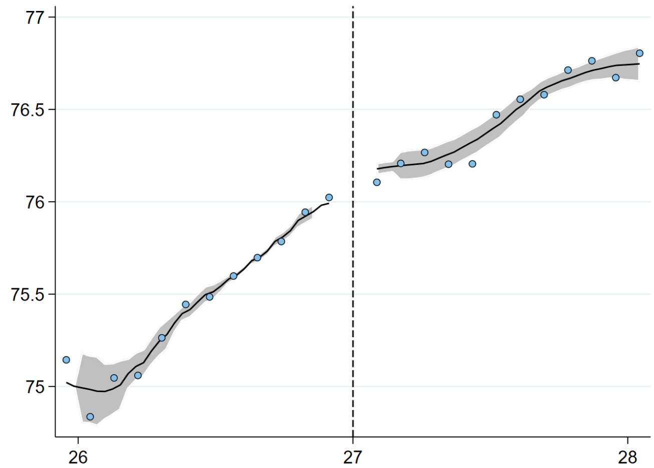
For future research, it would be interesting to investigate long-term effects. For instance, human capital effects are more likely to appear after program completion. Furthermore, the low crime rates among the sample under consideration restricted the analysis of separate crime categories. Differentiating 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.

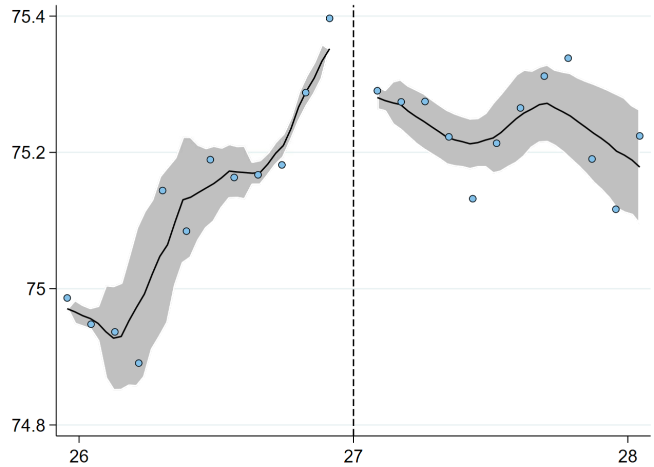
3.A Employment

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

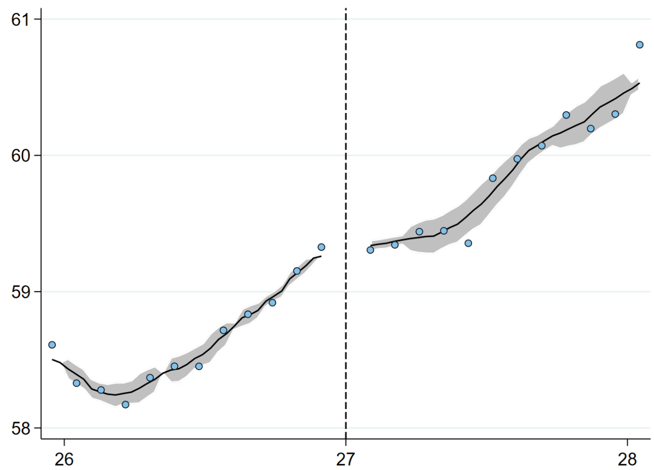


Table 3.10: Baseline probit estimates for employment status

	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)
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.

4 Crime over the welfare payment cycle: Assessing the relationship between welfare payment dates and criminal behavior among recipients

Abstract

Ample evidence suggests that many welfare recipients face serious financial constraints towards the end of the month. As this may induce criminal behavior, this study investigates the extent to which crime among welfare recipients is affected by the monthly welfare payment cycle. To this end, we exploit exogenous variation in payment dates over time and across Dutch municipalities. We find financially-motivated crime to increase by 17% over the welfare month, indicating an increase in supplementation of income through crime. Conversely, other crimes peak directly after benefits receipt, and decrease by 6% over the payment cycle. This may be attributable to a spike in consumption complementary to criminal behavior directly after disbursement, such as illicit drugs and alcohol.

Introduction 4.1

Previous studies suggest that many welfare recipients prematurely exhaust their welfare benefits, and lack savings to cover subsequent financial

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shortfalls.[1] Consumption among welfare recipients increases sharply after payment receipt, and decreases substantially towards the end of the month. Financial constraints towards the end of the month could substantially affect criminal behavior, yet little is known about the relationship between welfare benefits disbursement and crime. To address this paucity, this study assesses to what extent the amount of time that has passed since welfare benefits receipt affects criminal behavior among welfare recipients.

Time since welfare benefits receipt may affect crime through two distinct economic causal mechanisms. The first mechanism concerns the possibility that crime among welfare recipients increases towards the end of the welfare payment cycle, due to a reduction in available means of subsistence. If welfare recipients prematurely exhaust their benefits, subsequent financial shortfalls are likely to increase crime (see Agnew 1992, Becker 1968, Ehrlich 1973). Following the permanent income hypothesis (Friedman 1957), such financial shortfalls should not occur if the benefit is large enough, as recipients smooth consumption over the payment cycle. However, a substantial body of evidence points towards a decline in consumption near the end of the welfare month (Damon et al. 2013, Hamrick and Andrews 2016, Hastings and Washington 2010, Mastrobuoni and Weinberg 2009, Shapiro 2005, Wilde and Ranney 2000). Consumption of basic necessities, such as food, drops substantially towards the end of the payment cycle. While most studies focus on food spending, Hamrick and Andrews (2016) and Shapiro (2005) find an increased likelihood among SNAP recipients to report days without any nutritional intake, suggesting severe financial constraints. From a rational choice perspective, such financial constraints could motivate recipients to commit financially-motivated crime (Becker 1968). Becker states that individuals determine their behavior by rationally weighing the perceived costs and benefits (Becker 1968, Ehrlich 1973). Having less financial means available increases the likelihood of individuals committing crime for financial gains, as the relative benefits increase.[2] Following general strain theory, insufficient financial

[1]E.g., see Castellari et al. (2017), Damon et al. (2013), Hamrick and Andrews (2016), Hastings and Washington (2010), Mastrobuoni and Weinberg (2009), Shapiro (2005), Stephens Jr (2003), Wilde and Ranney (2000).

[2]This is supported by a substantial body of literature, such as studies into the relationship between changes in wages and crime (e.g. Gould et al. 2002, Machin and Meghir

means could stimulate criminal behavior in general, as it can be classified as a negative stimulus that may render an individual unable to achieve personal goals (Agnew 1992). The resulting emotional strain may increase both financially-motivated and other crime (e.g. drug-related and violent offenses).

The second mechanism through which time since welfare benefits receipt may affect crime pertains to the income shocks generated by the once monthly lump sum disbursement of welfare benefits. Ample evidence points towards a sharp increase in consumption among welfare recipients directly after benefits receipt (Castellari et al. 2017, Damon et al. 2013, Shapiro 2005, Stephens Jr 2003, Wilde and Ranney 2000). If this spike in consumption also concerns the consumption of alcohol, illicit drugs, and certain leisure activities, this may increase criminal behavior.[3][4] Routine activity theory (RAT) offers a theoretical mechanism through which payment receipt may affect crime (Cohen and Felson 1979). From this perspective, crime occurs through the culmination of three elements: the presence of a motivated offender, a suitable target, and the absence of a capable guardian. Benefits receipt may provide the means necessary for the consumption of alcohol and illicit drugs, which may reduce inhibitions to criminal behavior. Furthermore, benefits could finance participation in certain leisure activities that increase contact between motivated offenders and suitable targets, such as nightlife activities (see Miller 2013). In an extension of RAT, Felson (2006) classifies nightlife establishements as 'offender convergence settings' where individuals assemble in anticipation of criminal activity.

Supporting evidence for this second theoretical mechanism is presented by two studies, which find spikes in specific crime types upon benefits receipt. Hsu (2017) finds such a spike, by comparing temporal patterns

2004). More broadly, a study by Carvalho et al. (2016) finds that members of low-income households become more present-biased in their intertemporal choices surrounding monetary rewards, towards the next payout date.

[3] Beyond unlawful consumption.

[4] Castellari et al. (2017) assess the relationship between food stamp disbursement and purchasing patterns. In addition to the increase in consumption directly after receipt, they find the day of week upon which the benefits receipt takes place to affect purchasing choices. Disbursement on weekends produces an increase of 4 to 5% in beer purchases, compared to weekdays.

in certain types of intimate partner violence to payment schedules for the Temporary Assistance for Needy Families (TANF) program. Directly after benefits receipt, she finds an increase in male-on-female physical assault by intoxicated offenders, as well as increased intimidation perpetrated by men to gain control of household resources. The latter spike is not found in states where recipients receive TANF payments twice monthly (as opposed to once monthly). Relatedly, a study by Dobkin and Puller (2007) indicates that Supplemental Security Income (SSI) recipients appear to significantly increase their consumption of illicit drugs upon payment receipt. By analyzing temporal patterns in adverse health outcomes due to the consumption of illicit drugs among recipients of several US cash transfer programs, they find increases of 23% in drug-related hospitalizations, and 22% in drug-related hospital mortality, during the first five days after SSI disbursement.

While a vast body of evidence has accumulated on the relationship between welfare benefits disbursement and consumption, studies assessing the effects on crime are scarce. To the best of our knowledge, we build upon only one existing study that assesses the effects of the time since welfare benefits disbursement on comprehensive measures of crime. This study by Foley (2011) compares disbursement schedules of welfare benefits to daily-level aggregate crime data in twelve large US cities. Compared to the start of the payment cycle, he finds a significantly higher crime rate on the last day before payment (12%). This is attributeable to an increase of 14% in financially-motivated crime over the welfare month, as the rate of other offenses is unaffected. These findings support the hypothesis that financial shortfalls towards the end of the welfare payment cycle increase financially-motivated crime. Another closely related study by Watson et al. (2019) exploits exogeneity in Alaska's Permanent Fund Dividend payouts to all residents of Alaska, to assess the effects of an annual lump-sum universal cash transfer on crime. They find a 12% reduction in property crime, and 17% increase in substance-related incidents for up to two weeks after disbursement. These findings support both of our hypotheses in that the receipt of a cash transfer reduces the motivation to commit crime for financial gains, but also increases consumption conducive to criminal behavior.

If the payment cycle affects crime, determining an optimal disbursement strategy could prove to be an important and cost-effective crime prevention strategy. Multiple authors argue in favor of staggering disbursement *across* individuals (Carr and Packham 2019, Dobkin and Puller 2007, Foley 2011). The closely related study by Foley (2011) finds aggregate crime rates to stabilize in jurisdictions where disbursement are staggered across individuals (i.e., different recipients receive benefits on different days). Also noteworthy is a study by Carr and Packham (2019), which exploits a policy reform in the state of Indiana, and geographical policy differences, to assess the effect of staggering SNAP payments across individuals, to non-staggered disbursement. They find this form of staggering of payments to decrease crime in general by 17.5%, and grocery store theft by 20.9%. However, there is little research into staggering disbursement *within* individuals, which could potentially address the underlying causal mechanism of consumption smoothing by welfare recipients at the individual level. Hsu (2017) follows welfare recipients over time and finds that increasing the individual-level disbursement frequency to bi-monthly payouts causes spikes in domestic violence upon disbursement to disappear. These findings emphasize that disbursement policy can be an effective tool for crime prevention, and that an individual-level assessment of *if* and *how* welfare benefits disbursement affects crime is warranted to further our understanding of the underlying disbursement-crime dynamics.

This study is the first to analyze individual-level administrative data to assess the extent to which the time that has passed since the last received welfare benefits payment affects criminal behavior among welfare recipients. The availability of these unique data allows us to employ individual fixed effects linear probability models to exploit exogenous variation in welfare payment dates across 16 of the largest Dutch municipalities. These data also enable us to investigate heterogeneous effects across sex and different age groups. As our hypotheses differ across crime categories, we run the analyses separately for financially-motivated crime and other (non-financially-motivated) crime.

Our contribution to the literature is fourfold. First, we contribute to the scarce literature on welfare benefits disbursement by assessing the effects on criminal behavior at the individual level, contrasting this study with previous research that focuses on city-level crime rates (e.g. Foley 2011). This approach is facilitated by the availability of individual-level administrative data on both welfare receipt and criminal behavior, which enable us to select the welfare population and investigate their criminal behavior over the welfare payment cycle. These data also allow us to employ individual fixed effects to control for unobserved time-invariant heterogeneity. Combined with the exploitation of exogenous variation in payment dates within and across municipalities, we avoid bias from endogeneity induced by variation across individuals, time, and municipalities (e.g. from coinciding transactions, such as other benefits, wages, and rents).

Second, our unique individual-level data also enable us to assess the extent to which the effects of welfare benefits disbursement on criminal behavior are heterogeneous across age and sex. As detailed by Hsu (2017), the sex of welfare recipients plays an important role in the relationship between welfare benefits disbursement and both criminal behavior and victimization. Furthermore, both age and sex have been proven to be important determinants of welfare dependency and criminal behavior (e.g. see Corman et al. 2014, Holtfreter et al. 2004, Loeber and Farrington 2014, Steffensmeier and Allan 1996). If we find the treatment effects to differ across the included samples, this study may facilitate the formation of more cost-effective welfare policy targeting populations of interest.

Third, the comparative generosity of the Dutch welfare system enables us to shed light on the generalizability of earlier findings to welfare systems with higher benefits levels. The only other existing study into the effects of welfare payment timing on comprehensive crime measures is focused on the US (Foley 2011), where guaranteed minimum income benefits are much lower than the Netherlands (6% vs 60% of median disposable income, see OECD 2018a). The larger payments may reduce financial shortfalls at the end of the cycle, while inducing larger income shocks at the start. Theoretically, this may reduce financially-motivated crime at the end of the month, but cause more non-financially-motivated offenses upon disbursement.

Finally, research on welfare benefits disbursement is generally focused on directly-targeted economic outcomes. By assessing the evolution of crime over the welfare payment cycle at the individual level, our findings contribute to a more comprehensive overview of the costs and benefits of welfare payment regimes. This is especially relevant in light of the recent call to reduce welfare generosity in the Netherlands (Ministerie van Financiën 2020), and the overall trend of reduction of welfare accessibility in several Western countries (e.g. Dahlberg et al. 2009, Hernæs et al. 2017).

We find welfare recipients to commit 17% more financially-motivated crime at the end of the monthly welfare payment cycle, as compared to directly after benefits disbursement. The rate of other, non-financially-motivated crime however peaks directly after benefits receipt, and decreases by 6% over the welfare month. Overall, we find comparable effects across subsamples, although non-financially motivated crime is unaffected among women. Furthermore, higher baseline crime rates produce larger absolute changes in offenses among younger age groups and men.

Below, Section 4.2 will first shortly summarize the data on welfare disbursement in the Netherlands, and other included measures, followed by a discussion of the samples and some graphical evidence. Section 4.3 presents the methodology, followed by the estimation results in Section 4.4, and an additional robustness check in Section 4.5. We conclude and discuss the implications of the results in Section 4.6.

Data and graphical evidence 4.2

Welfare benefits 4.2.1

The Dutch welfare system can be considered very generous, in comparison to most other countries. Guaranteed minimum income benefits in the Netherlands are €933.65 for single-person households (Ministerie van Sociale Zaken en Werkgelegenheid 2017). This amounts to 60% of the median disposable income, which greatly exceeds the US (6%), and is only surpassed by Japan (65%), Ireland (64%), and Denmark (63%) (OECD 2018a). Every legally-registered adult Dutch citizen is guaranteed a minimum income for an unlimited duration. The benefits are means tested, however, which excludes eligibility of individuals with an income higher than the welfare norm and/or assets exceeding a certain maximum threshold (including other household members).[5] While these criteria are centrally defined under one national scheme, executive responsibilities lie with municipalities.[6] As the municipalities are responsible for the disbursement of welfare benefits, payout dates vary across municipalities. Within municipalities, the payout dates generally also vary from month to month. As these payout schemes are not centrally registered, we contacted the largest Dutch municipalities to provide us with the required data.

