The Effect of Social Assistance Generosity on Crime: Evidence from a Regression Discontinuity Design*

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Abstract

This paper studies the effects of decreased social assistance on criminal behavior among welfare recipients in Denmark. I analyse the 2014 reform of the Danish social assistance system, which introduced a sharp discontinuity at age 30, with lower benefits for individuals under 30. Using register data from 2014–2021 I employ a regression discontinuity design to identify the average treatment effect at the age-30 cutoff. The findings indicate that eligibility for lower benefits below age 30 increases both the likelihood of committing a crime and the number of crimes committed in a year. The probability of committing any crime in a year rises by 0.72 percentage points (a 9 % increase compared to the baseline at age 31), while the average number of crimes increases by 0.0122 (a 12 % increase). These effects are particularly pronounced for property and violent crime. The probability of committing a property crime rises by 0.57 percentage points (19 %), and violent crime by 0.30 percentage points (37 %). Heterogeneity analyses show that the effects are most pronounced among men, unmarried individuals, and those from disadvantaged family backgrounds. These findings show that reductions in financial support through the social assistance system can influence adverse behaviours such as criminal activity.

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1. Introduction

A central consideration when designing welfare policies is how they influence behaviour beyond the labour market, including in areas such as crime. In recent decades, successive Danish governments have pursued reforms to the transfer system, including the social assistance system,[1] with the aim of increasing labour supply by lowering benefits. However, while such policies may increase employment for some, they may also impose financial strain on others. For these individuals, changes in benefit levels may affect behaviours beyond job search, including criminal engagement. Crime imposes substantial costs on society. According to estimates from the Rockwool Foundation (2006), the direct costs of crime in Denmark amounted to around DKK 10 billion in 2005. When broader indirect costs are included, such as victim support, healthcare expenses etc. the true cost is likely far higher. For the individual, a criminal record significantly reduces the chances of stable labour market attachment, often resulting in prolonged dependency on public support and lower earnings. In short, crime carries large and long-lasting consequences for individuals and the society.

Despite this, the potential link between social assistance generosity and criminal behaviour has received limited attention. While existing research has examined the effects of welfare policies on employment, less is known about how changes in benefit levels affect non-economic outcomes. Notable exceptions are Dustmann et al. (2024a) and Dustmann et al. (2024b), who study Denmark's Start Aid welfare reform that reduced benefits for refugees. They find that the cut led to a signif-icant increase in crime among both the affected individuals and their children. Although these studies provide valuable insights, they focus on a relatively nar-row and specific population of primarily non-Western immigrants. Less is known about how changes in benefit levels affect crime within the much larger group of individuals receiving social assistance, the majority of whom are of Danish origin. Analysing the effects of welfare benefit changes in this broader population is highly relevant from a policy perspective. Even small behavioural responses can translate into substantial aggregate effects when the exposed group is large. Understanding these effects is essential for informing a more comprehensive eval-uation of welfare policies.

This paper therefore examines the effects of decreased social assistance on criminal behaviour among all social assistance recipients. In particular, I study the causal effect of reduced social assistance eligibility below age 30 on criminal behaviour among welfare recipients in Denmark. The analysis exploits a sharp decrease in transfer levels introduced as part of the 2014 reform of the Danish social

1. In Danish: »Kontanthjælpsystemet«.

assistance system, under which individuals under 30 became eligible for significantly lower social assistance compared to those aged 30 and above. Using register data cover-ing the full Danish population from 2014 to 2021 and focusing on social assistance recipients, I implement a regression discontinuity (RD) design to estimate the average treatment effect at the cutoff of this policy-induced reduction in transfers.

The two primary outcomes examined are: the probability of committing a crime in a given year and the number of crimes committed per individual per year. The analysis distinguishes between all crimes, property crimes and violent crimes.

The results show that eligibility for lower benefits below age 30 increases both the likelihood of committing a crime and the number of crimes committed in a year. In the preferred specification, the probability of committing any crime rises by 0.72 percentage points in a year, corresponding to a 9 % increase relative to the baseline at age 31, while the number of crimes committed increases by 0.0122, corresponding to a 12 % increase. These effects are particularly pronounced for property and violent crime. For property crime, the probability increases by 0.57 percentage points (25 %), and the number of crimes rises by 0.0068 (23 %). For violent crime, the probability increases by 0.30 percentage points (37 %), and the number of violent offences rises by 0.0032 (37 %). These findings are robust across multiple specifications, placebo tests, bandwidth choices and subsample restrictions. Heterogeneity analyses reveal that the treatment effects are concentrated among males, unmarried individuals and those from more disadvantaged backgrounds.

While the RD design identifies a local treatment effect around the age-30 threshold, the findings suggest broader considerations for how welfare policies are designed. The behavioural responses observed indicate that structural features of welfare systems, such as age-based eligibility thresholds, can shape individual decisions in meaningful ways. While further research is needed to assess whether similar effects are found in other institutional settings or persist over longer time horizons, the results show that reductions in benefit levels can lead to increased criminal activity among affected individuals. Policymakers designing or reforming wel-fare programs may benefit from considering such broader effects, including the potential for increased crime, alongside traditional economic objectives.

To the best of my knowledge, this paper provides the first causal evidence that the decrease in social assistance below age 30 increases criminal behaviour. It offers new empirical insights into the behavioural consequences of welfare policies, expanding on existing studies that examine the relationship between welfare benefits and crime. Additionally, it complements previous research that has primarily focused on the impact of benefit changes on labour market outcomes. In doing so, this paper enhances the understanding of how welfare policies not only influences economic self-sufficiency but also wider patterns of behaviour that carry significant costs for individuals and society.

2. Institutional Background

Cash benefits and educational assistance are key components of the Danish social assistance system. This system plays a central role in the Danish welfare model by ensuring economic security and promoting redistribution. However, since social assistance is publicly funded, it affects government costs negatively. Moreover, an increase in social assistance or other transfers reduces the financial incentive to work (DEC (2015)). This section outlines the key institutional features of the Danish social assistance system that are relevant to the analysis.

Social assistance in Denmark is a means-tested transfer available to all legal re-si-dents. It has no time limit, is not linked to previous income and benefit levels vary by age, education, family situation and duration of residence. Individuals qualify if they cannot support themselves or their family, provided their needs cannot be met through other benefit schemes. Eligibility is subject to asset limits – individu-als may not hold assets above DKK 10,000 – and both own and spousal income are deducted from payments. Full benefits also require meeting employment-related conditions. Once eligible, the municipality determines the type of benefit. To re-ceive cash or educational assistance, applicants must have resided in Denmark for at least nine of the past ten years and completed 2½ years of full-time employment within the same period.

Throughout this paper, several terms related to the Danish social assistance sys-tem are used. Understanding the distinctions between these terms is essential for interpreting the institutional context and the empirical strategy. Table A.1 provides explanations for the key terms used in the analysis.

2.1. The Reformed Social Assistance System from 2014

Educational assistance was introduced with the 2014 reform of the Danish social assistance system, effective January 1, 2014, with the aim of reducing the number of recipients. Before the reform, almost all individuals aged 25+ received the same cash benefit. The reform primarily affected those aged 25–29: benefit levels were reduced, and recipients under 30 without a vocational qualification faced an educational obligation to participate in programs to retain benefits. Under the new rules, individuals under 30 with a vocational education were eligible only for cash benefits at the youth rate, while those without such qualifications received educational assistance. By contrast, individuals aged 30+ became entitled to full cash benefits regardless of education (Nordic Co-operation).[2]

Eligibility is assessed at local job centres. For individuals with vocational edu-ca-tion, the job centre can classify them as either job-ready (able to work soon) or activity-ready (facing complex barriers such as health or social issues). All recipients who either have a vocational education or are aged 30+ are assessed using this two-category system. By contrast, individuals under 30 without vocational education are evaluated in three categories: clearly education-ready (can begin immediately), education-ready (able to start within a year) and activity-ready (unable to begin within a year) (Borger.dk).

The assessment category does not affect benefit levels for those 30+, but they matter for those under 30. In particular, under-30 recipients deemed activity-ready receive activity pay, which aligns their benefits with the 30+ level, regardless of education. Thus, the 2014 reform's benefit reduction only applies to under-30 individuals classified as job-ready, clearly education-ready, or education-ready (DEC (2015)). Assessment categories also determine the conditions recipients must fulfil to maintain eligibility.

Table 1 presents the monthly benefit levels that individuals could receive in 2021 – the most recent year in the analysis. It also shows the absolute and relative differences in monthly benefits for individuals below and above age 30 and reveals that the decrease in benefit levels below age 30 is greater for individuals without a vocational education.

Table 1. Social Assistance level in Denmark, 2021

	18-29 years		**> 30 years**	**Reduction**
	DKK pr. month			%
Educational assistance				
Not living with parents, whitout children	6,410	11,698	5,288	45
Living with parents, whitout children	2,762	11,698	8,936	76
Married or living together with children	8,973	15,547	6,574	42
Singel with children	12,821	15,547	2,726	18
Cash benefits				
Not living with parents, whitout children	7,541	11,698	4,157	36
Living with parents, whitout children	3,639	11,698	8,059	69
Married or living together with children	10,396	15,574	5,151	33
Singel with children	14,860	15,574	687	4

Note: The benefit level refers to the monthly amount received per person before taxes. The table does not include benefit levels for individuals under the age of 30 who are assessed as activity ready, since they receive activity pay. This means their benefit levels are equivalent to those of individuals aged 30.

Source: Retsinformation (2022).

2. These rules remain until July 2025, when a new reform will simplify the system The Municipality of Copenhagen (2025).

3. Literature Review

In this section I provide a brief overview of the existing literature associating criminal behavior and welfare benefits. Cross-country comparisons in this area are often complicated by institutional differences. Variations in public spending on law enforcement, such as policing and judicial systems, as well as differences in the severity and type of punishment, whether imprisonment, probation or fines, can all influence individual behavior and affect the degree to which crime responds to changes in welfare policy.

Despite the relevance of the topic, there is relatively limited research that examines the effects of welfare benefits on crime using Danish data. I therefore begin by reviewing international studies that explore this relationship in other settings. I then turn to what are, to my knowledge, the only two existing studies that use Danish data to estimate the effect of welfare benefit changes on crime. Both studies examine the Start Aid welfare reform, which was in effect from 2002 to 2012 and involved a substantial reduction in benefit levels, approximately 40 percent, for newly arrived refugee immigrants. One study investigates the direct consequences for the individuals who were affected by the policy, while the other focuses on the long-term impacts on the children of these families. The magnitude of the benefit reductions under the Start Aid reform is comparable to that introduced by the 2014 reform of the social assistance system examined in this paper.

3.1. Crime and Welfare Benefit Levels

Palmer et al. (2019) examines whether emergency financial assistance can reduce criminal behavior among individuals experiencing negative economic shocks in the United States. The study uses administrative data from individuals who contacted Chicago's Homelessness Prevention Call Center to request emergency financial support and exploits the fact that the availability of funding varies unpredictably over time. This variation provides a quasi-random setting that allows the authors to estimate the causal impact of receiving temporary assistance on subsequent criminal behavior. By comparing individuals who called when funding was available to those who called when no funds were available, the study finds that arrest rates for violent crimes were 0.87 percentage points (51 %) lower among those who received assistance. This reduction persists for up to three years following the call. The effects are especially pronounced for single individuals, among whom the reduction reaches 2.2 percentage points.

