SNAP Schedules and Domestic Violence

Jillian B. Carr
Analisa Packham

Abstract

This paper exploits a policy change in Illinois that altered monthly nutritional assistance benefits dates to estimate the impact of in-kind benefit receipt on domestic violence. We find that issuing SNAP benefits on days other than the first of the month increases domestic crimes. On average, we find the shifting benefit dates increases domestic abuse by 6.9 percent and child maltreatment by 30.0 percent. We posit that these effects are driven by increases in opportunities for conflict or changes in drug use.

INTRODUCTION

In 2015, over 680,000 children were victims of child maltreatment in the United States, and nearly 25 percent of these victims lived in households with reports of physical intimate partner violence (U.S. Department of Health and Human Services, 2017). Domestic violence, including child abuse, child neglect, and intimate partner violence, has large social and economic implications. Mistreated children are more likely to have poor physical and mental health, experience behavioral problems, and have worse economic outcomes in adulthood (Currie & Spatz Widom, 2010; Currie & Tekin, 2012; Fletcher, 2009). Moreover, instances of abuse fall disproportionately to women and children in high-poverty households, which perpetuates the fact that socioeconomic status is a key indicator of family violence (Stith et al., 2009; U.S. Department of Justice, 2014).

In this paper, we investigate the impact of changes in the timing of household benefits disbursements on domestic violence. The change in the timing of benefits disbursements may generate resource shocks to poor households and exacerbate the risk for domestic violence. Despite the well-established link between poverty and domestic violence, the theoretical relationship between *changes* in resources and violence is ambiguous, and conclusions from economic models largely depend on how the intent or expression of violence is modeled. Standard models of economic theory, for example, predict that within-household violence can be mitigated by the relaxation of liquidity constraints. In these classic household bargaining models, violence is limited by threat points, such as divorce (Manser & Brown, 1980; McElroy & Horney, 1981). When a woman's income or potential income increases, her outside options improve, which creates a more credible threat point, and, consequently, reduces incidents of abuse or threats of violence (Farmer & Tiefenthaler, 1997;

Journal of Policy Analysis and Management, Vol. 00, No. 0, 1–32 (2020)

Published by Wiley Periodicals, Inc. View this article online at wileyonlinelibrary.com/journal/pam
DOI:10.1002/pam.22235

Pollak, 2005).[1] Moreover, if intimate partner violence is used as a way to relieve stress, and enters into an abuser's utility function directly, income or benefit receipt by resource-constrained households would lead to fewer instances of violence due to a reduction in financial anxiety.

Other types of signaling models suggest that an influx of resources perpetuates domestic violence. This is particularly relevant when an abuser attempts to exert control over the victim's behavior or the allocation of scarce household resources, known as instrumental violence (Anderberg & Rainer, 2011). Consistent with theories of male backlash, abusers may alternatively use extractive violence to control current or prospective financial resources, such as wages or other wealth owned by the victim or their family (Bloch & Rao, 2002; Bobonis, Gonzalez-Brenes, & Castro, 2013).

These existing studies document that, depending on the underlying cause of domestic abuse, positive resource shocks, including government transfers, have the ability to increase or decrease violence. More specifically, changes in the timing of benefit receipt have the potential to affect domestic violence if resources help to alleviate stress (as in the case of expressive violence) or if changing the transfer date creates temporary financial disruptions or a new opportunity for potential conflict (as in the case of instrumental violence).

To test how the receipt of in-kind transfers affects domestic violence, we estimate the impact of nutritional assistance issuance schedules on the number of incidents and timing of intimate partner violence and child maltreatment, using variation in monthly household benefit disbursement dates from the Supplemental Nutrition Assistance Program (SNAP). Consistent with instrumental and extractive signaling models, we find a positive relationship between household resource shocks and domestic violence, suggesting that when SNAP issuance is misaligned with the distribution of other types of income, such as paychecks or TANF benefits, the schedule change introduces new opportunities for within-household conflict. Given that low-income families disproportionately receive benefits from federal welfare programs, this paper sheds new light on how policy affects the link between poverty and abuse.

This paper builds on a larger literature on household resources and domestic violence. Recent studies have documented that poor economic conditions and liquidity play a large role in the incidence of family violence, likely due to economic stress. Negative, unexpected changes in income, such as job loss, are associated with increases in child abuse, neglect, and domestic homicide (Brown & de Cao, 2017; Dugan, Nagin, & Rosenfeld, 1999; Lindo, Hansen, & Schaller, 2018). Similarly, economic downturns, like the Great Recession, result in greater instances of child trauma and abuse (Huang et al., 2011; Stephens-Davidowitz, 2013; Wood et al., 2012), while positive shocks can accordingly reduce domestic violence in some settings.[2]

Alternatively, existing evidence suggests that income from a large cash transfer program, Temporary Assistance for Needy Families (TANF), increases household violence right after receipt, driven by alcohol-related purchases (Hsu, 2016). These findings imply that monthly government benefits may affect within-household

[1] See Aizer (2010) for a model that shows under what assumptions an increase in women's income leads to a decline in violence. Specifically, if a women's utility function is increasing in her own consumption and safety, there is an upper bound to violence and the contract curve has a positive slope; increases in relative income lead to fewer instances of violence.

[2] For example, Aizer (2010) documents that historical reductions in the gender wage gap were responsible for a 9 percent decline in domestic violence from 1990 to 2003, suggesting that changes in the labor market can improve women's outside options and provide a more credible threat point.

bargaining decisions differently than changes in wages, and may increase purchases of goods that are complements to crime. In this paper, we estimate to what extent this behavior extends beyond cash by focusing on the distribution schedules of in-kind transfers.

This paper contributes to this important, policy-relevant discussion by exploring the monthly cyclicality of intimate partner violence and child maltreatment and analyzing how changes in the timing of benefits affect the levels and timing of abuse. In particular, our findings fit into a larger literature suggesting that such benefits are considered fungible, and that the timing effects of government transfers affect other outcomes, including crime (Carr & Packham, 2019; Foley, 2011; Hsu, 2016) and test scores (Cotti, Gordanier, & Ozturk, 2017).[3] We note that any perceived fungibility can contribute to the effects we find, especially if it influences purchases of alcohol or drugs, which are correlates of domestic violence.[4] Moreover, we note that domestic violence, unlike other types of crime, such as property crime, is often motivated less by criminal opportunity payoffs than by financial strain and power inequality. Therefore, by focusing on domestic abuse and child maltreatment, our analysis can build on and contribute to the body of work discussing the effectiveness of legal and household interventions on reducing the direct and external costs of within-household violence (Dugan, Nagin, & Rosenfeld, 1999, 2003; Stevenson & Wolfers, 2006).

To measure the impact of in-kind benefit transfers on domestic violence, we exploit variation in SNAP timing to measure the effect of household resource shocks on levels of violence. Specifically, in 2010, Illinois changed its SNAP benefit issuance from a primarily first-of-the-month distribution to a staggered distribution over 12 days, and we explore whether this change had an impact on various types of child maltreatment and domestic abuse. In previous work, we show that this policy change led to lower levels of crime and theft, with particularly stark results for those crimes occurring at grocery stores, and take this as evidence that families do respond to the policy change (Carr & Packham, 2019).[5] In this study, we leverage additional granular data on child maltreatment as well as domestic violence and family violence data for areas outside of Chicago to answer several new questions, including whether violent criminal activity *across* households responds to the change in SNAP timing, whether there is any evidence of impacts on child health, how these effects vary across neighborhoods, how short-lived these effects are, and what could be driving any observed changes in criminal activity. In doing so, we address how household shocks affect a fundamentally different type of crime—domestic violence—which is motivated more by within-household conflict, psychological and financial control, and strain, than by opportunities for financial gain.

We answer these questions using administrative datasets on domestic violence crimes. We estimate the effects on daily domestic violence incidents at the Census Tract-level using both regression discontinuity (RD) and difference-in-regression discontinuity (D-i-RD) approaches. Because domestic violence rates have fallen nearly continuously over the past three decades, such approaches allow us to separate out the effects of the change in SNAP disbursement dates from other factors.

[3] The distribution method of benefits also affects crime. Recent work shows that cash distribution of TANF was associated with more crime; switching to electronic benefits reduced street crime by 9.2 percent, suggesting that the desire and opportunities for acquiring cash motivates property crimes (Wright et al., 2017).

[4] Indeed, evidence suggests that SNAP timing does affect purchases of alcohol and drunk driving (Castellari et al., 2017; Cotti, Gordanier, & Ozturk, 2015).

[5] In Carr and Packham (2019) we use the maximum bandwidth available: three years. For a replication of those results using the smaller bandwidth of 12 months used in this paper, see Table A1 in the Appendix.

Notably, we do not observe which households are SNAP recipients. Therefore, all estimates will represent intent-to-treat effects.

Our estimates are based on detailed crime data from the city of Chicago from February 2009 to February 2011. The primary advantage of these administrative, day-level data is that they include reports of domestic violence even if no arrest is made. This feature allows us to contribute to and improve on the existing knowledge of occurrence and timing of domestic violence—a crime widely known to experience nonrandom underreporting (Ellsberg et al., 2001). Nearly all of the studies on domestic violence to date rely on ex-post survey data, which include only self-reported incidents of abuse and grossly underestimate the severity of maltreatment (Bondurant, 2018; Cicchetti & Carlson, 1989; McMillan, Jamieson, & Walsh, 2003; Swahn et al., 2006; Waldfogel, 1998; U.S. Department of Justice, 2005).

Although there is likely to still be considerable underreporting, our data constitute significant improvements on survey data since we are able to more accurately measure severity, timing, and exact locations of domestic violence, even when the victim chooses not to press charges. Using these data, estimates based on our primary difference-in-RD approach indicate that distributing nutritional assistance benefits later in the month increases domestic abuse by 6.9 percent and increases child maltreatment by 30.0 percent. Effects are largely driven by more serious types of abuse (domestic battery). We note that this change in violent family crimes across the course of the benefit month similarly tracks increases in drug-related crime, suggesting that in-kind benefits constitute enough of a household resource shock to alter drug-related behavior. We find that effects are short-lived; estimates indicate that increases in violence last more than a few months but less than one year, indicating that changing benefit cycles may cause short-run disruptions in household planning and cause undue financial stress.

These findings have several implications for policy and contribute to a growing literature on household shocks and domestic violence in five main ways. First, we find that policies that help families avoid food scarcity at the end of the benefit month do not lead to fewer reports of child malnourishment, nor do they justify less policing in low-income communities. Second, our results indicate that the null estimates of the effects of the SNAP issuance policy change on total crime reported in Carr and Packham (2019) obscure effects of within-household violence. We conclude that an influx of benefits in the middle of the month increases violence between partners, potentially driven by changes in drug use or household stress, and that this conflict induces negative spillovers to children. Third, we show that although distributing benefits later in the month has the potential to mitigate some first-of-the-month abuse, the decrease in domestic violence on the first does not fully compensate for the increases on later dates, indicating that new benefit dates create additional opportunities for family strife. Fourth, we analyze whether victims or third parties are more likely to report abuse after the policy change and provide some evidence to suggest that these results are not driven by systematic changes in reporting. Fifth, we discuss how changes in benefit timing can have both short- and long-run consequences. In doing so, our findings add to a growing discussion on the advantages and disadvantages of in-kind transfers and on the efficacy of distributing SNAP benefits only once per month.

BACKGROUND ON ILLINOIS SNAP POLICY

This section describes inner workings of the Supplemental Nutrition Assistance Program before providing background information on the 2010 Illinois SNAP policy change. To do so, we refer to much of the discussion in Carr and Packham (2019).

The Supplemental Nutrition Assistance Program (SNAP)

Although SNAP (formerly known as the Food Stamp Program) is a federally-funded program, states have the authority to determine eligibility requirements, calculate monthly benefits for qualifying households, and issue benefits through an electronic transfer system. As a result, the monthly timing of benefit disbursement varies greatly across states. Currently, all but seven states issue benefits on multiple days of the month, although no SNAP participant receives benefits more than once per month.

While we recognize that there is recent evidence that many SNAP families mentally earmark SNAP funds to be spent on food (Hastings & Shapiro, 2018), previous work has also documented that SNAP recipients view benefits as fungible, displacing cash expenditures on food such that more of a family's budget is available for nonfood purchases. Therefore, SNAP issuance serves as a general household income shock.[6] Nevertheless, participants increase food consumption right after benefit receipt, and subsequently reduce consumption throughout the month. Families that receive SNAP often face substantial resource limitations at the end of the month, just before they receive their next benefit payment, and many households find themselves going without food (Bruich, 2014; Castner & Henke, 2011; Goldin, Homonoff, & Meckel, 2016; Gregory & Smith, 2019; Hamrick & Andrews, 2016; Hastings & Washington, 2010; Kuhn, 2018; Shapiro, 2005; Wilde & Ranney, 2000).

In all states, benefits are issued to a recipient's debit-like program card on the same date each month. However, most states assign different groups of recipients to different issuance dates, using what is known as a "staggered" benefit schedule. There are many reasons why a state would choose to distribute SNAP benefits on multiple days during the month. First, staggering benefits could alleviate crowding at grocery stores on issuance dates. In doing so, staggered distribution schedules aid grocers in stocking and staffing decisions. Additionally, such policies protect consumers from grocery store price hikes due to demand shocks in low-income communities. This argument is especially compelling given the evidence that recipients spend a majority of their benefits in the first two weeks after issuance.[7] Second, recipients often receive other sources of income at the beginning of the month, from employment or other programs, such as Temporary Assistance for Needy Families (TANF) or the Women, Infants, and Children (WIC) program. Therefore, by distributing SNAP benefits later in the month, states can spread out administrative costs.

One potential demand-side advantage of staggered issuance policies is that receiving benefits later in the calendar month could assist families with consumption smoothing or maintaining stable levels of food availability. A majority of SNAP recipients also earn wages or other income, which are typically distributed, at least partially, on the first of the month. In distributing benefits later in the month, staggered SNAP schedules have the ability to reduce domestic violence by preventing a large influx of resources at one time. Therefore, not only could shifting monthly nutritional assistance benefit timing prevent high levels of stress associated with hunger at the end of the month but could also lower incentives for household conflict corresponding with a desire for control over a bundle of resources.