As shown in Table 4.1, we have access to data concerning exact disbursement dates for 16 municipalities. Although coverage varies, the majority of municipalities were able to provide us with data covering our full observation window (2005-2017). This includes three of the six largest Dutch municipalities (Rotterdam, The Hague, and Groningen), while the data for Amsterdam and Utrecht are available from 2010 and 2011 onwards, respectively. We determine on a daily level whether an individual resides in one of the included municipalities within the respective observation window.

[5]Further exclusion restrictions are limited to entitlement to other benefits (e.g. unemployment benefits), and being imprisoned.

[6]Supervision and support of re-integration is also carried out by the municipalities, which define job-search requirements and offer job-search assistance.

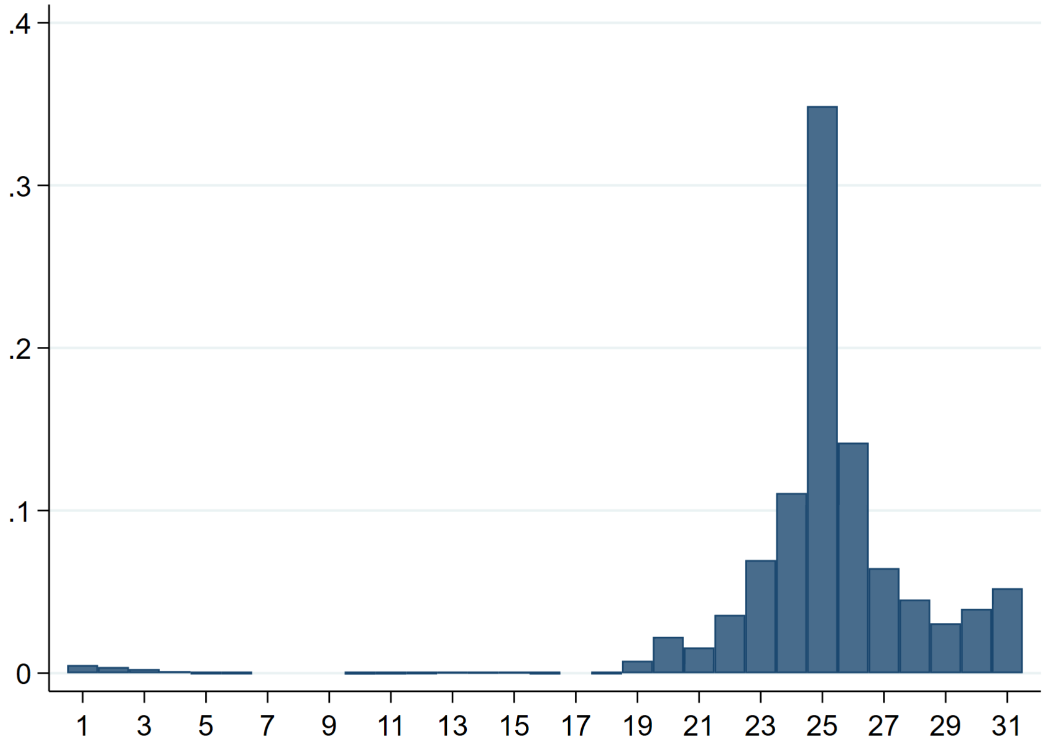
Table 4.1: Data coverage per included municipality

City	Sample period	Population size*	Welfare receipt rate (%)*
Almelo	2006-2017	72,495	4.46
Almere	2006, 2008-2017	201,051	3.15
Amsterdam	2010-2017	845,594	5.00
Arnhem	2005-2011, 2013-2017	155,763	5.46
Delft	2005-2017	101,217	3.32
Deventer	2005-2017	99,358	3.29
Groningen	2005-2017	202,324	5.47
Hengelo	2005-2017	80,757	3.21
s-Hertogenbosch	2005-2014	152,425	2.79
Leeuwarden	2005-2017	108,631	5.67
Leiden	2005-2017	123,571	3.08
Oss	2017	90,423	1.99
Rotterdam	2005-2017	634,887	6.52
The Hague	2005-2017	525,156	5.27
Utrecht	2011-2017	342,971	3.32
Zwolle	2005-2017	125,616	3.00

Notes. *The population data originate from Statistics Netherlands and concern municipal population sizes in January 2017 (Statistics Netherlands 2017a,b).

Figure 4.1 presents the distribution of welfare benefits payout days over days of the calendar month. The disbursement of welfare benefits generally takes place towards the end of the calendar month. The payout probability increases after the 18th and peaks on the 25th of the month, with approximately one-third of payouts taking place on this day. As subsidized housing rents and utility payments are generally due towards the end of the calendar month, the disbursement of welfare benefits around this time may be aimed at reducing the risk that recipients will be unable to make payments.

Figure 4.1: Payout day distribution



4.2.2 Sample and descriptive statistics

To estimate our models, we combine the disbursement schedule data with longitudinal individual-level data provided by Statistics Netherlands.[7] These data cover all registered welfare recipients in the municipalities mentioned in Table 4.1 over a thirteen-year observation window, from 2005 to 2017.

In addition to the data on disbursement dates, this study is facilitated by the availability of daily-level crime data. These data are derived from crime reports of the Dutch law enforcement agencies, which have been submitted to the public prosecutor. These reports contain information concerning crimes of which individuals are officially suspected and are

[7]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, bijstanduitkeringtab, bus, gbaadresobjectbus, gbapersoontab, integraal huishoudens inkomen, integraal persoonlijk inkomen, verdtab and vslgwbtab.

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, there is no reason to expect the unmeasured crime distribution to differ from the measured crime distribution over the welfare cycle. Administrative data on welfare benefits are derived from municipal payment registrations, which enable us to determine welfare receipt status on a monthly level.

Table 4.2 shows that the selection of all registered welfare recipients in the municipalities under consideration produces a full sample of 545,186 individuals and 545.9 million daily observations. This vast sample size facilitates the estimation of our models on the low daily-level crime probabilities, which range from 0.0164% to 0.0312% for financially-motivated crime and crime in general, respectively. On a yearly level, 4.62% of the selected welfare recipients commit any criminal offense, 2.51% a financially-motivated offense, and 3.00% an offense classified as 'other'. Other crime is defined as any criminal offense for which no theoretical financial incentive can be identified. Of these offenses, 60.07% can be classified as a violent offense, 42.35% as a sex offense, 12.56% as a drug offense, and 24.45% as belonging to miscellaneous categories (e.g. traffic offenses). Singular offenses can belong to multiple crime categories.

Of the full sample of welfare recipients, 49.42% are male, and 36.35% native-born Dutch citizens. As welfare benefits are means tested, the average annual personal primary income is very low (€ 1,127). So, individuals within our sample receive little income from legitimate employment. The mean annual standardized household income of € 13,233, which includes benefits, is less than half of the overall mean for the Netherlands (€ 28,800 in 2017, see Statistics Netherlands 2018b).

The individual-level data allow us to investigate potential heterogeneous effects. We run the baseline analyses over five subsamples, including three age groups, and men and women, separately. A vast body of evidence shows that the propensity to commit crime follows a skewed bell curve over age (see Loeber and Farrington 2014). As criminal behavior peaks around late adolescence, followed by a decline from the early 20s onwards, we select three age groups: 18 to 25 year olds, 26 to 39 year olds and individuals aged 40 and up. Men and women are considered separately, as men are more likely to commit offenses compared to women (violent offenses especially, e.g. see Kruttschnitt 2013, Steffensmeier and Allan 1996), whereas women show comparatively high poverty and welfare dependency rates. The latter emphasizes the importance of analyzing the effects of welfare receipt on crime among women, who have received little attention in the existing related literature (see Corman et al. 2014, Holtfreter et al. 2004).

Table 4.2 shows substantial differences in the propensities to commit crime across subsamples. The daily crime rate among men is more than five times higher than that among women, with 0.0566% and 0.0106% respectively. This gap widens when we consider other crime, of which the daily rate is more than seven times as high among men (0.0334%), as compared to women (0.0046%). On average, 7.67% of male welfare recipients commit crime in a given year, versus 1.97% of female welfare recipients. In line with the age-crime curve, we find lower rates as we move up the age groups for all crime categories. As compared to the oldest age group (40+ year olds), we find the daily crime rates among the youngest age group (18-25 year olds) to be approximately three times as high (e.g. 0.0211% versus 0.0631% for crime in general, respectively).

While annual standardized household incomes differ little across samples, we find substantial differences in annual personal primary incomes. The highest of which is found among the youngest age group (€ 1,757), whereas the oldest age group shows the lowest primary income (€ 815). This is indicative of the duration of unemployment spells increasing with age, which may be due to lower employability.

Table 4.2: Descriptive statistics, 2005-2017

	Full sample	18-25 yo	26-39 yo	40+ yo	Men	Women
Male (%)	49.42	49.19	49.64	48.47		
Native (%)	36.35	35.03	31.78	38.66	35.77	36.91
Crime (daily, %)	0.031	0.063	0.044	0.021	0.057	0.011
Crime (yearly, %)	4.621	6.395	5.786	3.418	7.668	1.967
Financially-motivated crime (daily, %)	0.016	0.032	0.022	0.012	0.029	0.007
Financially-motivated crime (yearly, %)	2.510	3.406	3.029	1.932	3.989	1.222
Other crime (daily, %)	0.018	0.036	0.026	0.011	0.033	0.005
Other crime (yearly, %)	3.002	4.231	3.925	2.104	5.357	0.952
Annual personal primary income	1,127	1,757	1,628	815	1,281	1,004
Annual standardized household income	13,233	13,919	13,062	13,236	12,909	13,492
Number of individuals	545,186	123,332	240,833	284,200	269,415	275,771
Number of observations	545.9M	40.3M	164.5M	341.1M	244.0M	301.9M

4.2.3 Graphical evidence

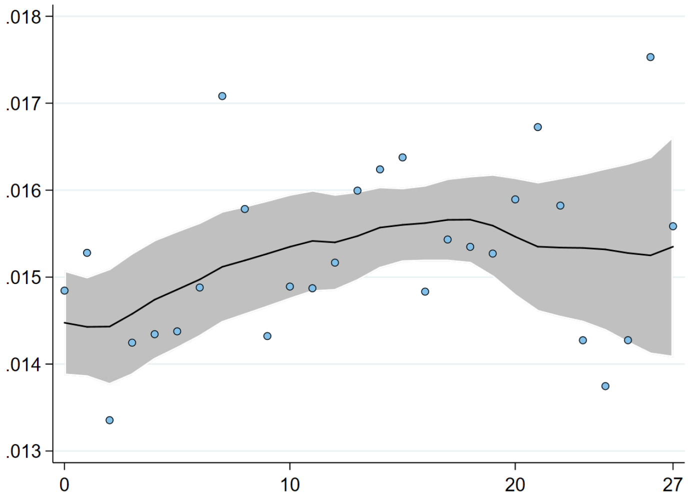
Before turning to the estimation results, we present exploratory graphs on the evolution of crime outcomes over the number of days that have passed since welfare payment receipt. Figures 4.2a to 4.2c present local polynomial smooth plots of average daily crime rates, including 95% confidence intervals.

Figure 4.2a presents the evolution of the daily financially-motivated crime rate over the welfare month among the full sample. In line with the descriptives shown in Table 4.2, the daily rate of financially-motivated crime is around 0.0150%. We can see that the financially-motivated crime rate is the lowest directly after benefits disbursement. The probabilities rise until approximately 20 days since payment, where it starts to slightly drop towards the end of the payment cycle. Conversely, as shown in Figure 4.2b, the rate of other crime is the highest directly after payment receipt, and declines as the time since disbursement increases.

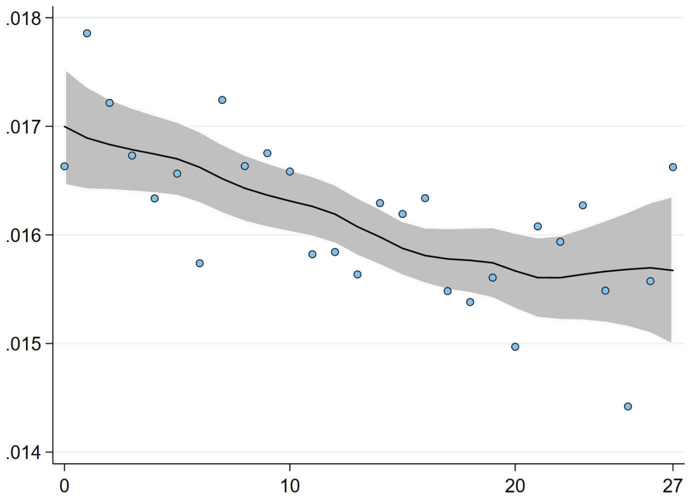
Figure 4.2c shows the change in crime in general to be more limited, as compared to Figures 4.2a and 4.2b. This may be attributable to the inversive trends of financially-motivated and other crime among the full sample. Although exacerbated by the larger y-axis scale, all crime appears to be less affected than the included subcategories.

Figure 4.2: Crime rates over days since payment

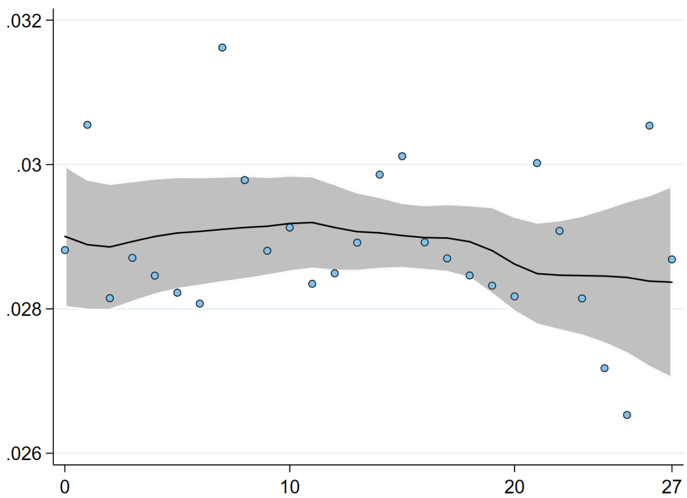
(a) Financially-motivated crime



(b) Other crime



(c) All crime



4.3 Empirical methodology

We analyze the evolution of crime over the payment cycle by exploiting exogenous variation in payment dates using a fixed effects linear probability model. The availability of individual-level data allows us to employ individual fixed effects to control for unobserved time-invariant heterogeneity. To further control for endogeneity, we account for time-varying, and municipality-specific, external influences, by including individual, day-of-week, calendar month, year, and municipality fixed effects in all fixed effects model specifications. Through this approach, we avoid potential biases induced by the disbursement coinciding with other monetary transactions (e.g. rents, wages, or other benefits), and unobserved heterogeneity across individuals and municipalities.[8] We cluster the standard errors on a calendarmonth*year*municipality combination to prevent overstatement of the significance of the estimated effects, and account for the group structure induced by potential specification errors. The baseline fixed effects model for financially-motivated crime, other crime, and crime in general, is specified as follows:

$$\begin{aligned} y_{it} = \delta DSP_{it} + \beta AGE_{it} + \alpha PERSON_{it} + \eta DOW_{it} \\ + \gamma MONTH_{it} + \rho YEAR_{it} + v_{it} \end{aligned} \tag{4.1}$$

Where y_{it} denotes the outcome that indicates whether individual i is suspected of crime in general, financially-motivated crime, or other crime on day t, DSP_{it} is the main variable of interest, a days-since-payment index that indicates the time that has passed since the last received welfare payment, β_{it} captures age, (α_{it}, η_{it}, γ_{it}, ρ_{it}) are individual, day-of-week, calendar month, and year fixed effects, and v_{it} is the error term. The error term v_{it} is assumed to follow a normal distribution with mean zero.

