Essays on welfare benefits, employment, and crime
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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.

4.1 Introduction

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.

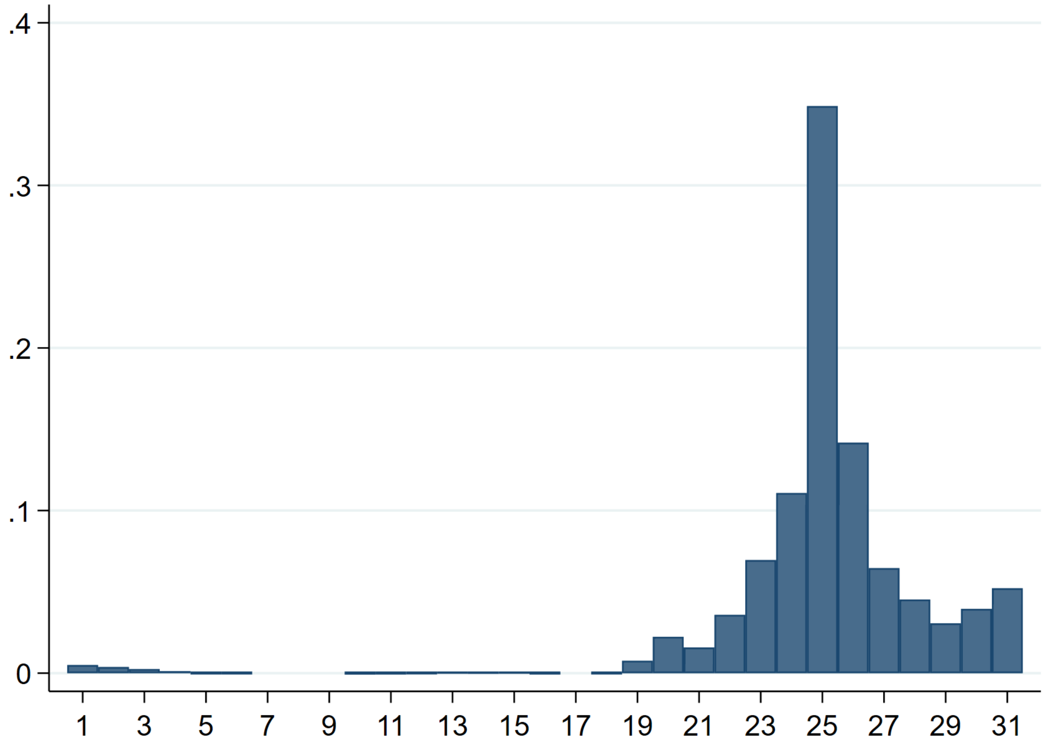
Table 4.1: Data coverage per included municipality

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

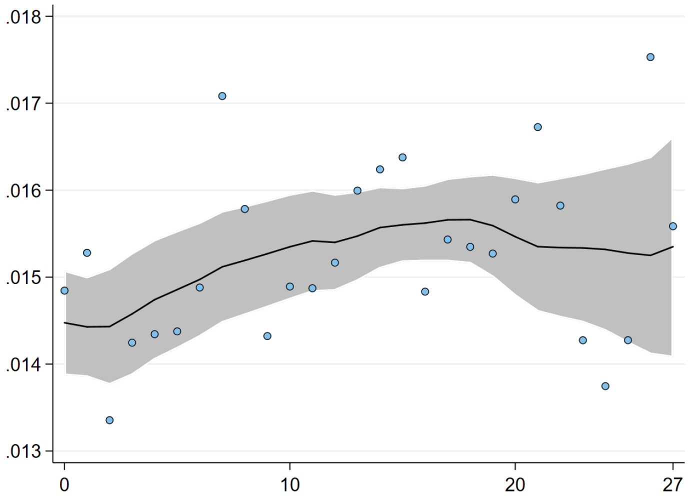
Before turning to the estimation results, we present exploratory graphs on the evolution of crime outcomes over the number of days that have passed since welfare payment receipt. Figures 4.2a to 4.2c present local polynomial smooth plots of average daily crime rates, including 95% confidence intervals.

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.

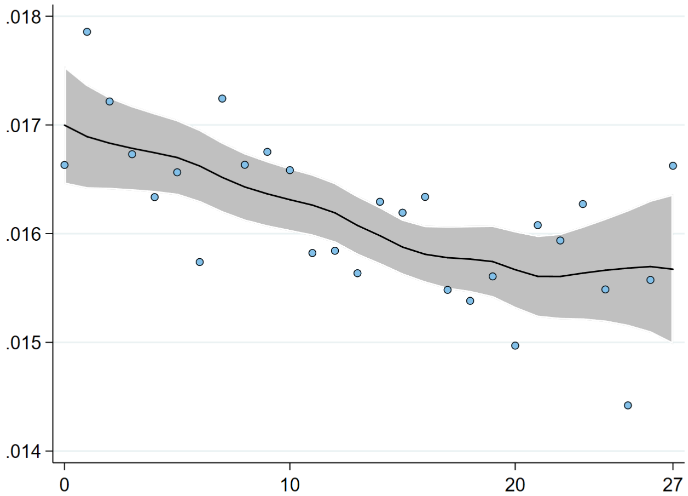
Figure 4.2c shows the change in crime in general to be more limited, as compared to Figures 4.2a and 4.2b. This may be attributable to the inversive trends of financially-motivated and other crime among the full sample. Although exacerbated by the larger y-axis scale, all crime appears to be less affected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

Figure 4.2: Crime rates over days since payment

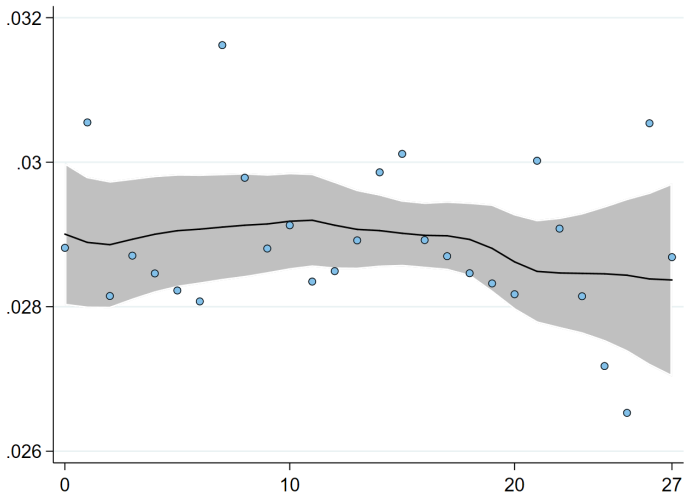
(a) Financially-motivated crime



(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.