[6] Fraud presents an opportunity for families to convert their benefits to cash, but in 2010 fraud only accounted for 0.4 percent of total benefits paid out nationally (U.S. Department of Agriculture, 2017). Regardless, the ability to convert benefits to cash makes it even more likely that the in-kind transfer will be seen as an income shock.

[7] See Figure A1 in the Appendix, which uses data from the Illinois Department of Health and Human Services to illustrate that consumers responded to the changes in Illinois SNAP distribution dates by reducing SNAP redemptions on the first of the month by nearly 50 percent.

Moreover, since staggered benefit policies have the potential to reduce first-of-the-month effects, we may expect that such policies also reduce negative outcomes related to alcohol or drug use often associated with these monthly resource shocks.[8] On the other hand, if benefit issuance constitutes a household income shock, and individuals view benefits as fungible, staggering benefits could create an extra day of potential struggle, as partners divvy up the additional resources, or could create another opportunity for alcohol or drug use.

The 2010 Illinois SNAP Policy Change

On February 16, 2010, as a way to reduce crowding in grocery stores, the State of Illinois enacted a staggered benefit issuance schedule. Prior to the policy change, 70 percent of benefits were distributed on the 1st, while the remaining 30 percent of cases were split between the 4th, 7th, and 10th. After the change, cases were added to the 4th, 7th, and 10th days of the month, with the full range of disbursement dates ranging from the 1st to the 23rd.[9,10] To minimize the impact of moving benefit dates, the change occurred over a three-month period, starting in February.[11] The Illinois Department of Human Services announced the change to the public 13 days before the policy change.

Importantly, some households still received benefits on the first after the policy change. However, a large proportion of families experienced a noticeable change in their benefit date from the 1st to the 4th, 7th, or 10th. No recipient ever received benefits more than once a month; the change was limited to shifting the household's benefit date to a later date in the month. In this analysis, we consider the aggregate effects of this policy change to study how SNAP receipt affects household violence. To do so, we will consider how the temporal patterns of domestic violence correspond to changes in monthly SNAP distribution timing.

DATA

In this paper, we focus on crimes sufficiently serious to warrant police response. Notably, domestic violence and child maltreatment reports could be instigated by individuals outside of the residence, and instances do not need to be contained in the home to be flagged as domestic abuse.[12] Offense-level data contain information on whether or not an arrest was made, and victims do not need to press charges for a record to appear in the data.

Specifically, we use administrative offense-level data from the City of Chicago's online data portal for February 1, 2009 to January 31, 2011, containing offenses

[8] See, for example, Cotti, Gordanier, and Ozturk (2015) and Watson, Guettabi, and Reimer (2018) on the effects of income timing on alcohol purchases and substance abuse-related crimes, respectively.
[9] Officially, Illinois SNAP benefits are made available on the 1st, 3rd, 4th, 7th, 8th, 10th, 11th, 14th, 17th, 19th, 21st, and 23rd of each month.
[10] After the policy change, the first of the month remained a primary distribution day, with over 30 percent of cases issued. The remaining 11 issuance dates each accounted for between 5 to 10 percent of caseloads. See Goldin, Homonoff, and Meckel (2016) for more information on Illinois SNAP issuance dates.
[11] To account for this phase-in period or any potential announcement effects, we estimate the policy change cutoff at February 1, 2010, although we also estimate some specifications that drop these three months.
[12] In the state of Illinois, domestic violence is considered any crime against family members related by blood, current or ex-spouses, those living in the same dwelling, people who are dating or engaged or used to date, including same-sex couples, and people with disabilities and their personal assistants, according to state statutes (Illinois Attorney General, 2018).

occurring one year before and one year after the SNAP policy change.[13,14] For placebo tests and bandwidth sensitivity tests, we expand our sample to include data from January 2007 to June 2013.[15]

One of the primary advantages of these data is the ability to pinpoint the location, date, and time that the crime was reported.[16] For our main analyses, we use coordinates to geocode the location of each crime and create a Census Tract-by-day panel. In doing so, we are able to estimate effects across areas with relatively more SNAP recipients, using annual Census Tract-level data on SNAP participation rates from the American Community Survey.

There are two major benefits to using crime-level data as opposed to survey data or reports of child abuse to public services. First, these data do not rely on ex-post descriptions of abuse severity or timing. Second, in our data, reporting of family violence is less likely to respond to frequency of interaction with mandatory reporters, as cases do not depend on individuals being legally required to report an incident. Therefore, our reports contain records of abuse and maltreatment for any first- or third-party observation that was reported to police. This is an important feature, especially when analyzing effects on child maltreatment, because if family resources affect the likelihood that a child interacts with responsible adults (such as teachers or community workers) who are obligated to report signs of abuse, it could be difficult for institutional reporting systems to disentangle reporting effects from actual changes in abuse patterns.

Nonetheless, we acknowledge that underreporting of child maltreatment is still likely in this context. We supplement our analysis with data from the National Child Abuse and Neglect Data System (NCANDS), which contains detailed information on child maltreatment reports for all counties in Illinois, to test whether we observe changes in child maltreatment more broadly across the state and across many types of reporting channels.

Finally, we use annual, county-level data from the FBI Uniform Crime Reports from 2007 to 2012, which contain counts of violence against families and children, as reported by local agencies. These data allow us to capture changes in crimes across Cook County and compare these trends to other urban counties in the U.S. in an effort to account for trends in domestic violence over time. By comparing areas surrounding Chicago to other areas across the country, we are able to analyze the relative effects of changing SNAP issuance schedules and observe their persistence. We use these data in conjunction with population counts from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER) to consider domestic crime rates in our analysis, and to construct county-level measures of demographics (fraction black and fraction Hispanic). We measure county-level economic conditions using unemployment rates from the Bureau of Labor Statistics.

In our main analysis, we select the set of crimes used in each category to reflect different mechanisms that can affect family violence. For domestic abuse, we consider all reports that are flagged for or indicate a case of domestic violence between adults and extend this analysis to separately estimate effects for domestic battery, assault,

[13] Available for download at https://data.cityofchicago.org/Public-Safety/Crimes-2001-to-present/ijzp-q8t2. Although more data are available, we limit our sample period to one year given that all optimal bandwidth estimates are less than 365 days.

[14] Specifically, all RD-optimal bandwidths range from 87 to 323 days.

[15] We do not use the full year of 2013 due to a subsequent SNAP policy change in the latter half of that year. RD Estimates using this larger sample yield comparable results to our preferred one-year sample.

[16] While the National Child Abuse and Neglect Data System (NCANDS) dataset has more detailed information about perpetrators and victims of child maltreatment, it does not contain the actual day of report or abuse. Instead, observations are aggregated temporally into the first half and second half of the month.

Table 1. Summary statistics.

	Mean	St. Dev.
Domestic Abuse	0.189	0.481
Domestic Abuse-Battery	0.102	0.341
Domestic Abuse-Assault	0.003	0.056
Domestic Abuse-Property	0.008	0.089
Domestic Abuse-Threat	0.051	0.232
Child Maltreatment	0.004	0.065

Notes: Chicago crime data are from the Chicago online Data portal (https://data.cityofchicago.org/Public-Safety/ Crimes-2001-to-present/ijzp-q8t2). Our sample includes 597,140 Census Tract-day observations that span February 1, 2009 to January 31, 2011.

property damage, or threats made. Battery, the most serious offense listed above, includes unlawful physical contact with the intent to cause injury, while assault reflects the presence or threat of harm to another individual. In our definition of child maltreatment, we include cases of child abuse and neglect.[17] Specifically, for counts of child maltreatment, we include any cases from the city of Chicago data that indicate battery or aggravated assault of a child, child abandonment, endangering the life or health of a child, or contributing to criminal delinquency of a child/juvenile. Lastly, to show that other factors related to the outcomes of interest are not driving our results, we use daily weather data on wind speed, temperature, and precipitation from the Global Historical Climatology Network measured at O'Hare Airport.

Table 1 contains summary statistics on the Census Tract-by-day level for these crime data. On average, a Census Tract has nearly 0.19 reports of domestic abuse per day, with battery making up 54 percent of these reports. This corresponds to approximately 154.6 cases of domestic violence across the city of Chicago per day, or around 56,400 per year. Child maltreatment, including abuse and neglect, is less common, with Census Tracts experiencing less than two incidents per year.[18]

METHODS

We exploit the timing of the SNAP policy change in Illinois to estimate the causal effect of benefit issuance on domestic violence and child maltreatment per Census Tract. In doing so, we operationalize a regression discontinuity model of the following form:[19]

$$crime_{it} = \beta_0 + \beta_1\, SNAP\ staggered_t + f(days\ from\ cutoff_t) + \pi_d + \gamma_m + \psi_y + \lambda_i + u_{it}, \quad (1)$$

where $crime_{it}$ represents the count of various domestic violence crimes in Census Tract i on date t, $SNAPstaggered_t$ represents a dummy variable equal to one for dates

[17] Child physical abuse is defined as non-accidental injury to a child inflicted by a parent or caregiver. Child neglect includes the failure of a parent or caregiver to provide adequate supervision, medical care, or other necessities, and includes cases of child abandonment and endangerment.
[18] Child abuse is the most common child-specific offense, with each Census Tract experiencing 0.003 per day on average (or about one per year), or approximately 896 annual cases of physical child abuse city-wide.
[19] While we refer to the model as a regression discontinuity model throughout the paper, one can also consider this approach to be akin to estimating an interrupted time series model.

after the initiation of the staggered SNAP issuance policy, and β_1 is the effect of the policy change on each outcome of interest, including incidents of domestic abuse or child maltreatment. The term f (*days from cutoff*$_i$) represents our treatment of the running variable, the number of days from the February 2010 policy change, which we allow to vary on either side of the cutoff. We include a host of fixed effects to control for cyclicality in crime: π_d is a set of day-of-week fixed effects, γ_m is day-of-month fixed effects, and ψ_y is year fixed effects. To account for variation across neighborhoods, we also control for Census Tract fixed effects, λ_i.[20] We control for the days from cutoff (running variable) in multiple ways and allow it to vary on either side of the cutoff. Standard errors are clustered on the Census Tract level.[21]

Given that the running variable is defined as "days from the policy change cutoff," we employ a number of additional tests to address issues related to the time-series nature of these models, as suggested by Hausman and Rapson (2018). As they recommend, we plot residuals of the data after removing covariates, and we consider alternate time trends and bandwidths. We also estimate placebo tests using different treatment dates and estimate RD models on covariates at the real time of treatment.

Specifically, for our main RD analysis, we estimate equation (1) using ordinary least squares, allowing for a linear function of the running variable, although we additionally fit models where the running variable enters the equation quadratically, allowing it to vary across the treatment threshold. Moreover, while we use a bandwidth of two years (February 1, 2009 to January 31, 2011) to estimate our baseline results, our preferred specifications will show estimates from a model that restricts our sample to observations with a mean square error-(MSERD)-optimal bandwidth, as suggested by Calonico et al. (2017), although we perform multiple robustness checks to test for stability across bandwidths.

The identification assumption underlying this model is that no other policy changes or other related events occur coincident with the policy, implying that all other determinants of domestic violence are smooth across the date of treatment. Since the policy change occurred in the middle of the month, we consider the full month to be treated in the following analyses, and normalize our running variable to be equal to zero on February 1, 2010, which yields more conservative estimates than a mid-month treatment definition, as it accounts for any announcement effects of the policy change.[22] The fact that SNAP recipients cannot manipulate SNAP issuance timing alleviates potential selection concerns. Nonetheless, we consider whether there may be additional policy changes or general disruptions related to domestic violence that coincide with the change in SNAP issuance timing. We find no evidence of such changes, and provide support that weather, county-level employment rates, and the timing of other sources of income do not drive our findings.

That being said, it is well known that crime levels follow recurring patterns based on seasonality. To account for this cyclicality even further, we use an alternative

[20] Because our crime incidents are count data, we also consider a Poisson model. However, in our main analysis, we estimate equation (1) using OLS, as this relaxes the condition that the mean and variance of the outcome variable are equal.

[21] This approach is more conservative than clustering on the running variable, which yields economically similar results.

[22] We have also considered a model that drops February 2010 entirely as well as a model that drops February 2010 and March 2010 to account for phase-in, as recommended by Hausman and Rapson (2018). When dropping February from the analysis, estimates indicate an increase in domestic abuse and battery by 28.3 and 32.2 percent, respectively, and an increase in child maltreatment by 20.0 percent. When omitting February and March, our estimates are even larger. These estimates are all similar, albeit larger in magnitude, to the estimates shown in column 2 of Table 2. Therefore, in subsequent analysis, we include data from February 2010 and March 2010 in an effort to produce more conservative estimates.

difference-in-regression discontinuity (D-i-RD) approach. To do so, we introduce observations with temporal variation to serve as comparison groups for our treated observations. This model includes all of the variables in a traditional RD but adds interactions of each with an indicator for whether the observation is from a treated or untreated period. Specifically, we estimate models of the following form:

$$\begin{aligned} crime_{it} = {} & \beta_0 + \beta_1 SNAP\ staggered_t + \beta_2 SNAP\ staggered_t * treatedyear_t \\ & + \beta_3 treatedyear_t + f(days\ from\ cutoff_t) + f(days\ from\ cutoff_t * treatedyear_t) \\ & + \pi_d + \gamma_m + \psi_y + \lambda_i + u_{it}, \end{aligned} \tag{2}$$

where $treatedyear_t$ is an indicator variable equal to one for observations in 2010, the year of the policy change. All other variables remain unchanged from equation (1). The primary coefficient of interest is β_2, which represents the effect of the policy change in 2010 relative to other years (i.e., 2008, 2009, 2011, and 2012). We allow all of the years to have different intercepts and introduce another separate control for the running variable in the year of treatment. In each relevant year, the running variable is normalized to February 1st. The identification assumption underlying this model is that trends in domestic abuse crimes are similar in 2010 to those of other years in the months prior to the policy change, and that no other policy changes occur simultaneously with the SNAP timing change in February 2010. Therefore, any estimated effects are relative to the dates just prior to the SNAP timing change as compared to any changes in crime across the threshold in February in other (untreated) years.