For the baseline analyses, we employ a days-since-payment (DSP) index, which captures the total change in the outcome variable over the

[8] All low-income households in the Netherlands are entitled to rent and healthcare benefits, which are disbursed on the 20th of every calendarmonth. As the disbursement of these benefits theoretically could affect our estimates, we assess the sensitivity of our estimates to such payouts. As will be discussed in Subsection 4.5.1, we do not find our results to be sensitive to these disbursements.

welfare payment cycle. This index ranges in value from 0 to 1, and is computed by dividing the number of days that have passed since the last received welfare payment by the maximum number of days since payment. Due to differences in processing times between banks, it is possible for recipients to receive their benefits up to 3 days before the guaranteed disbursement date. We therefore drop all observations within 3 days before the next disbursement, amounting to a total payment cycle duration of 28 days.[9] On the available system, it is not computationally feasible to estimate a fixed effects linear probability model over the entire dataset at once. We therefore run the regressions over two randomly selected halves of the dataset, after which we combine the estimates through a minimum distance approach.

In addition to the fixed effects linear probability model, we run (a) a linear probability model excluding individual fixed effects, and (b) a probit model specification. We compare the fixed effects linear probability estimates to a linear probability model excluding individual fixed effects, to assess the extent to which the inclusion of individual fixed effects affects our estimates. The probit model specification is applied as the outcome variables are dichotomous and have probabilities close to zero. A probit model may be preferable in such cases over a linear probability model, as the latter does not estimate non-linear structural parameters (see Horrace and Oaxaca 2006). We include municipality dummies in these model specifications, to control for heterogeneity across municipalities. Furthermore, apart from the exclusion of individual fixed effects, we specify the linear probability and probit model identically to the fixed effects linear probability model. For these robustness checks, day-of-the-week, calendar month and year are included as dummies, as opposed to fixed effects. This, however, is inconsequential, as least square dummy variables and fixed-effect estimators produce an identical result. Finally, we test for nonlinearity by employing a higher order model specification, as well as a model using multiple days-since-payment indicators.

[9]Welfare payment cycles consisting of more than 31 days occur infrequently in our dataset (e.g. due to a deviation in disbursement around holidays). As the inclusion of such observations substantially increases the noise in the tail-end of the distribution, we drop these observations from the analyses.

4.4 Estimation results

This section presents the main estimation results of this study. Subsection 4.4.1 firstly discusses the estimates produced by the baseline model specification, followed by a comparison to estimation results from a standard linear probability model, and a probit model. Subsections 4.4.2 and 4.4.3 summarize the results obtained from quadratic and indicator days-since-payment model specifications, respectively. Finally, Subsection 4.4.4 assesses the extent to which the effects are heterogeneous across subsamples.

4.4.1 Baseline estimation

Table 4.3 presents the estimates for the full sample of the baseline fixed effects linear probability model, as well as a standard linear probability, and a probit model specification. The use of the aforementioned days-since-payment (DSP) index means that the average marginal effects (AMEs) indicate how much the conditional probability of committing crime changes over the welfare month (i.e. a one unit increase spans the full payment cycle).

Starting with the baseline fixed effects linear probability model, we find the largest effect for financially-motivated crime, of which we find welfare recipients to commit 16.52% (0.0026 %points) more offenses at the end of the welfare month, as compared to directly after benefits disbursement. Conversely, other crime peaks directly after disbursement. Welfare recipients commit 5.69% less non-financially-motivated crime at the end of the payment cycle (-0.0010 %points), as compared to directly after payout. This supports the hypothesis that a spike in available means upon benefits disbursement increases consumption complementary to non-financially-motivated crime (e.g. drugs and alcohol). Furthermore, the findings for financially-motivated crime support the hypothesis that welfare recipients commit more crime for financial gains towards the end of the welfare month, to supplement their income.

The inversive effects on financially-motivated crime and other crime amount to a comparatively small change in crime in general over the payment cycle. Compared to directly after benefits disbursement, welfare recipients commit 4.86% (0.0015 %points) more crime in general at the end of the month. This increase in total crime is attributable to the relatively large increase in financially-motivated crime, which is approximately three times larger than the inverse effect of time since payment on other offenses.

Overall, we find highly comparable estimates from the standard linear probability model ('Ordinary least squares'), and the probit model ('Probit'), as compared to the baseline model ('Fixed effects'). The estimates remain highly statistically significant across the board ($p<0.01$ and $p<0.001$), and while both models produce slightly smaller estimates for crime in general and financially-motivated crime, we find slightly larger estimates for other crime. An additional robustness check can be found in Section 4.5.

Table 4.3: Baseline days-since-payment index estimates, full sample

	Financially-motivated crime	Other crime	All crime
Fixed effects			
DSP index	0.0026***	-0.0010***	0.0015***
	(0.0002)	(0.0002)	(0.0003)
Age	0.0001	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0003)
Constant	0.0129	0.0134	0.0255*
	(0.0090)	(0.0089)	(0.0123)
DSP index (%)	16.52	-5.69	4.86
Ordinary least squares			
DSP index	0.0023***	-0.0012***	0.0010**
	(0.0002)	(0.0002)	(0.0003)
Age	-0.0006***	-0.0008***	-0.0013***
	(0.0000)	(0.0000)	(0.0000)
Constant	0.0439***	0.0494***	0.0880***
	(0.0013)	(0.0013)	(0.0023)
DSP index (%)	14.27	-6.87	3.13
Probit			
DSP index	0.0023***	-0.0012***	0.0010**
	(0.0002)	(0.0002)	(0.0003)
Age	-0.0006***	-0.0008***	-0.0014***
	(0.0000)	(0.0000)	(0.0000)
Constant			
DSP index (%)	15.36	-6.91	3.33
Number of individuals	545,186	545,186	545,186
Number of observations	545.9M	545.9M	545.9M

Notes. The days-since-payment index ranges in value from 0 to 1, where 0 indicates the payout day, and 1 the last day before payment. The fixed effects model specification includes individual, day-of-the-week, calendar month, year, and municipality fixed effects, whereas the OLS and probit model specifications include day-of-the-week, calendar month, year, and municipality dummies. To increase interpretability, the shown fixed effects and OLS coefficients are multiplied by 100, and the shown probit estimates are average marginal effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Higher-order estimates 4.4.2

To assess the sensitivity of our estimates to a higher-order model specification, we run a fixed effects model including a quadratic days-since-payment term, the results of which are presented in Table 4.4. 'DSP index (combined)' captures the total change over the welfare payment cycle, and is computed through linear combination of 'DSP index' and 'DSP index squared', i.e. the linear and quadratic days-since-payment terms. Due to the negative covariance between the linear and quadratic terms, we find comparatively small standard errors for the combined DSP index. Hence, the combined DSP index is more precisely estimated than its subvariables.

We find the inclusion of a quadratic term to produce comparable estimates to the baseline model specification. While the coefficients slightly reduce in size, the estimates remain statistically highly significant for all crime outcomes ($p<0.01$ and $p<0.001$ respectively). The most notable reduction in effect size is for crime in general (3.30% vs 4.86%), whereas the relative change in effect size is more limited for financially-motivated crime (12.93% vs 16.52%), and other crime (-5.36% vs -5.69%). While this does reduce the statistical significance of the general crime estimates ($p<0.01$ vs $p<0.001$), we consider all estimates to be robust to the specification of a higher-order model.

Table 4.4: Fixed effects higher-order days-since-payment index estimates, full sample

	Financially-motivated crime	Other crime	All crime
DSP index	0.0072***	-0.0015†	0.0054***
	(0.0008)	(0.0008)	(0.0011)
DSP index squared	-0.0051***	0.0005	-0.0044***
	(0.0009)	(0.0009)	(0.0012)
Age	0.0001	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0003)
Constant	0.0116	0.0125	0.0247*
	(0.0090)	(0.0091)	(0.0123)
DSP index combined	0.0021***	-0.0010***	0.0010**
	(0.0002)	(0.0002)	(0.0003)
DSP index combined (%)	12.93	-5.36	3.30
Number of individuals	545,186	545,186	545,186
Number of observations	545.9M	545.9M	545.9M

Notes. The days-since-payment index ranges in value from 0 to 1, where 0 indicates the payout day, and 1 the last day before payment. To increase interpretability, the shown coefficients are multiplied by 100. The model specification includes individual, day-of-the-week, calendar month, year, and municipality fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

4.4.3 DSP indicator specification

The graphical evidence presented in Section 4.2.3 suggests that the relationship between days since payment and crime varies over the welfare payment cycle. To further assess the evolution of the probability to commit crime over the welfare payment cycle, we use indicators for four day increments in the number of days that have passed since the last-received payment. This approach enables us to assess to what extent the relationship between the time that has passed since disbursement and crime is non-linear (beyond a second degree polynomial). The average marginal effects (AMEs) capture the difference relative to the first four days after benefits disbursement, as the constant captures the crime rate in this period.

Table 4.5 presents the estimation results for the fixed effects model specification with days-since-payment (DSP) indicators. For financially-motivated crime, we find statistically highly significant positive estimates for all indicators ($p<0.001$). Although the financially-motivated crime rate continues to rise for most of the welfare month, the sharpest increase is found in the first approximately 12 days after benefits receipt. In line with Figure 4.2a, we find the rate to peak around two-thirds into the month, at 15.30% higher than the mean rate directly after disbursement. This is followed by a decrease in the tail-end of the cycle, which may indicate delaying of gratification by welfare recipients, in anticipation of the upcoming welfare benefits disbursement.

In line with Figure 4.2b, the negative coefficients for other crime show that the other crime rate is the highest in the first 8 days after benefits receipt. The reduction is more gradual, as the estimates are not statistically significant for the first approximately 12 days of the welfare month. As opposed to financially-motivated crime, we do not find an apparent inversion of this trend towards the end of the month. The consistent reduction in other crime over the welfare month supports the notion that the inversion in financially-motivated crime is due to delaying of gratification, as opposed to income from other sources (e.g. rent and healthcare benefits). As the latter would also cause an inversion of the trend in other crime, due to increased consumption complementary to criminal behavior (e.g. illicit drugs and alcohol). The lack of an increase in other crime near the end of the payment cycle supports the absence of income from other sources. The sensitivity analysis presented in Subsection 4.5.1 confirms that our estimates are not sensitive to the disbursement of rent and healthcare benefits.

The uniformly positive and statistically significant coefficients for all crime show that the rate of crime in general is at its lowest directly after benefits receipt. Crime in general peaks between 20 to 23 days after benefits receipt, at 5.24% higher than the mean rate at the start of the welfare month.

Table 4.5: Fixed effects days-since-payment index indicator estimates, full sample

	Financially-motivated crime	Other crime	All crime
Constant	0.0153†	0.0162†	0.0292*
	(0.0090)	(0.0089)	(0.0124)
$4 \leq DSP \leq 7$	0.0011***	0.0001	0.0011***
	(0.0002)	(0.0002)	(0.0003)
$8 \leq DSP \leq 11$	0.0015***	-0.0003	0.0012***
	(0.0002)	(0.0002)	(0.0003)
$12 \leq DSP \leq 15$	0.0017***	-0.0005*	0.0012***
	(0.0002)	(0.0002)	(0.0003)
$16 \leq DSP \leq 19$	0.0020***	-0.0009***	0.0011***
	(0.0002)	(0.0002)	(0.0003)
$20 \leq DSP \leq 23$	0.0024***	-0.0005*	0.0017***
	(0.0002)	(0.0002)	(0.0003)
$24 \leq DSP \leq 27$	0.0016***	-0.0007*	0.0010*
	(0.0003)	(0.0003)	(0.0004)
Age	-0.0000	0.0000	0.0000
	(0.0002)	(0.0002)	(0.0003)
AME(%)			
$4 \leq DSP \leq 7$	6.79	0.30	3.44
$8 \leq DSP \leq 11$	9.69	-1.37	3.71
$12 \leq DSP \leq 15$	11.06	-2.55	3.88
$16 \leq DSP \leq 19$	12.90	-4.81	3.41
$20 \leq DSP \leq 23$	15.52	-2.74	5.42
$24 \leq DSP \leq 27$	10.17	-3.98	3.09
Number of individuals	545,186	545,186	545,186
Number of observations	545.9M	545.9M	545.9M

Notes. The reference category includes 0 to 3 days since payment. The model specification includes day-of-the-week, calendar month, year, and municipality dummies. Standard errors are clustered by municipality, year, and calendar month, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Heterogeneous effects 4.4.4

Table 4.6 presents the estimation results produced by the baseline fixed effects analyses over multiple subsamples.

In line with the findings for the full sample, we find statistically highly significant estimates for financially-motivated crime across all included subsamples ($p<0.001$). Although financially-motivated crime is affected among all subsamples, we find substantial variation in absolute effect sizes, especially. Of all included samples, women show both the smallest absolute increase (0.0007 %points), as well as the smallest increase relative to their baseline rate (10.31%), whereas men show an almost twice as large relative increase (18.94%) and an absolute increase that is more than seven times larger than women (0.0051 %points). While the absolute effect size drops substantially over age, from 0.0049 %points (18-25 yo) to 0.0020 %points (40+ yo), relative to the baseline rates, we find the reductions to be highly comparable in size. Overall, we find financially-motivated crime among women to be the least affected. While the relative effect size is comparable across the remaining included samples, the largest absolute change in financially-motivated offenses is found among men and young-adults (18-25 yo).

We find the most heterogeneity in the estimates for other crime. For this outcome, we only find statistically significant inverse effects for men and 40+ year olds ($p<0.001$). The absolute reduction for men is the largest of all included subsamples (-0.0023 %points), whereas other crime is unaffected among women. Despite only finding statistically significant estimates for the oldest age group, we, again, find the coefficient size to drop as age increases. While the age group estimates therefore have to be interpreted with caution, we find the largest change in non-financially-motivated offenses among men.

For crime in general, we find statistically highly significant estimates for almost all of the included subsamples ($p<0.01$ and $p<0.001$). We especially find heterogeneity in the absolute effect sizes, as we find a more than three times larger increase for men (0.0025 %points) than for women (0.0007 %points), and a drop in coefficient size over age (groups). However, this is mainly attributable to heterogeneous baseline crime rates, as we find more comparable relative effect sizes (ranging from 4.28% to 6.65%). The time that has passed since benefits receipt hence appears to affect all included samples almost equally. Nevertheless, this comparable effect materializes into the largest change in offenses among men, due to their comparatively high baseline criminal activity.

Despite showing comparatively large coefficient sizes across all outcomes, we find the estimates for 18 to 25 year olds to only be statistically significant for financially-motivated crime. The substantial standard errors may be attributable to the relatively small sample size, combined with the low daily crime probabilities.

