Crime over the welfare payment cycle

Stam, M.T.C.; Knoef, M.G.; Ramakers, A.A.T.

Received: 30 January 2023 | Accepted: 19 March 2024

DOI: 10.1111/ecin.13223

ORIGINAL ARTICLE

Economic Inquiry

Crime over the welfare payment cycle

Marco T. C. Stam[1,2] | **Marike G. Knoef**[3] | **Anke A. T. Ramakers**[4]

[1]Department of Sociology, Tilburg University, Tilburg, The Netherlands

[2]Department of Economics, Tilburg University, Tilburg, The Netherlands

[3]Department of Econometrics & OR, Tilburg University, Tilburg, The Netherlands

[4]Department of Criminology, Leiden University, Leiden, The Netherlands

Correspondence
Marco T. C. Stam.
Email: m.t.c.stam@tilburguniversity.edu

Funding information
Nederlandse Organisatie voor Wetenschappelijk Onderzoek; Instituut Gak

Abstract

This study examines criminal behavior over the welfare payment cycle. Using unique individual-level administrative data on welfare recipients, we exploit exogenous variation in payment dates over time and across Dutch municipalities. The results suggest that financially motivated crime increases by 12% over the payment cycle, indicating serious financial constraints toward the end of the month. Non-financially motivated offenses, particularly sex and violent offenses and driving under the influence offenses (DUIs), peak directly after benefits receipt and decrease over the payment cycle, suggesting an underlying spike in consumption conducive to crime. Public order offenses and DUIs also increase with weekend disbursement.

KEYWORDS
benefit disbursement, crime, welfare benefits

JEL CLASSIFICATION
D90, H31, I38

1 | INTRODUCTION

Prior research suggests that welfare recipients often prematurely exhaust their welfare benefits, and lack savings to cover subsequent financial shortfalls.[1] Consumption among welfare recipients increases sharply after payment receipt, and decreases substantially toward the end of the month. Financial constraints at the end of the month could affect criminal behavior, yet little is known about criminal behavior over the welfare payment cycle. 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.

To the best of our knowledge, this study is the first to utilize individual-level administrative data to trace welfare recipients and assess the development of crime over the welfare payment cycle. The availability of these unique data allows us to follow criminal behavior among welfare recipients with different welfare payment dates (across municipalities and across time periods within municipalities). We estimate individual fixed effects linear probability models and investigate

Abbreviations: AME, average marginal effect; ATE, average treatment effect; ATT, average treatment effect on the treated; DSP, days-since-payment; DUI, driving under the influence; IPV, intimate partner violence; ITT, intent-to-treat; NWO, Netherlands Organization for Scientific Research; OECD, Organization for Economic Co-operation and Development; OLS, ordinary least squares; SNAP, Supplemental Nutrition Assistance Program; SSI, Supplemental Security Income; TANF, Temporary Assistance for Needy Families.

Managing Editor: Philip Curry

heterogeneous effects across gender and different age groups. To further investigate the underlying causal mechanisms, we run the analyses separately for financially motivated crime and other (non-financially motivated) crime.

The amount of time passed 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 toward the end of the welfare payment cycle, due to a reduction in available financial means. If welfare recipients prematurely exhaust their benefits, subsequent financial shortfalls are likely to increase crime (see Agnew, 1992; Becker, 1968; Ehrlich, 1973).[2] 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 toward a decline in consumption near the end of the welfare month (Damon et al., 2013; Hamrick & Andrews, 2016; Hastings & Washington, 2010; Mastrobuoni & Weinberg, 2009; Shapiro, 2005; Wilde & Ranney, 2000). Consumption of basic necessities, such as food, drops substantially toward the end of the payment cycle. Furthermore, Hamrick and Andrews (2016) and Shapiro (2005) find an increased likelihood among Supplemental Nutrition Assistance Program (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.[3] Following general strain theory, insufficient financial 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., violent, drug, and sex 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 toward a sharp increase in consumption among welfare recipients directly after benefit receipt (Castellari et al., 2015, 2017; Damon et al., 2013; Dobkin & Puller, 2007; Shapiro, 2005; Stephens Jr, 2003; Wilde & Ranney, 2000). If this spike in consumption also concerns the consumption of alcohol, illicit drugs, and certain leisure activities, this may increase criminal behavior.[4,5] Routine activity theory offers a theoretical mechanism through which payment receipt may affect crime (Cohen & 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 routine activity theory, Felson (2006) classifies nightlife establishments 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 benefit receipt. Hsu (2017) finds such a spike, by comparing temporal patterns in certain types of intimate partner violence to payment schedules for the Temporary Assistance for Needy Families (TANF) program.[6] 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 5 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 with daily-level aggregate crime data in 12 large US cities. He finds a significant increase of 14% in financially motivated crime over the welfare month. The rate of other offenses is unaffected. These findings support the hypothesis that financial shortfalls toward 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 2 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 & Packham, 2019; Dobkin & 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), who conclude that staggering SNAP benefits has the potential to reduce the presence of negative peer effects within communities caused by simultaneous income shocks. Increasing the number of SNAP distribution days may aid in community-level consumption smoothing, reducing the need to commit crimes to obtain basic resources. They find staggering of payments to decrease crime in general by 17.5%, and grocery store theft by 20.9%. However, later findings by Carr and Packham (2021) suggest that there are also short-term adverse consequences to staggering disbursement across individuals, as they find that shifting benefit dates increases domestic violence by 6.9% and child maltreatment by 30.0%. As they posit that these adverse effects are driven by changes in drug use and increased in opportunities for conflict, they argue that splitting the transfers into multiple smaller payments may reduce both resource scarcity at the end of the benefit month and reduce resource abundance upon benefit receipt. However, there is little research into so-called staggering disbursement *within* individuals, which is arguably the only form of staggering that addresses the underlying causal mechanism of inadequate consumption smoothing by welfare recipients. Hsu (2017) 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 policies can be an effective tool for crime prevention.

Our contribution to the scarce literature on criminal behavior over the welfare payment cycle is fourfold. First and foremost, our unique individual-level data allow us to select welfare recipients and follow their criminal behavior over the welfare payment cycle. Whereas prior research relates disbursement dates to aggregate crime data, we directly measure criminal activity of welfare recipients. In other words, we assess average treatment effects (ATEs) on the treated (ATTs) on criminal behavior among welfare recipients, as opposed to the intent-to-treat (ITT) effects on city-level crime rates investigated in prior research. This is especially important as welfare recipients constitute a relatively small proportion of the general population.[7] Furthermore, we exploit exogenous variation in welfare payment dates across time periods and municipalities.

Second, the rich data allow us to disentangle the underlying causal mechanisms. We provide further insight into the extent to which welfare benefit disbursement affects non-financially motivated crime through increased consumption conducive to criminal behavior (e.g., (il)licit substances and certain leisure activities), first, by separately analyzing sex and violent crime, public order crime and "driving under the influence" (DUI) offenses. In addition, as prior evidence suggests that benefit disbursement on weekend days affects purchasing decisions and stimulates the consumption of alcohol Castellari et al. (2017, 2015), we further examine this causal mechanism by investigating to which extent welfare benefit disbursement during weekends affects crime, in comparison to disbursement on weekdays. These insights may contribute to determining optimal disbursement strategies that take spillover effects on crime into account.

Third, our individual-level data enable us to assess heterogeneous effects. As detailed by Hsu (2017), the gender of welfare recipients plays an important role in the relationship between welfare benefits disbursement and criminal behavior.[8] Furthermore, both age and gender have proven to be important determinants of both welfare dependency and criminal behavior (e.g., see Corman et al., 2014; Holtfreter et al., 2004; Loeber & Farrington, 2014; Steffensmeier & Allan, 1996). Understanding heterogeneous effects may aid policymakers in targeting populations of interest.

Fourth, the comparative generosity of the Dutch welfare system enables us to shed light on the generalizability of earlier findings to welfare systems with higher benefit levels. The only other existing study into the effects of the welfare payment cycle on comprehensive crime measures is focused on the US (Foley, 2011), where guaranteed minimum income benefits are much lower than in the Netherlands (6% vs. 60% of median disposable income, see OECD, 2018a). More generous benefits 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.

The main findings show that welfare recipients commit 12% more financially motivated crime at the end of the monthly welfare payment cycle, as compared to directly after benefits disbursement. Similar effects have been found by Foley (2011) for the US. Conversely, we find the rate of other, non-financially motivated crime to peak directly after benefits receipt and decrease by 7% over the welfare month. Sex and violent crime and DUI offenses show the largest spikes upon disbursement, with subsequent 12% and 11% reductions over the payment cycle, respectively. We also find disbursement on weekend days to increase DUI crime (18%) and public order offenses (10%). While Foley (2011) does not find an effect for non-financially motivated crime at the city level, similar spikes after disbursement in the US are found in substance-abuse incidents (Dobkin & Puller, 2007; Watson et al., 2019), as well as domestic violence (Carr & Packham, 2021; Hsu, 2017). Overall, we primarily find comparable relative effects across subsamples, although non-financially motivated crime is unaffected among women. Higher baseline crime rates produce larger absolute changes in offenses among younger age groups and men.

Below, Section 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 3 presents the methodology, followed by the estimation results in Section 4, and an additional robustness check in Section 5. We conclude and discuss the implications of the results in Section 6.

2 | DATA AND GRAPHICAL EVIDENCE

2.1 | Welfare benefits

The Dutch welfare system plays a crucial role in preventing poverty and providing support to those facing financial difficulties. Welfare benefits serve as the ultimate social safety net once all other forms of assistance have been exhausted (e.g., unemployment benefits are restricted to a maximum duration of 2 years). Being means-tested, eligibility is restricted to households with an income lower than the welfare norm and household wealth may not exceed certain maximum thresholds (€5940 for single-person households and €11,880 for couples, in 2017). This means that welfare beneficiaries are a highly vulnerable group in terms of income and wealth, rendering them especially susceptible to (financially motivated) crime.

The Dutch welfare benefit level can be classified as generous, in comparison to most other countries. Guaranteed minimum income benefits in the Netherlands were €933.65 for single-person households in 2017 (Ministerie van Sociale Zaken en Werkgelegenheid, 2017). For other household compositions, the amount follows an equivalence scale that ensures comparability of the guaranteed minimum income across different household sizes and compositions. 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). If the aforementioned eligibility requirements are met, every legally-registered adult Dutch citizen is guaranteed a minimum income for an unlimited duration. While the eligibility conditions are centrally defined under one national scheme, executive responsibilities lie with municipalities.[9] 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 requested the largest Dutch municipalities to provide us with the required data.