Finally, we provide a number of checks to address the possibility that our results are driven by a systematic change in domestic violence reporting at the time of the policy change, including looking at the proportion of reports ending in arrest, and analyzing other county-level datasets containing family crimes and child maltreatment reports across the state of Illinois and the U.S.

RESULTS

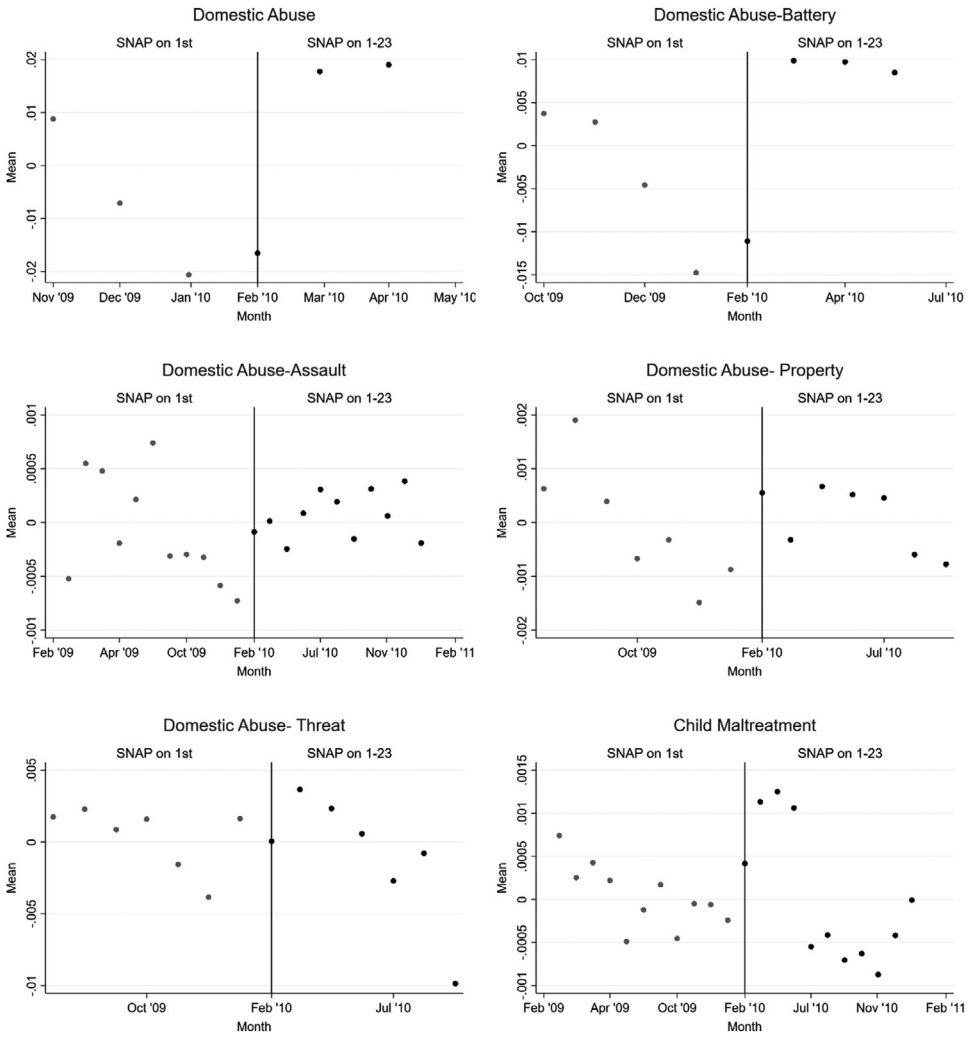
Main Results

In this section, we evaluate whether changes in SNAP disbursement dates affect domestic abuse and child maltreatment.

In Figure 1, we present visual evidence of the effects of the 2010 Illinois SNAP policy change on domestic violence, including overall levels of domestic abuse and child maltreatment. We additionally display effects for select categories of intimate partner violence, including battery, assault, threats, and property crimes. In these figures, we plot the monthly means of each outcome of interest, using the MSE-optimal bandwidth, and control for cyclical trends (year, day-of-week, and day-of-month fixed effects) and Census Tract fixed effects. The vertical line denotes the timing of the policy change. Overall, Figure 1 shows striking evidence of an increase in overall household violence after the policy change, including large effects on domestic battery and child maltreatment.[23]

In Table 2, we formalize the relationships presented in Figures 1 and A2. To do so, we estimate equation (1) using OLS with a full set of fixed effects for day of week, day of month, year, and Census Tract. Column 1 shows RD estimates from our full

[23] We additionally provide figures showing plots for each outcome based on a two-year bandwidth in Figure A2.



Notes: Each figure plots month-level means of residuals (after differencing out Census Tract, year, day-of-week and day-of-month fixed effects) of each of the crimes listed, using MSERD-optimal bandwidths. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. Crime data are from the city of Chicago from February 1, 2009 to January 31, 2011.

Figure 1. Effect of Illinois SNAP Disbursement Change on Domestic Violence, Using Optimal Bandwidth.

sample, February 1, 2009 to January 31, 2011. Column 2 replicates the estimates for column 1 using a MSERD-optimal bandwidth, and across nearly all models yields more conservative estimates. For this reason, in the following discussion we treat the MSERD-optimal bandwidth as our preferred specification. We additionally provide the pre-period means for all estimates to inform the magnitude of the effects.

Estimates in column 1 indicate that the change in SNAP disbursement dates increased domestic abuse by 23.9 percent with increases in domestic battery and assault of 28.9 percent and 26.7 percent, respectively. Effects for property crimes and

Table 2. The effect of staggering SNAP benefits on domestic violence, regression discontinuity estimates.

	Day of Month Range				
	Average Effect	Average Effect	1st of Month	Days 2–23	Days 24–31
Domestic Abuse					
SNAP Staggered	0.0461***	0.0114**	−0.2829***	0.0144**	0.0221*
	(0.0036)	(0.0048)	(0.0672)	(0.0058)	(0.0116)
Pre-Period Mean	0.193	0.170	0.237	0.169	0.168
N	597,140	143,150	4,090	104,704	34,356
Battery					
SNAP Staggered	0.0295***	0.0101***	−0.0876*	0.0136***	0.0061
	(0.0026)	(0.0031)	(0.0512)	(0.0037)	(0.0080)
Pre-Period Mean	0.102	0.092	0.125	0.089	0.097
N	597,140	177,506	5,726	130,062	41,718
Assault					
SNAP Staggered	0.0008**	0.0007*	0.0024	0.0006	0.0008
	(0.0004)	(0.0004)	(0.0024)	(0.0004)	(0.0008)
Pre-Period Mean	0.003	0.003	0.003	0.003	0.003
N	597,140	529,246	17,178	383,642	128,426
Property					
SNAP Staggered	0.0014**	0.0018**	−0.0014	0.0017**	0.0027*
	(0.0006)	(0.0007)	(0.0074)	(0.0008)	(0.0015)
Pre-Period Mean	0.008	0.008	0.010	0.008	0.007
N	597,140	316,566	10,634	226,586	79,346
Threat					
SNAP Staggered	0.0030*	0.0029	−0.0397*	0.0013	0.0125***
	(0.0017)	(0.0019)	(0.0237)	(0.0022)	(0.0039)
Pre-Period Mean	0.055	0.051	0.083	0.052	0.044
N	597,140	319,838	10,634	229,858	79,346
Child Maltreatment					
SNAP Staggered	0.0015***	0.0013***	−0.0016	0.0019***	0.0002
	(0.0005)	(0.0005)	(0.0039)	(0.0006)	(0.0010)
Pre-Period Mean	0.004	0.004	0.007	0.004	0.004
N	597,140	509,614	17,178	366,464	125,972
One-Sided Bandwidth	1 Year	Optimal	Optimal	Optimal	Optimal

Notes: Estimates are based on reported crime data from the city of Chicago. Each coefficient is generated by a separate Census Tract-by-day regression of equation (1) using the listed crime type as the dependent variable and using data from all days (columns 1 and 2) or the ranges listed at the top of each column. Each regression includes Census Tract, year, day-of-month, and day-of-week fixed effects and fits the running variable linearly. Standard errors are clustered on the Census Tract level and reported in parentheses. We also report the mean of each outcome for the period before the policy change (February 1, 2009 to February 15, 2010) for the relevant bandwidth.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

threats, which do not require medical attention and may be more likely to go unnoticed or unreported, also increased after the policy change by 17.5 and 5.5 percent, respectively, although estimates for threats are not consistent across all columns. Strikingly, estimates indicate an increase in child maltreatment by 37.5 percent.

In column 2, we estimate effects for observations within the MSERD-optimal bandwidth. We find that after the SNAP policy change, domestic abuse increased by 6.7 percent, driven by increases in domestic battery and assault of 11.0 percent and 23.3 percent, respectively. These estimates correspond to over nine more instances of domestic abuse across the city of Chicago per day, or 3,400 crimes per year.

Similarly, child maltreatment increased by 32.5 percent, which implies one additional instance of child maltreatment across the city of Chicago per day.

In Table 2, we additionally consider whether the days of the month most likely to be impacted by the policy change drive the observed effects. Given that the state shifted benefits for most recipients away from the 1st of the month to another date ranging from the 2nd to the 23rd, while the 24th to 31st remained untreated, we separately examine effects on these date ranges. Columns 3 through 5 present estimates based on the OLS model in equation (1), restricting the sample to the 1st of the month, 2nd to 23rd, and 24th to 31st, respectively.[24]

As shown in columns 3 through 5, we find that cases of domestic battery decrease at the beginning of the month followed by a 15.3 percent increase spanning days 2 to 23.[25] Given that battery is the most serious domestic abuse offense in terms of physical harm, these findings suggest that when there is an influx of resources later in the month, households respond by initiating violence or increasing the intensity of attacks during these weeks. These findings are consistent with other evidence showing that intimate partner violence increases by between 73 and 210 percent for TANF recipients in the first four days following TANF disbursement (Hsu, 2016).

We find similar patterns in child maltreatment, although we do not find subsequent decreases at the beginning of the month. Estimates indicate that staggering SNAP benefits leads to a 47.5 percent increase in child maltreatment crimes spanning days 2 to 23. Altogether, these results imply that when families experience resource shocks at different times, there are more opportunities for households to engage in bargaining, leading to more conflict. While some of these effects are mitigated by the reduction of income and violence on the first of the month, these effects are not fully offset and overall levels of violence increase. Below, we provide some insight as to possible mechanisms that could explain these findings.

Given that domestic crimes experience seasonality effects, we additionally present local average estimates from a difference-in-RD model, using years other than 2010 as controls.[26] This approach will allow us to estimate how much the SNAP timing change affected domestic crimes relative to years in which there was no policy change.

Specifically, columns 1 and 2 in Table 3 contain D-i-RD results using 2008, 2009, 2011, and 2012 as control years, and the baseline results are consistent with the results in Table 2. In particular, we find that staggering SNAP benefits leads to a 6.9 percent increase in domestic abuse, driven by large increases in domestic battery. Estimates also indicate that changes in SNAP timing increase child maltreatment by 30.0 percent, suggesting that increases in domestic crimes in 2010 are larger than what would have been expected in the absence of the policy change.

[24] Alternatively, in Figure A3, we present estimates in three-day intervals, similar to Foley (2011).

[25] We find that cases of domestic abuse follow a similar trend. Although the first-of-the-month decrease is relatively large, reflecting a drop of over 200 domestic abuse crimes across Chicago each month, it is smaller than the total increase in crimes on days later in the month. In particular, our estimates indicate an increase in approximately 400 crimes committed on later dates across Chicago per month. However, we note that the standard errors for these estimates suggest a large range of effects, spanning 64 to 174 percent.

[26] For a graphical representation of our main difference-in-RD estimates, comparing pre-period crime levels in 2008 and 2010, respectively, to those in the following months, before and after February 1, see Figure A5. Overall, graphs for domestic battery, assault, and child maltreatment show that, prior to the policy change, crime levels decreased on February 1, whereas levels in 2010 increased at the threshold, indicating that such increases are not typical each year.

Table 3. The effect of staggering SNAP benefits on domestic violence by location type, difference-in-RD estimates.

	Location			
	Average Effect	Average Effect	Non-Residence	Residence
Domestic Abuse				
Staggered*Treated Year	0.0146***	0.0117**	0.0038	0.0079*
	(0.0036)	(0.0052)	(0.0026)	(0.0044)
Pre-Period Mean	0.192	0.170	0.065	0.169
N	2,981,642	715,757	715,757	715,757
Battery				
Staggered*Treated Year	0.0120***	0.0110***	0.0088**	0.0179**
	(0.0027)	(0.0035)	(0.0043)	(0.0075)
Pre-Period Mean	0.102	0.092	0.078	0.213
N	2,981,642	887,540	269,072	269,072
Assault				
Staggered*Treated Year	0.0003	0.0002	−0.0004	0.0005
	(0.0004)	(0.0004)	(0.0006)	(0.0010)
Pre-Period Mean	0.003	0.003	0.004	0.006
N	2,981,642	2,646,258	835,120	835,120
Property				
Staggered*Treated Year	−0.0012	0.0006	0.0010	−0.0001
	(0.0007)	(0.0008)	(0.0011)	(0.0017)
Pre-Period Mean	0.008	0.008	0.008	0.016
N	2,981,642	1,582,846	497,945	497,945
Threat				
Staggered*Treated Year	0.0008	−0.0011	−0.0034	−0.0013
	(0.0019)	(0.0020)	(0.0022)	(0.0050)
Pre-Period Mean	0.054	0.051	0.016	0.112
N	2,981,642	1,599,206	503,442	503,442
Child Maltreatment				
Staggered*Treated Year	0.0013**	0.0012**	0.0002	0.0009
	(0.0006)	(0.0006)	(0.0003)	(0.0005)
Pre-Period Mean	0.004	0.004	0.001	0.005
N	2,981,642	2,548,097	2,548,097	2,548,097
One-Sided Bandwidth	1 Year	Optimal	Optimal	Optimal

Notes: Estimates are based on reported crime data from the city of Chicago. Each coefficient is generated by a separate Census Tract-by-day regression of equation (2) using the listed crime type as the dependent variable and using data from all days. Columns 1 and 2 contain results from a D-in-RD model where years 2008, 2009, 2011, and 2012 are used as the controls. Columns 3 and 4 report D-in-RD results by location type. "Non-Residence" refers to the subset of crimes occurring outside of a home. "Residence" refers to crimes occurring in a house, apartment, college dorm, or government housing. Each regression includes Census Tract, year, day-of-month, and day-of-week fixed effects. Standard errors are clustered on the Census Tract level and reported in parentheses. We also report the mean of each outcome for the period before the policy change (February 1, 2009 to February 15, 2010) for the relevant bandwidth.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

Differential Effects by Location

Although our main results suggest that staggering SNAP benefits leads to changes in domestic violence, on average, these results could mask information regarding where domestic crimes are happening, whether these crimes are more likely to happen at home, or in public, and whether they are more likely to be reported. In Table 3, we explore differential effects by location type using the D-i-RD model with the MSERD-optimal bandwidth. In particular, in columns 3 and 4, we separately show the effects of staggered SNAP policies on residential violence (crimes

Table 4. The effect of staggering SNAP benefits on domestic violence, difference-in-RD estimates.