Table 4.6: Fixed effects days-since-payment index estimates, multiple samples

	Financially-motivated crime	Other crime	All crime
18-25 yo			
DSP index	0.0049***	-0.0019†	0.0022
	(0.0011)	(0.0012)	(0.0016)
DSP index (%)	16.95	-6.03	3.89
Number of individuals	123,332	123,332	123,332
Number of observations	40.3M	40.3M	40.3M
26-39 yo			
DSP index	0.0034***	-0.0009†	0.0019**
	(0.0005)	(0.0005)	(0.0006)
DSP index (%)	15.55	-3.53	4.28
Number of individuals	240,833	240,833	240,833
Number of observations	164.5M	164.5M	164.5M
40+ yo			
DSP index	0.0020***	-0.0010***	0.0012***
	(0.0002)	(0.0002)	(0.0003)
DSP index (%)	17.55	-8.12	5.64
Number of individuals	284,200	284,200	284,200
Number of observations	341.1M	341.1M	341.1M
Men			
DSP index	0.0051***	-0.0023***	0.0025***
	(0.0004)	(0.0005)	(0.0006)
DSP index (%)	18.94	-6.67	4.51
Number of individuals	269,415	269,415	269,415
Number of observations	244.0M	244.0M	244.0M
Women			
DSP index	0.0007***	0.0000	0.0007**
	(0.0002)	(0.0002)	(0.0002)
DSP index (%)	10.31	0.65	6.65
Number of individuals	275,771	275,771	275,771
Number of observations	301.9M	301.9M	301.9M

Notes. The days-since-payment index ranges in value from 0 to 1, where 0 indicates the payout day, and 1 the last day before payment. To increase interpretability, the shown coefficients are multiplied by 100. The model specification includes individual, day-of-the-week, calendar month, year, and municipality fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

4.5 Robustness check

4.5.1 Rent and healthcare benefits exclusion

As discussed in Subsection 4.2.1, low-income households in the Netherlands are entitled to rent and healthcare benefits, which may alleviate financial constraints among welfare recipients. As this could be a potential source of bias, we test whether our estimates are sensitive to the disbursement of these benefits, by effectively excluding sections of the welfare payment cycle following rent and healthcare disbursement. As the monthly payout of rent and healthcare benefits takes place on the 20th, we run additional fixed effects models excluding (a) welfare cycles which start before the 20th of the month, and (b) observations on more than 20 days since payment.

Table 4.7 shows the estimates produced by the baseline fixed effects model, excluding observations following rent and healthcare benefits disbursement. We find that dropping these observations produces comparable estimates to the baseline analyses over the full dataset. The coefficients for all crime outcomes remain statistically highly significant ($p<0.001$). While we find a slightly smaller effect size for financially-motivated crime (14.62% vs 16.52%), and crime in general (4.08% vs 4.86%), the effect size for other crime is almost identical (-5.80% vs -5.69%).

Table 4.7: Fixed effects days-since-payment index estimates, rent and healthcare benefits exclusion, full sample

	Financially-motivated crime	Other crime	All crime
DSP index	0.0023***	-0.0010***	0.0013***
	(0.0002)	(0.0002)	(0.0003)
Age	-0.0002	0.0002	0.0000
	(0.0002)	(0.0002)	(0.0003)
Constant	0.0224*	0.0085	0.0302*
	(0.0099)	(0.0097)	(0.0135)
DSP index (%)	14.62	-5.80	4.08
Number of individuals	542,590	542,590	542,590
Number of observations	437.7M	437.7M	437.7M

Notes. The days-since-payment index ranges in value from 0 to 1, where 0 indicates the payout day, and 1 the last day before payment. The model specification includes day-of-the-week, calendar month, year, and municipality dummies. Standard errors are clustered by municipality, year, and calendar month, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Conclusion 4.6

This study assesses the extent to which crime among welfare recipients is affected by the monthly welfare payment cycle. Our unique data allow us to follow benefits receipt and criminal behavior of individual welfare recipients at the daily level. The availability of individual-level administrative data on the entire registered welfare population of 16 of the largest Dutch municipalities, furthermore facilitates the inclusion of individual fixed effects to control for unobserved time-invariant heterogeneity. To avoid bias from municipality-specific, and shared time-varying external influences, we exploit exogenous variation in welfare payment dates over time and across municipalities. We estimate various fixed effects model specifications, as well as a standard linear probability and probit model, for crime in general, financially-motivated crime, and other (non-financially-motivated) crime.

We find evidence of an increase in supplementation of income through crime towards the end of the welfare month, as financially-motivated crime increases by 17% over the payment cycle. Conversely, other offenses peak directly after benefits receipt, and decrease by 6% over the welfare month, which may be attributable to a spike in consumption complementary to criminal behavior, such as alcohol and illicit drugs. These inversive effects amount to an increase of 5% in crime in general over the payment cycle. Although higher baseline crime rates produce larger absolute changes in offenses among younger age groups and men, the relative effects are highly comparable across subsamples. For women, however, we find null effects on other crime.

The estimation results support two distinct theoretical economic causal mechanisms, derived from the first line of evidence on the relationship between welfare disbursement and consumption by welfare recipients. While the increase in financially-motivated crime over the payment cycle contrasts the permanent income hypothesis (Friedman 1957), it is in line with evidence on the inability of welfare recipients to sustain consumption towards the end of the welfare month (e.g. see Damon et al. 2013, Hamrick and Andrews 2016, Hastings and Washington 2010, Mastrobuoni and Weinberg 2009, Shapiro 2005, Wilde and Ranney 2000). Having insufficient means of subsistence available may prompt recipients to commit offenses from which financial gains can be obtained, to supplement their income. This is in line with Becker's rational choice theory (Becker 1968, Ehrlich 1973), which states that individuals rationally determine their behavior by weighing the perceived costs and benefits. The relative financial gains from crime increase when the available financial means are reduced towards the end of the welfare month. General strain theory predicts a similar pattern in non-financially-motivated offenses over the welfare month, as an increase in financial stress would increase crime in general as a coping mechanism (Agnew 1992). However, we find such offenses to peak directly after payment receipt, in line with related findings by Watson et al. (2019). This spike in non-financially-motivated offenses upon benefits receipt supports routine activity theory (Cohen and Felson 1979), as it is likely attributeable to a spike in consumption complementary to criminal behavior (e.g. alcohol, illicit drugs, and certain leisure activities). While not directly measured in this study, there is a notable body of evidence on such a spike in consumption, caused by the income shocks upon welfare benefits disbursement (see Castellari et al. 2017, Damon et al. 2013, Shapiro 2005, Stephens Jr 2003, Wilde and Ranney 2000).

Complementary to the existing literature, the availability of individual-level data allows us to estimate effects for welfare recipients at the individual-level (as opposed to aggregate crime rates). This enables us to investigate underlying theoretical mechanisms, which are more obscured in aggregate data. A related study by Foley (2011) finds increases in city-level rates of crime in general and financially-motivated crime over the welfare month. Contrary to our results, however, he finds non-financially-motivated crime to be unaffected. Direct comparison to our findings is not without flaws due to the difference in level of analysis. However, if the findings by Foley hold true at the individual-level, a potential explanation may lie in the comparatively generous Dutch welfare system, as the higher benefits levels produce larger spikes in the available financial means of welfare recipients upon disbursement. Subsequently, the theoretical causal mechanism of a spike in consumption complementary to criminal behavior may also be larger. While this may explain our findings for other crime, this study's effect size on financially-motivated crime (17%) is comparable to the effect found by Foley (14%). Despite the comparatively high benefits levels in the Netherlands, our findings indicate that welfare recipients face financial shortfalls towards the end of the month.

Finally, overall, we find little heterogeneity in the effects of welfare benefits disbursement on crime. Relative to their baseline rates, the effect sizes are highly comparable across age and sex. An explanation for the higher crime rates among men and the younger age groups may therefore not lie in a lower ability to smooth consumption, but in a higher prevalence of other criminogenic factors (e.g. lower self-control, opportunity costs, patience and risk aversion, see Kruttschnitt 2013, Loeber and Farrington 2014, Steffensmeier and Allan 1996). One exception is that the effects of welfare benefits disbursement on other, non-financially-motivated crime differ across sex. As opposed to men, we do not find other crime to be affected among women. This may simply be attributable to the fact that women commit very little violent offenses as compared to men, which constitute the majority of other crime. Nonetheless, as a crime prevention strategy, welfare disbursement policy changes targeting (young) men are likely to be the most effective.

Our findings indicate that welfare recipients face financial shortfalls towards the end of the month, despite comparatively high benefit levels. As this can be attributed to inadequate smoothing of consumption, welfare policy directly targeting consumption smoothing could potentially reduce crime. A viable crime prevention strategy may lie in disbursing welfare benefits more frequently than once monthly, which would effectively shorten the time window over which recipients are required to smooth consumption. Such measures may not only reduce the severe financial shortfalls that welfare recipients often face towards the end of the month, but also the size of the spikes in available financial means upon disbursement (i.e. the 'full wallet' effect). As such, both the increase in financially-motivated crime over the welfare payment cycle, as well as the size of the spikes in other offenses upon disbursement may be reduced. While the latter may increase in frequency, Hsu (2017) find that an increase in disbursement frequency causes spikes in domestic violence upon disbursement to disappear. In other words, the wallet may be full more frequently, but less full, leaving less money to be spent on non-essential consumption complementary to crime. However, while this could be an effective crime prevention strategy, it also reduces recipients' financial autonomy. The costs and benefits of the constraint of financial autonomy should therefore be comprehensively considered in the formation of such policy.

This study shows welfare benefits disbursement to affect criminal behavior among welfare recipients, and puts forth a viable policy response. The relevance of these insights is emphasized by the recent trend in several Western countries to cut social expenditures by reducing welfare generosity and accessibility (e.g. Dahlberg et al. 2009, Hernæs et al. 2017, Ministerie van Financiën 2020). Determining the optimal disbursement strategy could potentially reduce the substantial societal costs of crime, without notably increasing welfare expenditures. While further research is warranted to assess the efficacy of such a policy as a crime prevention strategy, this study contributes to a more comprehensive overview of the costs and benefits of welfare payment regimes.

5 Crime state dependence and employment: Modelling feedback effects to investigate crime continuity

Abstract

Substantial evidence suggests that past criminal behavior is one of the most significant predictors of future crime. However, estimating crime state dependence effects is empirically challenging due to unobserved population heterogeneity. By explicitly modelling feedback effects from past crime on current employment in a joint dynamic model, this study investigates crime state dependence and the role of employment. To control for unobserved population heterogeneity, we apply a correlated random effects bivariate probit model over individual-level data on a randomly-selected 5% subset of young adults in the Netherlands. Through this approach, we find subtantial state dependence effects across crime outcomes and sex, ranging from an increase of 47% for male non-financially-motivated crime to an increase of 100% for female financially-motivated crime. Reduced labor market opportunities following an offense appear to only minimally contribute to crime state dependence among men.

5.1 Introduction

Two-thirds of all registered yearly crime suspects in the Netherlands are repeat offenders (Statistics Netherlands 2020c). As prior criminal behavior is one of the most significant predictors of future criminal behavior (e.g. Campbell et al. 2009, Farrington 1998, Gendreau et al. 1996), a prominent question in criminological research is to what extent this relationship is causal or spurious. The substantial positive correlation between past – and future crime may be partially attributable to population heterogeneity, as individuals differ in personal characteristics that determine their latent tendency to commit crime (e.g. self-control). To the extent that these characteristics persist over time, they will also affect offending in future periods (i.e. *spurious* autocorrelation). However, past criminal behavior may also *causally* affect future criminal decision making (i.e. true state dependence). A prominent theoretical pathway through which such state dependence effects could arise is through adverse effects on employment. Past crime may reduce labor market opportunities, which in turn could incentivize individuals to commit crime, for financial gains especially. By implementing a model that controls for population heterogeneity and explicitly allows for potential feedback effects from past crime to current employment, this study investigates the role of employment in crime state dependence.

Seminal work by Heckman (1981b, 1991) first distinguished between population heterogeneity and state dependence as mechanisms underlying behavioral continuity. In following, criminological research has produced mixed results as to what extent these mechanisms explain the positive correlation between prior and future offending (see Nagin and Paternoster 2000). Some early studies find population heterogeneity to solely be the cause (e.g. Nagin and Paternoster 1991, Paternoster and Brame 1997), whereas findings from other studies support state dependence (e.g. Nagin and Farrington 1992a,b, Paternoster et al. 1997). The mixed findings from these studies may be attributeable to differences in data and sample characteristics. Population heterogeneity is supported by evidence based on administrative data on high-risk samples (i.e. ex-offenders), whereas state dependence is supported by evidence based on survey data on general population samples. One Dutch study by Blokland and Nieuwbeerta (2010a) finds support for both population heterogeneity and state dependence effects, by combining survey data on a general population sample with administrative data on a high-risk sample through hierarchical linear modeling. Quasi-experimental research using administrative data on a large general population sample, however, is scarce.

One pathway for crime state dependence is through adverse effects on employment. Prior criminal behavior may knife off opportunities for stable employment through multiple 'scarring' mechanisms.[1] Firstly, the acquirement of a criminal record may substantially reduce labor market opportunities (e.g. Pager et al. 2009, Selbin et al. 2018, Uggen et al. 2014). Signalling theory argues that a criminal record may signal certain undesirable personal characteristics towards potential employers, such as low discipline and work competency (Spence 1973). To a lesser extent, this also holds true for unemployment spells (Holzer et al. 2004), which can be the result of investing in a criminal career, as well as penal intervention (such as imprisonment). These voluntary or forced absences from the labor market may additionally adversely affect the accumulation of human capital and, consequently, employment opportunities (see Becker 2009). There is, however, a paucity in research into the labor market scarring effects of criminal behavior outside of the US. Compared to the Netherlands and most other European countries, criminal records are more accessible in the US (see Corda and Lageson 2020),[2] custodial sanctions are more often imposed, and average prison terms are (much) longer (see Aebi and Tiago 2020, Kaeble 2018, Motivans 2020). Hence, it remains unclear to what extent these findings can be generalized to other contexts.

Labor market scarring by criminal behavior may play an important role in crime state dependence, as a substantial body of micro-level evidence shows that being employed reduces criminal behavior (see Apel et al. 2008, Apel and Horney 2017, Van der Geest et al. 2011, Ramakers et al. 2020, Uggen 2000).[3] This reduction in crime may be achieved through various theoretical economic (Chalfin and Raphael 2011) and sociological (Lageson and Uggen 2013) mechanisms. From an economic perspective, the legiti-

[1]See Apel and Sweeten (2010), Bernburg and Krohn (2003), De Li (1999), Dobbie et al. (2018), Lopes et al. (2012), Pager et al. (2009), Selbin et al. (2018), Uggen et al. (2014).

[2]Criminal records are only accessible to criminal justice actors in the Netherlands. However, certain professions and employers may require a so-called 'certificate of conduct', and a criminal conviction can be ground to dismiss an application for this certificate. The aim of these regulations is to reduce labor market discrimination of ex-offenders, and evidence suggests that criminal records fulfill only a limited role in hiring decisions in the Netherlands (Van den Berg et al. 2017, Dirkzwager et al. 2015). Furthermore, a recent study by Ramakers (2020) shows that only 6% of Dutch ex-prisoners apply for a certificate of conduct in the first four years upon re-entry, of which approximately one-third is granted.

[3]There is also substantial evidence on effects of employment and and labor market prospects on aggregate crime rates (e.g. Gould et al. 2002, Lin 2008, Machin and Meghir 2004, Raphael and Winter-Ebmer 2001).

mate income from employment may reduce the relative financial benefits from financially-motivated crime (Becker 1968, Ehrlich 1973), as well as financial-strain-induced emotional stress conducive to criminal behavior in general (Agnew 1992). Employment can also have an incapacitative effect, by limiting ones time and opportunity to commit crime overall (Cohen and Felson 1979). By providing structure, responsibility and social bonds with non-deviant peers, a stable work environment may also reduce crime in general (e.g. Hirschi 1969, Laub and Sampson 1993). By reducing employment opportunities, prior criminal behavior may cause further criminal behavior through removal of such crime prevention mechanisms.