As shown in Table 1, we have access to data concerning exact disbursement dates for 15 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 to 2011 onwards, respectively. We determine on a daily level whether an individual resides in one of the included municipalities within the respective observation window.[10]

Figure 1 presents the distribution of welfare benefits payout days over days of the calendar month. The disbursement of welfare benefits always takes place once per month, and generally toward 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 are generally due toward 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. There is no evidence to suggest that dates of disbursement entail a time-varying change related to crime. In addition, given the low welfare receipt rate in the population, it is unlikely that law enforcement agencies (or other stakeholders) adjust their resources according to the (varying) welfare disbursement dates.

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.[11] These data cover all registered welfare recipients in the municipalities mentioned in Table 1 over a 13-year observation window, from 2005 to 2017.

TABLE 1 Data coverage per included municipality.

City	Sample period	Population size[a]	Welfare receipt rate (%)[a]
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
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

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

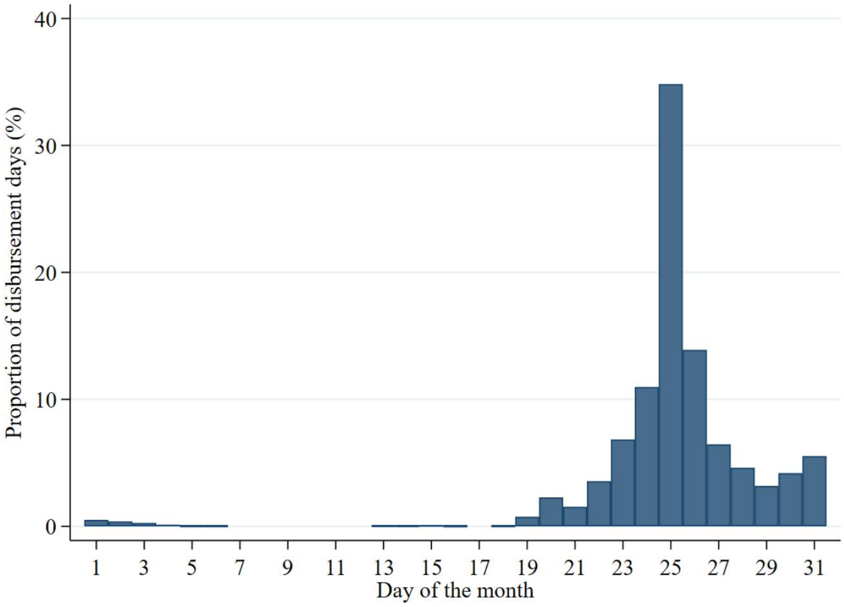

FIGURE 1 Payout day distribution.

In addition to the data on disbursement dates, this study is facilitated by the availability of daily-level data on welfare benefits receipt and crime. Administrative data on welfare benefits receipt are derived from municipal payment registrations, whereas the crime data originate from the Dutch law enforcement agencies. The crime measures are based upon official criminal charges in crime reports submitted to the public prosecutor. Approximately 90% of charges brought to trial result in a criminal conviction (Statistics Netherlands, WODC Research and Documentation Centre, & The Council for the Judiciary, 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. The crime categorization in this study is based on the offense characteristics that can be derived from the “Dutch Standard Crime Classifications” that are assigned to offenses by Statistics Netherlands. Any crime to which a financial motivation can be assigned with a fair degree of certainty is classified as a financially motivated offense, which primarily pertains to property offenses (see Appendix A for the specific crime classifications).

Table 2 shows that the selection of all registered welfare recipients in the municipalities under consideration produces a full sample of 528,981 individuals and 528.4 million daily observations. This vast sample size facilitates the estimation of our models on the low daily-level crime probabilities, which range from 0.017% to 0.032% for financially motivated crime and crime in general, respectively. On a yearly level, 4.73% of the included welfare recipients commit any criminal offense, 2.57% a financially motivated offense, and 3.07% an offense classified as “other”. Of these offenses, 43.25% can be classified as a sex and/or violent offense, 21.62% as a public order offense and 10.45% as a DUI offense, with the remainder of offenses falling under miscellaneous categories (e.g., other traffic offenses and weapon possession offenses, see Appendices A and B). Note that singular offenses can belong to multiple crime categories.

Of the full sample of welfare recipients, 49.43% are male, and 36.46% native-born Dutch citizens. As welfare benefits are means-tested, the average annual personal primary income is very low (€1155). So, individuals within our sample receive little income from legitimate employment. The mean annual standardized household income of €13,199, which includes benefits, is less than half of the overall mean for the Netherlands (€28,800 in 2017, see Statistics Netherlands, 2018).

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 & Farrington, 2014). As criminal behavior peaks around late adolescence, followed by a decline from the early twenties onwards, we select three age groups: 18–25 year olds, 26–39 year olds and individuals aged 40–64. 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 & 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 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 among women, with 0.058% and 0.011% respectively. This gap widens when we consider other crime, of which the daily rate is almost seven times as high among men (0.034%), as compared to women (0.005%). On average, 7.84% of male welfare recipients commit crime in a given year, versus 2.01% of female

TABLE 2 Descriptive statistics, 2005–2017.

	Full sample	Ages 18–25	Ages 26–39	Ages 40–64	Men	Women
Male (%)	49.43	49.18	49.64	48.44		
Native (%)	36.46	35.00	31.74	39.03	35.93	36.98
Crime (daily, %)	0.032	0.063	0.044	0.022	0.058	0.011
Crime (yearly, %)	4.727	6.399	5.786	3.542	7.838	2.013
Financially motivated crime (daily, %)	0.017	0.032	0.022	0.012	0.029	0.007
Financially motivated crime (yearly, %)	2.569	3.408	3.029	2.004	4.081	1.249
Other crime (daily, %)	0.018	0.364	0.026	0.012	0.034	0.005
Other crime (yearly, %)	3.074	4.233	3.925	2.183	5.477	0.977
Annual personal primary income	1155	1758	1628	844	1281	1004
Annual standardized household income	13,199	13,915	13,061	13,182	12,909	13,492
Number of individuals	528,981	122,951	240,086	269,046	261,471	267,510
Number of observations	528.4M	40.2M	164.4M	323.8M	236.5M	291.9M

Note: The full sample includes all welfare recipients in the municipalities under investigation. Financially motivated crime includes all property crimes, human trafficking, and drug offenses (e.g., manufacturing and trafficking). Other crime predominately consists of violent and sexual crimes, vandalism and crimes against public order and authority (e.g., public disorder), and traffic offenses. See Table A1 for a comprehensive list of crime classifications.

welfare recipients. While the yearly crime rate among women is the lowest of the investigated subsamples, it is approximately twice as high as the rate among the general population of the Netherlands (0.99% in 2017, see Statistics Netherlands, 2020). In line with the age-crime curve, we find lower rates as we move up the age groups for all crime categories. Compared to the highest age group (40–64 year olds), the daily crime rate among the lowest age group (18–25 year olds) is almost three times as high (0.022% vs. 0.063%, 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 lowest age group (€1758), whereas the highest age group shows the lowest primary income (€844). This is indicative of the duration of unemployment spells increasing with age, which may be due to lower employability.

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 2–4 present local polynomial smooth plots of average daily crime rates, including 95% confidence intervals.

Figure 2 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 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 toward the end of the payment cycle.

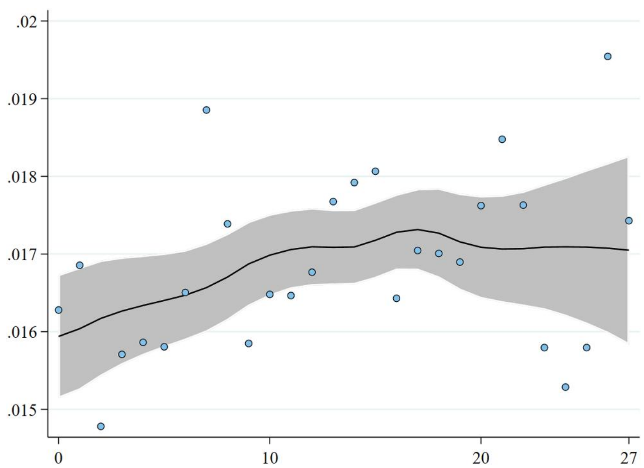

FIGURE 2 Financially motivated crime over days since payment.

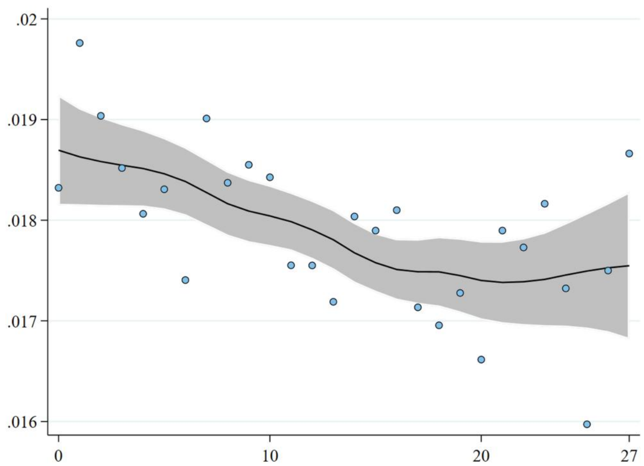

FIGURE 3 Other crime over days since payment.

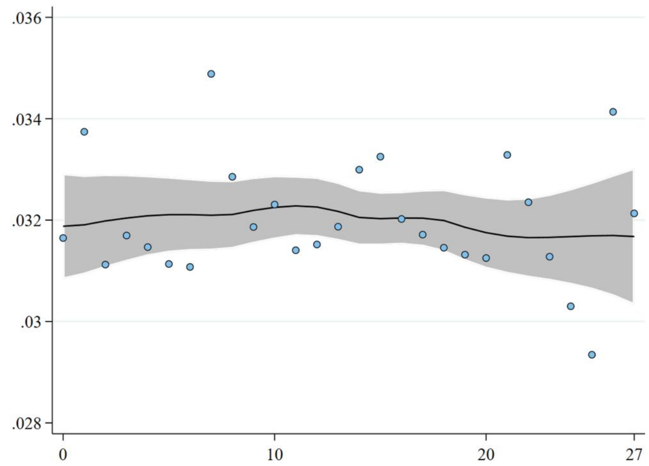

FIGURE 4 All crime over days since payment.