	Baseline	High-SNAP	Low-SNAP
Domestic Abuse			
Staggered*Treated Year	0.0117**	0.0186***	0.0047
	(0.0052)	(0.0049)	(0.0091)
Pre-Period Mean	0.170	0.076	0.263
N	715,757	357,875	357,882
Battery			
Staggered*Treated Year	0.0110***	0.0076**	0.0145**
	(0.0035)	(0.0031)	(0.0062)
Pre-Period Mean	0.092	0.038	0.146
N	887,540	443,765	443,775
Assault			
Staggered*Treated Year	0.0002	0.0001	0.0003
	(0.0004)	(0.0004)	(0.0008)
Pre-Period Mean	0.003	0.001	0.005
N	2,646,258	1,323,115	1,323,143
Property			
Staggered*Treated Year	0.0006	−0.0003	0.0015
	(0.0008)	(0.0008)	(0.0013)
Pre-Period Mean	0.008	0.003	0.013
N	1,582,846	791,415	791,431
Threat			
Staggered*Treated Year	−0.0011	−0.0039	0.0017
	(0.0020)	(0.0024)	(0.0033)
Pre-Period Mean	0.051	0.037	0.064
N	1,599,206	799,595	799,611
Child Maltreatment			
Staggered*Treated Year	0.0012**	0.0006	0.0018*
	(0.0006)	(0.0006)	(0.0010)
Pre-Period Mean	0.004	0.002	0.006
N	2,548,097	1,274,035	1,274,062
One-Sided Bandwidth	Optimal	Optimal	Optimal

Notes: Estimates are based on reported crime data from the city of Chicago. Each coefficient is generated by a separate Census Tract-by-day regression of equation (2) using the listed crime type as the dependent variable and using data from all days. Column 1 contains results from a D-in-RD model where years 2008, 2009, 2011, and 2012 are used as the controls. Columns 2 and 3 report D-in-RD results for Census Tracts with an above-median SNAP participation rate and a below-median SNAP participation rate, respectively. Each regression includes Census Tract, year, day-of-month, and day-of-week fixed effects. Standard errors are clustered on the Census Tract level and reported in parentheses. We also report the mean of each outcome for the period before the policy change (February 1, 2009 to February 15, 2010).
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

occurring in a house, apartment, college dorm room, or government housing) and non-residential crimes (all other locations).

Given that a large majority of domestic crimes occur at home, it is perhaps unsurprising that effects for domestic abuse are concentrated in residential locations. However, estimates for domestic battery are statistically significant both for residential and non-residential locations, and indicate effects of 8.4 to 11.2 percent, suggesting that domestic violence crimes after the policy change are more likely to occur both in public and at home. Estimates for child maltreatment by location are not statistically significant at conventional levels.

Similarly, in Table 4, we estimate effects for Census Tracts for high and low levels of SNAP participation, separately, based on tract-level data from the ACS. When we

split the Census Tracts at the median SNAP enrollment percentage (24.8 percent), and repeat the methodology described in the previous section, we find that domestic crimes increase more in high-SNAP enrollment areas. In particular, we find that shifting benefits later in the month increases domestic abuse and domestic battery by 24.5 percent and 20.0 percent, respectively, in Census Tracts with relatively high SNAP participation rates, although we note that coefficients across columns 2 and 3 are not statistically different at the 1 percent level.[27,28]

Alternatively, effects could suggest that after the change in SNAP timing such crimes are more likely to be reported.[29] This is especially relevant if changes in the frequency or severity of domestic violence increases the likelihood of reporting by a third party, and implies that our findings may represent a lower bound if victims are unlikely to report crimes that occur at home. In the next section, we address to what degree our results could be driven by changes in monitoring behavior and provide additional explanations for these findings.

Robustness

In this section, we consider the extent to which potential confounders, discussed in the previous sections, are empirically relevant to our analysis, as well as provide evidence that our preferred model specifications are not yielding an anomalous result. A common concern in regression discontinuity-type models is that the results are a product of over- or underfitting the data or a consequence of bandwidth selection. To address these concerns, we explore various alternative RD specifications in this section and show that our average estimates are not sensitive to these other specifications.

In Table 5, we perform a set of standard robustness tests. First, in columns 1 and 2, we report results from our baseline models from Table 2, which use observations from both estimated MSERD-optimal bandwidths and the full sample. In column 3, we fit the days from the policy change (the running variable) quadratically, while still allowing the fit to vary on either side of the cutoff. Estimates for child maltreatment are positive and statistically significant and indicate that the SNAP policy change led to large increases of 37.5 percent. All domestic abuse estimates but one in column 3 are statistically insignificant and relatively imprecise, indicating that models using higher-order polynomials may overfit the data.

Since the crime data are discrete, we estimate a corresponding Poisson model and display results in column 4. Notably, some Census Tracts may have no reported

[27] Estimates for child maltreatment are statistically significant only for low-SNAP areas, which may indicate that reporting of child maltreatment is more likely to be from a third party, rather than the victim. However, estimates when splitting our sample are less precise for this outcome, and we cannot reject large effects (up to 88.8 percent) in high-SNAP areas, nor can we reject that these two coefficients are statistically different from each other.

[28] Taking this analysis a step further, we can also compare crime levels in Census Tracts ranking in the highest quartile of SNAP participation rates to those in the lowest quartile. To do so, we use data from only the highest and lowest quartile Census Tracts and estimate a difference-in-differences specification controlling for day-of-month, day-of-week, month, year, and Census Tract fixed effects. Our variable of interest is an indicator variable equal to one for days after the February 2010 policy change for the Census Tracts in the highest quartile of the SNAP participation rate distribution and zero otherwise. Estimates indicate a 3.3 percent increase in domestic violence and a 22 percent increase in child maltreatment relative to the lowest-quartile SNAP Census Tracts. We present the corresponding event-study estimates for domestic abuse and child maltreatment in Figure A4. Importantly, estimates indicate that trends in domestic violence crimes in these areas track each other closely prior to the policy change and diverge sharply afterwards.

[29] Effects for non-residential crimes are largely driven by statistically significant increases in domestic battery at stores (by 10 percent) and on the street (by 65.6 percent).

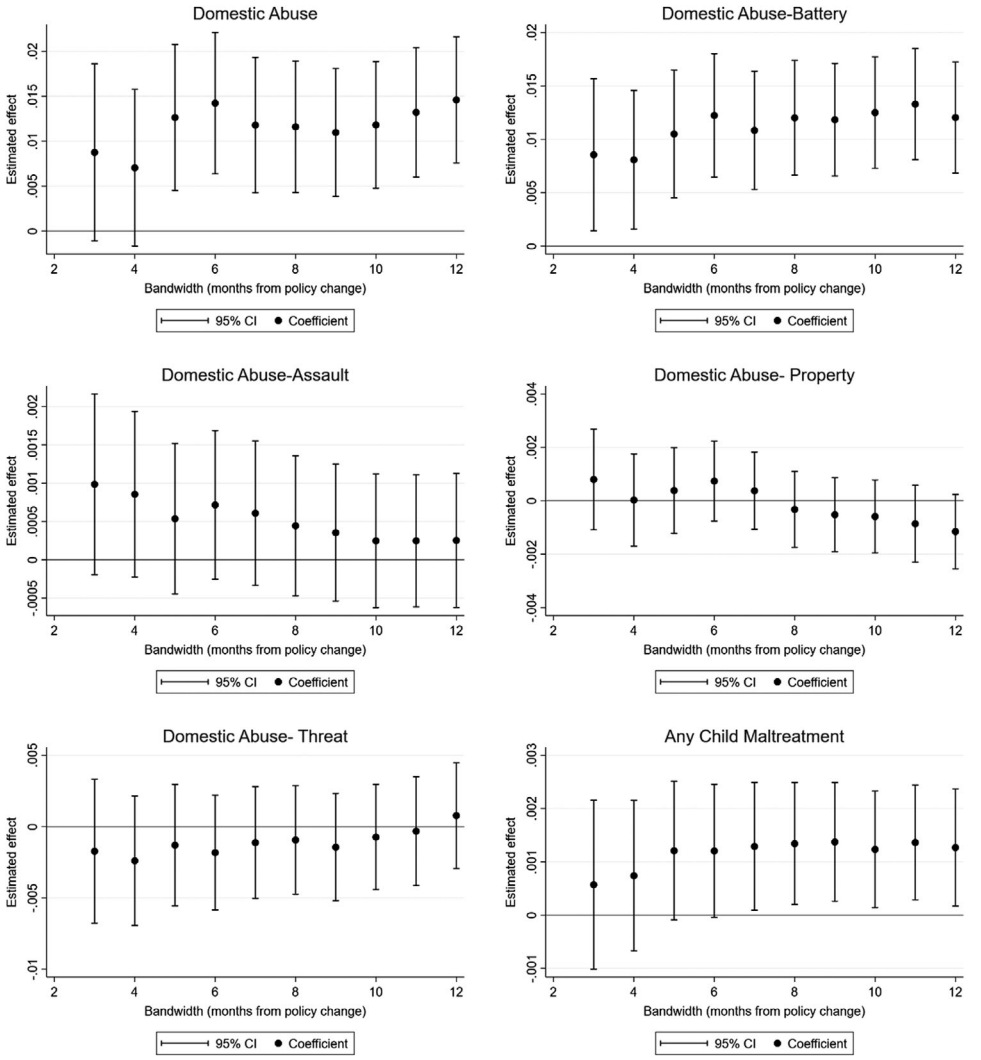
Table 5. Robustness checks, RD specification.

					Triangular Kernel	
	Optimal BW	Full BW	Quad Fit	Poisson	MSERD BW	Full BW
Domestic Abuse						
SNAP Staggered	0.0114**	0.0461***	−0.0090	0.0720***	0.0020	0.0363***
	(0.0048)	(0.0036)	(0.0086)	(0.0277)	(0.0050)	(0.0026)
N	143,150	597,140	143,150	138,250	597,140	597,140
Battery						
SNAP Staggered	0.0101***	0.0295***	−0.0041	0.1130***	0.0230***	0.0040
	(0.0031)	(0.0026)	(0.0054)	(0.0336)	(0.0018)	(0.0033)
N	177,506	597,140	177,506	170,128	597,140	597,140
Assault						
SNAP Staggered	0.0007*	0.0008**	0.0008*	0.2807*	0.0007**	0.0007**
	(0.0004)	(0.0004)	(0.0005)	(0.1546)	(0.0003)	(0.0003)
N	529,246	597,140	529,246	340,322	597,140	597,140
Property						
SNAP Staggered	0.0018**	0.0014**	0.0009	0.2389**	0.0014***	0.0015**
	(0.0007)	(0.0006)	(0.0009)	(0.0940)	(0.0005)	(0.0007)
N	316,566	597,140	316,566	220,203	597,140	597,140
Threat						
SNAP Staggered	0.0029	0.0030*	0.0011	0.0549	0.0028**	0.0019
	(0.0019)	(0.0017)	(0.0025)	(0.0356)	(0.0013)	(0.0017)
N	319,838	597,140	319,838	310,454	597,140	597,140
Child Maltreatment						
SNAP Staggered	0.0013***	0.0015***	0.0015**	0.2929***	0.0014***	0.0014***
	(0.0005)	(0.0005)	(0.0006)	(0.1128)	(0.0004)	(0.0004)
N	509,614	597,140	509,614	371,931	597,140	597,140

Notes: Each coefficient is generated by a separate Census Tract-by-day regression of equation (1) using the listed crime type as the dependent variable. Columns 1 and 2 replicate the baseline results for comparison. Column 3 allows for the days from the cutoff to vary quadratically (in addition to varying on either side of the threshold). Column 4 reports Poisson coefficients. Columns 5 and 6 fit the model using a triangular kernel instead of uniform kernel. In particular, column 5 uses a MSE-driven bandwidth, while column 6 reports estimates from the full sample. One-sided MSE-optimal bandwidths for domestic abuse, battery, assault, property crimes, threats, and child maltreatment when using a triangular kernel are 88, 105, 373, 190, 209, and 315 days, respectively. Crime data are from the city of Chicago.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

cases of child maltreatment or domestic abuse. Therefore, a number of observations are dropped in this model. Poisson estimates for all outcomes are statistically significant and similar to the baseline results. Finally, in columns 5 and 6, we test how sensitive these estimates are to kernel selection by estimating the model using a triangular kernel, as compared to the uniform kernel, which we assume for our main results. Estimates using a triangular kernel are positive and nearly all estimates are statistically similar to our baseline estimates in columns 1 and 2.

Second, to test how sensitive our main results are to bandwidth selection, we replicate our difference-in-RD and RD specifications under a range of bandwidths. We start by testing how robust our difference-in-RD estimates are to various bandwidths spanning three months on either side of the threshold up to 12 months, and show our results for a linear fit in Figure 2. Estimates for domestic abuse and child maltreatment are all positive and are statistically significant for bandwidths spanning five to 12 months (on each side); estimates using a one-sided bandwidth of three and four months are similar in sign and magnitude, but slightly less precise. All estimates for domestic battery are positive and statistically significant.