Previous studies that dynamically model crime state dependence through employment are scarce, and are forced to make concessions due to computational challenges. Imai and Krishna (2004) estimate a dynamic discrete-choice structural model on data from the 1958 Philadelphia birth cohort study,[4] to unveil a substantial deterrent effect of anticipated adverse labor market consequences on current criminal behavior. In addition to evidence of state dependence and population heterogeneity causing continuity in criminal behavior, they find criminal history to adversely affect current labor market outcomes. Different crime types are likely to differ in underlying motivation and societal response, and the current choice to engage in legitimate labor market activities is likely to correlate with both the current choice to commit crime as well as anticipated future labor market outcomes. However, while offenses differ substantially in both motivation and response by the criminal justice system, this study only considers crime in general. Furthermore, for computational reasons, they model the current choice to commit crime, but not the current choice to engage in legitimate labor market activities. This reciprocal effect is accommodated in a closely-related study on a high-risk sample of 270 Dutch young adult male ex-offenders (Mesters et al. 2016). By simultaneously modelling crime, employment, and welfare receipt, in a dynamic discrete-choice model, they find evidence of crime state dependence through employment. Potentially due to a difference in the societal or legal response, the adverse effect of violent crime on future employment appears to be substantially

[4]The 1958 Philadelphia birth cohort study collected data on the criminal careers of all boys born in 1945 who lived in Philadelphia from the age of 10 to 18.

larger than property crime. Conversely, they find employment to only cause a statistically significant reduction in property offenses. The current study aims to assess to what extent these findings apply to a general population of young adults.

To investigate the causal pathway of crime state dependence, this study employs a joint dynamic model of crime and employment that explicitly accommodates feedback effects from past crime on current employment. Facilitated by the availability of unique individual-level administrative data (2006-2017), we apply a correlated random effects bivariate probit model over a balanced panel of a randomly-selected 5% subset of young adults in the Netherlands (aged 18 to 29 in 2006). This approach allows us to assess to what extent crime state dependence works through adverse effects of past crime on employment, which is empirically challenging as the past-crime–future-crime relationship as well as employment status are highly endogenous. To control for all time-invariant observed and unobserved heterogeneity, we model individual-specific effects in the form of including individual-level correlated random effects and initial employment – and crime conditions. By instrumenting employment status on regional unemployment rates, we furthermore exploit exogenous variation in employment caused by regional labor market conditions. To investigate heterogeneous effects, we run the analyses separately for financially-motivated offenses and other (non-financially-motivated) offenses among the men and women.

After controlling for population heterogeneity, we find substantial state dependence effects for both financially-motivated crime and other crime. Financially-motivated criminal behavior in the prior month increases the monthly probabilities of such an offense by 92% and 100%, for men and women respectively. We find the other crime state dependence effect to be almost twice as large for women (78%) as compared to men (47%). Conversely, employment only appears to limitedly function as a causal pathway for crime state dependence among men. We find prior crime to reduce current employment among men slightly (-1%), whereas employment among women is unaffected. Furthermore, we find employment to only reduce financially-motivated crime among women (23%), whereas employment reduces both financially-motivated – (25%), and other crime (9%) among men.

The contribution of this study to the literature is threefold. First, complementary to the existing literature, we investigate crime state dependence through the use of a joint dynamic model, as opposed to dynamic discrete response models with unobserved heterogeneity. Hence, this study avoids the highly-restrictive exogeneity assumption, which does not allow the outcome of dependent variables to influence future outcomes of the regressors. This assumption is unlikely to hold as employment is likely to be influenced by past crime outcomes. Second, the availability of a vast individual-level administrative dataset enables us to investigate crime state dependence among a general, young adult sample. These data additionally enable us to separately investigate young adult women, whereas the existing literature focuses on higher-risk young male (ex-offender) samples (see Imai and Krishna 2004, Mesters et al. 2016). Criminal behavior, however, differs substantially between the sexes (e.g. see Steffensmeier and Allan 1996), and evidence also suggests that crime state dependence is heterogeneous across sex (Andersson 1990, Gushue et al. 2020, Mazerolle et al. 2000). Hence, this study adresses the paucity in crime literature focused on women, and contributes to the ongoing discussion of whether the same factors influence male and female crime through similar mechanisms (see Kruttschnitt 2013, Steffensmeier and Allan 1996). Third, the Netherlands offers a different context to assess crime state dependence, whereas most of the existing research focuses on the US. The Netherlands and most other European countries have comparatively lenient criminal justice systems (see Aebi and Tiago 2020, Kaeble 2018, Motivans 2020), and comparatively inaccessible criminal records (see Corda and Lageson 2020). As such, this study is able to assess the generalizability of US estimates for the labor market scarring effects of past crime to other contexts.

Below, Section 5.2 will first discuss the data, sample selection and relevant descriptive statistics. Section 5.3 describes the empirical model, after which we discuss the baseline estimation results in Section 5.4. We conclude and discuss the implications of our findings in Section 5.5. Appendix 5.A contains a comparison of the estimates obtained from our baseline joint dynamic model to those obtained from a standard dynamic probit approach.

Data 5.2

This study uses longitudinal individual-level administrative data from Statistics Netherlands on a randomly-selected subset of the Dutch population.[5] The monthly-level data covers the period of 2006 until 2017, which amounts to a twelve-year observation window.

Administrative data on crime are derived from crime reports of the Dutch law enforcement 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). The employment data are derived from the Employee Insurance Agency (i.e. 'UWV'), which is the central Dutch administrative authority that registers all employee insurances. As we suspect employment status to be endogenous, we use the regional unemployment rate to instrument employment. The regional unemployment data cover yearly unemployment rates at the 'COROP' level, which is a commonly used geographical division of the Netherlands into 40 supra-municipal regions.

Sample and descriptive statistics 5.2.1

This study focuses on investigating crime state dependence among young adults. To this end, we generate a balanced panel by randomly selecting 5% of all registered inhabitants of the Netherlands aged 18 to 29 in the year 2006. This approach produces a full sample of 93,428 individuals (aged

[5]While Statistics Netherlands provides data on the entire registered population of the Netherlands, it is computationally infeasible to estimate our models on such a large sample. 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 bijstanduitkeringtab, bijstanduitkeringint, bus, gbaadresobjectbus, gbapersoontab, integraal huishoudens inkomen, integraal persoonlijk inkomen, polisbus, spolisbus, verdtab and vslgwbtab.

18-40), and 13,453,632 monthly observations between January 2006 and December 2017. This large sample size enables us to investigate potential heterogeneity across sex, by running the analyses for men and women, separately.

Prior studies underline the importance of investigating the role of crime state dependence during early adulthood and across sex. Developmental criminological theory posits young adulthood to be an important life stage, due to a divergence that occurs in offending behavior (see Moffitt 1993). So-called 'adolescence-limited' offenders desist from criminal behavior around this age, whereas 'life-course persistent' offenders continue offending. Individuals also commonly enter the labor market around this age, which may make them particularly vulnerable to potential adverse effects of prior criminal behavior on labor market prospects. Sex differences in the developmental trajectories of criminal behavior have also become a focus of criminological research and theory (see Moffitt and Caspi 2001). Evidence suggests that men are more often repeat offenders (Moffitt and Caspi 2001), and that crime state dependence processes differ between men and women (Andersson 1990, Gushue et al. 2020, Mazerolle et al. 2000). More broadly, the underrepresentation of women in repeat offending, and offending in general (Steffensmeier and Allan 1996), has resulted in a paucity of research into crime state dependence among women. To adress this paucity, this study investigates heterogeneity across sex in the role of employment in crime state dependence during this transitionary life stage.

Table 5.1 presents descriptive statistics for the selected samples. To first explore the relationship between prior – and current crime and employment, we include monthly rates conditional on positive or negative lagged values of financially-motivated crime, other crime, and employment.[6]

While financially-motivated crime accounts for more than half of all crime committed in the Netherlands (Statistics Netherlands 2020a), we find other offenses to be more prevalent among young adults. In the full sample, we find 1.51% of individuals to commit non-financially-motivated crime in any given year within the observation window. This yearly rate

[6]Other crime is defined as any criminal offense for which no theoretical financial incentive can be identified.

is more than two times higher than the rate for financially-motivated crime (0.61%). We find much lower rates at the monthly level, with 0.06% and 0.15% for financially-motivated and other crime, respectively. If an individual has committed crime in the previous month, however, these rates increase substantially to 6.64% for financially-motivated crime and 4.03% for other crime. Conversely, the average employment rate of 78.24% is substantially lower among individuals who have committed financially-motivated crime (42.63%) or other crime (58.13%) in the previous month. Compared to unemployed individuals, those who are employed show substantially lower rates for other crime (0.11% vs 0.28%), and financially-motivated crime especially (0.03% vs 0.16%). We find a *gross* average personal primary income of €31,411 and a *net* average standardized household income of €25,779.

A comparison of the sexes shows that the gap in crime rates is especially large for other crime, as the monthly rate is around six times higher for men (0.25%) than for women (0.04%). This is likely mainly attributable to the overrepresentation of men in violent crime (Statistics Netherlands 2020b). While the gap narrows when we consider financially-motivated crime, we still find that men commit these offenses approximately three times as often as women, with monthly rates of 0.09% and 0.03%, respectively. The relative difference between men and women decreases when we consider rates conditional on having committed crime the previous month, both for financially-motivated crime (7.30% vs 4.62%) and other crime (4.29% vs 2.48%). This is in line with evidence that shows that although repeat offenders are much more prevalent among men, offending frequency among male and female repeat offenders is more comparable (Moffitt and Caspi 2001). Apart from repeat offenders, we find the highest crime rates among unemployed men, and the lowest crime rates among employed women. However, women are less often employed than men (77.09% vs 79.42%), and earn much less on average (€37,184 vs €25,749).

In line with our expectations, we find substantially higher crime – and lower employment rates if individuals have committed crime in the previous month, for both men and women. The differences in conditional crime rates are especially large, as crime in the prior month increases the current monthly rate by around factors of 20 up to 100. The size of these differences emphasize the importance of investigating as to what extent this is attributable to population heterogeneity, or true state dependence.

Table 5.1: Descriptive statistics, 2006-2017

	Full sample	Men	Women
Financially-motivated crime (yearly, %)	0.61	0.90	0.31
Financially-motivated crime (monthly, %)	0.06	0.09	0.03
if financially-motivated crime$_{t-1}$ = 1	6.64	7.30	4.62
if financially-motivated crime$_{t-1}$ = 0	0.06	0.08	0.03
if employment status$_t$ = 1	0.03	0.05	0.02
if employment status$_t$ = 0	0.16	0.25	0.08
Other crime (yearly, %)	1.51	2.59	0.46
Other crime (monthly, %)	0.15	0.25	0.04
if other crime$_{t-1}$ = 1	4.03	4.29	2.48
if other crime$_{t-1}$ = 0	0.14	0.24	0.04
if employment status$_t$ = 1	0.11	0.19	0.03
if employment status$_t$ = 0	0.28	0.49	0.08
Employment (monthly, %)	78.24	79.42	77.09
if employment status$_{t-1}$ = 1	98.33	98.39	98.28
if employment status$_{t-1}$ = 0	6.03	6.29	5.79
if financially-motivated crime$_{t-1}$ = 1	42.63	43.43	40.19
if financially-motivated crime$_{t-1}$ = 0	78.28	79.47	77.12
if other crime$_{t-1}$ = 1	58.13	59.12	52.18
if other crime$_{t-1}$ = 0	78.29	79.49	77.12
Native (%)	79.70	81.26	78.16
Welfare dependency rate (monthly, %)	2.94	2.21	3.65
Annual personal primary income	31,411	37,184	25,749
Annual standardized household income	25,779	26,387	25,183
Number of individuals	93,428	46,266	47,162
Number of observations	13,453,632	6,662,304	6,791,328

Empirical methodology 5.3

To estimate true crime state dependence, this study employs a joint dynamic model following the framework by Wooldridge (2000) and Biewen (2009), in the form of a correlated random effects bivariate probit. As we expect employment to have an important reciprocal relationship with crime, we explicitly accommodate feedback effects from past crime to contemporaneous employment. However, time-invariant unobserved heterogeneity affecting criminal behavior across time, makes estimating the effect of past crime on current crime particularly empirically challenging. Additionally, unobserved heterogeneity in particular individual characteristics may affect both criminal behavior, as well as employment status. This study adresses these endogeneity problems by instrumenting employment status using regional unemployment rates, and by estimating individual-specific effects. The individual-specific terms accommodate unobserved time-invariant determinants of crime, such as self-control, morality and intelligence. We account for correlation between individual-specific effects and the initial crime condition following Wooldridge (2005).[7]

Let y_{it} indicate individual crime status, and let w_{it} denote whether individual i is employed. Then the joint density of y_{i1},..., y_{iT}, w_{i1},..., w_{iT} given exogenous variables z_{it}, initial values y_{i0}, w_{i0}, and individual-specific effects c_{ij}, can be written as

$$f(y_{i1}, ..., y_{iT}, w_{i1}, ..., w_{iT} | z_{it}, y_{i0}, w_{i0}, c_{ij}, \theta, \gamma, \beta) \tag{5.1}$$

$$= \prod_{t=1}^{T} \Phi_2[(2y_{it} - 1)(\theta_1 z_{it} + \theta_2 w_{it} + \theta_3 y_{it-1} + c_{i1}), \tag{5.2}$$

$$(2w_{it} - 1)(\gamma_1 z_{it} + \gamma_2 y_{it-1} + \gamma_3 w_{it-1} + c_{i2}), \tag{5.3}$$

$$(2y_{it} - 1)(2w_{it} - 1)\rho]$$

In addition to initial crime condition values y_{i0}, the individual-specific effects c_{ij} include initial values for employment status w_{i0}. Through this approach, we avoid bias from unobserved personal characteristics that

[7]The implemented approach proposed by Wooldridge (2005) is a simplified alternative to the commonly implemented solution to the initial conditions problem by Heckman (1981a).

are correlated to z_{it}, as well as unobserved factors correlated to the initial states of crime y_{i0} and employment w_{i0}. The individual-specific effects are specified as follows:

$$c_{ij} = \alpha_{0j} + \alpha_{1j} y_{i0} + \alpha_{2j} w_{i0} + \alpha_{3j} \bar{z}_i + \alpha_{4j} y_{i0} \bar{z}_i + \alpha_{5j} w_{i0} \bar{z}_i + \alpha_{ij} \tag{5.4}$$
$$\text{For } j = 1, 2$$

Where it is assumed that the joint density of the random effects error terms follows a bivariate normal distribution.

Equation (5.3) includes lagged crime y_{it-1}, capturing potential feedback effects from crime on employment. Combined with the inclusion of contemporary employment status w_{it} in equation (5.2), this approach captures potentially adverse effects of crime on labor market opportunities, and vice versa. Equation (5.3) also accounts for state dependence effects in employment, and for possible correlations between employment status and unobserved time-invariant determinants of crime status c_{i2}. To avoid logical inconsistency, we exclude current crime from this equation (see Maddala 1986). We run the analyses separately for financially-motivated crime and other crime.

To further control for endogeneity, we instrument employment status on regional unemployment rates. We find the regional unemployment rate to be a suitable instrumental variable, as we consider both of the assumptions of instrument relevance and instrument exogeneity to hold. First, we find the regional unemployment rate to cause a sufficient amount of variation in employment (as will be discussed in Section 5.4). Second, we are unable to conceive of mechanisms through which the regional unemployment rate might affect criminal behavior other than through its effect on employment. Multiple prior studies have also used unemployment rates to instrument for various endogenous variables of interest. For example, local unemployment rates have previously been used as an instrumental variable to study the effect of youth unemployment on future wages and employment (Gregg 2001, Gregg and Tominey 2005), industry-age-year unemployment rates have been used to investigate the effect of becoming unemployed on psychological health (Gathergood 2013), and state unemployment rates have been used to estimate returns to schooling (Arkes 2010).