When we estimate a simple univariate linear regression model through the 28 points in Figure 2, we find a marginally significant 8% increase in financially motivated crime over the welfare month ($p < 0.10$). Conversely, Figure 3 shows that the rate of other crime is the highest directly after payment receipt, and that this rate declines as the time since disbursement increases. When we estimate a simple univariate linear regression through the 28 points in Figure 3, we find a statististically significant decrease in other crime of 9% over the welfare payment cycle ($p < 0.01$).

Finally, Figure 4 shows the change in crime in general (i.e., all crime, irrespective of crime category). Due to the inversive trends of financially motivated and other crime, we find this line to be more stable.

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 select welfare recipients and employ individual-specific fixed effects to control for unobserved time-invariant heterogeneity. Furthermore, we include day-of-week, calendar month, year, and municipality fixed effects in all model specifications.[12] We cluster the standard errors on a calendar month-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:

$$y_{it} = \alpha DSP_{it} + \beta AGE_{it} + \gamma' DOW_t + \delta' MONTH_t + \rho' YEAR_t + \sigma' MUNICIPALITY_m + \upsilon_i + \varepsilon_{it} \tag{1}$$

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, AGE_{it} captures age in years, DOW_t, $MONTH_t$, $YEAR_t$ and $MUNICIPALITY_m$ represent the day-of-week, calendar month, year, and municipality fixed effects, υ_i is the individual-specific fixed effect, and ε_{it} the error term.

For the baseline analyses, we use a days-since-payment (DSP) index, which captures the total change in the outcome variable over the welfare payment cycle. This index ranges in values between 0 and 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.[13] 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.[14] On the available system, it is not computationally feasible to estimate the fixed effects linear probability model over the entire dataset at once. We therefore estimate the model on two randomly-selected subsamples, which together include all observations, after which we combine the estimates by applying minimum distance (see Been & Knoef, 2017; Chamberlain, 1984).[15]

In addition to the fixed effects linear probability model, we run (a) a linear probability model excluding individual-specific fixed effects, and (b) a probit model specification. We compare the fixed effects linear probability estimates to a linear probability model excluding individual-specific fixed effects, to assess the extent to which the inclusion of individual-specific fixed effects affects our estimates. The probit model specification is applied as the outcome variables are dichotomous and have probabilities close to zero (see Horrace & Oaxaca, 2006). Apart from the exclusion of individual-specific fixed effects, we specify the linear probability and probit model identically to the fixed effects linear probability model. For these model specifications, municipality, day-of-week, calendar month and year fixed effects are included as vectors of dummy variables. Finally, we test for nonlinearity by estimating a model with a quadratic DSP term, as well as by estimating a model with multiple separate DSP indicators.

4 | ESTIMATION RESULTS

This section presents the main estimation results of this study. Section 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. Sections 4.2 and 4.3 summarize the results obtained from quadratic and indicator DSP model specifications, respectively. Finally, Sections 4.4 and 4.5 assess the extent to which the effects are heterogeneous across subsamples and crime subcategories.

4.1 | Baseline estimation

Table 3 presents the estimates for the full sample of the baseline fixed effects linear probability model, as well as a standard linear probability model and a probit model specification. The use of the aforementioned DSP index means that the shown average marginal effects (AMEs) capture 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. More specifically, we find welfare recipients to commit 12.37% (0.0020% 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 7.41% less non-financially motivated crime at the end of the payment cycle (−0.0014% 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., illicit drugs and alcohol). Furthermore, the findings for financially motivated crime support the hypothesis that welfare recipients commit more crime for financial gains toward the end of the welfare month, to supplement their income. The inversive effects on financially motivated crime and other crime limit the change in aggregate crime over the payment cycle. Compared to directly after benefits disbursement, welfare recipients commit 2.02% (0.0006% points) more crime in general at the end of the month. While small and statistically only marginally significant ($p < 0.10$), this upward trend is attributable to the increase in financially motivated crime being larger than the reduction in other crime.

Compared to the baseline fixed effects linear probability model ("Fixed effects"), we find that the linear probability model without fixed effects ("Ordinary least squares") and probit model ("Probit") yield similar results. While the effect sizes for other crime are nearly identical, we find that the baseline fixed effects linear probability model produces slightly more conservative effect sizes for financially motivated crime than the models without fixed effects. As the estimates for financially motivated crime and other crime also remain statistically highly significant across the board ($p < 0.001$), we consider these estimates to be robust to these changes in model specification.

4.2 | Higher-order estimation results

To assess the sensitivity of our estimates to a higher-order model specification, we run a fixed effects linear probability model including a quadratic DSP term, the results of which are presented in Table 4. "DSP index (combined)" captures the total change over the welfare payment cycle, and is computed through the linear combination of "DSP index" and "DSP index squared", that is, the linear and quadratic DSP terms.

TABLE 3 Baseline days-since-payment index estimates, full sample.

	Financially motivated crime	Other crime	All crime
Fixed effects			
DSP index	0.0020***	−0.0014***	0.0006†
	(0.0003)	(0.0002)	(0.0003)
Age	−0.0296***	−0.0142	−0.0399***
	(0.0030)	(0.0027)	(0.0039)
Constant	1.3092***	0.6364***	1.7720***
	(0.1319)	(0.1187)	(0.1711)
DSP index (%)	12.37	−7.41	2.02
Ordinary least squares			
DSP index	0.0023***	−0.0013***	0.0009**
	(0.0003)	(0.0003)	(0.0004)
Age	−0.0006***	−0.0008***	−0.0013***
	(0.0000)	(0.0000)	(0.0002)
Constant	0.0442***	0.0530***	0.0891***
	(0.0014)	(0.0014)	(0.0024)
DSP index (%)	14.11	−7.00	2.99
Probit			
DSP index	0.0023***	−0.0013***	0.0010**
	(0.0003)	(0.0003)	(0.0004)
Age	−0.0006***	−0.0008***	−0.0014***
	(0.0000)	(0.0000)	(0.0000)
DSP index (%)	15.09	−7.04	3.13
Number of individuals	528,981	528,981	528,981
Number of observations	528.4M	528.4M	528.4M

Note: The days-since-payment (DSP) index ranges in value from 0 to 1, where 0 indicates the payout day, and 1 the last day before payment. As such, the DSP index coefficient captures the percentage point change in crime over the full welfare payment cycle. The baseline fixed effects model includes individual-specific, municipality, day-of-week, calendar month, and year fixed effects, and the ordinary least squares (OLS) and probit models include vectors of municipality, day-of-week, calendar month, and year dummy variables. To increase interpretability, the fixed effects and OLS coefficients are multiplied by 100, and the probit estimates are average marginal effects. Standard errors are clustered by municipality, year, and calendar month. Conclusions do not change when we cluster by year and calendar month, *** indicates $p < 0.001$, **$p < 0.01$ and †$p < 0.10$.

Abbreviation: DSP, days-since-payment.

We find the inclusion of a quadratic term to produce comparable estimates to the baseline model specification. The estimates for financially motivated crime and other crime remain statistically highly significant ($p < 0.001$). While the effect on other crime decreases in size (−5.56% vs. −7.41%), we find a slightly larger effect size for financially motivated crime (12.79% vs. 12.37%). Although the inversive effects on financially motivated crime and other crime still limit the effect on aggregate crime, we find an increase in both effect size (3.11% vs. 2.02%), as well as statistical significance ($p < 0.01$ vs. $p < 0.10$). As such, we consider all estimates to be robust to the specification of a higher-order model.

4.3 | DSP indicator specification

The graphical evidence presented in Section 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 4 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

TABLE 4 Fixed effects higher-order days-since-payment index estimates, full sample.

	Financially motivated crime	Other crime	All crime
DSP index	0.0011	−0.0058***	−0.0037*
	(0.0011)	(0.0011)	(0.0015)
DSP index squared	0.0010	0.0048***	0.0047**
	(0.0012)	(0.0011)	(0.0016)
Age	−0.0311***	−0.0218***	−0.0474***
	(0.0036)	(0.0033)	(0.0048)
Constant	1.3745***	0.9726***	2.1011***
	(0.1574)	(0.1445)	(0.2080)
DSP index combined	0.0021***	−0.0010***	0.0010**
	(0.0003)	(0.0003)	(0.0004)
DSP index combined (%)	12.79	−5.56	3.11
Number of individuals	528,981	528,981	528,981
Number of observations	528.4M	528.4M	528.4M

Note: 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. As such, the DSP index coefficient captures the percentage point change in crime over the full welfare payment cycle. To increase interpretability, the coefficients are multiplied by 100. The model specification includes individual-specific, municipality, day-of-week, calendar month, and year fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates $p < 0.001$, **$p < 0.01$ and *$p < 0.05$.

Abbreviation: DSP, days-since-payment.

since disbursement and crime is nonlinear. The AMEs capture the difference relative to the first 4 days after benefits disbursement, as the constant captures the crime rate in this period.

Table 5 presents the estimation results for the fixed effects model specification with 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 11 days after benefits receipt. In line with Figure 2, we find the rate to peak around two-thirds into the month, at 15.44% 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 3, the negative coefficients for other crime show that the other crime rate is the highest in the first 3 days after benefits receipt. The reduction is more gradual, as the estimates are not statistically significant for the first approximately 11 days of the welfare month. As opposed to financially motivated crime, we do not find an apparent inversion of this trend toward 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). 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 robustness check presented in Section 5 confirms that our estimates are not sensitive to the disbursement of rent and healthcare benefits.

Similar to the baseline estimation results, we find the inversive effects on financially motivated crime and other crime to limit the change in aggregate crime over the welfare month. While the uniformly positive and statistically significant coefficients show that crime in general is at its lowest directly after benefits receipt, we find the relative effects to be comparatively limited in size. In line with financially motivated crime, crime in general peaks between 20 and 23 days after benefits receipt, at 5.20% higher than the mean rate at the start of the welfare month.

4.4 | Heterogeneous effects

Table 6 presents the estimation results produced by the baseline fixed effects linear probability model over multiple subsamples.