Notes: Each dot represents the coefficient of interest generated by a separate regression as specified by equation (2). The various bandwidths on which these regressions were performed are represented on the x-axis, such that the x-value indicates the bandwidth used on each side of the cutoff. We also report the 95 percent confidence interval of the coefficient. Reported crime data are from the city of Chicago.

Figure 2. Effect of Varying Bandwidth on Difference-in-RD Estimates.

Our difference-in-RD estimates are limited to this 12-month window because other years are used as controls in that approach. RD estimates are not limited in this way, so we provide an additional test of our RD estimates using bandwidths spanning three to 39 months in Figure A6. Estimated effects for domestic abuse and domestic battery are positive and stable across all bandwidths, and estimates are always statistically significant at the 5 percent level. Similarly, effects on child maltreatment are positive and nearly all estimates are statistically significant.

Third, although our preferred differences-in-RD specification accounts for the recurring monthly fluctuations in crime, we perform additional checks to test to what extent any RD estimates are driven by existing crime cyclicality.

Table 6. February placebo RD estimates.

	February 2010 (actual)	February 2008	February 2009	February 2011	February 2012
Domestic Abuse					
SNAP Staggered	0.0114**	−0.0118**	0.0008	−0.0056	0.0045
	(0.0036)	(0.0049)	(0.0029)	(0.0060)	(0.0046)
N	143,150	143,150	143,150	143,150	143,150
Child Maltreatment					
SNAP Staggered	0.0013**	−0.0004	−0.0002	0.0007	0.0006
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0004)
N	509,614	509,614	509,614	509,614	509,614
One-Sided Bandwidth	Optimal	Optimal	Optimal	Optimal	Optimal

Notes: Each coefficient is generated by a separate Census Tract-by-day regression of equation (1), assigning a different year as the treatment cutoff and using the listed crime type as the dependent variable. Column 1 replicates the baseline results from Table 2, column 2 for comparison, using February 1, 2010, as the treatment date. Columns 2, 3, 4, and 5 reassign the treatment cutoff to February 1, 2008, 2009, 2011, and 2012, respectively. Crime data are from the city of Chicago.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

In Table 6, we test whether the discontinuity observed in crime levels after the policy change is a season regularity, or "February effect." Specifically, we estimate equation (1), assigning February 2008, 2009, 2011, and 2012 separately as treatment cutoff dates. None of the estimates in columns 2 through 5 indicate a statistically significant increase in domestic abuse or child maltreatment, which implies that our findings are not a result of typical monthly fluctuations in crime rates.[30,31]

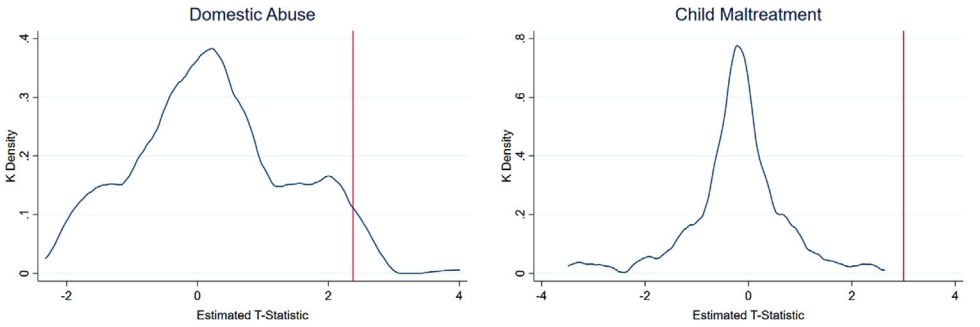
Furthermore, we conduct permutation inference using placebo RD estimates from pre-period crime data to provide more evidence that the discontinuity observed in Chicago is a result of the SNAP policy change and not an artifact of the data. To do so, we randomly select a date from 2007 to 2010, and assign it as a treatment cutoff date, without replacement.[32] We then generate distributions of estimates and standard errors based on these RD estimates, using the preferred specification in equation (1) and MSE-optimal bandwidths associated with Table 2, to determine what percent of the simulated estimates from 1,000 random draws are greater than the estimate reported in column 2 of Table 2.

The distributions of t-statistics, based on placebo estimates for domestic abuse and child maltreatment, are shown in Figure 3. Based on these placebo distributions, 4.4 percent and 0.2 percent of t-statistics are greater than the reported estimates for domestic abuse and child maltreatment, respectively, which provides additional support for the idea that the policy change is driving these reported results.

[30] While the estimate in Table 6, column 2, is statistically significant for domestic abuse, the coefficient is negative. We hypothesize that this may be due to changes in employment in 2008 or reporting of domestic abuse during the Great Recession.

[31] Similarly, when we include month fixed effects into our main RD specification, estimates indicate a statistically significant increase in domestic abuse by 36.4 percent and an increase in child maltreatment by 17.5 percent. We do not control for month fixed effects in our main results, given that the MSE-optimal bandwidths are less than one year for all outcomes and relatively small for some outcomes (e.g., 45 days for domestic abuse). Therefore, many samples do not contain more than two months, and controlling for month fixed effects in this context would yield estimates from likely misspecified models.

[32] When randomly selecting a treatment date, we drop observations that would be included within the optimal bandwidth according to our true treatment date, February 1, 2010.



Notes: Each figure plots the distribution of 1,000 t-scores from placebo regressions of the regression discontinuity specification (equation 1) using randomly drawn discontinuities and pre-period crime data from 2007 to 2010. For domestic abuse and child maltreatment, 4.4 percent and 0.2 percent of t-statistics (in absolute value) are larger than those reported in Table 2, respectively. Reported crime data are from the city of Chicago.

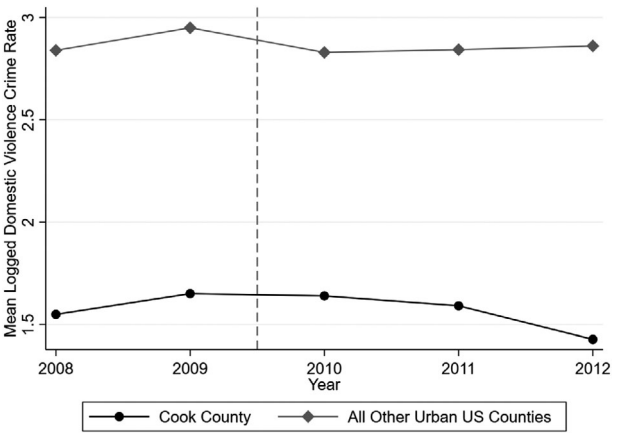
Figure 3. Empirical Distribution of Placebo Estimates.

Finally, we address the possibility that both systematic weather changes and changes in economic factors are biasing our results. If any of these variables experience a discontinuity after the policy change, we would worry that any estimates that do not account for these factors would over- or understate the true effects of staggering SNAP benefits. In Figures A7 and A8, we test these outcomes formally, using weather data on precipitation, temperature, and wind from the Global Historical Climatology Network and unemployment rate data from the Bureau of Labor Statistics, respectively. All weather variables and unemployment rates are smooth across the treatment threshold when tested at conventional levels of statistical significance. Visually, however, because the amount of snowfall in inches increased in February 2010 from 0.2 to 0.4 inches in December and January to approximately 0.6 inches in February, we have alternatively estimated models that add weather controls to account for any possible weather effects. When we do, estimates for domestic abuse and child maltreatment are statistically similar at the 1 percent level to our preferred estimates in Table 2 and Table 3.

Longer-Run Effects

Although our methodology focuses on local linear effects, it is possible that any increases in domestic abuse observed near the policy change eventually phase out as households adjust to new disbursement dates. Therefore, it's not only critical to focus on the discontinuity at the cutoff, but also to observe how the slope varies on either side of the threshold, especially considering the nearly continuous decades-long decline in domestic violence crimes nationwide. As shown in Figure A2, trends in domestic abuse and child maltreatment decrease at a faster rate after the policy change, falling to levels below those of early 2009. These figures suggest that there may be a transition period that households experience after a change in income timing in which violence is more frequent.

To further investigate the effects of SNAP policy changes over time, in Figure 4 and Table 7 we use annual, county-level incident data from the FBI Uniform Crime Reports (UCR) to analyze the changes in domestic abuse crimes in Cook County as compared to urban counties in other states. UCR data classify domestic violence crimes as "offenses against family and children." The advantage of these data is that we are able to capture changes in crimes at a broader level and compare



Notes: Annual, county-level data from 2007 to 2012 is from the FBI. Domestic violence crimes are crimes reported as "offenses against family and children." The above graph plots mean domestic violence arrests for Cook County versus other U.S. urban counties. "Urban" is defined according the USDA rural-urban continuum codes and includes all counties in metro areas with at least 250,000 population.

Figure 4. Effect of Illinois SNAP Disbursement Change on Domestic Violence Arrests, Difference-in-Differences using FBI Uniform Crime Reports.

these changes to other urban counties in the U.S. in an effort to account for trends in domestic violence over time. In focusing on the entirety of Cook County, we are able to get a more comprehensive picture of the effects of such policy changes.[33]

Figure 4 shows that domestic violence crimes in Cook County tracked trends in other areas prior to the policy change, then increased in 2010 relative to other U.S. counties, before falling in the following years. In comparison, domestic violence crimes across the U.S. remained relatively unchanged from 2007 to 2012, and, if anything, appear to fall in 2010.

We formalize this relationship using a difference-in-differences approach to compare crime rates in Cook County and other urban U.S. counties over time and present these estimates in Table 7. Specifically, we estimate the following model:

$$DV_{ct} = \beta_0 + \sum_{k=1}^{3} \theta_k \, SNAP\ staggered_{c,t-k} + \delta X_{ct} + \gamma_c + \alpha_t + u_{ct}, \tag{3}$$

where DV_{ct} is the logged domestic violence crime rate in county c in year t, $SNAP\ staggered_{c,t-k}$ is an indicator variable that takes a value of one for Cook County k years after 2009 and zero otherwise, γ_c are county fixed effects to control for any systematic differences across counties, α_t are year fixed effects to control for shocks to crimes that are common to all counties in a year, and X_{ct} can include time-varying county-level economic and demographic controls. Rates are constructed per 100,000 population. All analyses allow errors to be correlated within counties over time when constructing standard-error estimates.

[33] Ideally, we would be able to track crimes across all Illinois counties; however, Illinois incident data on family crimes is available only for Cook County and Winnebago County. Illinois National Incident-Based Reporting System (NIBRS) data are available only for Rockford County.

Table 7. Difference-in-differences estimates of the effect of the 2010 Illinois SNAP policy change on logged domestic violence crime rates in urban counties.

	(1)	(2)	(3)
Effect in 2010	0.109***	0.097**	0.136**
	(0.035)	(0.046)	(0.064)
Effect in 2011	0.085**	0.063	0.102
	(0.042)	(0.062)	(0.079)
Effect in 2012	−0.088*	−0.120	−0.081
	(0.048)	(0.075)	(0.091)
One-Year Lead			0.078
			(0.050)
Average Effect	0.04	0.01	0.05
P-Value (test average effect = 0)	0.33	0.81	0.48
Mean	2.86	2.86	2.86
Observations	1,776	1,776	1,776
County Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Controls	No	Yes	Yes

Notes: Domestic violence crimes are crimes reported as "offenses against family and children." Annual, county-level data from 2007 to 2012 is from the FBI Uniform Crime Reports. Rates are constructed per 100,000 population. County-level demographic and economic controls include percent black, percent Hispanic, and unemployment rate. Standard errors are clustered at the county level. "Urban" counties are according the USDA rural-urban continuum codes and include all counties in metro areas with at least 250,000 population.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

We use data from urban counties (defined by the USDA as metro areas with at least 250,000 population) in an attempt to compare Chicago to other areas that may be similar on observable characteristics and experiencing similar pre-2010 trends in crime. The identifying assumption underlying this approach is that the proportional changes in domestic violence crimes in the comparison counties provide a good counterfactual for the proportional changes that would have been observed in Cook County in the absence of the 2010 SNAP policy change.

Importantly, we allow the estimated effects to vary across years with a set of indicator variables rather than considering the coefficient on a single "post-treatment" indicator to observe both short-run and longer-run effects of the policy change. However, we note that in the event that the policy change has lasting effects, we may prefer to focus on the average effect across years and on the statistical significance of the effect across years. Therefore, we additionally report the average lagged effect and p-value of a joint significance test in all columns.

Difference-in-differences estimates are shown in Table 7. Estimates in columns 1 through 3 indicate domestic violence crimes in Cook County did not increase, on average, relative to other counties in the three years after the policy change. However, effects for the year of the policy change are positive and statistically significant and indicate an increase in domestic violence crimes of 9.7 to 13.6 percent, which is similar to our baseline results of 6.9 percent.[34] Moreover, estimates for a one-year leading indicator variable are statistically insignificant, providing additional support for the identification assumption. Estimates in columns 2 and 3, measuring effects two to three years after the policy change, are statistically insignificant at the

[34] Our findings are qualitatively similar when calculating raw counts of domestic violence.

Table 8. The effect of staggering SNAP benefits on household violence, controlling for weekend income, difference-in-RD estimates.

	Average Effect	Weekend Payday	Weekend SNAP
Domestic Abuse			
Staggered*Treated Year	0.0117**	0.0124**	0.0115**
	(0.0052)	(0.0052)	(0.0052)
Weekend Payday		0.0118**	
		(0.0053)	
Weekend SNAP			−0.0017
			(0.0023)
Pre-Period Mean	0.170	0.170	0.170
N	715,757	715,757	715,757
Child Maltreatment			
Staggered*Treated Year	0.0012**	0.0011**	0.0012**
	(0.0006)	(0.0006)	(0.0006)
Weekend Payday		−0.0007	
Weekend SNAP		(0.0005)	−0.0002
			(0.0002)
Pre-Period Mean	0.004	0.004	0.004
N	2,548,097	2,548,097	2,548,097
One-Sided Bandwidth	Optimal	Optimal	Optimal

Notes: Estimates are based on reported crime data from the city of Chicago. Each coefficient is generated by a separate Census Tract-by-day regression of equation (2) using the listed crime type as the dependent variable. Each regression includes Census Tract, year, day-of-month, and day-of-week fixed effects and fits the running variable linearly. "Weekend SNAP" represents a dummy variable equal to one if any potential SNAP disbursement day of the month corresponds to a Friday or Saturday. "Weekend Payday" represents a dummy variable equal to one if the 1st or 15th day of the month corresponds to a Friday or Saturday. Standard errors are clustered on the Census Tract level and reported in parentheses. We also report the mean of each outcome for the period before the policy change.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

5 percent level. Overall, these findings suggest that the policy change had immediate effects that phased out over the following years.