The correlated random effects bivariate probit model endogenizes employment status w_{it} upon which crime status is conditioned. Simulation evidence shows that when the average probability of the dependent variable is close to 1 or 0 (which clearly applies to crime outcomes in this study), this approach is preferable to (fixed effects) two-step or linear probability estimators (Bhattacharya et al. 2006, Chiburis et al. 2012).

To determine the absolute size of the crime state dependence and feedback effects, we calculate average partial effects (APEs). APEs respresent the change in the dependent variable from a one unit increase in the explanatory variable averaged over the distribution of other population characteristics. For the baseline model, the APE of the crime state dependence effect is given by:

$$\begin{aligned} APE = E[&P(y_{it} = 1|z_{it}, w_{it}, y_{it-1} = 1, w_{it-1}, y_{i0}, w_{i0}) \\ &-P(y_{it} = 1|z_{it}, w_{it}, y_{it-1} = 0, w_{it-1}, y_{i0}, w_{i0})] \end{aligned} \tag{5.5}$$

While the inclusion of separate individual-specific effects c_{ij} for the crime and employment equations avoids the imposition of severe restrictions on cross-equation unobserved correlations, it does impose a serious computational burden. On the available system, it is consequently not computationally feasible to estimate the model over the full 5% subset of the population at once. To adress this issue, we employ a minimum distance approach to combine estimates obtained from five separate regressions over 1% subsets. Another limitation related to computation feasiblity is that the model does not accommodate error term serial correlation. Evidence from a related study to estimate welfare benefits receipt state dependence, however, suggests that controlling for serial correlation has little effect (Chay and Hyslop 2014). Standard errors are clustered at the individual level, to account for the group structure induced by potential specification errors and prevent overstatement of the significance of the estimated effects. To avoid potential bias from non-exogenous panel attrition, we fit the models on balanced panel data (see Biewen 2009). Finally, to assess the extent to which feedback of prior crime on employment influences our estimates, we compare the results from the baseline model to those obtained from a dynamic correlated random effects probit model in Appendix 5.A.1 (e.g. see Wooldridge 2010).

5.4 Estimation results

Tables 5.2 and 5.3 present the estimation results produced by the baseline joint dynamic model, for financially-motivated crime and other crime among men and women separately. To enhance the interpretability of the estimation results, we compute the average partial effects (APEs) for the variables of interest. The APEs indicate how much the dependent variable changes due to a one unit increase in the variable of interest averaged over the distribution of other population characteristics. The statistically significant estimates for the unemployment rate ($p<.001$) suggest that the regional unemployment rate is a sufficiently strong instrument for employment. The APEs show that a one percentage point increase in the regional unemployment rate reduces employment by approximately 0.09 percentage points.

Table 5.2 shows statistically significant and sizeable positive estimates for the financially-motivated crime state dependence effects, for both men (0.5449) and women (0.5721). This indicates that past criminal behavior substantially increases future criminal behavior, even after controlling for observed and unobserved population heterogeneity. The crime equation estimates furthermore show that employment significantly reduces financially-motivated crime (-0.1727 and -0.1631 for men and women, respectively). However, when we consider the employment equation estimates, we find prior financially-motivated crime to significantly reduce employment among men only (-0.1332). Employment therefore does not appear to be a pathway for financially-motivated crime state dependence among women.

The APEs show that prior financially-motivated crime increases current financially-motivated crime by 92% and 100%, for men and women respectively. In other words, committing a financially-motivated offense in the prior month approximately doubles the probability of committing such an offense in the current month. This relative increase is slightly larger for women as compared to men. In absolute terms, however, the increase is more than twice as large for men (0.0877 vs 0.0357 percentage points). This is attributable to the substantially higher baseline rate for men. While the state dependence effects are very large, they are dwarfed by the differences in the corresponding conditional crime probabilities, shown in Table 5.1. This implies that population heterogeneity explains a substantial proportion of the positive correlation between past – and future crime. The relative reduction in financially-motivated crime by employment is also comparably-sized across sex, whereas the absolute reduction is around three times as large for men (-0.0289 percentage points or 25%) as compared to women (-0.0095 percentage points or -23%). In addition to the small and statistically non-significant effect for women, we find the reduction by prior crime on employment among men to be very limited in size (-0.94%). Despite the substantial inverse effects of employment on financially-motivated crime, this leads to the conclusion that the role of employment in crime state dependence among men is limited.

Table 5.3 presents the estimates for other (non-financially-motivated) crime among men and women. In line with financially-motivated crime, the other crime state dependence estimates are statistically significant and sizeable for both men (0.2165) and women (0.3385). Employment, however, only significantly reduces other crime among men (-0.0441). Furthermore, the feedback effect of prior other offenses on current employment is only statistically significant for men (-0.0836). In line with the findings for financially-motivated crime, employment therefore does not appear to be a pathway for other crime state dependence among women. Conversely, relative to their baseline rates, the APEs show that the other crime state dependence effect is almost twice as large for women (78.19% or 0.0333 percentage points) as compared to men (46.67% or 0.1194 percentage points). As opposed to the relatively small and statistically non-significant effect for women, we find employment to noteably reduce other crime among men (-9%). The small size of the feedback effect of prior other crime on employment (-0.57%), however, also limits the role of employment in state dependence among men.

To summarize, the estimation results show that crime state dependence effects are substantial across sex and crime outcomes. Across sex, we find comparably-sized relative state dependence effects for financially-motivated crime, while for other crime the effect is almost twice as large for women. Interestingly, employment appears to only reduce financially-motivated crime among women, while it reduces all crime among men. As we furthermore do not find statistically significant feedback effects of any prior crime on current employment for women, employment does not appear to be a pathway for crime state dependence among women. While statistically significant, we find the feedback effects of prior crime on current employment among men to be limited in size. Despite the noteable reductions by employment in male crime, we therefore conclude that employment fulfills only a limited role in male crime state dependence.

Table 5.2: Correlated random effects bivariate probit estimates for financially-motivated crime

	MEN		WOMEN	
Financially-motivated crime				
Crime_{t-1}	0.5449***	(0.0323)	0.5721***	(0.0559)
Employment	-0.1727***	(0.0174)	-0.1631***	(0.0277)
Crime_{i0}	-5.8558	(6.5650)	-14.1145	(11.5638)
Employment_{i0}	0.3901*	(0.1516)	-0.1360	(0.2129)
Crime_{i0}*employment_{i0}	43.8266***	(1.8378)	53.7407***	(3.9681)
Crime_{i0}*$\overline{\text{age}}$	0.7053**	(0.2258)	1.5161***	(0.3842)
Employment_{i0}*$\overline{\text{age}}$	-0.0263***	(0.0051)	-0.0110	(0.0071)
Age	-0.0300***	(0.0017)	-0.0130***	(0.0024)
$\overline{\text{Age}}$	0.0322***	(0.0044)	-0.0045	(0.0054)
Native	-0.2144***	(0.0124)	-0.1056***	(0.0189)
Constant	-3.1829***	(0.1224)	-2.9465***	(0.1501)
Employment				
Unemployment rate	-0.0169***	(0.0012)	-0.0156***	(0.0012)
Crime_{t-1}	-0.1332***	(0.0350)	-0.0989	(0.0600)
Employment_{t-1}	3.0628***	(0.0056)	3.0822***	(0.0052)
Crime_{i0}	-0.9759	(1.9494)	-4.9841	(4.1490)
Employment_{i0}	-0.3198***	(0.0750)	0.1651*	(0.0701)
Crime_{i0}*employment_{i0}	-10.5999***	(0.7739)	-7.8417***	(1.4165)
Crime_{i0}*$\overline{\text{age}}$	0.0597	(0.0668)	0.1180	(0.1479)
Employment_{i0}*$\overline{\text{age}}$	0.0780***	(0.0026)	0.0584***	(0.0024)
Age	0.0020**	(0.0006)	-0.0037***	(0.0006)
$\overline{\text{Age}}$	-0.0580***	(0.0022)	-0.0359***	(0.0020)
Native	0.0725***	(0.0052)	0.0515***	(0.0049)
Constant	-0.8061***	(0.0607)	-1.2598***	(0.0559)
Random effects parameters				
Crime	0.4117	(0.0290)	0.4210	(0.0284)
Employment	0.3494	(0.0043)	0.3434	(0.0041)
Correlation	-0.3170	(0.0361)	-0.2129	(0.0567)
Average partial effects				
Financially-motivated crime				
Crime_{t-1} (%point)	0.0877***	(0.0061)	0.0357***	(0.0041)
Crime_{t-1} (%)	91.96		100.35	
Employment (%point)	-0.0289***	(0.0032)	-0.0095***	(0.0017)
Employment (%)	-24.92		-23.03	
Employment				
Unemployment rate (%point)	-0.0909***	(0.0067)	-0.0864***	(0.0066)
Unemployment rate (%)	-0.11		-0.11	
Crime_{t-1} (%point)	-0.7510***	(0.1910)	-0.6673	(0.3487)
Crime_{t-1} (%)	-0.94		-0.86	
Observations	6,662,304		6,791,328	
Individuals	46,266		47,162	

Notes. Standard errors are clustered by individual, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 5.3: Correlated random effects bivariate probit estimates for other crime

	MEN		WOMEN	
Other crime				
Crime$_{t-1}$	0.2165***	(0.0212)	0.3385***	(0.0613)
Employment	-0.0441***	(0.0111)	-0.0316	(0.0231)
Crime$_{i0}$	1.0354	(3.6953)	-9.2386	(8.4454)
Employment$_{i0}$	0.5767***	(0.1001)	-0.3246*	(0.1646)
Crime$_{i0}$*employment$_{i0}$	17.6316***	(0.8746)	35.6382***	(2.6887)
Crime$_{i0}$*$\overline{\text{age}}$	0.4745***	(0.1312)	1.4958***	(0.3023)
Employment$_{i0}$*$\overline{\text{age}}$	-0.0304***	(0.0033)	-0.0016	(0.0054)
Age	-0.0388***	(0.0010)	-0.0136***	(0.0019)
$\overline{\text{Age}}$	0.0463***	(0.0032)	-0.0025	(0.0046)
Native	-0.1538***	(0.0082)	-0.1305***	(0.0149)
Constant	-3.0775***	(0.0904)	-2.8569***	(0.1253)
Employment				
Unemployment rate	-0.0171***	(0.0012)	-0.0156***	(0.0012)
Crime$_{t-1}$	-0.0836***	(0.0221)	-0.0382	(0.0526)
Employment$_{t-1}$	3.0633***	(0.0056)	3.0826***	(0.0052)
Crime$_{i0}$	-0.7550	(1.4198)	2.2607	(4.6646)
Employment$_{i0}$	-0.2385**	(0.0748)	0.1670*	(0.0700)
Crime$_{i0}$*employment$_{i0}$	-7.7478***	(0.5106)	-6.9261***	(1.2969)
Crime$_{i0}$*$\overline{\text{age}}$	0.0499	(0.0485)	-0.1209	(0.1664)
Employment$_{i0}$*$\overline{\text{age}}$	0.0756***	(0.0026)	0.0584***	(0.0024)
Age	0.0021***	(0.0006)	-0.0038***	(0.0006)
$\overline{\text{Age}}$	-0.0573***	(0.0022)	-0.0358***	(0.0020)
Native	0.0688***	(0.0052)	0.0516***	(0.0049)
Constant	-0.8232***	(0.0610)	-1.2626***	(0.0560)
Random effects parameters				
Crime	0.2891	(0.0126)	0.3072	(0.0280)
Employment	0.3464	(0.0043)	0.3433	(0.0041)
Correlation	-0.2385	(0.0371)	-0.2919	(0.0605)
Average partial effects				
Other crime				
Crime$_{t-1}$ (%point)	0.1194***	(0.0117)	0.0333***	(0.0061)
Crime$_{t-1}$ (%)	46.67		78.19	
Employment (%point)	-0.0238***	(0.0061)	-0.0036	(0.0022)
Employment (%)	-8.70		-7.98	
Employment				
Unemployment rate (%point)	-0.0909***	(0.0066)	-0.0863***	(0.0066)
Unemployment rate (%)	-0.11		-0.11	
Crime$_{t-1}$ (%point)	-0.4557***	(0.1192)	-0.2900	(0.2990)
Crime$_{t-1}$ (%)	-0.57		-0.37	
Observations	6,662,304		6,791,328	
Individuals	46,266		47,162	

Notes. Standard errors are clustered by individual, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Conclusion 5.5

This study investigates crime state dependence by explicitly modelling feedback effects from past crime on current employment in a joint dynamic model. We apply a correlated random effects bivariate probit model over a balanced panel of a randomly-selected 5% subset of all Dutch citizens aged 18 to 29 in 2006, covering a twelve year observation window (2006-2017). Through this approach, we assess to what extent adverse effects of past crime on employment is a causal pathway for crime state dependence. This is empirically challenging as evidence indicates that both employment status as well as the past-crime–future-crime relationship are highly endogenous. Following Wooldridge (2005), we control for all time-invariant observed and unobserved heterogeneity by modelling individual-specific effects, including individual-level correlated random effects and initial crime – and employment conditions. We additionally exploit exogenous variation in employment caused by regional labor market conditions, by instrumenting employment status on regional unemployment rates. To investigate heterogeneous effects, we analyze financially-motivated crime and other (non-financially-motivated) crime for men and women, separately.

We find substantial state dependence effects for both financially-motivated crime and other crime, after controlling for observed and unobserved population heterogeneity. Regardless of sex, financially-motivated criminal behavior in the prior month approximately doubles the probability of committing financially-motivated crime in the current month (92% and 100% for men and women, respectively). Relative to their baseline rates, however, we find the other crime state dependence effect to be almost twice as large for women (78.19% or 0.0333 percentage points) as compared to men (46.67% or 0.1194 percentage points).

Conversely, employment appears to only fulfill a limited role in male crime state dependence. We find a statistically significant reduction in current employment among men by prior criminal behavior ($p<.001$). While this feedback effect is limited in size (-1%), we find employment to substantially reduce both financially-motivated crime (25%) and other offenses (9%) among men. For women however, we do not find feedback effects of prior criminal behavior on current employment status. Despite a notable inverse effect of employment on financially-motivated crime among women (-23%), we therefore conclude that employment does not function as a causal pathway for female crime state dependence.

The state dependence effects unveiled in this study are in line with the most closely-related study on a male-only sample (e.g. Imai and Krishna 2004, Mesters et al. 2016), and expand upon this by unveiling substantial crime state dependence among women. The estimation results for the contemporaneous effects of employment on crime mainly support Becker's rational choice theory (Becker 1968, Ehrlich 1973), over routine activity – (Cohen and Felson 1979) and social control theory (Hirschi 1969, Laub and Sampson 1993). The much larger inverse effects of employment on financially-motivated crime as compared to other offenses (in line with Mesters et al. 2016), suggest that employment mainly affects criminal behavior through an income effect (Becker 1968, Ehrlich 1973). Nonetheless, while to a lesser extent, our findings for men also support the routine activity – (Cohen and Felson 1979) and social control perspective (Hirschi 1969, Laub and Sampson 1993), from which follows that employment reduces all crime through incapacitative and social control mechanisms.