However, the study also finds that property crime, particularly shoplifting, increases among recipients one year after receiving assistance. This effect is largely driven by individuals who requested support for security deposits to enter new rental contracts. The authors suggest that while the aid may facilitate access to housing, it may also place some individuals in financially precarious situations they struggle to maintain, thereby potentially increasing incentives for property crime. Overall, the findings indicate that temporary financial assistance can significantly reduce violent crime, but may simultaneously introduce risks that lead to increases in certain types of property crime, highlighting the importance of the design and duration of support programs.

A study by Deshpande and Mueller-Smith (2022) investigates the long-term effects of welfare benefits on criminal justice involvement among disadvantaged youth. The analysis focuses on the Supplemental Security Income (SSI) program in the United States, which provides cash assistance to low-income individuals with disabilities. The study exploits a policy reform that introduced a reevaluation process for children receiving SSI upon turning 18. As a result of this reform, approximately 40 percent of youth who previously received SSI are removed from the program following their reevaluation, while nearly all individuals who turned 18 prior to the reform were allowed to continue receiving benefits. Using this institutional change within a regression discontinuity design, the study compares individuals who turned 18 just before and just after the reform. The findings reveal that the removal of SSI significantly increases criminal behavior in adulthood. Specifically, individuals who lost SSI benefits experienced a 20 % increase in the total number of criminal charges against them between the ages of 18 and 38. The effect is particularly strong for income-generating offences, such as burglary and theft, which increased by 60 %. In contrast, non-income-related offences increased by only 10 % and were not statistically significant. These results suggest that long-term welfare support plays an important role in reducing economic-motivated criminal behavior, and that the removal of such support can have persistent adverse effects on crime outcomes later in life.

3.2. Crime and Welfare Benefit Levels – Evidence from Danish Data

To my knowledge only two studies exist that use Danish data to examine the relationship between changes in welfare benefits and criminal behavior. The first, by Dustmann et al. (2024a), investigates the Start Aid welfare reform which significantly reduced benefit levels for newly arrived refugees by approximately 40 %. This reduction applied only to refugees who were granted residency after the reform came into effect. Using a RD design that exploits this cutoff, the study finds that the reform increased both the likelihood of committing a crime and the number of crimes committed by refugees by about 125 % in the first year following residency. The increase is entirely driven by property crimes, with no observable impact on violent crimes. Furthermore, the effect is mainly explained by an extensive margin response, where individuals who would otherwise not have committed a crime began doing so following the benefit cut. The authors also find particularly strong effects among women, whose number of criminal convictions nearly triples after the reform, again driven by property-related offences. In addition to crime outcomes, the study estimates labour market effects and finds that a one percent increase in benefit level reduces crime by the same percentage as it decreases labour earnings. They therefore conclude that the adverse responses following large welfare cuts can in relative terms be at least as large as the labour supply responses that were intended by policymakers.

The second study, Dustmann et al. (2024b), examines the longer-term effects of the same reform on the children of refugee families. It focuses on adolescents aged 14 to 18 at the time of residency and compares those exposed to the reduced benefit regime to those who arrived before the reform. The results show that within the first five years after residency, the share of adolescents with at least one criminal conviction increased with 12 percentage points, and the average number of convictions per individual rose by 0.37. These effects are mainly driven by property and violent crimes, with conviction probabilities increasing by approximately 11 and 7 percentage points, respectively.

In the longer term, particularly during years five to ten after arrival, the study finds that the rise in crime is increasingly driven by violent offences. The estimated reform effects on the number of violent crime convictions doubled from years five to 10, while the impact on the number of property crimes remained unchanged. Moreover, the rise in crime was concentrated among males, whose crime conviction probability increased by almost 20 percentage points during the first ten years after residency.

Together, these two studies provide robust evidence that large reductions in wel-fare benefits can have significant and lasting effects on crime, both for the directly affected individuals and for their children. These findings underline the importance of considering adverse consequences when designing welfare policies.

4. Empirical Strategy

The aim of this study is to examine the effect of becoming eligible for lower social assistance benefits below age 30 on crime outcomes. A concern is that criminal behavior tends to decline with age after the teenage years (Farrington (1986)). This trend is also evident in Danish data (see Figure A.1). If not properly accounted for, this age trend would create a upward bias in the estimation of the treatment effect of receiving lower benefits, as crime outcomes would already be higher at younger ages regardless of benefit status.

To address the selection bias, I employ a Regression Discontinuity (RD) design that takes advantage of the age-dependent eligibility rule in the Danish social assistance system. The rule creates a natural experiment in which individuals below age 30 are assigned to the treatment group while those aged 30 and above are assigned to the control group. Since treatment status is a deterministic and discontinuous function of the covariate age, a_i, this setup represents a sharp RD de sign. The variable that determines treatment, here age, is commonly referred to as the running variable. The treatment effect can be estimated using the following regression

$$Y_i = \alpha_0 + \rho D_i + f_0(a_i - c) + D_i \cdot f_1(a_i - c) + a_1 X_i + \varepsilon_i \qquad (1)$$

In this specification, Y_i is the outcome variable of interest, X_i represents a set of baseline covariates, c represents the cutoff at age 30 and D_i is the treatment indicator, which takes the value 1 if the individual is below age 30, and 0 otherwise. The coefficient ρ then captures the causal effect of treatment at the cutoff, that is, the effect of eligibility for lower social assistance on the outcome being studied. The main identifying assumption is that, apart from the treatment itself, there are no other discontinuities in the running variable around the cutoff. This assumption is validated in Section 7.. To ensure that the treatment effect at $a_i = c$ is correctly estimated as the coefficient on D_i, the age variable is centred at the cutoff $c = 30$.

In practice, when estimating the regression, the relationship between the outcome variable and the running variable may differ before and after the cutoff. A simple way of implementing the RD design is therefore to estimate two separate regres-sions on each side of the cutoff. The functions $f_0(a_i)$ and $f_1(a_i)$ therefore represent smooth approximations that flexibly model the relationship between the running variable and the outcome variable on each side of the threshold. The functions are modelled using a p^{th}-order polynomial. Since the estimated treatment effect may be sensitive to the choice of polynomial order, I estimate the model using first, second and third order polynomials.

4.1. Regression Discontinuity Estimation

To ensure sufficient data on criminal behaviour, I analyse both the number of crimes committed and whether an individual commits any crime during a full calendar year.[3] Accordingly age is measured in years. Since age is discrete, outcomes cannot be compared in very narrow bins around the cutoff, instead, regressions are used to estimate the conditional expectation of the outcome variable at the threshold. This approach is standard even with continuous running variables, so discreteness poses no major complication. However, a complication arises regarding how to treat individuals who turn 30 during the year. To see why this might be a problem consider an individual who turn 30 in January, she would thus only be exposed to the treatment in 1/12 of the year. Following Lemieux and Milligan (2008), I measure treatment as the fraction of the year during which the individual is age 29.

3. Figure A.2 shows that the monthly probability of crime is very low.

To determine the appropriate polynomial order, Lee and Lemieux (2010) re-com-mend using the Akaike Information Criterion (AIC) defined as $AIC = N \ln(\hat{\sigma}^2) + 2p$, where N is number of observations, $\hat{\sigma}^2$ the mean squared error and p the number of parameters. The model with the lowest AIC value is preferred.

Of the six models, three use binary outcomes, estimated with a linear probability model. While alternative estimators exist, results are typically similar, and the linear probability model is preferred for its simplicity and interpretability (Angrist and Pischke (2008)).

Standard error estimation raises additional considerations. For discrete running variables, Lee and Card (2008) recommend clustering at the running variable level. However, when few clusters exist, this can bias standard errors more than heteroscedasticity-robust alternatives. To remain conservative, I report the largest estimates, which in practice are the heteroscedasticity-robust standard errors. This also addresses heteroscedasticity from the linear probability model.

Even though the RD design is implemented within a panel framework, year or individuals fixed effects are not required for identification in a RD design (Lee and Lemieux (2010)). Year dummies are still included to reduce sampling variance and absorb year-specific shocks. As argued by Lee and Lemieux (2010), time dummies can be treated like baseline covariates without violating continuity assumptions.

Inclusion of baseline covariates can reduce sampling variability and improve precision. To serve this role, they should not be affected by the treatment, but should be correlated with the outcome variables. While covariates do not reduce bias at the cutoff, they help when observations farther from the threshold are included (Imbens and Lemieux (2008)).

5. Description of Data

In this paper I use registry data for the full Danish population, provided by Statistics Denmark. The registers used are the population register, the education register, the Central Crime Register and the DREAM database, spanning the period from 2014 to 2021. All registers are assessed annually at the end of each calendar year, with the exception of the DREAM database, which is updated on a monthly basis (dst.dk). The data allow for individual-level linkage across different years and registers using unique personal identification numbers. A more detailed description of the individual datasets used is provided in Section A2.

5.1. Variables

The treatment indicator is constructed from the age variable in the population register, taking the value 1 if the individual is aged 31 and 0 if under 30. For those turning 30 during the year, it is defined as the fraction of the year spent at age 29. Parental and address identifiers are used to determine provider status, with single providers identified by household type. Parental information on education and social assistance history is obtained by linking to the education and DREAM registers. Social assistance receipt is identified from DREAM, which also records quarterly assessment categories assigned by caseworkers.

Criminal behavior is measured using the Central Crime Register. Consistent with Dustmann et al. (2024a), only convictions are included, and crimes are attributed to the year of offence. Offence codes allow classification into three categories: all crimes (excluding traffic offences), property crimes and violent crimes.

5.2. Combining the Data and Restricting the Sample

I construct the dataset from the population register for each year, restricting the main analysis to individuals aged 21–40. Single providers are excluded, as their benefit reduction is relatively small.[4] Using the DREAM database, I further restrict to individuals who received cash benefits or educational support at least one week during the year.[5] Individuals categorised as activity-ready in all four quarters of the year are excluded, since they are unaffected by the reform. The sample is then merged with the crime register for information on convictions, and with education and DREAM data via parental identifiers to construct background variables.

To ensure a sufficiently large sample, I pool data from 2014–2021.[6] This yields 360,681 individual-year observations on 172,615 unique individuals. The aggregated dataset is unbalanced: individuals may drop out in years where they emigrate, die, are classified as activity-ready all year or qualify as single providers, but reappear if they later meet the inclusion criteria.

The final sample for each year thus includes individuals aged 21–40 who are not single providers, received social assistance ≥1 week and were assessed as job-ready, clearly education-ready, or education-ready for ≥1 quarter. Treatment is assigned based on age and date of birth. Five binary covariates are defined: female, married, Danish origin,[7] parent with vocational education and parent with social assistance history (≥1 week in 1991–2022). Six outcomes are measured: indicators for committing all, property or violent crimes, plus counts of crimes in each category in a given year.

4. Including them reduces the estimated effects; see Table A.6.
5. Restricting instead to those with ≥12 weeks of support yields even stronger effects, see Section A5.1.
6. Yearly estimates are mostly insignificant due to small samples; see Table A.7.
7. Defined as not being an immigrant or a descendant of immigrants.