TABLE 5 Fixed effects days-since-payment index indicator estimates, full sample.

	Financially motivated crime	Other crime	All crime
4 ≤ DSP ≤ 7	0.0008***	−0.0001	0.0008**
	(0.0002)	(0.0002)	(0.0003)
8 ≤ DSP ≤ 11	0.0015***	−0.0003	0.0010**
	(0.0002)	(0.0002)	(0.0003)
12 ≤ DSP ≤ 15	0.0016***	−0.0007**	0.0010**
	(0.0002)	(0.0002)	(0.0003)
16 ≤ DSP ≤ 19	0.0021***	−0.0011***	0.0010***
	(0.0002)	(0.0002)	(0.0003)
20 ≤ DSP ≤ 23	0.0025***	−0.0006**	0.0017***
	(0.0002)	(0.0002)	(0.0003)
24 ≤ DSP ≤ 27	0.0017***	−0.0009**	0.0009*
	(0.0003)	(0.0003)	(0.0004)
Age	−0.0049***	−0.0040***	−0.0093***
	(0.0011)	(0.0011)	(0.0015)
Constant	0.2312***	0.1929***	0.4376***
	(0.0459)	(0.0457)	(0.0670)
AME (%)			
4 ≤ DSP ≤7	4.84	−0.44	2.37
8 ≤ DSP ≤11	9.25	−1.82	3.12
12 ≤ DSP ≤15	10.27	−3.58	3.02
16 ≤ DSP ≤19	12.96	−5.54	3.07
20 ≤ DSP ≤23	15.44	−3.27	5.20
24 ≤ DSP ≤27	10.84	−4.73	2.75
Number of individuals	528,981	528,981	528,981
Number of observations	528.4M	528.4M	528.4M

Note: The reference category includes 0–3 days since payment receipt. As such, the DSP coefficients capture the percentage point difference relative to the first 4 days after payment receipt. To increase interpretability, the coefficients are multiplied by 100. The model specification includes individual-specific, municipality, day-of-week, calendar month, and year fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates $p < 0.001$, ** $p < 0.01$ and * $p < 0.05$.

Abbreviation: DSP, days-since-payment.

In line with the findings for the full sample, we find statistically highly significant estimates for financially motivated crime across all included subsamples ($p < 0.01$ and $p < 0.001$). 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 (0.0006% points), as well as the smallest increase relative to their baseline rate (8.07%), whereas men show an almost twice as large relative increase (15.11%) and an absolute increase that is around seven times larger than women (0.0041% points). While the absolute effect size drops substantially over age, from 0.0032% points (ages 18–25) to 0.0019% points (ages 40–64), we find the relative effect size to increase from 11.02% (ages 18–25) to 15.96% (ages 40–64). Overall, the smallest absolute changes in financially motivated offenses are found among women and the highest age group (ages 40–64), whereas the largest absolute changes are found among men and young adults (ages 18–25).

We find the effects on other crime to be more heterogeneous than financially motivated crime. Other crime is unaffected among women, whereas the reduction among men is the second largest of all included subsamples in absolute terms (−0.0028% points). In line with the larger difference between the genders, we find greater variation in effect size across age. Although the relative effect sizes are more comparable, the absolute reduction in other crime is three

TABLE 6 Fixed effects days-since-payment index estimates, multiple samples.

	Financially motivated crime	Other crime	All crime
Ages 18–25			
DSP index	0.0032**	−0.0033**	−0.0006
	(0.0012)	(0.0012)	(0.0017)
DSP index (%)	11.02	−10.41	−1.06
Number of individuals	122,951	122,951	122,951
Number of observations	40.2M	40.2M	40.2M
Ages 26–39			
DSP index	0.0029***	−0.0011*	0.0011
	(0.0005)	(0.0005)	(0.0007)
DSP index (%)	13.23	−4.30	2.45
Number of individuals	240,086	240,086	240,086
Number of observations	164.4M	164.4M	164.4M
Ages 40–64			
DSP index	0.0019***	−0.0011***	0.0009**
	(0.0003)	(0.0002)	(0.0003)
DSP index (%)	15.96	−9.12	4.25
Number of individuals	269,046	269,046	269,046
Number of observations	323.8M	323.8M	323.8M
Men			
DSP index	0.0041***	−0.0028***	0.0012†
	(0.0005)	(0.0005)	(0.0007)
DSP index (%)	15.11	−7.97	2.10
Number of individuals	261,471	261,471	261,471
Number of observations	236.5M	236.5M	236.5M
Women			
DSP index	0.0006**	−0.0001	0.0005†
	(0.0002)	(0.0002)	(0.0003)
DSP index (%)	8.07	−2.16	4.63
Number of individuals	267,510	267,510	267,510
Number of observations	291.9M	291.9M	291.9M

Note: 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. As such, the DSP index coefficient captures the percentage point change in crime over the full welfare payment cycle. To increase interpretability, the coefficients are multiplied by 100. The model specification includes individual-specific, municipality, day-of-week, calendar month, and year fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates $p < 0.001$, **$p < 0.01$, *$p < 0.05$ and $^{\dagger}p < 0.10$.

Abbreviation: DSP, days-since-payment.

times larger among young adults than both higher age groups (−0.0033% points vs. −0.0011% points, respectively). As such, we find the largest absolute changes among men and young adults (ages 18–25).

In line with the estimation results for the full sample, we find crime in general to be the least affected across the included subsamples. For this aggregate crime measure, the estimates are only statistically significant for subsamples among which the increase in financially motivated crime over the welfare month is sufficiently larger than the reduction in other crime. While we find the largest absolute increases in financially motivated crime among men and young adults, they also show the largest absolute reductions in other crime. As a result, we do not find crime in general to be affected among these subsamples. Conversely, financially motivated crime is the second least affected in absolute terms among the highest age group (ages 40–64), but that is also true for other crime. Consequently, we only find a statistically significant increase in crime in general among the highest age group (4.25%).

In summary, higher baseline crime rates produce substantially larger absolute changes in both financially motivated offenses and other offenses among younger age groups and men. Although the relative effect sizes are more comparable across subsamples, we find non-financially motivated crime to be unaffected among women.

4.5 | Crime subcategory estimates

As prior evidence suggests that benefit disbursement causes spikes in both the consumption of alcohol[16] and illicit drugs,[17] as well as substance-related offenses,[18] we investigate the so-called consumption spike hypothesis by separately estimating the baseline fixed effects linear probability model for three non-financially motivated crime subcategories: sex and violent crime, public order crime and DUI offenses.

Among the investigated non-financially motivated crime subcategories (shown in Table 7), we only find statistically significant estimates for sex and violent crime ($p < 0.001$), and DUI crime ($p < 0.01$). The largest effect size is found for sex and violent crime, with a 11.91% reduction in the baseline fixed effects linear probability model. This is in line with the graphical evidence presented in Appendix B, as sex and violent offenses show the sharpest decrease over the payment cycle (see Figure B1). DUI crime also peaks directly after disbursement, followed by a 11.30% reduction over the welfare month. Although the graphical evidence suggests public order crime to be the highest directly after benefits receipt (see Figure B2), we do not find the estimates to be statistically significant.

Overall, the estimates and graphical evidence for sex and violent crime are the most in line with the spike in non-financially motivated crime upon disbursement expected from the consumption spike hypothesis. Yet, we also find a comparably-sized, more linear downward trend over the payment cycle among DUI offenses.

4.6 | Payment weekend indicator estimates

Prior research suggests that welfare benefit disbursement during weekends affects purchasing choices and increases consumption conducive to criminal behavior (e.g., alcohol[19] and illicit drugs[20]), yet we find 34.59% of welfare benefit

TABLE 7 Crime subcategory days-since-payment index estimates, full sample.

	Sex & violent crime	Public order crime	DUI crime
Fixed effects			
DSP index	−0.0009***	−0.0000	−0.0002**
	(0.0002)	(0.0001)	(0.0001)
Age	−0.0115***	0.0009	0.0006
	(0.0018)	(0.0012)	(0.0008)
Constant	0.5084***	−0.0371	−0.0246
	(0.0785)	(0.0532)	(0.0363)
DSP index (%)	−11.91	−0.44	−11.30
Number of individuals	528,981	528,981	528,981
Number of observations	528.4M	528.4M	528.4M

Note: Sex and violent crime includes all sexual and violent crimes apart from human trafficking (e.g., assault & battery, threatening & stalking, and homicide). Public order crime primarily consists of public disorder and vandalism & criminal damaging. Driving under the influence (DUI) crime pertains to the crime of operating a motor vehicle while being affected by alcohol or other (il)licit drugs. See Table A1 for a comprehensive list of crime classifications. 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. As such, the DSP index coefficient captures the percentage point change in crime over the full welfare payment cycle. To increase interpretability, the coefficients are multiplied by 100. The model specification includes individual-specific, municipality, day-of-week, calendar month, and year fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates $p < 0.001$ and **$p < 0.01$.

Abbreviations: DSP, days-since-payment; DUI, driving under the influence.

disbursements to take place on a Friday, Saturday or Sunday. As such an increase in consumption conducive to criminal behavior should increase non-financially motivated crime under the consumption spike hypothesis, we investigate whether welfare benefit disbursement during weekends also affects crime. Tables 8 and 9 present estimation results from a fixed effects linear probability model including an indicator that captures payment receipt on Friday, Saturday or Sunday.

TABLE 8 Fixed effects days-since-payment index and disbursement weekend indicator estimates, full sample.

	Financially motivated crime	Other crime	All crime
DSP index	0.0020***	−0.0012***	0.0008*
	(0.0003)	(0.0003)	(0.0004)
Payment weekend	−0.0000	0.0011**	0.0009†
	(0.0003)	(0.0004)	(0.0005)
Age	−0.0297***	−0.0155***	−0.0411***
	(0.0030)	(0.0027)	(0.0039)
Constant	1.3113***	0.6951***	1.8246***
	(0.1310)	(0.1197)	(0.1710)
DSP index (%)	12.32	−6.27	2.58
Payment weekend (%)	−0.17	5.90	2.94
Number of individuals	528,981	528,981	528,981
Number of observations	528.4M	528.4M	528.4M

Note: 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. As such, the DSP index coefficient captures the percentage point change in crime over the full welfare payment cycle. To increase interpretability, the coefficients are multiplied by 100. The model specification includes individual-specific, municipality, day-of-week, calendar month, and year fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates $p < 0.001$, **$p < 0.01$, *$p < 0.05$ and †$p < 0.10$.