The limited effects of prior crime on future employment deviate from the substantively reduced labor market opportunities found in other studies (e.g. Apel and Sweeten 2010, Bernburg and Krohn 2003, De Li 1999, Dobbie et al. 2018, Lopes et al. 2012, Pager et al. 2009, Selbin et al. 2018, Uggen et al. 2014). This may be attributable to the focus of these studies on the US, where the criminal justice system is comparatively punitive, and criminal records are more accessible to parties outside of criminal justice actors (see Aebi and Tiago 2020, Corda and Lageson 2020, Kaeble 2018, Motivans 2020). Compared to the US, custodial sanctions are less often imposed, and the average duration of imprisonment is around ten times shorter in the Netherlands (2.6 and 4.5 years for US state and federal prisoners versus 3.8 months for Dutch prisoners, see Aebi and Tiago 2020, Kaeble 2018, Motivans 2020). Furthermore, only criminal justice actors can directly access criminal records in the Netherlands (in line with most other European countries, see Corda and Lageson 2020). Both factors may reduce adverse effects of criminal history on human capital and future labor market prospects. Hence, the prior crime–employment estimates of this study are likely more representative of the EU context.

The application of a novel joint dynamic approach to unique administrative data on a general population sample enables this study to disentangle crime state dependence through employment from crime state dependence via alternative mechanisms. However, a limitation of this study is that we are unable to further investigate heterogeneity in the prior crime–employment relationship across penal interventions. Theoretically, labor market scarring may materialize through the obtainment of a criminal record and unemployment induced by penal intervention or investment in a criminal career (see Dobbie et al. 2018, Pager et al. 2009, Selbin et al. 2018, Uggen et al. 2014). While the available data allows us to control for prior employment, we do not possess data on sentencing decisions (e.g. sanction type and severity). As evidence suggests that employment prospects worsen as crime severity (Uggen et al. 2014) and sentence length (Ramakers et al. 2014) increases, further research is warranted as to what extent the role of scarring effects in crime state dependence is heterogeneous across crime – and sanction characteristics.

Further research is also warranted to address the paucity in evidence on alternative pathways for crime state dependence. From an economic perspective, the choice to commit crime is oftenly approached as a purely objective, economic process through which individuals maximize utility defined in monetary terms (e.g. Becker 1968). As such, a rational assessment of risk-return trade-off determines the decision to pursue legal or 'illegal employment', i.e. a criminal career (Ehrlich 1973). However, this decision-making process could be influenced by various subjective, psychological factors. An extensive literature review by Piquero et al. (2011) finds the deterrent effect of potential punishment on criminal behavior to be influenced by heterogeneity in personal traits, such as moral inhibitions, impulsivity, heuristic biases, and hyperbolic discounting. Direct experience with the criminal justice system may influence an individual's perception of the celerity, certainty, and severity of punishment, and potentially reduce the deterrent effect of possible penal intervention (Stafford and Warr 1993). While multiple studies show psychological and cognitive personal traits to mediate crime state dependence (e.g. Walters 2016a,b), causal evidence of state dependence through psychological and cognitive mechanisms is scarce. This may be attributable to the considerable empirical challenges that this question poses, which ostensibly require comprehensive individual-level longitudinal data on crime as well as relevant psychological and cognitive measures.

This study furthers the insight into crime state dependence across sex, and its causal pathway. Secondary punishment by the labor market does not uniformly appear to be a factor of importance in crime state dependence, as the unveiled adverse effects of prior crime on employment are not substantive. Nonetheless, the notable reductions in financially-motivated crime from being employed emphasize the importance of labor market activation efforts. Furthermore, the substantial crime state dependence effects through alternative mechanisms also carry major policy implications. As criminal behavior causes a sizeable increase in future criminal behavior, efforts to prevent the commission of a first offense are pertinent. Such crime prevention strategies may be more cost-effective than initially apparent, as the benefits of such efforts stretch beyond the prevention of a singular offense.

5.A Standard dynamic probit model

To investigate the impact of modelling feedback of prior crime on employment on the state dependence estimates, we present estimates in this appendix obtained from standard dynamic correlated random effects probit analyses (e.g. see Wooldridge 2010). This model is specified as follows:

$$f(y_{i1},...,y_{iT}|z_{it},w_{it},c_i,\theta) = \prod_{t=1}^{T} f(y_{it}|z_{it},w_{it},y_{it-1},c_i,\theta) \tag{5.6}$$

Equation (5.6) is effectively specified analogously to the crime equation of the baseline model, including the specification of an individual-specific effect c_i (as shown in equation (5.4)). Note that, in contrast to the baseline model, this approach is subject to the strict exogeneity assumption. This means that conditional on the individual-specific characteristics c_i and the crime status in the previous period y_{it-1}, the contemporaneous crime status y_t must not be correlated to past or future values of other variables. In other words, current crime status may not affect future employment (among others). As such, the estimates produced by this approach need to be interpreted with caution, as the baseline estimates indicate that the strict exogeneity assumption does not hold. More specifically, the joint dynamic model estimates show prior crime to limitedly affect current employment, conditional on the individual-specific characteristics (as shown in Section 5.4). Furthermore, it is not possible to instrument employment and to control for employment state dependence through this approach. Nevertheless, we relax these conditions, to compare the results from the baseline joint dynamic model to those obtained from a standard dynamic approach.

Standard dynamic probit estimation results 5.A.1

A comparison of the estimates shown in Tables 5.4 and 5.5 to the baseline estimates (Tables 5.2 and 5.3), reveals that standard dynamic correlated random effects probit analysis severely overstates crime state dependence. Modelling feedback effects of prior crime on employment substantially reduces the size of the state dependence estimates, ranging from almost threefold for male other crime (0.3234 to 0.1194 percentage points) to more than fivefold for female financially-motivated crime (0.1934 to 0.0357 percentage points). Furthermore, this single-equation approach also produces estimates for employment status which are multitudes larger than the baseline estimates.

Table 5.4: Correlated random effects probit estimates for financially-motivated crime

	MEN		WOMEN	
Financially-motivated crime				
Crime_{t-1}	0.6525***	(0.0343)	0.7410***	(0.0619)
Employment	-0.3136***	(0.0132)	-0.3058***	(0.0200)
Crime_{i0}	2.2775**	(0.6985)	0.3926	(1.4291)
Employment_{i0}	0.8085***	(0.1384)	0.1104	(0.1786)
Crime_{i0}*employment_{i0}	0.0620	(0.1580)	-0.4383	(0.4532)
Crime_{i0}*$\overline{\text{age}}$	-0.0455†	(0.0238)	0.0267	(0.0455)
Employment_{i0}*$\overline{\text{age}}$	-0.0325***	(0.0048)	-0.0109†	(0.0062)
Age	-0.0339***	(0.0017)	-0.0179***	(0.0024)
$\overline{\text{Age}}$	0.0295***	(0.0042)	0.0034	(0.0052)
Native	-0.3158***	(0.0158)	-0.2000***	(0.0216)
Constant	-3.1276***	(0.1148)	-3.2024***	(0.1398)
Average treatment effects				
Crime_{t-1} (%point)	0.3555***	(0.0359)	0.1934***	(0.0358)
Crime_{t-1} (%)	493.19		801.24	
Employment (%point)	-0.0844***	(0.0044)	-0.0292***	(0.0023)
Employment (%)	-59.31		-61.83	
Observations	6,662,304		6,791,328	
Individuals	46,266		47,162	

Notes. Standard errors are clustered by individual, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 5.5: Correlated random effects probit estimates for other crime

	MEN		WOMEN	
Other crime				
Crime_{t-1}	0.3356***	(0.0208)	0.5585***	(0.0601)
Employment	-0.1508***	(0.0085)	-0.1954***	(0.0159)
Crime_{i0}	0.6310	(0.4510)	1.8484	(1.2861)
Employment_{i0}	1.0959***	(0.0962)	0.5215***	(0.1431)
Crime_{i0}*employment_{i0}	-0.1198	(0.0923)	0.1144	(0.3136)
Crime_{i0}*$\overline{\text{age}}$	0.0090	(0.0155)	-0.0311	(0.0414)
Employment_{i0}*$\overline{\text{age}}$	-0.0410***	(0.0033)	-0.0233***	(0.0049)
Age	-0.0426***	(0.0010)	-0.0203***	(0.0019)
$\overline{\text{Age}}$	0.0423***	(0.0030)	0.0175***	(0.0043)
Native	-0.2769***	(0.0108)	-0.1744***	(0.0172)
Constant	-2.8790***	(0.0826)	-3.3970***	(0.1158)
Average treatment effects				
Crime_{t-1} (%point)	0.3234***	(0.0284)	0.1618***	(0.0331)
Crime_{t-1} (%)	142.78		437.39	
Employment (%point)	-0.1058***	(0.0066)	-0.0267***	(0.0025)
Employment (%)	-33.66		-46.13	
Observations	6,662,304		6,791,328	
Individuals	46,266		47,162	

Notes. Standard errors are clustered by individual, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

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Nederlandse samenvatting

Onderzoek naar bijstandsuitkeringen, arbeidsparticipatie, en criminaliteit

Dit proefschrift omvat vier studies met betrekking tot *bijstandsuitkeringen, arbeidsparticipatie, en criminaliteit*, welke onafhankelijk van elkaar gelezen kunnen worden. Deze vier studies hebben als doel om bij te dragen aan onze kennis van de neveneffecten op criminaliteit van bijstandsontvangst, actief arbeidsmarktbeleid, en arbeidsparticipatie.

De bijstand heeft als doel om mensen een financieel vangnet te bieden tegen armoede, wanneer het recht op overige socialezekerheidsuitkeringen ontbreekt. Echter, stijgende overheidsuitgaven zijn in recente jaren in toenemende mate beantwoord met de inkrimping van veel Europese verzorgingsstaten, waarbij hervormingen zich veelal richten op bijstandsvoorzieningen (zie Jensen et al. 2018). Hoewel bezuinigingen op de bijstand minder electoraal riskant zijn dan bezuinigingen op andere socialezekerheidsregelingen, kunnen zij ook worden beschouwd als een verzwakking van de inkomensbescherming van de meest kwetsbaren in de samenleving. Waar de effecten van dergelijke hervormingen op economische uitkomsten veelal worden onderzocht, is dat echter zelden het geval voor neveneffecten op criminaliteit.

Om een compleet beeld te vormen van de maatschappelijke kosten en baten van bijstands- en arbeidsmarktbeleid, dienen neveneffecten op criminaliteit in overweging te worden genomen. Het bijstandssysteem en het strafrechtssysteem kunnen worden beschouwd als twee tegenovergestelde instituties die beiden een grote invloed uitoefenen op de levens van mensen aan de onderkant van de inkomensverdeling. Zij worden dan ook vaak geduid als de linker- en rechterhand van de staat, oftewel de zachte en harde zijde van de overheid (Wacquant 2009). Vanuit een dergelijk perspectief, kan de trend van inkrimping van de verzorgingsstaat gelijkgesteld worden aan een meer punitieve benadering van criminaliteit, met een focus op repressie in plaats van preventie. Eerder onderzoek op macroniveau heeft reeds aangetoond dat bijstandsuitgaven criminaliteit reduceren (e.g. Chamlin et al. 2002, Grant and Martinez Jr 1997, Meloni 2014, Worrall 2009). Een afname in de toegankelijkheid van de bijstand zal bijstandsuitgaven reduceren, maar mogelijk dan ook kunnen zorgen voor een toename in criminaliteit met bijbehorende substantiële maatschappelijke kosten.

Deze multidisciplinaire thesis combineert inzichten uit de economische en criminologische wetenschappen, om causale relaties te onderzoeken tussen bijstandsontvangst, actief arbeidsmarktbeleid, arbeidsparticipatie, en criminaliteit. Waar economen veelal de effecten van bijstandsgerelateerd beleid op arbeidsmarktuitkomsten evalueren, worden neveneffecten op criminaliteit vaak genegeerd. Criminologen benutten daarentegen zelden variatie in economisch beleid. De studies in deze dissertatie onderzoeken theoriën ten aanzien van de *economics of crime*, door variatie in bijstands- en arbeidsmarktbeleid te benutten middels het gebruik van econometrische technieken. Deze benadering wordt gefaciliteerd door de beschikbaarheid van unieke microdata verzameld door het Centraal Bureau voor de Statistiek over de volledige Nederlands bevolking. Deze grootschalige data staan ons toe om criminaliteits- en arbeidsmarktuitkomsten te onderscheiden tot op dagniveau. Nederland biedt ook een waardevolle institutionele context om deze relaties te onderzoeken, vanwege het hoge bijstandsniveau en milde strafklimaat. Waar de meeste gerelateerde literatuur gericht is op de VS, werpt deze thesis licht op de generaliseerbaarheid van eerdere bevindingen naar een context die meer representatief is voor Scandinavische en West-Europese landen.

Het onderzoeken van causale relaties tussen de bijstand, de arbeidsmarkt, en crimineel gedrag is empirisch uitdagend vanwege de veelheid aan ongeobserveerde factoren die gelijktijdig deze uitkomsten beïnvloeden. Door hiervoor te corrigeren, adresseert deze thesis de schaarste aan causaal bewijs omtrent de volgende onderzoeksvragen: 1) Reduceert bijstandsontvangst criminaliteit middels haar minimuminkomensgarantie? 2) Zo ja, in hoeverre beïnvloeden striktere activeringseisen voor bijstandsgerechtigdheid criminaliteit? 3) Beïnvloedt de uitbetaling van bijstandsuitkeringen criminaliteit over de bijstandsmaand? 4) In hoeverre materialiseert continuïteit in crimineel gedrag zich via nadelige arbeidsmarktgevolgen van het plegen van criminaliteit? Middels de beantwoording van deze vragen, poogt deze thesis het begrip te vergroten ten aanzien van de causale relaties tussen bijstandsafhankelijkheid, arbeidsactivering, arbeidsparticipatie, en criminaliteit.

Hoofdstuk 2 adresseert de leemte in causaal bewijs omtrent de relatie tussen bijstandsontvangst en criminaliteit, door de onderzoeksvraag te beantwoorden *In hoeverre beïnvloedt bijstandsontvangst criminaliteit onder jongvolwassenen?*. Theoretisch kan het ontbreken van de minimuminkomensgarantie geboden door de bijstand criminaliteit via verschillende mechanismen beïnvloeden. De rationele keuze theorie voorspelt dat een afname van inkomen zal zorgen voor een toename aan vermogenscriminaliteit, gelet op de grotere relatieve financiële baten (Becker 1968, Ehrlich 1973). Het beschikken over onvoldoende inkomen kan daarnaast ook psychologische druk veroorzaken, wat crimineel gedrag in het algemeen als copingmechanisme kan stimuleren (Agnew 1992). Hoewel er een theoretische consensus heerst dat bijstandsontvangst criminaliteit reduceert middels inkomensbescherming, zijn deze hypotheses niet eerder rigoureus getest met microdata over een algemene populatie.

Om te corrigeren voor ongeobserveerde persoonskenmerken die samenhangen met zowel bijstandsafhankelijkheid als crimineel gedrag, benut hoofdstuk 2 de leeftijdsgrens in bijstandsbeleid waaronder bijstandsaanvragers in Nederland onderhevig zijn aan de zogenoemde ‘zoekperiode’. Bij aanvraag van een bijstandsuitkering, dienen aanvragers onder de 27 jaar eerst vier weken te zoeken naar werk voordat zij recht krijgen op een bijstandsuitkering. Een meerderheid van bijstandsaanvragers blijkt na deze zoekperiode af te zien van de aanvraag van een uitkering (Ministerie van Sociale Zaken en Werkgelegenheid 2015, Van Dodeweerd 2014), en zij die hun aanvraag doorzetten ontvangen geen legitiem inkomen voor een periode tot acht weken. Het benutten van deze leeftijdsgrens als instrumentele variabele, stelt ons in staat om het effect te meten van het ontvangen van een bijstandsuitkering ten opzichte van het niet ontvangen van bijstand door het zoekperiodebeleid.