To present an overview of the sample population, Table A.2 provides descriptive statistics for the full sample, the treatment group (age 21-29) and the control group (age 30-40). Some baseline covariates vary with age, underlining the importance of formally testing for discontinuities in these. In addition, the treatment group is, on average, less likely to commit crimes, which may simply reflect the general decline in crime rates with age.

As a robustness check, I replicate the analysis using data from the pre-reform period, 2006 to 2013, to verify that no discontinuities in crime outcomes are present. The dataset for this period is constructed in the same manner as in the main analysis. The only notable difference is that the assessment categories were defined differently prior to the 2014 reform.

6. Empirical Results

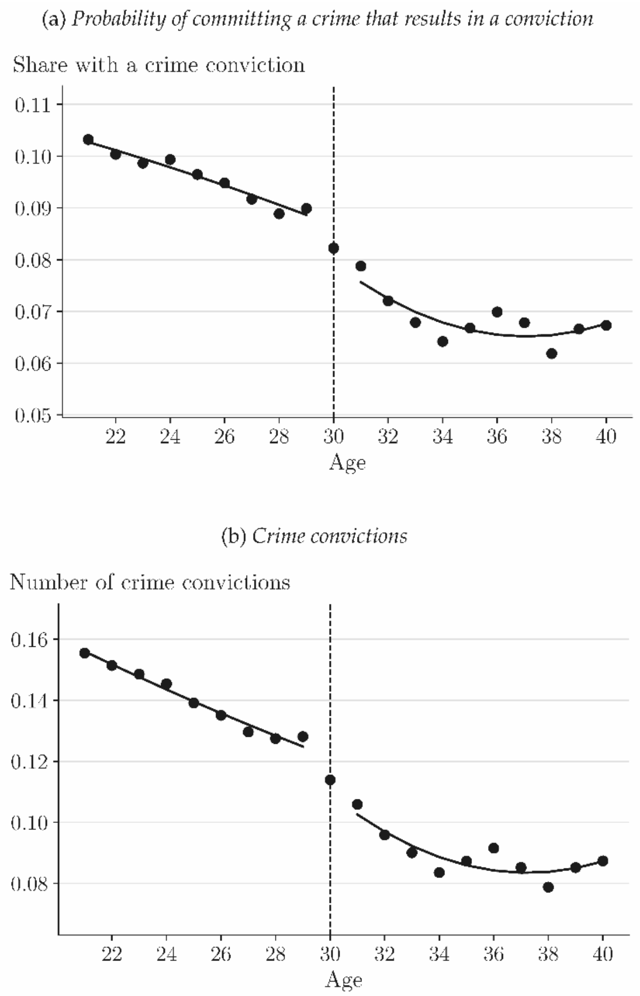
This section presents the main analysis, where I estimate the effects of decreased social assistance on criminal behavior. Criminal behavior is measured by two indicators: the probability of committing a crime that results in conviction (pr(crime)) and the number of crimes committed (#crimes) per individual per year. Three categories are considered: all crime (excluding traffic offences), property crime, and violent crime. I then examine heterogeneous effects across subgroups defined by baseline covariates.

6.1. Regression discontinuity estimates

As an initial illustration, Figure 1 shows the probability of committing a crime and the number of crimes committed by age for all crimes, with Figures 2 and 3 displaying corresponding outcomes for property and violent crime. For pri-vacy reasons I am confined to show violent crime in two-year bins. Each figure plots quadratic fits of the predicted outcomes before and after the discontinuity at age 30, estimated from equation (1) without covariates or year fixed effects. The quadratic specification is chosen based on the lowest AIC value (see Table A.3).[8] Since individuals at exactly age 30 include both treated and untreated observations, they are excluded from the fitted lines for all crime and property crime but included in violent crime estimates due to the two-year aggregation. Across all categories, the figures reveal increases in both the likelihood and number of crimes below age 30.

8. The quadratic specification is preferred for all and property crime, while the linear specification fits best for violent crime. Visual inspection supports these choices. Since linear and quadratic estimates for violent crime are not statistically different, I adopt the quadratic specification as the preferred for consistency across outcomes.

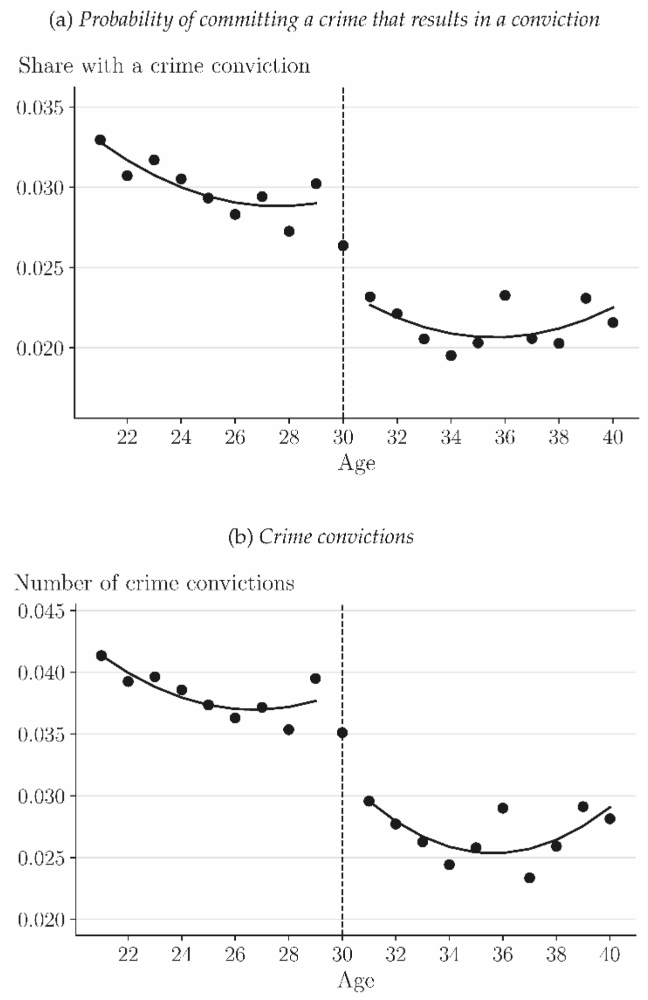
Figur 1. *All crime*



Note: The figure plots average outcome by age. All crime includes all offences except traffic related offences. The dashed vertical line indicates the discontinuity point at age 30. The figure contains quadratic slopes of the outcomes before and after age 30 based on equation (1) without including baseline covariates and year fixed effects. The average outcome at age 30 is shown, but is not included in any of the slopes in the plot.

Source: Own calculations based on register data from Statistics Denmark.

Figur 2. *Property crime*

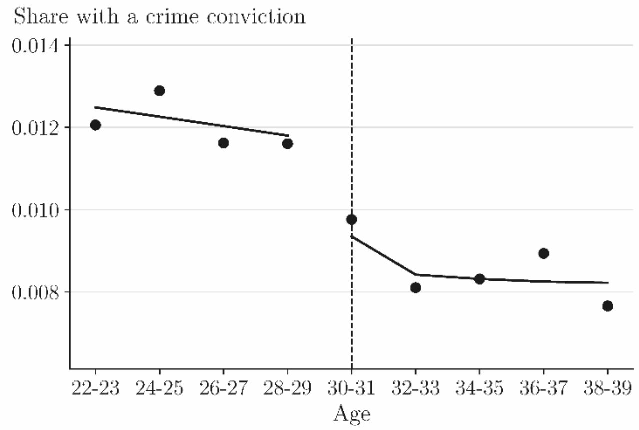


Note: The figure plots average outcome by age. The dashed vertical line indicates the discontinuity point at age 30. The figure contains quadratic slopes of the outcomes before and after age 30 based on equation (1) without including baseline covariates and year fixed effects. The average outcome at age 30 is shown, but is not included in any of the slopes in the plot.

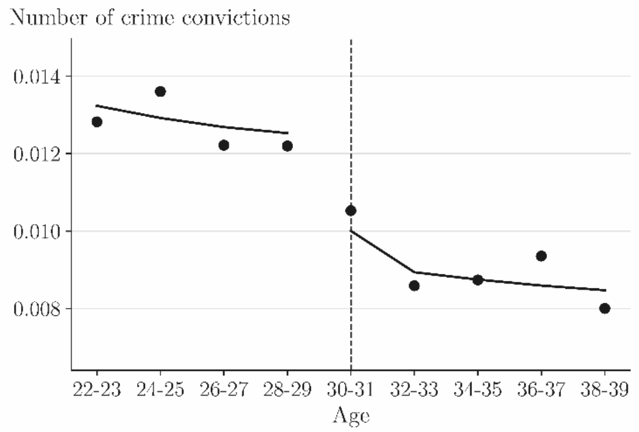
Source: Own calculations based on register data from Statistics Denmark.

Figur 3. *Violent crime*

(a) *Probability of committing a crime that results in a conviction*



(b) *Crime convictions*



Note: The figure plots average outcome by age in bins of two years. The dashed vertical line indicates the discontinuity point at age 30. The figure contains quadratic slopes of the outcomes before and after age 30 based on equation (1) without including the baseline covariates and year fixed effects.

Source: Own calculations based on register data from Statistics Denmark.

The treatment effects from equation (1) without including additional controls are reported in Table 2. Without additional assumptions, these estimates are interpreted as the average treatment effects at the cutoff (Imbens and Lemieux (2008)). In line with Bargain and Doorley (2011) the table also reports the mean of the dependent variable at age 31 as a benchmark. Heteroscedasticity-robust standard errors are used, as they are larger than those from clustering by age, personal identification numbers or both.

The results in Table 2 indicate that lower social assistance benefits increase the probability of committing a crime during a year by 0.72 percentage points, an 9 % rise relative to the baseline at age 31. The average number of crimes committed increases by 0.0122, or about 12 %. These results point to both extensive and intensive margin responses (Dustmann et al. (2024a)). Since the effect on the number of crimes is larger than the effect on the probability, the findings imply that lower benefits not only increase the proportion of individuals who engage in criminal behavior, but also raise the frequency of criminal activity among those who do.

For property crime, the probability increases by 0.57 percentage points, a 25 % rise, while the average number of property crimes increases by 0.0068 — a 23 % increase. For violent crime, the probability increases by 0.3 percentage points and the number of crimes by 0.0032, both corresponding to increases of 37 %. The similarity between the estimated effects in columns (3) and (7) suggests that the rise in violent crime is mainly driven by an extensive margin response.

Table 2. *Effects of treatment om crime outcomes*

	Pr(crime)	#crimes
All crime		
Treatment effect	0.0072**	0.0122**
	(0.0034)	(0.0056)
Mean	0.0788	0.1057
Relative change (%)	9.1	11.5
Property crime		
Treatment effect	0.0057***	0.0068**
	(0.0020)	(0.0030)
Mean	0.0232	0.0296
Relative change (%)	24.6	23.0
Violent crime		
Treatment effect	0.0030**	0.0032**
	(0.0012)	(0.0014)
Mean	0.0082	0.0086
Relative change (%)	36.6	37.2
Observations	360,681	360,681

Note: The table shows treatment effects based on a quadratic specification, without including covariates and year fixed effects and allowing for different slopes in the running variable on each side of the cutoff. Sample means at age 31 of the probability of having committed a crime and the number of crimes committed in a year is also presented. The relative change is computed as (treatment effect/mean)*100. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

6.1.1. Implications of Results

In this section, I present simple back-of-the-envelope calculations to illustrate and contextualize the main findings. The estimated RD effects capture the average treatment effect for individuals at the cutoff. Assuming this effect is representative of the whole analysis sample, it is possible to approximate how many individuals and crimes were affected by the treatment. This assumption is strong, as individuals further from the cutoff may differ systematically from those near it. These calculations should therefore be interpreted with caution and are intended primarily for illustrative purposes.