This evidence points to two arguments explaining why domestic violence crimes spike after initiating a staggered SNAP policy but fall over time. First, it's possible that families take time to adjust to a new income schedule. Second, if domestic violence is increasing in 2010 along with *arrests* of offenders, then it's possible that the removal of assailants in the household reduces domestic violence over time. We explore the latter possibility in greater detail below.

ALTERNATIVE EXPLANATIONS

Weekend Income Shocks

To test whether our main results are sensitive to the particular types of days on which beneficiaries experience income shocks, we provide additional results, controlling separately for weekend SNAP receipt (i.e., when benefits are distributed on a Friday or Saturday), and weekend paydays (i.e., when the 1st or 15th of the month falls on a Friday or Saturday) in Table 8.[35]

[35] We use these dates since over 36 percent of American businesses (and 72.9 percent of businesses with over 1,000 employees) have a biweekly pay schedule (Burgess, 2014).

We do so in an effort to account for the fact that increasing the number of SNAP issuance dates raises the probability that every month some proportion of total recipients receive benefits on the weekend or on a day that recipients receive income from a job. This may point to an alternative channel, if, for instance, recipients purchase more complements to crime (such as alcohol or drugs) when receiving benefits on the weekend, or if, on the contrary, individuals are more likely to stay home, which could lead to more instances of household violence. Similarly, receiving benefits on paydays has the potential to affect within-household tension by increasing the amount of resources at stake.

In Table 8, we display estimates from equation (2), accounting for SNAP distribution on paydays and weekends, respectively. Specifically, column 1 replicates our main difference-in-RD baseline estimates, while column 2 includes an indicator for common paydays falling on a Friday or Saturday, and column 3 instead includes an indicator if any SNAP issuance date corresponds to a Friday or Saturday. When controlling for weekend income or benefit receipt, models yield similar findings to our main results: namely, that shifting SNAP benefits later in the month results in an increase in household violence, on average, and this uptick in violence is not driven by weekend SNAP transfers or paydays. Results for child maltreatment are similar to those in Tables 2 and 3, and indicate that weekend benefit issuance does not play a substantial role in affecting a recipient's interactions with children. Notably, the coefficient on the interacted term in column 2 suggests that weekend paydays are correlated with higher levels of intimate partner violence, which may be explained both by the notion that partners spend more time together on the weekends and also by recent work showing that weekend SNAP disbursement dates drive alcohol purchases (Castellari et al., 2017).

These findings are consistent with recent work showing that the timing of non-SNAP income streams do not mitigate or exacerbate the SNAP cycle (Beatty et al., 2019). In particular, they support the above conclusions that issuing benefits later in the month provide more opportunities for abusive spouses or live-in partners to use violence as a bargaining mechanism, regardless of weekend benefit receipt.

Changes in Drug-Related Behavior

To more directly explore the changes observed in violent behavior, we now turn to estimates of the SNAP policy change on drug-related crimes. Recent work suggests that SNAP issuance is linked to alcohol purchases and drinking behavior (Castellari et al., 2017; Cotti, Gordanier, & Ozturk, 2015), as well as drug misuse (Allen et al., 2019).[36] While highly relevant, we are unable to observe alcohol-related crimes in our data. Therefore, we focus solely on drug crimes for this analysis, while acknowledging that these products may indeed be complements and that violence may be fueled by either drugs, alcohol, or a combination of both.

In Table A2, we replicate our main difference-in-RD results for crimes indicating drug possession, selling, or manufacturing. We find that staggered SNAP policies increase overall drug crimes by 8.3 percent, or approximately nine more drug crimes per day, and that effects are concentrated in non-residential areas.[37]

[36] Specifically, Allen et al. (2019) find that staggering welfare benefits reduces the likelihood of substance use events by 13 percent.

[37] Moreover, we estimate that these crimes not only shift to different parts of the month, but increase on net, which could suggest that allowing families to better consumption smooth could lead individuals to engage in more risky behavior and could be one driver of the net increase in violence.

Domestic Abuse Reporting

To the extent that staggered SNAP policies increase the number of days that households experience an income shock, it is possible that such policies change a victim's incentives to report violence. We address this in a few ways.[38] First, we consider differential effects by crimes that end in arrest. Since a victim is most likely to press charges against a perpetrator when they report the crime themselves, an increase in arrests may also correspond to an increase in reporting. However, if more instances of domestic abuse are occurring but these crimes do not end in arrest, it's likely that victims are experiencing more violence after benefit receipt but are fearful of potential backlash from sending a partner or spouse to jail. On the other hand, if crimes become more frequent or violent as a result of the policy change, it could increase the likelihood both of the crime being noticed by a third party and the likelihood of the perpetrator going to jail.

In Table 9, we show effects for domestic violence crimes by arrest indicator. Column 1 presents the baseline estimates from our main difference-in-RD specification. Columns 2 and 3 display separate difference-in-RD estimates for crimes ending in arrest and not ending in arrest, respectively.

Estimates in column 2 indicate a 7.0 percent increase in domestic abuse crimes ending in arrest. These effects for domestic abuse are driven entirely by increases in arrests for domestic battery, which lends support to the notion that giving benefits later in the month may both increase seriousness and detectability of domestic violence crimes. In particular, out of the nine additional domestic abuse crimes across the city of Chicago per day, estimates indicate approximately three more arrests for domestic abuse, which represents not only an increase in total arrests, but also in the proportion of domestic crimes ending in arrest.[39]

Although results in columns 1 and 2 reinforce the idea that the changes in SNAP distribution timing leads to more crimes and more arrests, in column 3 we find that staggered SNAP policies also increase the number of crimes that do not end in arrest by 4.4 percent, likely due to the fact that these crime types most frequently end without an arrest. Estimates for child maltreatment are positive across all columns, although effects are driven primarily by crimes not ending in arrest, suggesting that child victims are not more likely to report abuse after the policy change.

Overall, results in Tables 3 and 9 imply that changing a recipient's benefit date increases domestic violence crimes, and for serious crimes such as battery and child maltreatment, they are more likely to be noticed and reported. However, our findings suggest that the effects are not entirely driven by third-party reporting, since both reports for incidents occurring at a residence increase and arrests for domestic battery increase after the policy change, which implies a subsequent increase in victims choosing to press charges against a violent offender.

Taken with our findings in Figure A2, these results suggest something else: staggered SNAP policies could increase tension in the household, which leads to more violent behavior both in and outside of the home in the short run. This increase in violence leads to more visibility and seriousness of domestic violence, which increases the likelihood of reporting by both the victim and a third party. Despite the increase in violence, victims are less likely to press charges, indicating that estimated effects

[38] Ideally, to more directly address reporting bias, we could use data on 911 calls. However, these data are not available, even via a Freedom of Information Act Request, according to correspondence with the Chicago Office of Emergency Management and Communications, which states they "have no records in our database prior to March of 2015."

[39] On average, around 20 percent of reported domestic abuse crimes end in arrest.

Table 9. The effect of staggering SNAP benefits on domestic violence by arrest indicator, difference-in-RD estimates.

	Average Effect	Arrest	No Arrest
Domestic Abuse			
Staggered*Treated Year	0.0117**	0.0040*	0.0077*
	(0.0052)	(0.0023)	(0.0045)
Pre-Period Mean	0.170	0.057	0.175
N	715,757	715,757	715,757
Battery			
Staggered*Treated Year	0.0110***	0.0025*	0.0085***
	(0.0035)	(0.0015)	(0.0030)
Pre-Period Mean	0.092	0.035	0.095
N	887,540	887,540	887,540
Assault			
Staggered*Treated Year	0.0002	−0.0003	0.0005
	(0.0004)	(0.0002)	(0.0004)
Pre-Period Mean	0.003	0.002	0.003
N	2,646,258	2,646,258	2,646,258
Property			
Staggered*Treated Year	0.0006	−0.0001	0.0007
	(0.0008)	(0.0002)	(0.0007)
Pre-Period Mean	0.008	0.001	0.010
N	1,582,846	1,582,846	1,582,846
Threat			
Staggered*Treated Year	−0.0011	−0.0005*	−0.0006
	(0.0020)	(0.0003)	(0.0020)
Pre-Period Mean	0.051	0.001	0.057
N	1,599,206	1,599,206	1,599,206
Child Maltreatment			
Staggered*Treated Year	0.0012**	0.0001	0.0010*
	(0.0006)	(0.0003)	(0.0005)
Pre-Period Mean	0.004	0.002	0.004
N	2,548,097	2,548,097	2,548,097
One-Sided Bandwidth	Optimal	Optimal	Optimal

Notes: Estimates are based on reported crime data from the city of Chicago. Each coefficient is generated by a separate Census Tract-by-day regression of equation (2) using the listed crime type as the dependent variable and using data from all days. Column 1 contains results from a D-in-RD model where years 2008, 2009, 2011, and 2012 are used as the controls. Columns 2 and 3 report D-in-RD results by arrest indicator. "No Arrest" refers to the subset of crimes in which an assailant is not arrested. "Arrest" refers to crimes that end in arrest. Each regression includes Census Tract, year, day-of-month, and day-of-week fixed effects and fits the running variable linearly. Standard errors are clustered on the Census Tract level and reported in parentheses. We also report the mean of each outcome for the period before the policy change.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

are likely due to increases in violence, and not simply a result of increases in victim reporting.

Child Abuse Reporting

Across all of our analyses, we find consistent evidence that changing SNAP issuance timing affects child maltreatment. However, to the extent that children are less capable than adults of reporting violence against them, these estimates may not be picking up true effects of policy changes on child maltreatment. It could be the case that our average effects are disguising changes in physical abuse and neglect to

particularly vulnerable groups of children. To address these possibilities, we additionally analyze the effects of child maltreatment using data from the National Child Abuse and Neglect Data System (NCANDS), which is the most centralized and thorough child abuse reporting system to date. These data contain bimonthly, county-level data on child maltreatment, and contain detailed reports on child abuse and neglect, as well as information on characteristics of the child, household, and perpetrator. Although these data provide greater in-depth accounts of child maltreatment than the Chicago case-level data, they do not contain day-level reports, nor do they include more detailed location information.

In Figure 5, we test if either reports of child maltreatment or the number of child victims in Illinois increased as a result of the SNAP policy change.[40] Across all crime types, including abuse and neglect, we estimate large effects on child maltreatment for both reported and adjudicated crimes. These findings provide additional evidence that the effects of the state-level policy were not concentrated on Chicago alone, but impacted the entire state of Illinois.

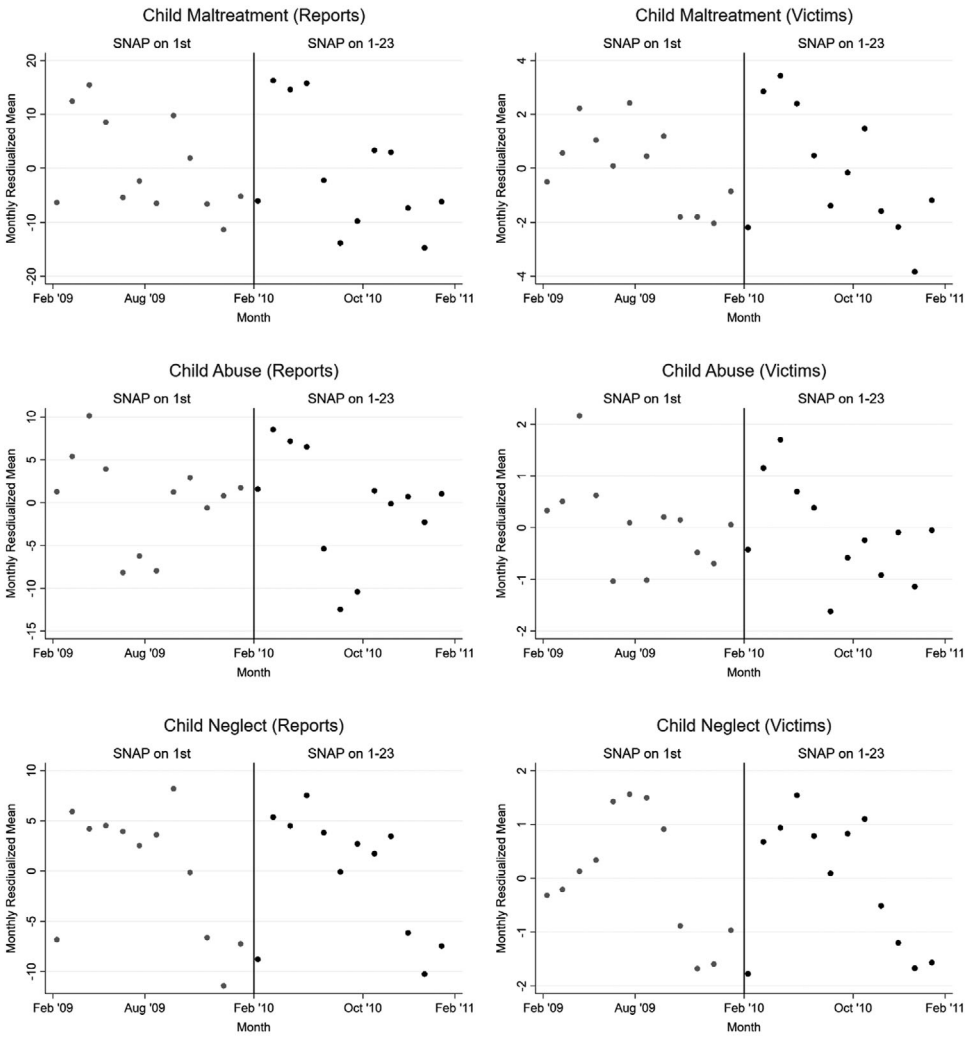
In Table A3, we extend this RD analysis to separately examine effects by victim and perpetrator characteristics. Overall, we find that effects are largest for more vulnerable groups of children, namely prior victims, children under the age of five, and females. We also find that effects are driven by maltreatment committed by parents.