Abbreviation: DSP, days-since-payment.

TABLE 9 Fixed effects days-since-payment index and disbursement weekend indicator estimates, full sample.

	Sex & violent crime	Public order crime	DUI crime
DSP index	−0.0009***	0.0001	−0.0001†
	(0.0002)	(0.0001)	(0.0001)
Payment weekend	0.0001	0.0004*	0.0003*
	(0.0002)	(0.0002)	(0.0001)
Age	−0.0116***	0.0004	0.0002
	(0.0018)	(0.0012)	(0.0008)
Constant	0.5160***	−0.0149	−0.0058
	(0.0795)	(0.0542)	(0.0362)
DSP index (%)	−11.63	1.53	−7.95
Payment weekend (%)	1.66	10.43	17.86
Number of individuals	528,981	528,981	528,981
Number of observations	528.4M	528.4M	528.4M

Note: 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. As such, the DSP index coefficient captures the percentage point change in crime over the full welfare payment cycle. To increase interpretability, the coefficients are multiplied by 100. The model specification includes individual-specific, municipality, day-of-week, calendar month, and year fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates $p < 0.001$, *$p < 0.05$ and †$p < 0.10$.

Abbreviations: DSP, days-since-payment; DUI, driving under the influence.

Among the main crime categories (shown in Table 8), we only find statistically significant estimates for non-financially motivated crime ($p < 0.01$). Disbursement during a weekend increases non-financially motivated crime by 5.90% compared to disbursement on a weekday, whereas financially motivated crime appears unaffected. This is in line with an increase in consumption conducive to criminal behavior under the consumption spike hypothesis. Furthermore, the DSP index estimates remain statistically significant and similarly sized across all outcomes.

To further investigate the effect of benefit disbursement during weekends on non-financially motivated crime, Table 9 presents estimation results for three separate crime subcategories. We find statistically significant estimates for public order crime and DUI crime ($p < 0.05$). Disbursement on weekend days increases public order crime and DUI offenses by 10.43% and 17.86%, respectively. While sex and violent crime shows the sharpest decrease over the payment cycle, it appears unaffected by disbursement during weekends. Following the consumption spike hypothesis, these findings suggest that disbursement on weekend days stimulates not only the consumption of (il)licit substances, but also certain leisure activities (e.g., committing public disorder in nightlife areas).

4.7 | Costs of crime

Crime reduction is an important public policy concern, due to its vast social and economic costs. As we find the welfare payment cycle to substantially affect crime among recipients, the question arises as to the economic significance of these effects. To address this question, we include a back-of-the-envelope calculation on the changes in absolute numbers of offenses and relate this to the average costs per offense estimated in prior research.

Table 10 presents an overview of the calculation of the changes in absolute numbers of offenses. To calculate all daily changes in offenses, we multiply the ATEs by the average numbers of committed offenses. As the welfare month estimates capture the total change over the welfare payment cycle (on a daily level), we subsequently divide by two to capture the average daily change across the welfare month. To approximate the yearly changes in offenses, the daily changes are multiplied by 365 for the welfare month estimates and 36 for the payment weekend estimates (i.e., the 3-day payment weekend multiplied by 12). As such, the latter captures a scenario where all disbursements within a year take place during weekends. Following this approach, 3.96 financially motivated offenses per 1000 welfare recipients annually could be avoided if one could stop the rise in financially motivated crime at the end of a monthly welfare payment cycle. Furthermore, 2.75 other offenses per 1000 welfare recipients annually could be avoided if one could stop the peak in offenses at the beginning of the welfare payment cycle. Note that it theoretically might be possible to lessen both the peak in offenses at the start of the payment cycle and the increase in financially motivated crime at the end of the payment cycle. For example, by disbursing welfare benefits more frequently than once a month, one may lower both

TABLE 10 Costs of crime.

	Financially motivated crime	Other crime
Welfare month		
ATE (daily, %point)	0.0020	0.0014
ATE (yearly, %point)	0.7337	0.4964
Average offense count (if crime = 1)	1.0791	1.1091
Change in offenses per 1000 recipients (daily)	0.0108	0.0075
Change in offenses per 1000 recipients (yearly)	3.9584	2.7528
Payment weekend		
ATE (daily, %point)		0.0011
ATE (yearly, %point)		0.0389
Average offense count (if crime = 1)		1.1210
Change in offenses per 1000 recipients (daily)		0.0121
Change in offenses per 1000 recipients (yearly)		0.4358

Note: The shown values are derived from the fixed effects linear probability model estimates for the full sample presented in Tables 3 and 8. As these estimates capture the change in crime over the welfare month, the negative coefficient for other (non-financially motivated) crime is inverted for the shown ATE to capture the increase in these offenses upon disbursement.

the spike in available financial means upon disbursement and the financial shortfalls that arise toward the end of the payment cycle, by shortening the time window over which recipients are required to smooth consumption. Finally, 0.44 other offenses per 1000 recipients annually could be prevented if all disbursements would be scheduled on weekdays (Monday-Thursday) instead of weekends (Friday-Sunday).

An evaluation of the economic significance of the effects of the welfare payment cycle on crime among recipients requires a comprehensive approximation of the costs per offense. However, while the direct costs per offense may be directly measureable (e.g., criminal justice costs and direct financial damages), it is empirically challenging to quantify the indirect costs per offense, which may include non-financial damages and adverse effects on the labor market outcomes for offenders, victims, and the labor market in general (likely through a fear of victimization, see Velásquez, 2020). Consequently, studies that comprehensively estimate the costs of crime are scarce. The limited available estimates also vary substantially across studies, due to differences in included costs and methodologies.[21] For example, one prior study by Cornaglia et al. (2014) on the adverse effects of crime on mental well-being estimates that the society-wide costs of crime are about 80 times larger than the direct costs for the victim, due to increased fears of victimization. Another seminal investigation by Cohen et al. (2004) uses a contingent valuation method to comprehensively estimate all costs per offense for several types of crime in the US. Converted to 2017 euros,[22] they find costs per offense ranging from €38,864 for household burglary, €94,026 for serious assault, €317,181 for armed robbery and €344,762 for rape or sexual assault to €12,411,415 for homicide. While the costs per murder greatly exceed all other crime categories, these findings show that lesser offenses also incur substantial social and economic costs.

5 | ROBUSTNESS CHECK

Low-income households in the Netherlands are entitled to rent and healthcare benefits, which may alleviate financial constraints among welfare recipients.[23] 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 both 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 11 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 for financially motivated crime and other crime to the baseline analyses over the full dataset. The coefficients for these crime outcomes remain statistically highly significant ($p < 0.001$). While we find a smaller effect size for financially motivated crime (10.50% vs. 12.37%), the effect size for other crime has increased (−8.25% vs. −7.41%). As these analyses produce more similarly-sized inversive effects on these crime categories, we find the estimates for aggregate crime to become close to zero and statistically nonsignificant.

TABLE 11 Fixed effects days-since-payment index estimates, rent and healthcare benefits exclusion, full sample.

	Financially motivated crime	Other crime	All crime
DSP index	0.0017***	−0.0015***	0.0002
	(0.0002)	(0.0002)	(0.0003)
Age	−0.0187***	−0.0111***	−0.0298***
	(0.0024)	(0.0022)	(0.0034)
Constant	0.8336***	0.5022***	1.3330***
	(0.1037)	(0.0965)	(0.1474)
DSP index (%)	10.50	−8.25	0.57
Number of individuals	527,027	527,027	527,027
Number of observations	424.2M	424.2M	424.2M

Note: 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. As such, the DSP index coefficient captures the percentage point change in crime over the full welfare payment cycle. To increase interpretability, the coefficients are multiplied by 100. The model specification includes individual-specific, municipality, day-of-week, calendar month, and year fixed effects. Standard errors are clustered by municipality, year, and calendar month, *** indicates $p < 0.001$.

We conclude that the disbursement of rent and healthcare benefits do not substantively affect the estimates for financially motivated crime and other crime. On the expenditure side, rents and utility bills are also unlikely to drive the patterns in crime found over the welfare payment cycle. Even in a hypothetical situation where the welfare payment cycle would be perfectly correlated with calendar months, our findings are incongruent with these recurring events. Rent is generally due before the first day of the calendar month, which would conflict with the observed spikes in crime categories related to consumption conducive to criminal behavior at the beginning of the welfare payment cycle. Neither can such expenditures at the start of the calendar month explain the increase in financially motivated crime toward the end of the welfare payment cycle. Energy bills are paid on a yearly basis with monthly advances on dates that can generally be chosen by the clients themselves. Finally, note that welfare benefits are means-tested at the household level, which rules out notable income from other sources (e.g., employment).

6 | CONCLUSION

This study assesses the development of crime over the welfare payment cycle. Unique individual-level administrative data allow us to follow benefits receipt and criminal behavior of individual welfare recipients at the daily level, and enable the inclusion of individual-specific fixed effects to control for unobserved time-invariant heterogeneity. Furthermore, 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 models for financially motivated crime, other (non-financially motivated) crime, and crime in general.

We find evidence of an increase in supplementation of income through crime toward the end of the welfare month, as financially motivated crime increases by 12% over the payment cycle. Conversely, other offenses peak directly after benefits receipt and decrease by 7% over the welfare month, which may be attributable to a spike in consumption conducive to criminal behavior, such as alcohol, illicit drugs and certain leisure activities. Sex and violent crime and DUI offenses show the largest reductions over the payment cycle (−12% and −11%, respectively). Evidence also suggests that disbursement during weekends increases both DUI crime (18%) and public order offenses (10%). Although higher baseline crime rates produce much larger absolute changes in offenses among lower age groups and men, the relative effects are more comparable across subsamples. For women, however, we find null effects for non-financially motivated 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. The increase in financially motivated crime over the payment cycle contrasts the permanent income hypothesis (Friedman, 1957), but it is in line with evidence on the inability of welfare recipients to sustain consumption toward the end of the welfare month (e.g., see Damon et al., 2013; Hamrick & Andrews, 2016; Hastings & Washington, 2010; Mastrobuoni & Weinberg, 2009; Shapiro, 2005; Wilde & 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 also aligns with several theories, including 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 toward 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).