The treatment group (ages 21–29) includes 240,541 individual-year observations, while the control group (ages 30–40) includes 120,140 individual-year observa-tions. Applying the estimated effect on the probability of committing a crime, 0.72 percentage points, to the treatment group implies an increase of approximately 1,732 individuals committing a crime in a given year during the eight-year anal-ysis period ($240,541 \cdot 0.0072 = 1732$). Since the data represent individual-year observations and not unique individuals, this increase reflects the total number of individuals engaging in crime in a given year, summed across all years. In other words, the same person committing crimes over multiple years would contribute more than once to this total.

Applying the same effect to the control group suggests that 865 fewer individuals in this group committed a crime than would have been the case had they received the lower benefit levels of the treatment group ($120,140 \cdot (-0.0072) = -865$). Table 3 presents these estimated changes in the number of individuals committing crimes, as well as the corresponding changes in the number of crimes committed, for each type of crime.

Table 3. *Implications of RD estimates on number of individuals committing a crime and number of crimes committed*

	All crime	Property crime	Violent crime
Number of individual committing a crime			
Treatment group (21-29 years olds)	1,732	1,371	722
Control group (30-40 year olds)	-865	-685	-360
Number of ceimes committed			
Treatment group (21-29 years olds)	2,935	1,636	770
Control group (30-40 year olds)	-1,466	-817	-384

Note: The table shows the implications from the RD estimates on number of individuals committing a crime and number of crimes committed for the treatment and control group.

Source: Own calculations based on register data from Statistics Denmark.

The results in Table 3 indicate that the reduced transfer level below age 30 increased the number of individuals committing a property crime by 1,371 and a violent crime by 722 among those aged 21–29 over the eight-year period. In contrast, the number of individuals aged 30–40 committing property and violent crimes was lower by 685 and 360, respectively, relative to a scenario in which they had received the same (lower) benefit levels. Regarding the number of crimes committed, the lower benefits led to 2,935 additional offences among individuals aged 21–29, of which 1,636 were property crimes and 770 were violent crimes. Compared to a situation where individuals aged 30–40 received the same transfer levels as those under 30, the number of offences in this group would have risen by 1,466, including 817 property crimes and 384 violent crimes.

6.2. Heterogenous Effects

Prior research shows that welfare effects on crime can vary across demographic and socioeconomic groups (Dustmann et al. (2024a), Deshpande and Mueller-Smith (2022)). Understanding these heterogeneous effects on crime is important for identifying which groups are more likely to alter their criminal behavior in response to changes in welfare generosity. To explore this, I estimate treatment effects separately by covariates including gender, marital status, origin and parental background. The results are presented in Table 4.

The effects are strongest for males, who show significant increases across all outcomes (1.34 percentage points in crime probability; 0.0215 crimes), including both property (0.87 percentage points; 0.0101) and violent crime (0.52 percentage points; 0.0056). For females, effects are near zero and insignificant.

Among unmarried individuals, the probability of committing any crime rises by 0.72 percentage points and the number of crimes by 0.0135, with consistent increases in property and violent crime. No significant effects are found for married individuals.

By origin, Danes show moderate but significant increases in all crime and property crime, though not in violent crime. Non-Danes also exhibit significant increases across both property and violent crime, but with larger standard errors due to smaller sample size.

Looking at parental education, individuals whose parents lack vocational training show increases in crime probability (0.64 percentage points) and crime count (0.0168), although both are not significant at the 5 % level. When examining property crime, there is a significant increase both in probability and in number of crimes. Those with vocationally educated parents show no significant changes.

Finally, the effects are stronger among individuals whose parents have received social assistance: 1.14 percentage points in crime probability and 0.0173 in crime count, with robust and significant increases in both property and violent crime. No effects are found for those whose parents never received social assistance.

These heterogeneous effects suggest that the overall treatment impact observed in the main analysis is driven by certain vulnerable subpopulations, particularly males, unmarried individuals, those from disadvantaged backgrounds (e.g., low parental education or receipt of social assistance), and individuals not of Danish origin.

Table 4. *Effects of treatment on crime outcomes*

	All crime		Property crime		Violent crime		Observations
	Pr(crime)	#crimes	Pr(crime)	#crimes	Pr(crime)	#crimes	
	(1)	(2)	(3)	(4)	(5)	(6)	
All	0.0072**	0.0122**	0.0057***	0.0068**	0.0030**	0.0032**	360,681
	(0.0034)	(0.0056)	(0.0020)	(0.0030)	(0.0012)	(0.0014)	
Female	0.0011	0.0035	0.0018	0.0030	-0.0001	-0.0002	146,628
	(0.0032)	(0.0045)	(0.0021)	(0.0031)	(0.0008)	(0.0009)	
Males	0.0134***	0.0215**	0.0087***	0.0101**	0.0052***	0.0056***	214,053
	(0.0051)	(0.0086)	(0.0029)	(0.0045)	(0.0019)	(0.0022)	
Married	-0.0001	.0.0141	0.0034	0.0011	-0.0023	-0.0023	30,187
	(0.0082)	(0.0110)	(0.0049)	(0.0058)	(0.0027)	(0.0028)	
Unmarried	0.0072*	0.0135**	0.0060***	0.0074**	0.0036***	0.0038	330,494
	(0.0037)	(0.0062)	(0.0022)	(0.0033)	(0.0014)	(0.0015)	
Danish Origin	0.0064*	0.0117*	0.0045**	0.0054	0.0016	0.0020	286,974
	(0.0038)	(0.0061)	(0.0023)	(0.0034)	(0.0013)	(0.0015)	
Not Danish	0.0069	0.0090	0.0103**	0.0128*	0.0076**	0.0071**	73,707
	(0.0079)	(0.0138)	(0.0043)	(0.0067)	(0.0031)	(0.0033)	
Parents has	0.0071*	0.0089	0.0033	0.0021	0.0014	0.0010	241,338
vocational education	(0.0043)	(0.0070)	(0.0025)	(0.0038)	(0.0015)	(0.0017)	
Parent has no	0.0064	0.0168*	0.0099***	0.0148***	0.0056***	0.0068***	119,343
vocational education	(0.0059)	(0.0097)	(0.0034)	(0.0052)	(0.0022)	(0.0023)	
Parent has received	0.0114**	0.0173*	0.0075**	0.0094**	0.0054***	0.0053**	200,015
Social assistance	(0.0053)	(0.0089)	(0.0031)	(0.0047)	(0.0020)	(0.0022)	
Parent has not received	0.0003	0.0026	0.0031	0.0029	-0.0001	0.0004	160,666
Social assistance	(0.0043)	(0.0066)	(0.0025)	(0.0036)	(0.0014)	(0.0016)	

Note: The table shows treatment effects and sample means of the probability of committing a crime and the number of crime committed in a year. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

6.3. Comparison with Previous Literature

My findings align with the broader literature showing that welfare generosity affects crime. Consistent with Deshpande and Mueller-Smith (2022), Dustmann et al. (2024a), and Dustmann et al. (2024b), I find that lower benefits increases crime.

Property crime emerges as the most responsive across studies. I find a 0.57 percentage point increase in its probability, comparable to Dustmann et al. (2024a), who document a 2.2 percentage points increase among refugees in Denmark, and Deshpande and Mueller-Smith (2022), who report a 3.4 percentage point rise in income-generating offences, as a result of SSI removal in the U.S. Differences in magnitude likely reflect variations in populations and institutional settings, which makes the results harder to compare.

For violent crime, I find significant increases, especially among men and disadvan-taged groups. While Dustmann et al. (2024a) and Deshpande and Mueller-Smith (2022) report no significant effects on non-income crimes, Dustmann et al. (2024b) show increases in violent crime among adolescent refugees after benefit cuts.

Subgroup patterns also differ: my effects are strongest for men, whereas Dustmann et al. (2024a) find them for women, and Deshpande and Mueller-Smith (2022) report large relative increases for women. These contrasts likely reflect differences in baseline risks, vulnerabilities and policy contexts.

7. Validity and Robustness Check

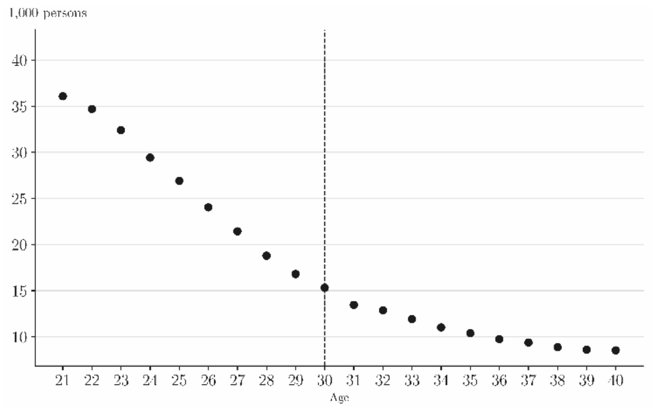
In this section I test the validity of the RD design and assess the robustness of the main findings. The main assumption of a valid RD design is that all covariates are balanced in the neighbourhood of the discontinuity. I validate this assumption by testing for manipulation in the running variable and by examining whether the observed baseline covariates are balanced on either side of the cutoff. The assumption could also be violated if other policies change discontinuously at age 30 and affect crime in my study period. To my knowledge, no such policies existed. I then assess the robustness of the main findings to a range of alternative specifications and sample restrictions. The goal is to examine whether the estimated effects of decreased social assistance below age 30 on crime outcomes are sensitive to key modeling choices.

7.1. Manipulation of the Running Variable

The RD design relies on the assumption that individuals cannot manipulate the running variable, here age (Lee and Lemieux (2010)). Misreporting age is highly unlikely, but manipulation could occur through other channels. One concern is that individuals delay applying for social assistance until turning 30, though this is rarely financially advantageous. Similarly, higher transfers at age 30 might discourage work or education just before the threshold. Since the sample excludes »activity ready« recipients, manipulation could arise if individuals can influence their classification to qualify for higher benefits. A related concern applies to excluding single providers: if individuals adjust childbirth or partnership decisions strategically, this too could be seen as manipulation. However, evidence suggests limited responsiveness of fertility and family structure to welfare benefits (Bargain and Doorley (2011)).

Lee and Lemieux (2010) argue that the RD design remains valid if individuals cannot precisely sort at the threshold, even with some influence. To ensure this is indeed the case, I do test for manipulation here by examining whether the aggregate distribution of the running variable is discontinuous at the cutoff. Figure 4 illustrates a first evidence of no manipulation, since no discontinuity in the age distribution of the analysed population is seen.