DISCUSSION AND CONCLUSION

In this paper, we use incident-level crime data from the city of Chicago to study the effect of SNAP receipt on household violence. In particular, we estimate changes in intimate partner violence and child maltreatment due to a policy change that shifted SNAP benefit issuance from the first of the month to a range of dates later in the month. Our findings indicate that changes to SNAP issuance timing resulted in an increase in domestic abuse by 6.9 percent and an increase in child maltreatment by 30.0 percent, driven by increases in crimes in the last three weeks of the month. These estimates correspond to nine additional cases of domestic violence per day, or 3,400 per year, across the city of Chicago. Our results have important policy implications, as they suggest that in-kind transfers are a within-household motivator of domestic violence, and monthly disbursement of electronic benefits can have large, unintended consequences. Despite the fact that our findings speak to violence in the context of Chicago, domestic violence remains a prevalent problem that is relevant for all major cities, and our findings suggest that more research is needed to better understand the effects of policies on the prevalence of domestic violence.

These findings may be surprising, given that in previous work we find that changes in SNAP benefit timing have economically meaningful reductions on theft, and these changes are largest for older individuals and females (Carr & Packham, 2019). Moreover, theoretical models of household bargaining, as well as models that predict first-of-the-month effects for crime, suggest that recipients would react to such policy changes by shifting criminal behavior timing or restraining from household violence altogether due to the increased availability of resources at the end of the month (Bloch & Rao, 2002; Foley, 2011). However, our findings, combined with recent work on staggered TANF policies (e.g., Hsu, 2016), suggests that although staggering SNAP benefits can allow families to better consumption smooth, this reduction in scarcity at the end of the month does not lead to less conflict between

[40] "Child victim" includes a child for whom the state determined at least one maltreatment was substantiated or indicated, including a child who died of abuse or neglect.



Notes: Each figure plots month-level residualized means (accounting for bimonthly and county fixed effects) of each of the crimes listed. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. In the left column, data contains information on reported child maltreatment crimes, while the right column shows adjudicated child maltreatment crimes for which a victim was found. Bimonthly county-level crime data from February 1, 2009 to January 31, 2011 for the state of Illinois are from the National Child Abuse and Neglect Data System.

Figure 5. Child Maltreatment Reports and Victims (NCANDS).

partners. To the extent that staggered SNAP issuance is less likely to be aligned with disbursement of other types of income, such as paychecks or other government transfers, the schedule change may create more opportunities for conflict due to potential resource struggles.

While we are unable to speak to psychological motivators in this paper, other studies have suggested that a partner's household finances do serve as an economically significant incentive for domestic violence (Aizer, 2010; Bloch & Rao, 2002; Hsu, 2016; McMillan & Gartner, 1999). Additionally, individuals may face internal social

norms in which they do not see their behavior as abnormal, but simply as a way to assert control, when making choices about engaging in domestic violence. Another possible contributing mechanism behind these findings is that such behavior could be fueled by alcohol or drug consumption, which may depend on the cyclicality of household finances. Previous studies have shown that SNAP receipt affects alcohol purchases and drunk driving accidents, suggesting that consumption patterns of social drinkers are tied to benefit timing (Castellari et al., 2017; Cotti, Gordanier, & Ozturk, 2015). Indeed, although we are unable to directly study the effects on alcohol crimes, we do present some evidence that the policy change increased drug crimes, which suggests that this is one potential channel through which domestic abuse is increasing.

Overall, our results provide new evidence that there are adverse consequences when implementing staggered SNAP issuance schedules, and policymakers must consider trade-offs between consumption smoothing and within-household violence when making decisions about the timing of government transfer payments. In particular, while staggering SNAP payments has been shown to result in long-lasting reductions in theft, in this paper we show that such policies also lead to large, short-run increases in domestic abuse. The fact that these impacts are transitory thus implies that any change in disbursements leads to unintended short-term consequences, which should be of concern for policymakers. Importantly, splitting recipients' benefits into multiple smaller payments, such as biweekly increments, may be one way to both eliminate resource scarcity at the end of the benefit month as well as reduce abundance, and, therefore, incentives for household violence on benefit dates. Given that the costs of violent crime are approximately $87,000 per serious assault (Heaton, 2010), with much lower costs for larceny, we note that studying the channels of how in-kind transfers can affect criminal behavior and comparing the costs and benefits of such policies on various types of crime is an important avenue for future research.

JILLIAN B. CARR is an Assistant Professor of Economics in the Krannert School of Management at Purdue University, West Lafayette, IN 47907 (e-mail: carr56@purdue.edu).

ANALISA PACKHAM is an Assistant Professor of Economics at Vanderbilt University, Department of Economics, VU Station B #351819, 2301 Vanderbilt Place, Nashville, TN 37235 (e-mail: analisa.packham@vanderbilt.edu). She is a Faculty Research Fellow at the National Bureau of Economic Research (NBER).

ACKNOWLEDGMENTS

We thank Greg DeAngelo, Kendall Houghton, Ria Ivandic, Jason Lindo, Ben Hansen, Marianne Bitler, Marianne Page, participants at the 2017 Meetings of the Southern Economic Association, 2017 Association of Policy Analysis and Management meetings, and 2018 Western Economics Association International meetings, and seminar attendees at Miami University, Case Western Reserve University, University of California–Davis, Vanderbilt University, and University of Tennessee for useful feedback on work in progress. We note that we have no financial or material interests related to the research contained in this paper. The Chicago crime data used in this article are available online at https://data.cityofchicago.org/Public-Safety/Crimes-2001-to-present/ijzp-q8t2. This paper also uses confidential data from the National Child Abuse and Neglect Data System (NCANDS), which can be obtained through the application process described here: https://www.ndacan.cornell.edu/datasets/request-restricted-data.cfm.

REFERENCES

Aizer, A. (2010). The gender wage gap and domestic violence. American Economic Review, 100, 1847–1859.

Allen, L., Atwood, A., Young, S., Pauly, N., & Harrington, R. (2019). The impact of staggered benefit distribution on opioid use. Working Paper.

Anderberg, D., & Rainer, H. (2011). Domestic abuse: Instrumental violence and economic incentives. CESifo Working Paper No. 3673. CESifo Group Munich. Available at http://www.cesifo-group.de/DocDL/cesifo1_wp3673.pdf.

Beatty, T. K., Bitler, M. P., Cheng, X. H., & van der Werf, C. (2019). SNAP and paycheck cycles. Southern Economic Journal, 86, 18–48.

Bloch, F., & Rao, V. (2002). Terror as a bargaining instrument: A case study of dowry violence in rural India. American Economic Review, 92, 1029–1043.

Bobonis, G., Gonzalez-Brenes, M., & Castro, R. (2013). Public transfers and domestic violence: The roles of private information and spousal control. American Economic Journal: Economic Policy, 5, 179–205.

Bondurant, S. R. (2018). The role of educators in the reporting of child maltreatment: Causal effects of school attendance. Working Paper. Available at https://drive.google.com/file/d/0B_m7LOBEtfwpeVdDMmhSODROSzg/view.

Brown, D., & de Cao, E. (2017). The impact of unemployment on child maltreatment in the United States. Oxford, UK: University of Oxford, Department of Economics. Discussion Paper Series.

Bruich, G. A. (2014). The effect of SNAP benefits on expenditures: New evidence from scanner data and the November 2013 benefit cuts. Working Paper. Available at http://scholar.harvard.edu/files/bruich/files/bruich_2014b.pdf.

Burgess, M. (2014). How frequently do private businesses pay workers? Beyond the Numbers: Pay & Benefits, 3(11). U.S. Bureau of Labor Statistics.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2017). rdrobust: Software for regression discontinuity designs. The Stata Journal, 17, 372–404.

Carr, J. B., & Packham, A. (2019). SNAP benefits and crime: Evidence from changing disbursement schedules. Review of Economics and Statistics, 101, 310–325.

Castellari, E., Cotti, C., Gordanier, J. M., & Ozturk, O. D. (2017). Does the timing of food stamp distribution matter? A panel-data analysis of monthly purchasing patterns of U.S. households. Health Economics, 26, 1380–1393.

Castner, L., & Henke, J. (2011). Benefit redemption patterns in the Supplemental Nutrition Assistance Program. U.S. Department of Agriculture, Food and Nutrition Service, Office of Research and Analysis. Project officer: Anita Singh, Alexandria, VA.

Cicchetti, D., & Carlson, V. (1989). Child maltreatment: Theory and research on the causes and consequences of child abuse and neglect. New York, NY: Cambridge University Press.

Cotti, C., Gordanier, J. M., & Ozturk, O. D. (2015). Eat (and drink) better tonight: Food stamp benefit timing and drunk driving fatalities. Available at SSRN 2589553.

Cotti, C., Gordanier, J. M., & Ozturk, O. D. (2017). When does it count? The timing of food stamp receipt and educational performance. Available at SSRN 2992390.

Currie, J., & Spatz Widom, C. (2010). Long-term consequences of child abuse and neglect on adult economic well-being. Child Maltreatment, 15, 111–120.

Currie, J., & Tekin, E. (2012). Understanding the cycle: Childhood maltreatment and future crime. Journal of Human Resources, 47, 509–549.

Dugan, L., Nagin, D., & Rosenfeld, R. (1999). Explaining the decline in intimate partner homicide: The effect of changing domesticity, women's status and domestic violence resources. Homicide Studies, 3, 1–16.

Dugan, L., Nagin, D., & Rosenfeld, R. (2003). Exposure reduction or retaliation? The effects of domestic violence resources on intimate-partner homicide. Law & Society Review, 37, 169–198.

Ellsberg, M., Heise, L., Pena, R., Agurto, S., & Winkvist, A. (2001). Researching domestic violence against women: Methodological and ethical considerations. Studies in Family Planning, 32, 1–16.

Farmer, A., & Tiefenthaler, J. (1997). An economic analysis of domestic violence. Review of Social Economy, 55, 335–358.

Fletcher, J. (2009). Childhood mistreatment and adolescent and young adult depression. Social Science & Medicine, 68, 799–806.

Foley, C. (2011). Welfare payments and crime. Review of Economics and Statistics, 93, 97–112.

Goldin, J., Homonoff, T., & Meckel, K. (2016). Is there an nth of the month effect? The timing of SNAP issuance, food expenditures, and grocery prices. Working Paper. Retrieved October 21, 2019, from https://wagner.nyu.edu/files/faculty/publications/Goldin%2C%20Homonoff%2C%20Meckel%20-%20Nth%20of%20the%20Month.pdf.

Gregory, C. A., & Smith, T. A. (2019). Salience, food security, and SNAP receipt. Journal of Policy Analysis and Management, 38, 124–154.

Hamrick, K. S., & Andrews, M. (2016). SNAP participants' eating patterns over the benefit month: A time use perspective. PloS one, 11, e0158422.

Hastings, J., & Shapiro, J. M. (2018). How are SNAP benefits spent? Evidence from a retail panel. American Economic Review, 108, 3493–3540.

Hastings, J., & Washington, E. (2010). The first of the month effect: Consumer behavior and store responses. American Economic Journal: Economic Policy, 2, 142–162.

Hausman, C., & Rapson, D. S. (2018). Regression discontinuity in time: Considerations for empirical applications. Annual Review of Resource Economics, 10, 533–552.

Heaton, P. (2010). Hidden in plain sight: What cost-of-crime research can tell us about investing in police. Retrieved November 27, 2017, from https://www.rand.org/pubs/occasional_papers/OP279.html.

Hsu, L.-C. (2016). The timing of welfare payments and intimate partner violence. Economic Inquiry, 55, 1017–1031.

Huang, M., O'Riordan, M., Fitzenrider, E., McDavid, L., Cohen, A., & Robinson, S. (2011). Increased incidence of nonaccidental head trauma in infants associated with the economic recession. Journal of Neurosurgery: Pediatrics, 8, 171–176.

Illinois Attorney General. (2018). Violence against women: Illinois domestic violence act. Retrieved March 11, 2018, from http://www.illinoisattorneygeneral.gov/women/idva.html.

Kuhn, M. (2018). Cyclical food insecurity and electronic benefit transfer. Working Paper. Retrieved July 11, 2019, from https://static1.squarespace.com/static/5c79b3d29b8fe82f5cb96360/t/5c7c47fdfa0d60728a81d779/1551648769579/FoodStamps_WP.pdf.

Lindo, J. M., Hansen, B., & Schaller, J. (2018). Caution! Men not at work: Gender-specific labor market conditions and child maltreatment. Journal of Public Economics, 163, 77–98.

Manser, M., & Brown, M. (1980). Marriage and household decision-making: A bargaining analysis. International Economic Review, 21, 31–44.

McElroy, M., & Horney, M. (1981). Nash-bargained household decisions: Toward a generalization of the theory of demand. International Economic Review, 22, 333–349.

McMillan, H., Jamieson, E., & Walsh, C. (2003). Reported contact with child protection services among those reporting child physical and sexual abuse: Results from a community survey. Child Abuse & Neglect, 27, 1397–1408.

McMillan, R., & Gartner, R. (1999). When she brings home the bacon: Labor-force participation and the risk of spousal violence against women. Journal of Marriage and the Family, 61, 947.

Pollak, R. A. (2005). Bargaining power in marriage: Earnings, wage rates and household production. NBER Working Paper No. 11239. Cambridge, MA: National Bureau of Economic Research.

Shapiro, J. M. (2005). Is there a daily discount rate? Evidence from the food stamp nutrition cycle. Journal of Public Economics, 89, 303–325.

Stephens-Davidowitz, S. (2013). Unreported victims of an economic downturn. Working Paper. Retrieved February 25, 2018, from https://static1.squarespace.com/static/51d894bee4b01caf88ccb4f3/t/51e22f38e4b0502fe211fab7/1373777720363/childabusepaper13.pdf.