Notably, we find non-financially motivated offenses to peak directly after payment receipt. These spikes in offenses upon benefits receipt are more in line with routine activity theory (Cohen & Felson, 1979), as it is likely attributable to a spike in consumption complementary to criminal behavior (e.g., alcohol, illicit drugs, and certain leisure activities). There is a notable body of evidence suggesting that the income shocks upon welfare benefits disbursement cause spikes in the consumption of both alcohol (Castellari et al., 2015, 2017) and illicit drugs (Dobkin & Puller, 2007). We do not measure consumption but find notable suggestive evidence to this effect from our analyses of separate non-financially motivated crime subcategories, as well as our analyses on disbursement during weekends. First and foremost, we find DUI offenses to peak upon benefits receipt, followed by an 11% reduction over the welfare payment cycle. While a direct comparison is not without flaws, this finding is in line with the 17% increase in substance-abuse incidents for up to 2 weeks after the annual lump-sum payment to all Alaskans found by Watson et al. (2019). Second, we find the sharpest spike upon benefits receipt and largest reduction over the welfare month to be among sex and violent offenses (−12%). Although Watson et al. (2019) find violent crime to be unaffected, this is in line with prior findings of spikes in domestic violence upon disbursement in the US (see Carr & Packham, 2021; Hsu, 2017). Finally, we find disbursement on weekend days to mainly increase DUI crime (18%), followed by public order offenses (10%). This is in line with prior findings of increased alcohol purchases from benefit disbursement during weekends (Castellari et al., 2015, 2017). These findings suggest that weekend disbursement stimulates consumption complementary to criminal behavior, both in the form of the consumption of (il)licit substances, as well as certain leisure activities (e.g., committing public disorder in nightlife areas).

Complementary to the existing literature, the availability of individual-level data allows us to follow welfare payments and criminal behavior over time at the individual level (as opposed to aggregate crime rates in prior studies). Combined with the exploitation of exogenous variation in welfare payment dates, this approach makes our results less vulnerable to the monthly disbursement of wages or other benefits, which may also affect crime rates. A related study by Foley (2011) finds increases in US 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. A potential explanation may lie in the comparatively generous Dutch welfare system, as the higher benefit 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, the increase in financially motivated crime over the payment cycle that we find in this study (12%) is surprisingly similar to the effect found by Foley (14%). Despite the comparatively high benefit levels in the Netherlands, our findings indicate that welfare recipients face financial shortfalls toward the end of the month.

Finally, overall, we find limited heterogeneity in the effects of welfare benefits disbursement on crime. Relative to their baseline rates, the effect sizes are comparable across age and gender. An explanation for the higher crime rates among men and lower age groups may therefore not lie in differences in consumption smoothing, but in a higher prevalence of other criminogenic factors (e.g., lower self-control, opportunity costs and risk aversion, see Kruttschnitt, 2013; Loeber & Farrington, 2014; Steffensmeier & Allan, 1996). One notable exception is that the effects of welfare benefits disbursement on other, non-financially motivated crime differ across gender. As opposed to men, we do not find other crime to be affected among women.

Welfare policies aimed at consumption smoothing could potentially reduce crime. Disbursing welfare benefits more frequently than once monthly would 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 toward the end of the month, but also the size of the spikes in available financial means upon disbursement (i.e., the "full wallet" effect). The wallet may be full more frequently, but less full, leaving less money to be spent on non-essential consumption complementary to crime. 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.[24] However, an ethical consideration of increasing the disbursement frequency pertains to the reduction in the financial autonomy of welfare recipients. Alternatively, policies could be aimed at stimulating individuals to commit themselves, for example, by reducing their daily limit on payment cards. This could work if recipients are aware of their own present bias (i.e., sophisticated hyperbolic discounters). Considering our findings, policies regarding consumption smoothing are expected to be the most effective as a crime prevention strategy when aimed at (young) men.

This study shows welfare benefits disbursement to affect criminal behavior among welfare recipients, and puts forth a viable policy response. Determining the optimal disbursement strategy could potentially reduce the 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 effects of welfare payment regimes.

ACKNOWLEDGMENTS

We thank Statistics Netherlands for providing us access to their data. Furthermore, we are grateful for comments and suggestions by Lars Andersen, Koen Caminada, Ruben van Gaalen, Paul Nieuwbeerta, Hilde Wermink, conference participants and the anonymous referees. Remaining errors are our own. Marco T. C. Stam and Marike G. Knoef gratefully acknowledge financial support from Instituut Gak. Anke A. T. Ramakers gratefully acknowledges financial support of the Innovational Research Incentives Scheme Veni, which is financed by the Netherlands Organization for Scientific Research (NWO).

CONFLICT OF INTEREST STATEMENT

The authors declare no conflicts of interest.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available on request from Statistics Netherlands. Restrictions apply to the availability of these data, which were used under license for this study. Instructions on data access and all code necessary to reproduce tables and figures are openly available in the openICPSR Western Economic Association International Data and Code Repository at https://doi.org/10.3886/E198424V2, Stam et al. (2024).

ORCID

Marco T. C. Stam https://orcid.org/0000-0003-1956-268X

ENDNOTES

[1] For example, see Castellari et al. (2017, 2015); Damon et al. (2013); Dobkin and Puller (2007); Hamrick and Andrews (2016); Hastings and Washington (2010); Mastrobuoni and Weinberg (2009); Shapiro (2005); Stephens Jr (2003); Wilde and Ranney (2000).

[2] A notable body of (quasi-)experimental evidence also suggests that cash transfers substantially reduce crime by providing a minimum income guarantee (e.g., Berk et al., 1980; Deshpande & Mueller-Smith, 2022; d'Este & Harvey, 2022; Mallar & Thornton, 1978; Palmer et al., 2019; Rauma & Berk, 1987; Tuttle, 2019; Yang, 2017).

[3] 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 & Meghir, 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, toward the next payout date.

[4] Beyond unlawful consumption, such as illicit drugs.

[5] Castellari et al. (2015, 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 weekend days produces an increase of 4%–7% in beer purchases, compared to weekdays.

[6] Cesur et al. (2022) provide further evidence on the cash transfer-IPV relationship, by exploiting an expansion of the Earned Income Tax Credit through a difference-in-differences approach. Using US National Crime Victimization Survey data, they find an increase of $1000 in after-tax income to reduce sexual and physical IPV incidence among unmarried women by approximately 21%.

[7] While welfare benefits are one of the most important transfers for preventing poverty in the Netherlands, as the ultimate social safety net once other benefits have been exhausted, we find only 3%–7% of the population of the cities under consideration to receive welfare benefits. Foley (2011) shows 2%–9% of the population of the investigated US cities to receive TANF benefits.

[8] Compared to Hsu (2017), who focuses on domestic violence, we investigate comprehensive measures of financially motivated and non-financially motivated crimes, to further investigate the underlying causal mechanisms.

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

[10] If a recipient moves from one municipality to another during the month, we consider the individual to be subject to the welfare disbursement schedule of the first municipality of residence for that month, and the welfare disbursement schedule of the second municipality of residence in the month following the address change.

[11] 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.

[12] The municipality fixed effects are identified by individuals who move during the sample period. Omitting the municipality fixed effects from the analysis does not substantively change the estimation results.

[13] All low-income households in the Netherlands are entitled to rent and healthcare benefits, which are disbursed on the 20th of every calendar month. As the disbursement of these benefits theoretically could affect our estimates, we assess the sensitivity of our estimates to such payouts by restricting the payment cycle. As will be discussed in Section 5, we do not find our results to be sensitive to these disbursements.

[14] 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.

[15] Minimum distance is suitable for any kind of situation that requires the estimation of (complex) models on large datasets. The estimation results from the randomly-selected subsamples lead to the same conclusions as the combined estimates.

[16] See Castellari et al. (2017, 2015).

[17] See Dobkin and Puller (2007).

[18] See Watson et al. (2019).

[19] More specifically, Castellari et al. (2015, 2017) find food stamp disbursement on weekend days to produce an increase of 4%–7% in beer purchases, compared to food stamp disbursement on weekdays.

[20] Note that, although we can measure drug crimes separately, we classify such offenses as financially motivated crime because they primarily consist of manufacturing and trafficking. That is because the possession of limited quantities of illicit substances for personal use is tolerated in the Netherlands.

[21] For example, M. A. Cohen (1988); McCollister et al. (2010); Rajkumar and French (1997).

[22] Based on US Bureau of Labor Statistics consumer price index data (US Bureau of Labor Statistics, 2018), we used an inflation rate of 1.417 to convert 2000 dollars to 2017 dollars. The resulting amounts were subsequently converted to euros by using the dollar/euro conversion rate of 0.885, as reported by the Organization for Economic Co-operation and Development (OECD) for 2017 (OECD, 2018b).

[23] While subject to higher thresholds than welfare benefits, rent and healthcare benefits are also means-tested in the Netherlands. To receive rent and healthcare benefits, individuals are required to fulfill an application (online) at the tax office, which is also the organization that disburses these benefits.

[24] Hsu (2017) finds that an increase in disbursement frequency causes spikes in domestic violence upon disbursement to disappear.

REFERENCES

Agnew, R. (1992) Foundation for a general strain theory of crime and delinquency. *Criminology*, 30(1), 47–88. Available from: https://doi.org/10.1111/j.1745-9125.1992.tb01093.x

Becker, G.S. (1968) Crime and punishment: an economic approach. *Journal of Political Economy*, 76(2), 169–217. Available from: https://doi.org/10.1086/259394

Been, J. & Knoef, M.G. (2017) Job-search requirements for unemployed at the end of working life effects on unemployment dynamics and self-employment probabilities. *Journal of Human Resources*, 52(2), 491–530. Available from: https://doi.org/10.3368/jhr.52.2.0415-7063r1

Berk, R.A., Lenihan, K.J. & Rossi, P.H. (1980) Crime and poverty: some experimental evidence from ex-offenders. *American Sociological Review*, 45(5), 766–786. Available from: https://doi.org/10.2307/2094894

Carr, J.B. & Packham, A. (2019) SNAP benefits and crime: evidence from changing disbursement schedules. *The Review of Economics and Statistics*, 101(2), 310–325. Available from: https://doi.org/10.1162/rest_a_00757

Carr, J.B. & Packham, A. (2021) SNAP schedules and domestic violence. *Journal of Policy Analysis and Management*, 40(2), 412–452. Available from: https://doi.org/10.1002/pam.22235

Carvalho, L.S., Meier, S. & Wang, S.W. (2016) Poverty and economic decision-making: evidence from changes in financial resources at payday. *The American Economic Review*, 106(2), 260–284. Available from: https://doi.org/10.1257/aer.20140481

Castellari, E., Cotti, C., Gordanier, J. & Ozturk, O. (2017) Does the timing of food stamp distribution matter? A panel-data analysis of monthly purchasing patterns of US households. *Health Economics*, 26(11), 1380–1393. Available from: https://doi.org/10.1002/hec.3428

Castellari, E., Cotti, C., Gordanier, J.M. & Ozturk, O.D. (2015) One more Saturday night: food stamp timing and monthly consumption patterns. Available at SSRN 2559615.