Figur 4. *Distribution of running variable*



Note: The dashed vertical line indicates the discontinuity point at age 30.
Source: Own calculations based on register data from Statistics Denmark.

McCrary (2008) proposes a formal density test to detect manipulation of the running variable, based on local linear regressions of the number of individuals in each age group just below and above the cutoff (Lemieux and Milligan (2008)). Following this approach, I estimate the regressions using a triangular kernel, as recommended by McCrary (2008). Table 5 reports results for different bandwidths around the cutoff, none of which reveal significant differences. Thus, the null of equal density of age on either side of the cutoff cannot be rejected, supporting the validity of the RD design.

Table 5. *Formal McCrary tests for discontinuity in running variable across different bandwidth choices*

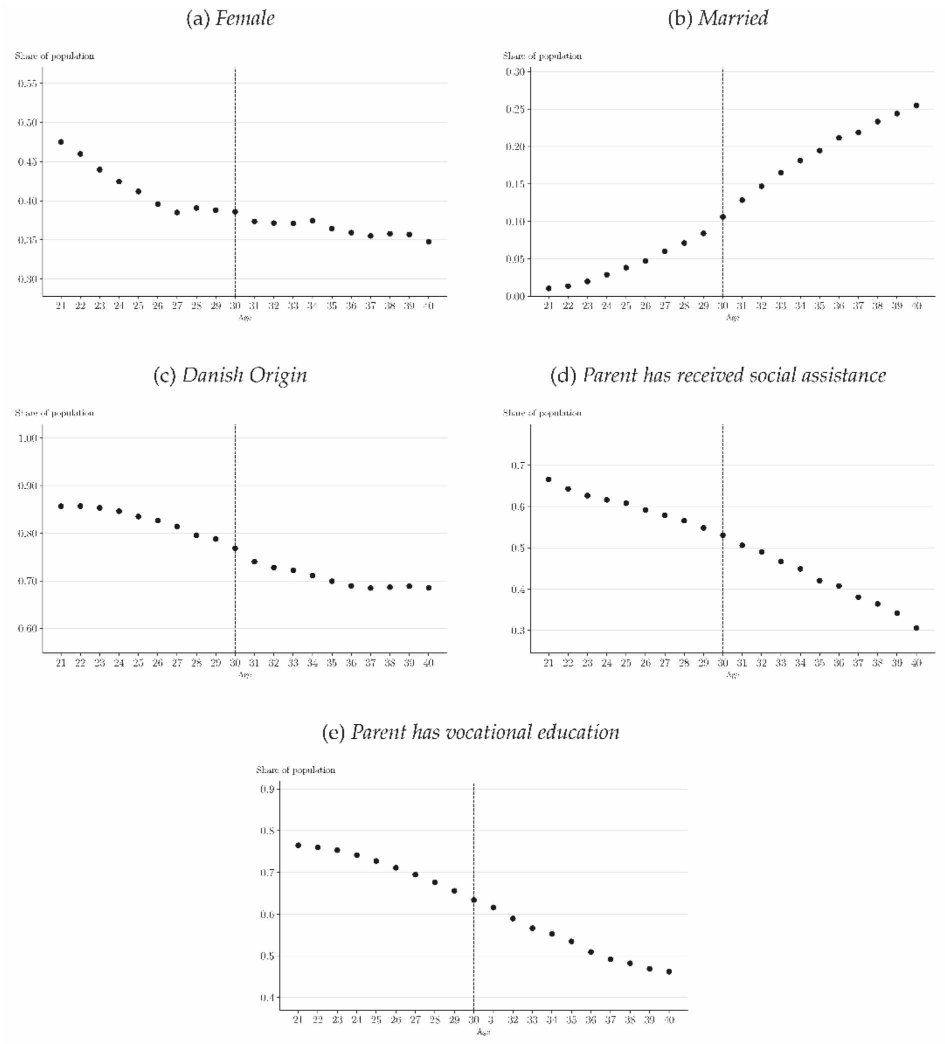
	2	3	4	5	6	7	8	8
Log difference	0.027	0.017	0.013	0.013	0.012	0.011	0.008	0.000
Standard error	0.017	0.014	0.012	0.011	0.010	0.009	0.009	0.008
T-statistic	1.56	1.23	1.05	1.17	1.21	1.18	0.90	0.02

Note: The table shows the log difference in density of the running variable around the cutoff and the corresponding standard errors. * p<0.1, ** p<0.05, *** p<0.01.
Source: Own calculations based on register data from Statistics Denmark.

7.2. Discontinuities in Covariates

Another way to test the validity of the RD design is to check whether baseline covariates are balanced around the cutoff (Lee and Lemieux (2010)). If treatment assignment is locally random, no discontinuities should appear. I assess this both graphically by plotting mean covariates by age and formally, by re-estimating equation (1) with each covariate as the outcome. A significant treatment effect would indicate a violation. Figure 5 shows the graphical results, which reveal no discontinuities except for the female indicator.

Figure 5. *Age distribution of covariates*



Note: The dashed vertical line indicates the discontinuity point at age 30.
Source: Own calculations based on register data from Statistics Denmark.

Table 6 reports the formal test results. At the age-30 cutoff in column 3, discontinuities are significant for female at a 10 % level and for marital status, Danish origin, and parent receiving social assistance (all at a 1 % level). This raises concerns about validity, but the large sample size lowers standard errors, which may explain why these effects appear statistically significant despite no clear discontinuities in the graphical analysis. Supporting this, estimates at alternative cutoffs also show significant effects of varying size.

Overall, both tests suggest possible discontinuities in most baseline covariates except parental vocational education, though the evidence likely reflects large-sample sensitivity rather than true imbalances. The dataset is therefore still used in the main analysis, with the reservation that baseline discontinuities may exist. To address this further, I later test robustness in a more homogeneous subsample in Section 7.5.

Table 6. *Formal test for discontinuity in covariates for different cutoffs*

	28 years (1)	29 years (2)	30 years (3)	31 years (4)	32 years (5)
Female	-0.0184***	-0.0051	0.0111*	0.0136*	0.0067
	(0.0064)	(0.0065)	(0.0061)	(0.0071)	(0.0076)
Married	0.0121***	0.0003	-0.0115***	-0.0088*	-0.0038
.	(0.0035)	(0.0039)	(0.0039)	(0.0050)	(0.0057)
Danish Origin	-0.0111**	-0.0058	0.0142***	0.0021	-0.0171**
	(0.0053)	(0.0055)	(0.0053)	(0.0064)	(0.0070)
Parent has vocational education	-0.0114*	-0.0092	-0.0028	-0.0072	-0.0073
	(0.0061)	(0.0063)	(0.0060)	(0.0071)	(0.0077)
Parent has received social assistance	0.067	0.116*	0.0204***	0.0140*	0.0134*
	(0.0065)	(0.0066)	(0.0062)	(0.0072)	(0.0078)

Note: The table shows estimation results of regressing each observable characteristic on the treatment dummy, using a quadratic specification and allowing for different slopes in the running variable on each side of the cutoff. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

7.3. Pseudo Cutoff

To further validate the RD design, I conduct pseudo-cutoff tests following Imbens and Lemieux (2008). These tests examine whether discontinuities similar to the estimated treatment effect appear at other points in the age distribution. Significant effects at arbitrary cutoffs would suggest that the main results do not reflect a causal impact of increased transfers. As recommended by Imbens and Lemieux (2008), I split the sample into two subsets below and above age 30 and estimate discontinuities at the median age in each (25 and 35, respectively), using the same specifications as in the main analysis. This approach avoids testing at the actual cutoff and increases power by focusing on central points within each subsample. Table 7 presents the results alongside those at the true cutoff. At age 30, significant treatment effects are found across all outcomes. In contrast, no consistent or significant effects emerge at the pseudo cutoffs. This confirms that the estimated effect is indeed a consequence of the policy.

Table 7. *Effects of treatment on crime outcomes – Pseudo cutoffs*

	Pr(crime)			#crimes		
	Actual	Pseudo		Actual	Pseudo	
	30 years	25 years	35 years	30 years	25 years	35 years
	(1)	(2)	(3)	(4)	(5)	(6)
All Crime						
Treatment effect	0.0072**	0.0027	-0.0085	0.0122**	0.0078	-0.0121
	(0.0034)	(0.0047)	(0.0056)	(0.0056)	(0.0081)	(0.0083)
Property crime						
Treatment effect	0.0057***	0.0046*	-0.0034	0.0068**	0.0027	-0.0044
	(0.0020)	(0.0027)	(0.0031)	(0.0030)	(0.0041)	(0.0044)
Violent crime						
Treatment effect	0.0030**	0.0019	0.0012	0.0032**	0.0007	0.0002
	(0.0012)	(0.0018)	(0.0020)	(0.0014)	(0.0019)	(0.0023)
Observations	360,681	240,541	120,140	360,681	240,541	120,140

Note: The table shows treatment effects of the probability of committing a crime and the number of crimes committed in a year. The effects is based on a quadratic specification without control variables. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

7.4. Placebo Test Using Pre-Reform Period

As a further test of the causal interpretation of the treatment effects, I run a placebo analysis using data from 2006–2013, when no increase in benefits occurred at age 30 (the discontinuity was instead at age 25). I replicate the main RD specification on the pre-reform data and results are reported in Table 8. Across almost all specifications, there is no evidence of a significant discontinuity at age 30. In the few cases where significant effects do appear (e.g. in the linear model with controls), they are negative and inconsistent across specifications. This placebo test thus strongly supports the validity of the main findings.

Table 8. *Effects of treatment on crime outcomes – Prereform years 2006-2013*

	Pr(crime)				#crime			
	Linear		Quadratic		Linear		Quadratic	
	(1)		(2)		(3)		(4)	
All crime								
Treatment effect	-0.0007	-0.0045***	0.0005	-0.0010	-0.0017	-0.0075***	0.0019	-0.0003
	(0.0018)	(0.0018)	(0.0029)	(0.0029)	(0.0029)	(0.0029)	(0.0048)	(0.0047)
Property crime								
Treatment effect	-0.0008	-0.0023*	0.0024	0.0018	-0.0023	-0.0042**	0.0022	0.0014
	(0.0012)	(0.0012)	(0.0020)	(0.0020)	(0.0017)	(0.0017)	(0.0029)	(0.0029)
Violent crime								
Treatment effect	0.0005	-0.0002	0.0022*	0.0019	0.0004	-0.0003	0.0022*	0.0018
	(0.0007)	(0.00078)	(0.0012)	(0.0012)	(0.0008)	(0.001)	(0.0013)	(0.0013)
Including controls	No	Yea	No	Yes	No	Yes	No	Yes
Observations	467.191	467.191	467.191	467.191	467.191	467.191	467.191	467.191

Note: The table shows treatment effects and sample means of the probability of committing a crime and the number of crime committed in a year. Robust standard errors in parentheses. Whenever indicated regressions include controls for covariates and year fixed effects. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

7.5. Restricting the Sample to a More Homogeneous Population

A key identifying assumption in the RD design is that baseline characteristics do not exhibit discontinuous changes around the cutoff. In Section 7.2., I investigated thus assumption by examining the distribution of covariates both graphically and formally. While the graphical analysis showed no major visible jumps except for female, the formal analysis revealed significant discontinuities at age 30 in several covariates.