Analyse van microdata over de volledige jongvolwassen bevolking van Nederland rond de leeftijdsgrens van 27 jaar bevestigt de hypothese dat bijstandsontvangst criminaliteit substantieel reduceert. Voor mannen vinden wij een grotere reductie in vermogenscriminaliteit dan andersoortige delicten, waar de reductie onder vrouwen van gelijke grootte is voor vermogensdelicten en criminaliteit in het algemeen. Onze resultaten suggereren dan ook dat het onderliggende causale mechanisme verschilt tussen mannen en vrouwen, maar wij vinden geen bewijs van heterogeniteit tussen opleidingsniveaus. Zodoende, lijkt een lagere weerbaarheid tegen financiële stress niet de hogere prevalentie van criminaliteit onder lageropgeleiden te kunnen verklaren. Eerdere studies naar de causale effecten van bijstandsontvangst op criminaliteit onder een algemene populatie zijn schaars, maar eerder onderzoek heeft wel vergelijkbare afnames in criminaliteit gevonden voor ex-gedetineerden (zie Yang 2017a). Hoewel een *back-of-the-envelope* berekening toont dat bijstandsvoorzieningen niet kosteneffectief zijn als criminaliteitspreventiestrategie, wijzen de bevindingen in hoofdstuk 2 uit dat de substantiële neveneffecten op criminaliteit meegewogen dienen te worden in een kosten-batenanalyse van bijstandsbeleid.

Waar hoofdstuk 2 de effecten van bijstandsontvangst onderzoekt op criminaliteit, bouwt *Hoofdstuk 3* hierop voort door de effecten te analyseren van strictere activeringseisen voor bijstandsgerechtigdheid. Om de onderzoeksvragen te adresseren *In hoeverre beïnvloedde recent Nederlands actief arbeidsmarktbeleid criminaliteit onder jongvolwassenen? en via welk causaal mechanisme?*, behandelt dit hoofdstuk de beleidsrespons in Nederland op de stijgende werkloosheid onder jongvolwassenen naar aanleiding van de Grote Recessie van 2007. Beleidsmakers in meerdere OECD-landen vonden jeugdwerkloosheid bijzonder verontrustend, mede vanwege de grotere potentiële nadelige gevolgen dan werkloosheid onder oudere leeftijdsgroepen (Scarpetta et al. 2010). Naast meer nadelige langetermijneffecten op arbeidsmarktuitkomsten door *labor market scarring*, werd hieronder ook een grotere verwachte toename in criminaliteit geschaard (e.g. Gould et al. 2002). In navolging van het succes van vergelijkbaar activeringsbeleid in de VS (*Job Corps*) en het VK (*The New Deal for Young People*), implementeerde de Nederlandse overheid activeringsbeleid gericht op de arbeidsactivering van jongvolwassenen onder de leeftijd van 27. Per oktober 2009, verving de Wet investeren in Jongeren het recht op een bijstandsuitkering met het recht op een zogenoemd 'werkleeraanbod', bestaande uit een voorziening gericht op arbeidsinschakeling ofwel een aanbod van algemeen geaccepteerde arbeid. Hoewel meerdere OECD-landen vergelijkbaar activeringsbeleid hebben geïmplementeerd, heeft effectevaluatie van deze programma's zich tot dusverre primair gericht op arbeidsmarktuitkomsten. Hoofdstuk 3 complementeert eerder onderzoek door neveneffecten van een verplicht activeringsprogramma op criminaliteit te analyseren.

De analyses gepresenteerd in hoofdstuk 3 vinden bewijs voor een incapacitatie-effect van het werkleeraanbod op criminaliteit. Meer specifiek, reduceerde het activeringsbeleid criminaliteit op werkdagen met 12% onder Nederlanders met een migratieachtergrond. Het ontbreken van een effect op criminaliteit gepleegd gedurende weekenden sluit menselijk kapitaal en socialisatie-effecten uit als onderliggend mechanisme. Hoofdstuk 3 behandelt de analyse van potentiële causale effecten en mechanismen in meer detail, en presenteert bewijs dat het activeringsbeleid geen substantieve invloed had op inkomen en reguliere arbeidsparticipatie. Hoewel onsuccesvol in haar doel van arbeidsactivering (zie ook Cammeraat et al. 2017), heeft het werkleeraanbod-beleid criminaliteit gereduceerd gedurende een periode van relatief lage baankansen.

De resultaten in hoofdstuk 3 suggereren dat een relatief kwetsbare groep het meeste profiteerde van het werkleeraanbod. We vinden enkel een effect op criminaliteit onder Nederlanders met een migratieachtergrond, de groep waarvoor wij ook de hoogste bijstandsafhankelijkheidgraad vinden. De substantiële afname in criminaliteit onder deze groep is in lijn met eerdere studies naar de effecten van vergelijkbaar activeringsbeleid op kwetsbare jongeren (Bratsberg et al. 2019, Schochet et al. 2008). Het ontbreken van een effect op criminaliteit onder individuën zonder een migratieachtergrond kan mogelijk verklaard worden door de lagere programmaparticipatie onder deze groep. Een aanvullende verklaring kan liggen in de grotere kans dat Nederlanders met een migratieachtergrond in meer gesegregeerde, criminogene gemeenschappen wonen (Peterson and Krivo 2005). Dit kan het incapacitatie-effect van programmaparticipatie versterken, gezien participanten minder tijd doorbrengen in een relatief criminogene omgeving. De substantiële neveneffecten op criminaliteit onder deze relatief kwetsbare groep verdient overweging in de ontwikkeling en evaluatie van gericht actief arbeidsmarktbeleid.

Hoofdstuk 4 bouwt voort op het onderzoek naar bijstandsontvangst als minimuminkomensgarantie in hoofdstuk 2, door crimineel gedrag onder bijstandsontvangers te analyseren over de maandelijkse betalingscyclus van bijstandsuitkeringen. De onderzoeksvraag die hierbij centraal staat luidt *In hoeverre beïnvloedt de tijd sinds bijstandsuitbetaling criminaliteit onder bijstandsontvangers?* Bewijs omtrent ontoereikende consumptiespreiding onder bijstandsontvangers[8] suggereert dat de betalingscyclus criminaliteit via twee verschillende causale mechanismen kan beïnvloeden: Eén mechanisme heeft betrekking op de toenemende mate waarin bijstandsontvangers geconfronteerd worden met serieuze geldtekorten naarmate het einde van de maand nadert. Dergelijke financiële beperkingen kunnen ontvangers stimuleren om hun inkomen aan te vullen middels vermogenscriminaliteit (Becker 1968, Ehrlich 1973). Ook kan dit de kans op criminaliteit in het algemeen vergroten als copingmechanisme tegen de resulterende psychologische stress (Agnew 1992). Een tweede mechanisme heeft betrekking op de aangetoonde pieken in consumptie onder bijstandsontvangers direct na uitbetaling. Dergelijke pieken in consumptie complementair aan criminaliteit (e.g. alcohol en drugs) kunnen gepaard gaan met pieken in geweldscriminaliteit in het bijzonder (zie Dobkin and Puller 2007, Hsu 2017, Watson et al. 2019). Om beide theoretische mechanismen te onderzoeken, analyseren wij afzonderlijk financieel gemotiveerde en niet-financieel gemotiveerde criminaliteit.

[8]Veel studies hebben reeds aangetoond dat consumptie onder bijstandsontvangers direct na bijstandsuitbetaling piekt, om daaropvolgend sterk af te nemen richting het einde van de maand (zie Castellari et al. 2017, Damon et al. 2013, Hamrick and Andrews 2016, Hastings and Washington 2010, Shapiro 2005, Stephens Jr 2003, Wilde and Ranney 2000).

Door variatie in uitbetalingsdata over tijd en tussen Nederlandse gemeenten te benutten, vinden wij bewijs voor beide hypotheses. Met betrekking tot het eerste mechanisme, vinden we dat financieel gemotiveerde criminaliteit met 17% toeneemt onder bijstandsontvangers over de betalingscyclus. Dit suggereert dat een afname in beschikbare financiële middelen over de bijstandsmaand ontvangers aanspoort tot het plegen van criminaliteit ter aanvulling op hun inkomen. De uitgevoerde analyses wijzen echter ook op een omgekeerde trend in niet-financieel gemotiveerde delicten, welke direct na uitbetaling pieken en daaropvolgend met 6% afnemen over de betalingscyclus. Deze tegenovergestelde effecten vlakken de trend in criminaliteit in het algemeen gedeeltelijk af, tot een toename van 5% over de bijstandsmaand. Gezien wij geen heterogene effecten vinden, lijken verschillen in het vermogen tot consumptiespreiding niet de verschillen in criminaliteit tussen leeftijdsgroepen en geslacht te kunnen verklaren. Hoewel nader onderzoek nodig is, suggereren onze bevindingen dat een meer gespreide betaling van bijstandsuitkeringen mogelijk criminaliteit kan reduceren door de tijd te verkorten waarover consumptie moet worden gespreid. Gezien dit echter de financiële autonomie van bijstandsontvangers zou beperken, dienen de kosten en baten van een dergelijke beleidswijziging grondig in beschouwing te worden genomen.

Hoofdstuk 5 complementeert de voorgaande hoofdstukken omtrent bijstand en criminaliteit, door te onderzoeken wat de rol is van nadelige arbeidsmarktgevolgen in de continuïteit van crimineel gedrag. De onderzoeksvraag die centraal staat in hoofdstuk 5 is *In hoeverre beïnvloedt eerder crimineel gedrag huidig crimineel gedrag via arbeidsmarktgevolgen?* Eerder onderzoek suggereert dat de arbeidsmarkt feitelijk een aanvullende straf oplegt voor crimineel gedrag, waarbij baankansen afnemen via meerdere *scarring* mechanismen, waaronder het verkrijgen van een strafblad en een verminderde opbouw van menselijk kapitaal.[9] Gelet op het bewijs omtrent de preventieve effecten van het hebben van een stabiele baan op crimineel gedrag,[10] kan hierbij een vicieuze cirkel ontstaan waarbij eerdere

[9] Zie Apel and Sweeten (2010), Bernburg and Krohn (2003), Dobbie et al. (2018), Holzer et al. (2004), Pager et al. (2009), Paternoster and Iovanni (1989).

[10] Zie Apel et al. (2008), Apel and Horney (2017), Van der Geest et al. (2011), Ramakers et al. (2020), Uggen (2000).

delinquentie toekomstig crimineel gedrag stimuleert. Direct bewijs voor de rol van arbeid in de padafhankelijkheid van crimineel gedrag is echter schaars, mede vanwege de empirische uitdagingen die voortkomen uit de wederkerige relatie tussen arbeid en criminaliteit. Om deze uitdagingen te overkomen, past hoofdstuk 5 een *joint dynamic model* toe over criminaliteit en arbeid, waarin expliciet terugkoppelingseffecten van eerder crimineel gedrag op de huidige arbeidsmarktstatus worden gemodelleerd. Zodoende worden de volgende drie testbare hypotheses geanalyseerd: 1) eerder crimineel gedrag reduceert huidige baankansen, 2) arbeidsparticipatie reduceert huidig crimineel gedrag, en 3) eerder crimineel gedrag vergroot de kans op huidig crimineel gedrag via alternatieve mechanismen, na correctie voor populatieheterogeniteit.

De resultaten bieden geen ondersteuning voor de centrale hypothese dat nadelige arbeidsmarktgevolgen een causale rol spelen in padafhankelijkheid van crimineel gedrag. Ten aanzien van de eerste hypothese, vinden wij geen substantieve effecten van eerder crimineel gedrag op het hebben van een huidige baan. Dit suggereert dat de lage arbeidsparticipatie onder ex-delinquenten wordt veroorzaakt door persoonskenmerken die gerelateerd zijn aan zowel de kans op criminaliteit als de de kans op werk. Wel vinden we steun voor de tweede hypothese, in de vorm van een substantiële reductie in criminaliteit door arbeidsparticipatie. Tot slot, ondersteunen de resultaten de hypothese dat eerder crimineel gedrag de kans op toekomstig crimineel gedrag substantieel vergroot, los van enige invloed van persoonskenmerken.

Concluderend toont hoofdstuk 5 dat de substantiële nadelige effecten van crimineel gedrag op toekomstige criminele besluitvorming niet materialiseren via arbeidsmarktgevolgen. Hoewel huidig crimineel gedrag substantieel gereduceerd wordt door arbeidsparticipatie, vinden wij geen bewijs voor substantieve nadelige effecten van eerder crimineel gedrag op de huidige kans op werk. Dit sluit niet aan bij bevindingen van eerdere studies, welke tonen dat crimineel gedrag arbeidsmarktkansen verlaagt. Een mogelijke verklaring hiervoor kan liggen in de focus van eerder onderzoek op de VS, waar een stricter strafklimaat heerst dan de meeste Europese landen, waaronder met betrekking tot de lengte van gevangenisstraffen en de toegankelijkheid van strafbladen (Zie Aebi and Tiago 2020, Corda and Lageson 2020, Kaeble 2018, Motivans 2020).

Bijeengenomen, tonen de resultaten in hoofdstuk 2 dat bijstandsontvangst criminaliteit substantieel vermindert via een minimuminkomensgarantie. De maandelijkse uitkering van de bijstand zorgt echter wel voor een piek in niet-financieel gemotiveerde criminaliteit direct na ontvangst (zoals getoond in hoofdstuk 4). Financieel gemotiveerde criminaliteit neemt daarentegen toe over de betalingscyclus, wat suggereert dat bijstandsontvangers financiële beperkingen ervaren richting het einde van de bijstandsmaand. Tezamen met hoofdstuk 2, suggereert hoofdstuk 3 dat de invloed van actief arbeidsmarktbeleid op criminaliteit afhankelijk is van de aanwezigheid van ontmoedigings- versus incapacitatie-effecten. Striktere activeringseisen die bijstandsafhankelijkheid verlagen, maar arbeidsparticipatie niet verhogen, zorgen voor een toename in criminaliteit (hoofdstuk 2), waar participatie in een verplicht activeringsprogramma criminaliteit reduceert onder kwetsbare jongvolwassenen (hoofdstuk 3). Tot slot, suggereert hoofdstuk 5 dat een recente criminele geschiedenis niet noodzakelijk werkloosheid veroorzaakt, gezien we geen substantieve invloed van eerder crimineel gedrag op arbeidsparticipatie vinden.

Curriculum Vitae

Marco Theodorus Cornelis Stam was born in Langedijk, the Netherlands on the 23rd of July, 1990. After obtaining a BSc degree in Criminology, he received a MSc degree in Investigative Criminology (*cum laude*) and the Research Track Criminology (*cum laude*) from the *Vrije Universiteit Amsterdam*. His MSc thesis, written during an internship at the Netherlands Institute for the Study of Crime and Law Enforcement, was presented and published at the *Benelux Conference on Artificial Intelligence*.

In 2016, Marco became a PhD candidate at the Department of Economics and the Department of Criminology at Leiden University. His doctoral research was part of the *Reform of Social Legislation* and *Criminal Justice: Legitimacy, Accountability and Effectivity* research programs, and received financial support from the Gak institute. The resulting papers were presented at various international conferences, including the annual meetings of the *EALE*, *ESPE* and *ASC*. Policy relevant work was presented at the Netherlands Economists Day and the Ministry of Social Affairs.

Besides his doctoral research, Marco taught several courses in criminology and economics. He also received formal education in Econometrics and Applied Microeconometrics at the Tinbergen Institute. During the finalization of his PhD dissertation, he participated as a researcher in the Leiden University project of *Punishment: What Works?* and the Centre for BOLD Cities project of *Reintegration in BOLD Cities*, funded by the Gratama foundation and the Netherlands Organisation for Health Research and Development, respectively. In addition to research and teaching, he contributed to data source integration into a national data infrastructure for academic research, through his affiliation as Data Scout at *ODISSEI*.