Cesur, R., Rodriguez-Planas, N., Roff, J. & Simon, D. (2022) *Domestic violence and income: quasi-experimental evidence from the earned income tax credit*. National Bureau of Economic Research. Working Paper No. 29930.

Chamberlain, G. (1984) Panel data. In Griliches, Z. & Intriligator, M. (Eds.) *Handbook of econometrics*. Amsterdam: North-Holland Publishing Company, 2.

Cohen, L.E. & Felson, M. (1979) Social change and crime rate trends: a routine activity approach. *American Sociological Review*, 44(4), 588–608. Available from: https://doi.org/10.2307/2094589

Cohen, M.A. (1988) Pain, suffering, and jury awards: a study of the cost of crime to victims. *Law & Society Review*, 22(3), 537. Available from: https://doi.org/10.2307/3053629

Cohen, M.A., Rust, R.T., Steen, S. & Tidd, S.T. (2004) Willingness-to-pay for crime control programs. *Criminology*, 42(1), 89–110. Available from: https://doi.org/10.1111/j.1745-9125.2004.tb00514.x

Corman, H., Dave, D.M. & Reichman, N.E. (2014) Effects of welfare reform on women's crime. *International Review of Law and Economics*, 40, 1–14. Available from: https://doi.org/10.1016/j.irle.2014.06.003

Cornaglia, F., Feldman, N.E. & Leigh, A. (2014) Crime and mental well-being. *Journal of Human Resources*, 49(1), 110–140. Available from: https://doi.org/10.3368/jhr.49.1.110

Damon, A.L., King, R.P. & Leibtag, E. (2013) First of the month effect: does it apply across food retail channels? *Food Policy*, 41, 18–27. Available from: https://doi.org/10.1016/j.foodpol.2013.04.005

Deshpande, M. & Mueller-Smith, M. (2022) Does welfare prevent crime? The criminal justice outcomes of youth removed from SSI. *Quarterly Journal of Economics*, 137(4), 2263–2307. Available from: https://doi.org/10.1093/qje/qjac017

d'Este, R. & Harvey, A. (2022) The unintended consequences of welfare reforms: Universal Credit, financial insecurity, and crime. *Journal of Law, Economics, and Organization*, 40(1), 129–181. Available from: https://doi.org/10.1093/jleo/ewac009

Dobkin, C. & Puller, S.L. (2007) The effects of government transfers on monthly cycles in drug abuse, hospitalization and mortality. *Journal of Public Economics*, 91(11-12), 2137–2157. Available from: https://doi.org/10.1016/j.jpubeco.2007.04.007

Ehrlich, I. (1973) Participation in illegitimate activities: a theoretical and empirical investigation. *Journal of Political Economy*, 81(3), 521–565. Available from: https://doi.org/10.1086/260058

Felson, M. (2006) *Crime and nature*. SAGE Publications.

Foley, C.F. (2011) Welfare payments and crime. *The Review of Economics and Statistics*, 93(1), 97–112. Available from: https://doi.org/10.1162/rest_a_00068

Friedman, M. (1957) The permanent income hypothesis. In: *A theory of the consumption function*. Princeton University Press, pp. 20–37.

Gould, E.D., Weinberg, B.A. & Mustard, D.B. (2002) Crime rates and local labor market opportunities in the United States: 1979–1997. *The Review of Economics and Statistics*, 84(1), 45–61. Available from: https://doi.org/10.1162/003465302317331919

Hamrick, K.S. & Andrews, M. (2016) SNAP participants' eating patterns over the benefit month: a time use perspective. *PLoS One*, 11(7), e0158422. Available from: https://doi.org/10.1371/journal.pone.0158422

Hastings, J. & Washington, E. (2010) The first of the month effect: consumer behavior and store responses. *American Economic Journal: Economic Policy*, 2(2), 142–162. Available from: https://doi.org/10.1257/pol.2.2.142

Holtfreter, K., Reisig, M.D. & Morash, M. (2004) Poverty, state capital, and recidivism among women offenders. *Criminology & Public Policy*, 3(2), 185–208. Available from: https://doi.org/10.1111/j.1745-9133.2004.tb00035.x

Horrace, W.C. & Oaxaca, R.L. (2006) Results on the bias and inconsistency of ordinary least squares for the linear probability model. *Economics Letters*, 90(3), 321–327. Available from: https://doi.org/10.1016/j.econlet.2005.08.024

Hsu, L.-C. (2017) The timing of welfare payments and intimate partner violence. *Economic Inquiry*, 55(2), 1017–1031. Available from: https://doi.org/10.1111/ecin.12413

Kruttschnitt, C. (2013) Gender and crime. *Annual Review of Sociology*, 39(1), 291–308. Available from: https://doi.org/10.1146/annurev-soc-071312-145605

Loeber, R. & Farrington, D.P. (2014) Age-crime curve. In Bruinsma, G. & Weisburd, D. (Eds.) *Encyclopedia of criminology and criminal justice*. New York: Springer, pp. 12–18.

Machin, S. & Meghir, C. (2004) Crime and economic incentives. *Journal of Human Resources*, 39(4), 958–979. Available from: https://doi.org/10.3368/jhr.xxxix.4.958

Mallar, C.D. & Thornton, C.V. (1978) Transitional aid for released prisoners: evidence from the LIFE experiment. *Journal of Human Resources*, 13(2), 208–236. Available from: https://doi.org/10.2307/145359

Mastrobuoni, G. & Weinberg, M. (2009) Heterogeneity in intra-monthly consumption patterns, self-control, and savings at retirement. *American Economic Journal: Economic Policy*, 1(2), 163–189. Available from: https://doi.org/10.1257/pol.1.2.163

McCollister, K.E., French, M.T. & Fang, H. (2010) The cost of crime to society: new crime-specific estimates for policy and program evaluation. *Drug and Alcohol Dependence*, 108(1), 98–109. Available from: https://doi.org/10.1016/j.drugalcdep.2009.12.002

Miller, J. (2013) Individual offending, routine activities, and activity settings: revisiting the routine activity theory of general deviance. *Journal of Research in Crime and Delinquency*, 50(3), 390–416. Available from: https://doi.org/10.1177/0022427811432641

Ministerie van Sociale Zaken en Werkgelegenheid. (2017) Uitkeringsbedragen per 1 januari 2017. Retrieved from https://archief28.sitearchief.nl/archives/sitearchief/20170629152234; https://www.rijksoverheid.nl/binaries/rijksoverheid/documenten/brochures/2016/12/22/uitkeringsbedragen-per-1-januari-2017/uitkeringsbedragenper1januari2017-1.pdf

OECD. (2018a) Adequacy of guaranteed minimum income benefits. Retrieved from https://stats.oecd.org/Index.aspx?DataSetCode=IA

OECD. (2018b) Exchange rates: total, national currency units/US dollar, 2000–2017. Retrieved from https://data.oecd.org/conversion/exchange-rates.htm

Palmer, C., Phillips, D.C. & Sullivan, J.X. (2019) Does emergency financial assistance reduce crime? *Journal of Public Economics*, 169, 34–51. Available from: https://doi.org/10.1016/j.jpubeco.2018.10.012

Rajkumar, A.S. & French, M.T. (1997) Drug abuse, crime costs, and the economic benefits of treatment. *Journal of Quantitative Criminology*, 13(3), 291–323. Available from: https://doi.org/10.1007/bf02221094

Rauma, D. & Berk, R.A. (1987) Remuneration and recidivism: the long-term impact of unemployment compensation on ex-offenders. *Journal of Quantitative Criminology*, 3(1), 3–27. Available from: https://doi.org/10.1007/bf01065198

Shapiro, J.M. (2005) Is there a daily discount rate? Evidence from the food stamp nutrition cycle. *Journal of Public Economics*, 89(2-3), 303–325. Available from: https://doi.org/10.1016/j.jpubeco.2004.05.003

Stam, M.T., Knoef, M.G. & Ramakers, A.A. (2024) *ECIN replication package for "Crime over the welfare payment cycle" (repository)*. Ann Arbor: Inter-university Consortium for Political and Social Research [distributor]. Available from: https://doi.org/10.3886/E198424V2

Statistics Netherlands. (2017a) Bevolkingsontwikkeling; regio per maand; januari 2017. Retrieved from https://opendata.cbs.nl/statline//CBS/nl/dataset/37230ned/table?dl=318D6

Statistics Netherlands. (2017b) Personen met een uitkering; uitkeringsontvangers per regio; januari 2017. Retrieved from https://opendata.cbs.nl//CBS/nl/dataset/80794ned/table?ts=1586294122434

Statistics Netherlands. (2018) Verdeling van inkomens, 2017. Retrieved from https://www.cbs.nl/nl-nl/visualisaties/inkomensverdeling

Statistics Netherlands. (2020) Verdachten van misdrijven; leeftijd, geslacht en recidive. Retrieved from https://opendata.cbs.nl/statline//CBS/nl/dataset/81997NED/table?ts=1597785869994

Statistics Netherlands, WODC Research and Documentation CentreThe Council for the Judiciary. (2013) Criminaliteit en Rechtshandhaving 2013. Retrieved from https://www.cbs.nl/nl-nl/publicatie/2014/45/criminaliteit-en-rechtshandhaving-2013

Steffensmeier, D. & Allan, E. (1996) Gender and crime: toward a gendered theory of female offending. *Annual Review of Sociology*, 22(1), 459–487. Available from: https://doi.org/10.1146/annurev.soc.22.1.459

Stephens, M., Jr (2003) "3rd of tha Month": do social security recipients smooth consumption between checks? *The American Economic Review*, 93(1), 406–422. Available from: https://doi.org/10.1257/000282803321455386

Tuttle, C. (2019) Snapping back: food stamp bans and criminal recidivism. *American Economic Journal: Economic Policy*, 11(2), 301–327. Available from: https://doi.org/10.1257/pol.20170490

US Bureau of Labor Statistics. (2018) Historical Consumer Price Index for all urban consumers (CPI-U): US city average, all items. by month. Retrieved from https://www.bls.gov/cpi/tables/supplemental-files/historical-cpi-u-201807.pdf

Velásquez, A. (2020) The economic burden of crime. *Journal of Human Resources*, 55(4), 1287–1318. Available from: https://doi.org/10.3368/jhr.55.4.0716-8072r2

Watson, B., Guettabi, M. & Reimer, M. (2019) Universal cash and crime. *The Review of Economics and Statistics*, 1–45.