To further assess the robustness of the main results, I reestimate the treatment effects within a subsample of individuals who are homogeneous with respect to these covariates. In particular, I focus on unmarried males of Danish origin whose parent(s) have received social assistance. This subsample is selected for two reasons. First, it helps eliminate any potential bias arising from discontinu-ities in these covariates at the cutoff. Second, the heterogeneous effects analysis previously showed that these characteristics are associated with the largest treat-ment effects. While significant effects were also found among individuals not of Danish origin, I restrict the sample to Danes to avoid narrowing the population too severely.

Table 9 presents the results based on this more homogeneous group. The findings in general show strong and statistically significant treatment effects across all crime categories. The effect on property crime remains significant in terms of probability, while the effect on the number of property crimes is larger in magnitude to the main sample it is not significant. For violent crime, both the probability and number of offences rise, with the latter significant at the 10 % level. It is worth noting that while the estimated effects are larger in magnitude compared to the full sample, reflecting the high responsiveness of this more disadvantaged group, the standard errors have also increased, likely due to the smaller sample size. This reduced precision may explain why some estimates are not significant despite showing substantively meaningful reductions.

These results confirm that the main findings are not driven by imbalances in covariates around the cutoff. Even when restricting the analysis to a relatively uniform population, the treatment effects remain large and statistically significant, particularly for all crime and probability of committing a property crime. This strengthens the credibility of the causal interpretation and highlights that the policy's impact is especially pronounced among disadvantaged subgroups.

Table 9. *Effects of treatment on crime outcomes – Homogenous population*

	All crime (1)	**Property crime** (2)	**Violent crime** (3)
Pr(crime)	0.0236***	0.0121**(0.0053)	0.0060*(0.0035)
	(0.0089)	(0.0053)	(0.0035)
#crimes	0.0390***	0.0122	0.0066*
	(0.0148)	(0.0080)	(0.0039)
Observations	86,908	86,908	86,908

Note: The table shows treatment effects for a population consisting of unmarried male individuals with a Danish origin and at least one parent who has received social assistance. The effects is based on a quadratic specification without control variables. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

7.6. Additional robustness checks

Additional robustness checks are reported in the appendix and summarized below to further assess the reliability of the main results. Because results from a parametric approach can be sensitive to functional form, I compare linear and quadratic specifications with and without covariates (Table A.4), allowing for different slopes on each side of the cutoff. Across all outcomes, estimates remain positive and statistically significant, though magnitudes vary by polynomial order. Imbens and Lemieux (2008) note that including covariates and year fixed effects can improve precision and reduce bias. In large samples they should not affect the discontinuity if the design is valid and functional form correctly specified. This holds here: in the quadratic model, adding covariates leaves estimates and standard errors unchanged, while statistically significant differences appear in the linear case for all and property crime. The robustness of the quadratic specification supports its use as the preferred functional form.

I also test sensitivity to the choice of bandwidth by re-estimating the main speci-fication with narrower and wider windows around the age-30 cutoff. Bandwidth choice involves a trade-off between bias and precision. Wider windows increase observations and precision but risk bias if the functional form is misspecified, while narrower windows reduce bias but raise standard errors due to smaller sample sizes. Table A.8 reports results for different bandwidths. Across all win-dows, estimated effects remain positive and stable in magnitude. For all crime, the treatment effect is positive throughout, significant at the 10 % level in some narrower windows but not others. For property crime, results are consistently significant at least at the 10 % level. For violent crime, estimated effects are con-sistently positive but only significant at the 10 % level in most cases. Overall, the findings for property and violent crime are particularly robust, confirming that lower welfare benefits increase criminal behavior.

The analysis is also repeated for individuals assessed as activity-ready in all four quarters during the years 2014–2021 (Table A.5). These individuals were excluded from the main analysis in Table 2, as they were not subject to reductions in social assistance. The results for this group are consistently smaller and statistically insignificant across nearly all specifications, which further supports the validity of the main findings.

A donut specification excluding individuals who turn 30 during the calendar year (Table A.9) produces estimates similar to the baseline. Effects on property and violent crime remain strong and significant, while results for all crime are somewhat weaker, reflecting the reduced sample size.

Finally, when limiting the sample to individuals who received social assistance for at least 12 weeks during the year (Section A.10, Table A.10), the estimated effects become larger in magnitude across all crime categories, consistent with stronger responses among those more financially dependent on transfers.

8. Discussion and Concluding Remarks

In this paper, I study the causal effect of reduced social assistance eligibility below age 30 on criminal behavior among welfare recipients in Denmark. Using administrative data covering the full Danish population from 2014 to 2021, and exploiting a sharp discontinuity in benefit levels at age 30, I implement a regression discontinuity (RD) design to estimate the local average treatment effect of lower transfers on two primary crime outcomes: the probability of committing a crime that is later convicted and the number of crimes committed per year. The analysis considers three crime categories, all crime (excluding traffic offences), property crime, and violent crime, and includes a range of robustness checks and heterogeneity analyses. To my knowledge, this is the first study to estimate the causal effects of the lower social assistance below age 30 on crime.

The results reveal a clear and consistent pattern: eligibility for lower social assistance below age 30 significantly increases criminal behavior among welfare recipients. The estimated effect corresponds to an increase of approximately 0.72 percentage points in the probability of committing any crime and a 0.0122 increase in the number of crimes committed annually. These effects represent increases of around 8-10 % relative to the crime rates of individuals just above the threshold. The analysis further shows that the increase in crime are particularly pronounced for property and violent crimes. For property crime, the probability of committing an offence rise by 0.57 percentage points, and the number of of-fences by 0.0068, corresponding to relative increases of 17-19 %. For violent crime, the increases are 0.3 percentage points and 0.0032 in probability and number, re-spectively, amounting to increases of 27-28 %. Heterogeneity analyses reveal that the treatment effects are concentrated among males, unmarried individuals, and those from more disadvantaged backgrounds.

These findings are robust across multiple specifications, alternative bandwidths, placebo tests, and subsample restrictions. The effects on property and violent crimes remain consistently statistically significant and stable across robustness checks, suggesting that these categories are especially responsive to changes in benefit levels.

One potential concern in regression RD designs where treatment is assigned based on age is the possibility of anticipation effects. Since individuals can foresee the increase in benefits occurring at age 30, they may adjust their behavior prior to becoming eligible. In this context, if individuals reduce their engagement in crime in anticipation of receiving higher transfers at age 30, the estimated discontinuity would be biased downward, and the true effect of the benefit decrease might be understated.

This issue has been discussed in related literature studying labour market responses to age-based eligibility rules. Jonassen (2013), examining the effect of higher social assistance at age 25 on benefit uptake, raises a similar concern but finds no clear evidence of behavioural changes just before the cutoff. His interpretation is that individuals receiving or at risk of receiving social assistance are typically liquidity constrained and thus unlikely to engage in consumption smoothing. Behavioural adjustments are therefore more likely to occur immediately after becoming eligible for higher transfers. This interpretation is consistent with my findings. The graphical evidence in Section 6.1. shows no notable change in criminal behavior at age 29. This suggests that individuals do not appear to respond to the expected benefit increase in advance.

While the RD design offers strong internal validity, the estimates reflect local treat-ment effects around the age-30 threshold and cannot be easily generalized beyond this context without further assumptions. Moreover, as the analysis is based on conviction data, the outcomes capture changes in recorded and prosecuted crime rather than total offending.

Nonetheless, the findings point to potential implications for how welfare policies are designed. The observed behavioural responses to decrease in benefits suggest that structural features of welfare systems, such as eligibility thresholds, can shape individual decisions in meaningful ways. While further research is needed to explore whether similar effects emerge in other contexts or persist over time, the results indicate that changes in benefit structures may affect not only factors like labour market participation, but also other aspects of individual behavior, including involvement in crime. As such, when designing welfare programs, policymakers might benefit from considering the broader consequences of eligibility rules, including the potential for increased criminal behavior, alongside traditional labour market incentives. This study adds to a growing body of evidence suggesting that income support policies may carry wider social benefits beyond their economic objectives.

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A. Appendix

A1. Danish Social Assistance System: Terms

Table A.1. *Definition of Terms in Relation til the Danish Social Assistance System*

Term	In Danish	Explanation
Social assistance	–	A general term covering different types of municipal financial support for individuals who are unable to support themselves or their families. Includes: cash benefits, ed-ucational assistance, transition benefits, and self-sufficiency and return benefits.
Social assistance system	»Kontanthjælpssystemet«	The overall system that provides the various types of financial support.
Vocational Education	»Erhvervskompetencegivende Uddannelse«	Practical qualifications that relate to a specific job or industry, or a tertiary education.
Cash Benefits	»Kontanthjælp«	Financial support granted to indivi-duals aged 30 and above who meet the eligibility criteria.
Cash benefits at youth rate	»Kontanthjælp på ungesats«	A reduced version of cash benefits provided to individuals under 30 who have completed a vocational education.
Educational assistance	»Uddannelseshjælp«	Financial support given to individu-als under 30 who have not completed a vocational education.
Job-ready	»Jobparat«	An assessment category for reci-pients of cash benefits (including youth rate) who are deemed by the job centre to be able to work within a short period of time.
Clearly education-ready	»Åbenlys Uddannelsesparat«	An assessment category for reci-pients of educational assistance who are considered able to begin an edu-cation immediately and com-plete it.
Education-ready	»Uddannelsesparat«	An assessment category for reci-pients of educational assistance who are expected to be ready to start and complete an education within appro-ximately one year.
Activity-ready	»Aktivitetsparat«	An assessment category for reci-pients of any type of social assistance who are not expected to be able to work within a short period of time or begin an education within 1 year.

Source: Lifeinddenmark.dk.

A2. Description of Data – The different registers

The population register (BEF) provides demographic information on all individuals residing in Denmark. This includes variables such as age, date of birth, gender, marital status, household type, address identifier, origin status (Danish or not) and personal identification numbers for the individual's mother and father.

The parental identification numbers and address identifiers are used to determine whether an individual is a provider for children. Individuals with at least one child under the age of 18 living at the same address as themselves are defined as providers. Single providers are identified using the variable for household type. The parental identifiers are also used in conjunction with the education and DREAM register to obtain information about the individual's parents, such as whether they hold a vocational qualification or have ever received social assistance.

The primary measure of criminal behavior is based on records from the Central Crime Register. This register contains comprehensive data on all arrests, charges and court verdicts. Each entry in the dataset contains case-specific identifiers as well as the individual's personal identification number, which makes it possible to match each crime to individuals in the population data. The register also provides detailed information on the offence and conviction dates, along with offence codes that makes it possible to classify crimes by type. Following the approach used in Dustmann et al. (2024a), only crimes that result in a conviction are included in the analysis. Additionally, crimes are attributed to the year in which the offence occurred rather than the year of conviction.

The offence codes enable differentiation between types of crimes. In this study, crimes are categorised into three groups: all crimes, property crimes, and violent crimes, consistent with the classifications used in Dustmann et al. (2024a) and Albæk et al. (2017).[9] The category of all crimes encompasses all violations of the criminal code, excluding traffic offences. This exclusion is justified by the high frequency of traffic-related convictions relative to other types of crime. Including traffic offences does not produce a discontinuity at age 30, as shown in Figure A.3.

To identify which individuals receive social assistance, I use data from the DREAM register, which provides weekly records of all public income transfers in Denmark. All individuals who receive transfer payments are recorded, along with informa-tion about the type of benefit received during each week. The register records a transfer even if the individual only receives the benefit for a single day within that week. In cases where multiple types of transfers are received in the same week, only one is recorded according to a prioritised hierarchy. In addition to income transfers, the DREAM database contains quarterly data on assessment

9. Offence codes are drawn from the variable »AFG_GER7«.

categories assigned by caseworkers to individuals receiving cash benefits or educational as-sistance. These categories reflect the most recent evaluation at the end of each quarter (dst.dk).

Before 2014, all recipients of social assistance were classified into three so-called match groups, regardless of age or educational background. For individuals re-ceiving social assistance after the reform, the assessment category »job ready« corresponds to the former match group 1, while »activity ready« corresponds to match groups 2 and 3 (see DEC (2015) for further details). However, infor-mation on match group classification is not available for the years 2006 to 2008. Consequently, for the years 2009 to 2013, I exclude individuals who are placed in match groups 2 or 3 for all four quarters of the year. This may limit the comparability between the pre-reform sample and the sample used in the main analysis from 2014 to 2022. Nonetheless, since benefit levels did not differ between match groups prior to the reform, any potential bias from this limitation is expected to be minimal.

A3. Descriptive Statistics

Table A.2. *Descriptive statistics for the main analysis sample*

	All		Treatment (Age 21-29)		Control (Age 30- 40)	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std.dev.
Treatment = 1	0.667	(0.471)	1.000	–	0.000	–
Age	27.71	(5.467)	24.367	(2.497)	34.403	(3.172)
Covariates:						
Married	0.084	(0.277)	0.035	(0.185)	0.180	(0.385)
Female	0.407	(0.491)	0.426	(0.495)	0.367	(0.482)
Danish origin	0.796	(0.403)	0.836	(0.370)	0.714	(0.452)
Parent has vocational education	0.669	(0.471)	0.730	(0.444)	0.548	(0.498)
Parent has received social assistance	0.555	(0.497)	0.613	(0.487)	0.437	(0.496)
Crime variables:						
All crime – probability	0.088	(0.284)	0.097	(0.296)	0.071	(0.256)
All crime – number	0.126	(0.479)	0.142	(0.517)	0.093	(0.391)
Property crime – probability	0.028	(0.164)	0.030	(0.172)	0.022	(0.147)
Property crime – number	0.035	(0.237)	0.039	(0.248)	0.028	(0.214)
Violent crime – probability	0.011	(0.104)	0.012	(0.110)	0.009	(0.093)
Violent crime – number	0.012	(0.114)	0.013	(0.120	0.009	(0.101)
Observations	360,681		240,541		120,140	

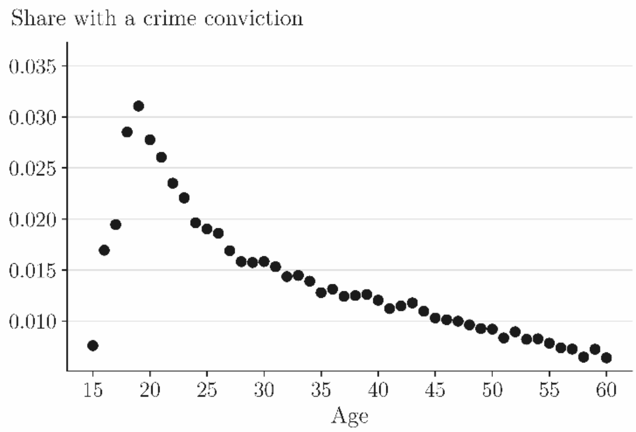
Note: The table shows sample means for the main sample, the treatment group of individuals aged 21-29 and the control group of individuals aged 30-40. Standard deviations in parentheses.

Source: Own calculations based on register data from Statistics Denmark.

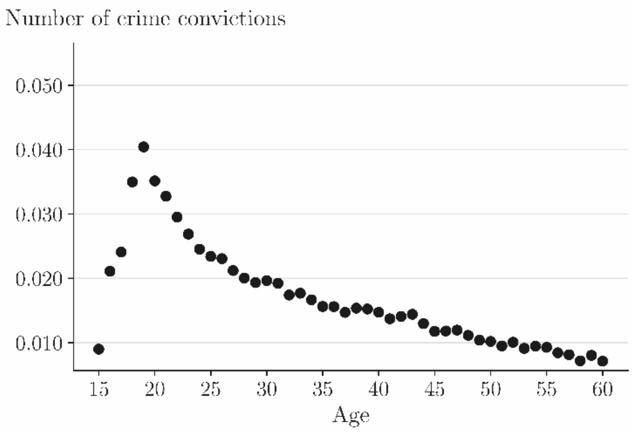
A4. Figures

Figure A.1. *All crime – Whole Danish population in 2021*

(a) *Probability of committing a crime that results in a conviction*

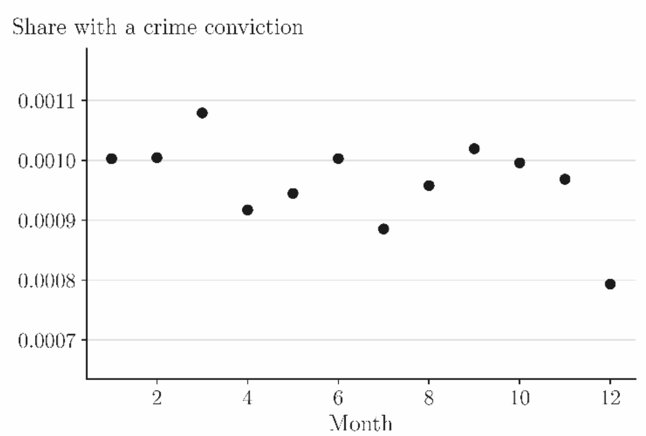


(b) *Crime convictions*



Note: The figure plots average outcome by age for the whole Danish population in 2021. All crime includes all offences against the criminal code except all traffic related offences.
Source: Own calcula-tions based on register data from Statistics Denmark.

Figure A.2. *Monthly probability of committing a crime – Whole Danish population in 2021*

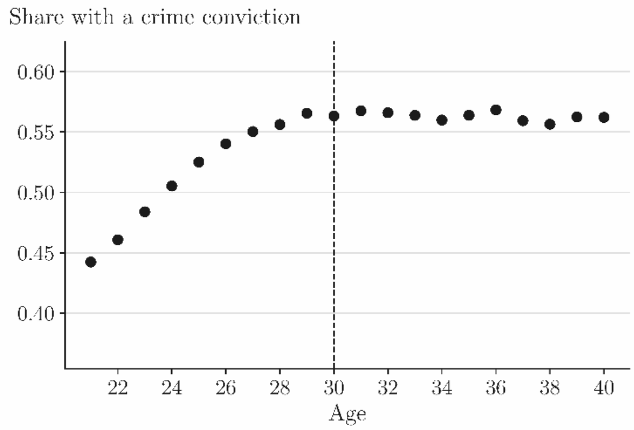


Note: The figure plots the share of individuals committing a crime that later let to a conviction in each month of 2021.

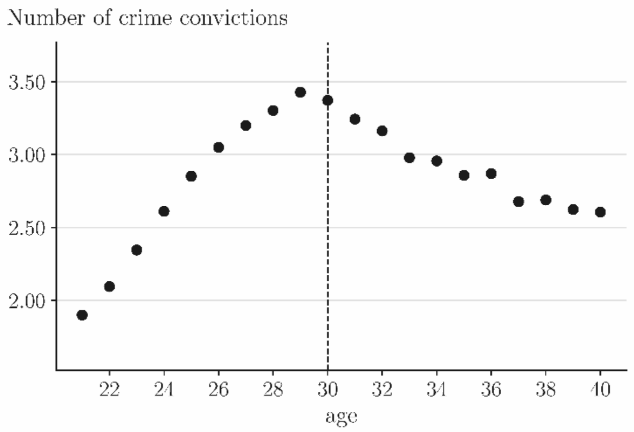
Source: Own calculations based on register data from Statistics Denmark.

Figure A.3. *All crime including traffic related offences*

(a) *Probability of committing a crime that results in a conviction*



(b) *Crime convictions*

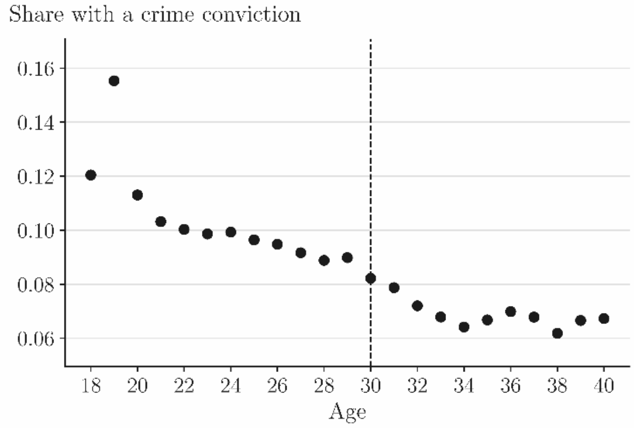


Note: The figure plots average outcome by age. The dashed vertical line indicates the discontinuity point at age 30, where individuals are eligible for the higher benefits.

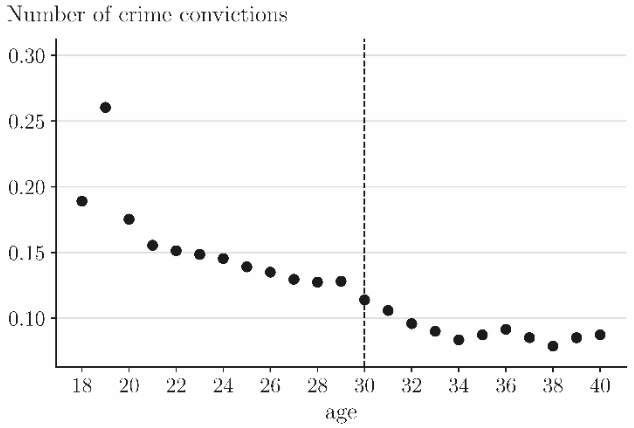
Source: Own calculations based on register data from Statistics Denmark.

Figure A.4. *All crime – Ages 18-40*

(a) *Probability of committing a crime that results in a conviction*



(b) *Crime convictions*



Note: The figure plots average outcome by age for ages 18 to 40. All crime includes all offences against the criminal code except all traffic related offences.The dashed vertical line indicates the discontinuity point at age 30, where individuals are eligible for the higher benefits.

Source: Own calculations based on register data from Statistics Denmark.

A5. Tables and Robustness Check

Table A.3. *Akaike information criteria (AIC) – Different polynomial specifications*

	Linear	Quadratic	Cubic	Specification with lowest AIC
All crime				
Pr(crime)	113,713	113,710	113,713	Quadratic
#crimes	491,691	491,687	491,690	Quadratic
Property crime				
Pr(crime)	-281,600	-281,602	-281,598	Quadratic
#crimes	-14,433	-14,438	-14,434	Quadratic
Violent crime				
Pr(crime)	-606,109	-606,105	-606,101	Linear
#crimes	-545,205	-545,201	-545,199	Linear

Note: The shows the Akaike information criteria for different polynomial specifications based on equation (1) without control variates. The specifications allow for different slopes before and after the cutoff.