Wilde, P.E. & Ranney, C.K. (2000) The monthly food stamp cycle: shopping frequency and food intake decisions in an endogenous switching regression framework. *American Journal of Agricultural Economics*, 82(1), 200–213. Available from: https://doi.org/10.1111/0002-9092.00016

Yang, C.S. (2017) Does public assistance reduce recidivism? *The American Economic Review*, 107(5), 551–555. Available from: https://doi.org/10.1257/aer.p20171001

SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Stam, M. T. C., Knoef, M. G. & Ramakers, A. A. T. (2024) Crime over the welfare payment cycle. *Economic Inquiry*, 62(3), 1309–1334. Available from: https://doi.org/10.1111/ecin.13223

APPENDIX A

Crime classification

TABLE A1 Crime classification.

Crime category[a]	Operationalization
1. Property crimes	Financially motivated
1.1 Theft, embezzlement & burglary (WvSr art. 310–312, 321–325)	Financially motivated
1.2 Deceit (WvSr art 326–338)	Financially motivated
1.3 Forgery (WvSr art. 208–214, 216–223, 225–234)	Financially motivated
1.4 Fencing (WvSr art. 416-417bis)	Financially motivated
1.5 Extortion & threat (WvSr art. 317–318)	Financially motivated
1.6 Bankruptcy fraud (WvSr art. 340–343)	Financially motivated
1.7 Money laundering (WvSr art. 420bis-420quater)	Financially motivated
1.8 Property crime (other) (WvSr art. 314–315, 344–348)	Financially motivated
2. Vandalism and crimes against public order and authority	Other
2.1 Vandalism & criminal damaging (WvSr art. 350–352)	Other
2.2 Public disorder (WvSr art. 131–136, 138-151c)	Other
2.3 Arson/explosion (WvSr art. 157–158)	Other
2.4 Public authority crime (WvSr art. 177–182, 184–206)	Other
3. Violent and sexual crimes	Other
3.1 Assault & battery (WvSr art. 300–306)	Other
3.2 Threatening & stalking (WvSr art. 284a-285b)	Other
3.3 Sex crimes (WvSr art. 239–250)	Other
3.4 Homicide (WvSr art. 287–296)	Other
3.5 Deprivation of liberty (WvSr art. 282-282a)	Other
3.6 Human trafficking (WvSr art. 273f)	Financially motivated
3.7 Violent crimes (other) (WvSr art. 274–281, 307–308)	Other

(Continues)

TABLE A1 (Continued)

Crime category[a]	Operationalization
4. Penal code crimes (other)	Other
5. Traffic crimes	Other
5.1 Hit & run (WVW art. 7)	Other
5.2 Driving under the influence (WVW art. 8)	Other
5.3 Driving while disqualified (WVW art. 9)	Other
5.4 Driving during driving ban (WVW art. 162)	Other
5.5 Vehicle registration fraud (WVW art. 41)	Other
5.6 Joyriding (WVW art. 11)	Other
5.7 Refusal to provide chemical samples (WVW art. 163)	Other
5.8 Traffic crimes (other) (WVW art. 6, 51, 61, 74, 114, 138)	Other
6. Drug crimes	Financially motivated[b]
6.1 Hard drugs (Opiumwet art. 2)	Financially motivated[b]
6.2 Soft drugs (Opiumwet art. 3)	Financially motivated[b]
7. Weapon possession crimes	Other
9. Other legal code crimes	Other
9.1 Military crimes (WvMSr art. 96–166)	Other
9.2 Crimes (other)	Other
9.9 Nature of crime unknown	Other

[a]As defined by Statistics Netherlands in the Dutch Standard Crime Classification (*Standaardclassificatie Misdrijven 2010*).

[b]As the possession of limited quantities of illicit substances for personal use is tolerated in the Netherlands, drug offenses primarily consist of manufacturing and trafficking and are therefore operationalized as financially motivated crime.

see Table A2

TABLE A2 Daily crime rates by days since payment, Dutch Standard Crime Classification.

Crime category[a]	Overall	First 3 days	Last 3 days
1. Property crimes (%)	0.0144	0.0135	0.0147
2. Vandalism and crimes against public order and authority (%)	0.0039	0.0042	0.0039
3. Violent and sexual crimes (%)	0.0078	0.0081	0.0072
4. Penal code crimes (other) (%)	0.0010	0.0011	0.0008
5. Traffic crimes (%)	0.0028	0.0031	0.0024
6. Drug crimes (%)	0.0024	0.0025	0.0024
7. Weapon possession crimes (%)	0.0007	0.0006	0.0008
9. Other legal code crimes (%)	0.0001	0.0002	0.0002

[a]As defined by Statistics Netherlands in the Dutch Standard Crime Classification *(Standaardclassificatie Misdrijven 2010)*.

APPENDIX B

Other crime subcategories

see Table B1

TABLE B1 Crime rates by non-financially motivated crime subcategory, multiple samples.

	Full sample	18–25 yo	26–39 yo	40+ yo	Men	Women
Sex & violent crime (daily, %)	0.008	0.016	0.011	0.005	0.015	0.002
Sex & violent crime (yearly, %)	1.483	2.006	1.915	1.043	2.650	0.464
Public order crime (daily, %)	0.004	0.009	0.005	0.003	0.007	0.001
Public order crime (yearly, %)	0.703	1.109	0.894	0.471	1.270	0.208
DUI crime (daily, %)	0.002	0.003	0.003	0.001	0.004	0.001
DUI crime (yearly, %)	0.422	0.507	0.584	0.285	0.783	0.107
Number of individuals	528,981	122,951	240,086	269,046	261,471	267,510
Number of observations	528.4M	40.2M	164.4M	323.8M	236.5M	291.9M

Note: Sex and violent crime includes all sexual and violent crimes apart from human trafficking (e.g., assault & battery, threatening & stalking, and homicide). Public order crime primarily consists of public disorder and vandalism & criminal damaging. Driving under the influence (DUI) crime pertains to the crime of operating a motor vehicle while being affected by alcohol or other (il)licit drugs. See Table A1 for a comprehensive list of crime classifications.

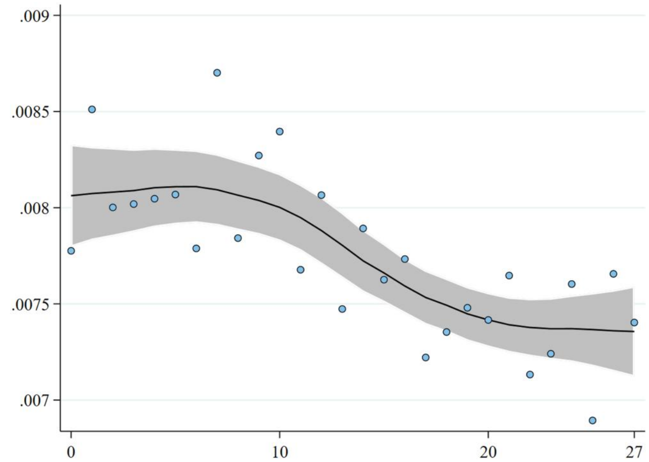

FIGURE B1 Sex and violent crime over days since payment.

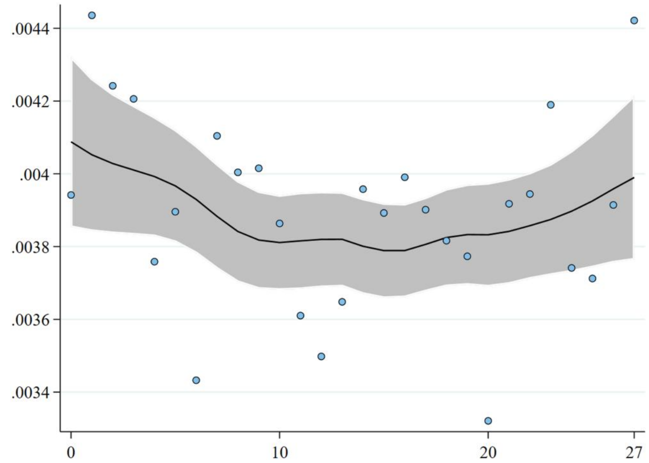

FIGURE B2 Public order crime over days since payment.

see Figure B3

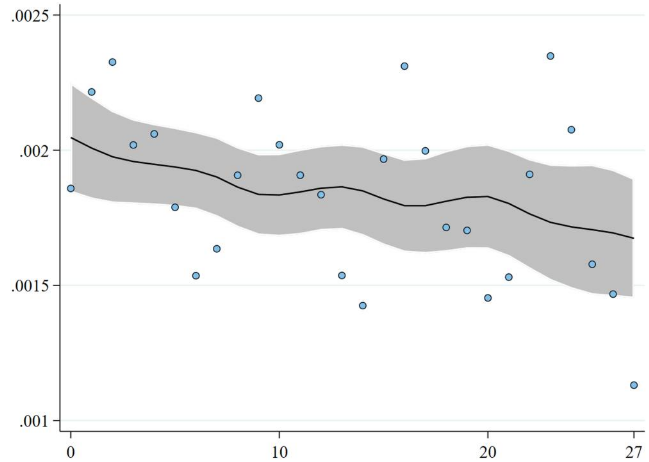

FIGURE B3 Driving under the influence offense crime over days since payment.