Source: Own calculations based on register data from Statistics Denmark.

Table A.4. *Effects of treatment on crime outcomes – Different Polynomial Specifications*

	Quadratic		Linear	
	(1)	(2)	(3)	(4)
All crime				
Pr(crime)	0.0072**	0.0075**	0.0129***	0.0118***
#crimes	(0.0034)	(0.0034)	(0.0021)	(0.0021)
	0.0122**	0.0128**	0.0199***	0.0187***
	(0.0056)	(0.0056)	(0.0034)	(0.0034)
Property crime				
Pr(crime)	0.0057***	0.0056***	0.0055***	0.0052***
#crimes	(0.0020)	(0.0020)	(0.0012)	(0.0012)
	0.0068**	0.0067**	0.0076***	0.0071***
	(0.0030)	(0.0030)	(0.0018)	(0.0018)
Violent crime				
Pr(crime)	0.0030**	0.0031**	0.0031***	0.0029***
#crimes	(0.0012)	(0.0012)	(0.0008)	(0.0008)
	0.0032**	0.0033**	0.0031***	0.0029***
	(0.0014)	(0.0014)	(0.0008)	(0.0008)
Including controls	No	Yes	No	Yes
Observations	360,681	360,681	360,681	360,681

Note: The table shows treatment effects of the probability of having committed a crime and the number of crimes committed in a year. Column (1) presents the results from the preferred specification used in the main analysis. Robust standard errors in parentheses. Whenever indicated regressions include controls for covariates and year fixed effects. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

Table A.5. *Effects of treatment on crime outcomes – Only activity ready*

	Pr(crime)				#crimes			
	Linear		Quadratic		Linear		Quadratic	
	(1)		(2)		(3)		(4)	
All crime								
Treatment effect	0.0019	0.0052**	0.0038	0.004	0.006	0.0116**	0.0035	0.0038
	(0.0025)	(0.0024)	(0.0040)	(0.0039)	(0.0046)	(0.0046)	(0.0076)	(0.0074)
Property crime								
Treatment effect	0.0008	0.0019	0.004	0.0041	0.0005	0.0022	0.0008	0.0008
	(0.0015)	(0.0015)	(0.0025)	(0.0025)	(0.0027)	(0.0027)	(0.0043)	(0.0043)
Violent crime								
Treatment effect	-0.0001	0.0005	0.0011	0.0011	0.0002	0.0009	0.0016	0.0016
	(0.0010)	(0.0010)	(0.0017)	(0.0017)	(0.0011)	(0.0011)	(0.0018)	(0.0018)
Including controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	287,419	287,419	287,419	287,419	287,419	287,419	287,419	287,419

Note: The table shows treatment effects of the probability of committing a crime and the number of crimes committed in a year. Robust standard errors in parentheses. Whenever indicated regressions include controls for covariates: Female, Married, Danish Origin, parent has received social assistance and parent has a vocational education. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

Table A.6: *Effects of treatment on crime outcomes – With single providers*

	Pr(crime)				#crimes			
	Linear		Quadratic		Linear		Quadratic	
	(1)		(2)		(1)		(2)	
All crime								
Treatment effect	0.0088***	0.0092***	0.0051*	0.0068**	0.0136***	0.0147***	0.0092*	0.0119**
	(0.0019)	(0.0019)	(0.0031)	(0.0030)	(0.0030)	(0.0030)	(0.0050)	(0.0049)
Property crime								
Treatment effect	0.0043***	0.0044***	0.0053***	0.0056***	0.0060***	0.0060***	0.0065**	0.0068**
	(0.0011)	(0.0011)	(0.0018)	(0.0018)	(0.0016)	(0.0016)	(0.0027)	(0.0027)
Violent crime								
Treatment effect	0.0023***	0.0023***	0.0022**	0.0024**	0.0022***	0.0023***	0.0024*	0.0026**
	(0.0007)	(0.0007)	(0.0011)	(0.0011)	(0.0007)	(0.0007)	(0.0012)	(0.0012)
Including controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	403,560	403,560	403,560	403,560	403,560	403,560	403,560	403,560

Note: The table shows treatment effects of the probability of committing a crime and the number of crimes committed in a year. Robust standard errors in parentheses. Whenever indicated regressions include controls for covariates: Female, Married, Danish Origin, parent has received social assistance and parent has a vocational education. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

Table A.7. *Effects of treatment on crime outcomes – For each year*

	2014	2015	2016	2017	2018	2019	2020	2021
All crime								
Pr(crime)	-0.0025	0.0280***	0.0048	0.0182*	0.0087	-0.0032	-0.0078	0.0096
	(0.0091)	(0.0090)	(0.0089)	(0.0094)	(0.0102)	(0.0108)	(0.0101)	(0.0110)
#crimes	-0.0133	0.0360**	0.0052	0.0312**	0.0191	0.0127	-0.0043	0.0107
	(0.0149)	(0.0142)	(0.0147)	(0.0154)	(0.0166)	(0.0179)	(0.0167)	(0.0178)
Property crime								
Pr(crime)	-0.0006	0.0152***	0.0015	0.0105*	0.0054	0.0016	0.0023	0.0115*
	(0.0054)	(0.0055)	(0.0054)	(0.0055)	(0.0056)	(0.0058)	(0.0058)	(0.0062)
#crimes	0.0030	0.0225***	0.0050	0.0138*	0.0068	0.0006	-0.0017	0.0108
	(0.0086)	(0.0079)	(0.0085)	(0.0079)	(0.0088)	(0.0084)	(0.0085)	(0.0095)
Violent crime								
Pr(crime)	0.0004	0.0045	-0.0005	0.0076**	0.0039	0.0060	0.0023	-0.0006
	(0.0030)	(0.0029)	(0.0033)	(0.0034)	(0.0040)	(0.0042)	(0.0036)	(0.0042)
#crimes	0.0004	0.0041	-0.0001	0.0069*	0.0063	0.0064	0.0034	-0.0022
	(0.0032)	(0.0033)	(0.0036)	(0.0036)	(0.0045)	(0.0046)	(0.0041)	(0.0046)
Observations	56,700	53,520	50,342	44,719	41,780	40,279	41,644	31,697

Note: The table shows treatment effects of the probability of committing a crime and the number of crimes committed in a year. The effects is based on a quadratic specification without control variables. Robust standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Source: Own calculations based on register data from Statistics Denmark.

Table A.8. *Effects of treatment on crime outcomes – Different window around cutoff*

	Ages							
	Main sample, 21-40	18-40	22-38 (±8)	23-37 (±7)	24-36 (±6)	25-35 (±5)	26- 34 (±4)	27-33 (±3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All Crime								
Pr(crime)	0.0072**	0.0158***	0.0066*	0.0045	0.0052	0.0068	0.0107*	0.0124*
	(0.0034)	(0.0032)	(0.0038)	(0.0041)	(0.0046)	(0.0051)	(0.0058)	(0.0068)
#crimes	0.0122**	0.0273***	0.0131**	0.0118*	0.0106	0.0116	0.0147	0.0126
	(0.0056)	(0.0052)	(0.0062)	(0.0068)	(0.0075)	(0.0084)	(0.0096)	(0.0112)
Property crime								
Pr(crime)	0.0057***	0.0098***	0.0054**	0.0060**	0.0052*	0.0060**	0.0066*	0.0082**
	(0.0020)	(0.0018)	(0.0022)	(0.0024)	(0.0027)	(0.0030)	(0.0034)	(0.0040)
#crimes	0.0068**	0.0124***	0.0068**	0.0078**	0.0062	0.0069	0.0073	0.0075
	(0.0030)	(0.0028)	(0.0034)	(0.0037)	(0.0041)	(0.0046)	(0.0053)	(0.0063)
Violent crime								
Pr(crime)	0.0030**	0.0057***	0.0027*	0.0025*	0.0032*	0.0032*	0.0026	0.0028
	(0.0012)	(0.0011)	(0.0014)	(0.0015)	(0.0017)	(0.0019)	(0.0021)	(0.0025)
#crimes	0.0032**	0.0060***	0.0027*	0.0027	0.0032*	0.0035*	0.0033	0.0037
	(0.0014)	(0.0013)	(0.0015)	(0.0017)	(0.0019)	(0.0021)	(0.0025)	(0.0030)
Observations	360,681	446,514	307,453	263,877	222,104	182,923	145,632	110,595

Note: The table shows treatment effects and sample means of the probability of committing a crime and the number of crime committed in a year. The effects is based on a quadratic specification without control variables. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

Table A.9. *Effects of treatment on crime outcomes – Donut Specification*

	All crime	**Property crime**	**Violent crime**
	(1)	(2)	(3)
Pr(crime)	0.0056	0.0057***	0.0030**
	(0.0038)	(0.0022)	(0.0014)
#crimes	0.0120*	0.0077**	0.0031**
	(0.0062)	(0.0033)	(0.0015)
Observations	345,357	345,357	345,357

Note: The table shows treatment effects and sample means of the probability of committing a crime and the number of crimes committed in a year using a donut specification, where individuals aged 30 are removed. The effects is based on a quadratic specification without control variables. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.

A5.1. Restricting to Individuals who Receives Social Assistance for 12 Weeks

In the main analysis, the sample includes all individuals who received social assistance for at least one week during the year. While this captures the broader population exposed to the transfer system, one might expect that individuals who are more dependent on social assistance, and thus more financially constrained, would be more strongly affected by the decrease in benefits below age 30. To explore this possibility, I restrict the sample to individuals who received social assistance for at least 12 weeks (not necessarily consecutively) during the year.

Table A.10 presents the results from this sample. The findings indicate that the treatment effects remain strong and statistically significant, and are in fact larger in magnitude compared to the baseline results as expected. The probability of committing any crime rise by 0.85 percentage points, and the number of crimes increases by 0.0164, both significant at the 5 % level. The effects are particularly pronounced for property crime and violent crime, with all estimates significant at the 5 or 1 % level.

These results support the intuition that individuals with greater exposure to the welfare system respond more strongly to changes in benefit levels. This is consis-tent with economic theory, as those who rely more heavily on transfers are likely to experience a larger change in effective income and incentives when benefits decrease. The robustness of the findings in this more economically vulnerable subgroup further strengthens the credibility of the main results.

Table A.10. *Effects of treatment on crime outcomes – Individuals receiving social assis-tance for at least 12 week*

	All crime	**Property crime**	**Violent crime**
	(1)	(2)	(3)
Pr(crime)	0.0085**	0.0077***	0.0050***
	(0.0041)	(0.0024)	(0.0014)
#crimes	0.0164**	0.0089**	0.0051***
	(0.0068)	(0.0038)	(0.0016)
Observations	258,727	258,727	258,727

Note: The table shows treatment effects for individuals receiving social assistance at least 12 weeks of the year. The effects is based on a quadratic specification without control variables. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Source: Own calculations based on register data from Statistics Denmark.