Stevenson, B., & Wolfers, J. (2006). Bargaining in the shadow of the law: Divorce laws and family distress. Quarterly Journal of Economics, 121, 267–288.

Stith, S., Liu, T., Davies, L., Boykin, E., Alder, M., Harris, J., Som, A., McPherson, M., & Dees, J. (2009). Risk factors in child maltreatment. A meta-analytic review of the literature. Aggression and Violent Behavior, 14, 13–29.

Swahn, M. H., Whitaker, D. J., Pippen, C. B., Leeb, R. T., Teplin, L. A., Abram, K. M., & McClelland, G. M. (2006). Concordance between self-reported maltreatment and court records of abuse or neglect among high-risk youths. American Journal of Public Health, 96, 1849–1853.

U.S. Department of Agriculture. (2017). Supplemental Nutrition Assistance Program state activity report fiscal year 2016. Retrieved November 26, 2018, from https://fns-prod.azureedge.net/sites/default/files/snap/FY16-State-Activity-Report.pdf.

U.S. Department of Health and Human Services. (2017). Child Maltreatment 2015. Accessed October 25, 2017, at https://www.acf.hhs.gov/sites/default/files/cb/cm2015.pdf.

U.S. Department of Justice. (2005). Family violence statistics. Retrieved February 23, 2018, from https://www.bjs.gov/content/pub/pdf/fvs02.pdf.

U.S. Department of Justice. (2014). Household poverty and nonfatal violent victimization, 2008–2012. Retrieved October 30, 2017, from https://www.bjs.gov/content/pub/pdf/hpnvv0812.pdf.

Waldfogel, J. (1998). The future of child protection: How to break the cycle of abuse and neglect. Cambridge, MA: Harvard University Press.

Watson, B., Guettabi, M., & Reimer, M. (2018). Universal cash and crime. Working Paper.

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, 200–213.

Wood, J. N., Medina, S. P., Feudtner, C., Luan, X., Localio, R., Fieldston, E. S., & Rubin, D. M. (2012). Local macroeconomic trends and hospital admissions for child abuse, 2000–2009. Pediatrics, 130, e358–e364.

Wright, R., Tekin, E., Topalli, V., McClellan, C., Dickinson, T., & Rosenfeld, R. (2017). Less cash, less crime: Evidence from the Electronic Benefit Transfer Program. Journal of Law and Economics, 60, 361–383.

APPENDIX

Table A1. The effect of staggering SNAP benefits on theft.

	Day of Month Range				
	Average Effect	Average Effect	1st of Month	Days 2–23	Days 24–31
SNAP Staggered	−0.0188**	−0.0653***	−0.2890*	−0.0857***	−0.1655***
	(0.0089)	(0.0197)	(0.1708)	(0.0302)	(0.0616)
Pre-Period Mean	0.662	0.634	0.739	0.640	0.597
N	236,463	38,674	1,800	28,117	8,757
One-Sided Bandwidth	1 Year	Optimal	Optimal	Optimal	Optimal

Notes: Estimates are based on reported crime data from the city of Chicago. Each coefficient is generated by a separate Census Tract-by-day regression of equation (1) using the listed crime type as the dependent variable and using data from all days (columns 1 and 2) or the ranges listed at the top of each column. Each regression includes Census Tract, year, day-of-month, and day-of-week fixed effects. Standard errors are clustered on the Census Tract level and reported in parentheses. We also report the mean of each outcome for the period before the policy change.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A2. The effect of staggering SNAP benefits on drug crimes by location type, difference-in-RD estimates.

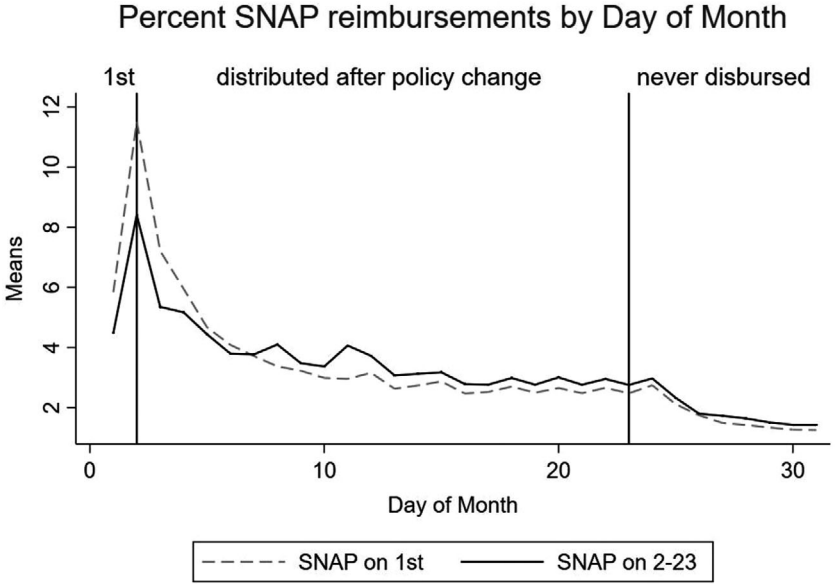
			Location	
	Average Effect	Average Effect	Non-Residence	Residence
Staggered*Treated Year	0.0113***	0.0094	0.0349**	−0.0065
	(0.0040)	(0.0075)	(0.0172)	(0.0066)
Pre-Period Mean	0.137	0.132	0.443	0.038
N	2,981,642	404,914	151,845	151,845
One-Sided Bandwidth	1 Year	Optimal	Optimal	Optimal

Notes: Estimates are based on reported crime data from the city of Chicago. Each coefficient is generated by a separate Census Tract-by-day regression of equation (2) using drug crimes as the dependent variable and using data from all days. Columns 1 and 2 contain results from a D-in-RD model where years 2008, 2009, 2011, and 2012 are used as the controls. Columns 3 and 4 report D-in-RD results by location type. "Non-Residence" refers to the subset of crimes occurring outside of a home. "Residence" refers to crimes occurring in a house, apartment, college dorm, or government housing. Each regression includes Census Tract, year, day-of-month, and day-of-week fixed effects. Standard errors are clustered on the Census Tract level and reported in parentheses. We also report the mean of drug crimes for the period before the policy change (February 1, 2009 to February 15, 2010).
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A3. The effect of staggering SNAP benefits on child maltreatment, by victim and perpetrator characteristics.

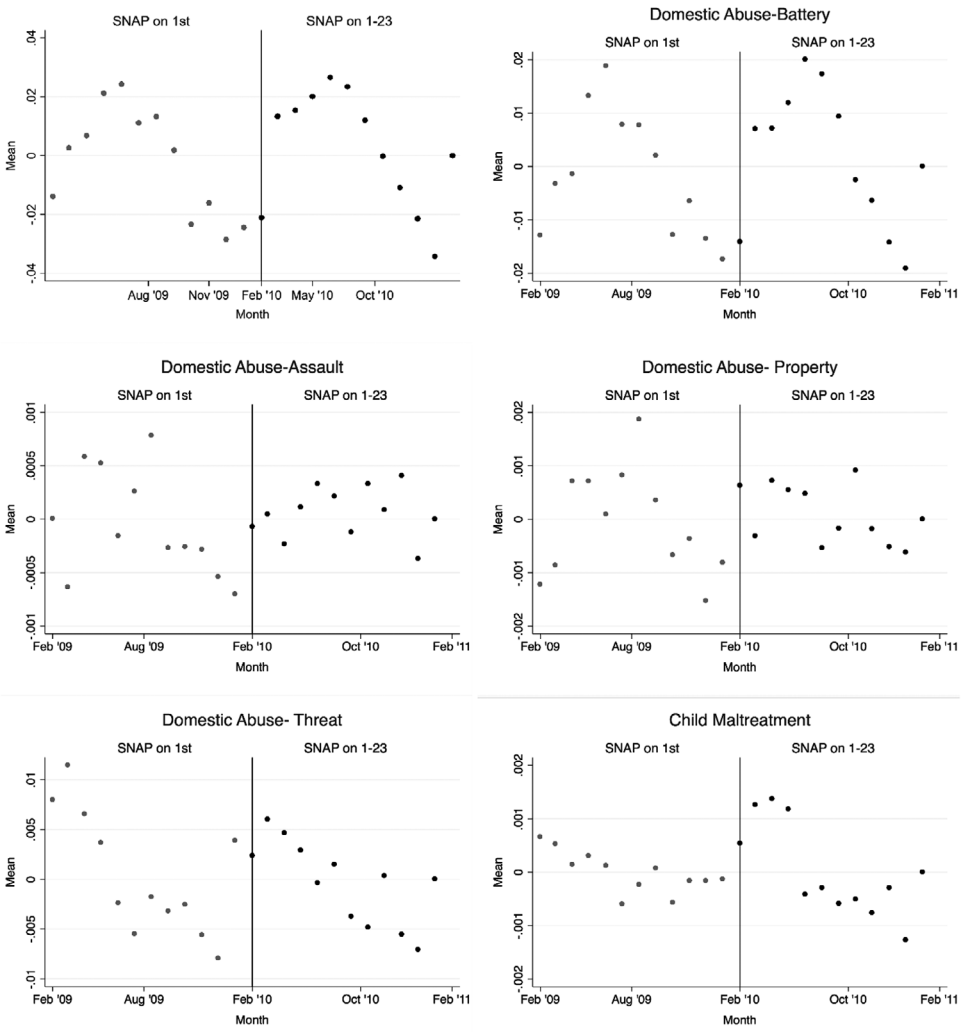
		Victim Characteristics				Perpetrator Characteristics	
	All Reports	Adjudicated Victims	Prior Victims	Less than 5 Years Old	Female	Parent	Partner of Parent
SNAP Staggered	22.7365***	4.3512	5.2061***	10.2471**	9.7491**	4.0182*	0.0619
	(8.0669)	(2.8712)	(1.7834)	(4.2162)	(3.5409)	(1.9966)	(0.4161)
Pre-Period Mean	135.181	22.048	30.713	56.042	67.513	15.717	1.716
N	1,304	1,304	1,304	1,304	1,304	1,304	1,304

Notes: Estimates are based on NCANDS data. Each coefficient is generated by a separate county-by-day regression of equation (1) using the listed crime type as the dependent variable and using data for all Illinois counties from February 1, 2009 to January 31, 2011. Each regression includes Census Tract, year, and bimonthly fixed effects. Standard errors are clustered on the county level and reported in parentheses. We also report the mean of each outcome for the period before the policy change.
*, **, and *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively.



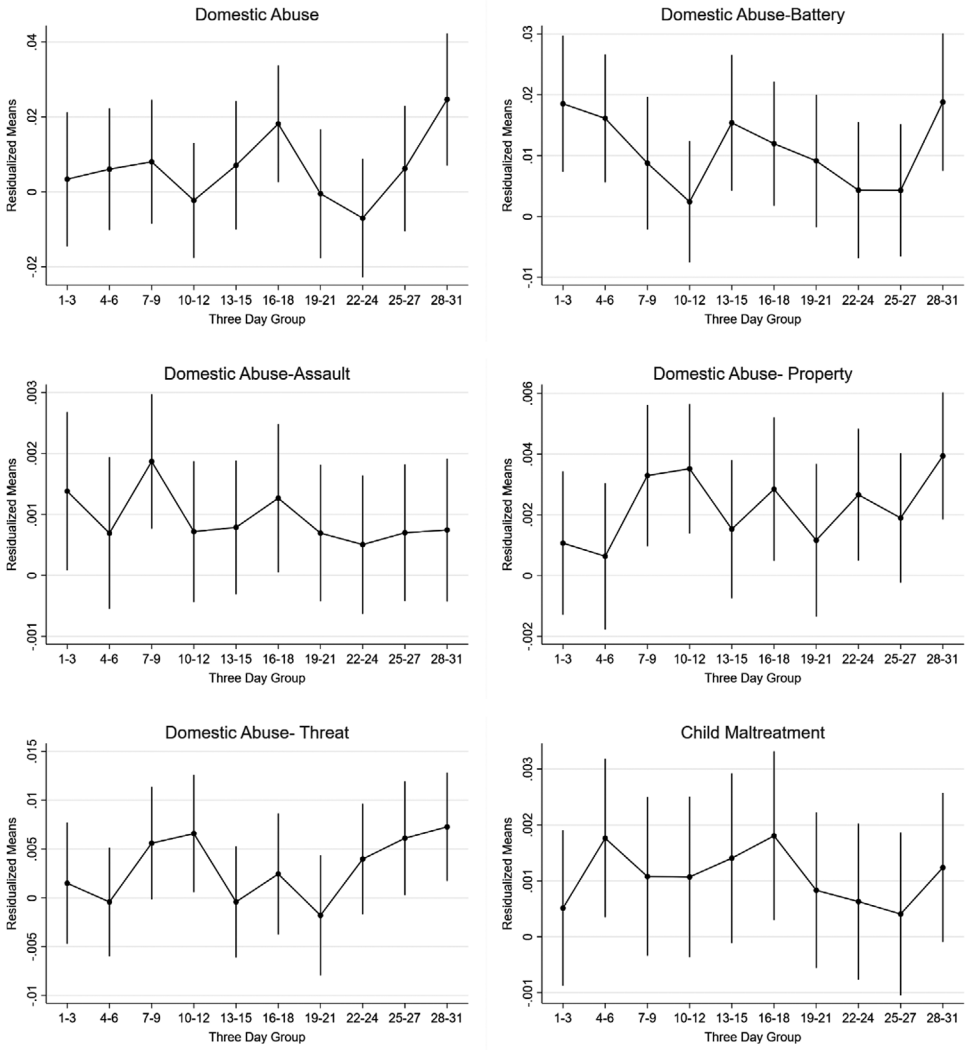
Notes: Authors' calculation based on daily SNAP redemptions data from the Illinois Department of Health and Human Services. The dotted line is calculated for February 2009 to January 2010. The solid line, indicating the post-period after the policy change, is calculated for February 2010 to February 2011.

Figure A1. Effect of Illinois SNAP Disbursement Change on SNAP Redemptions.



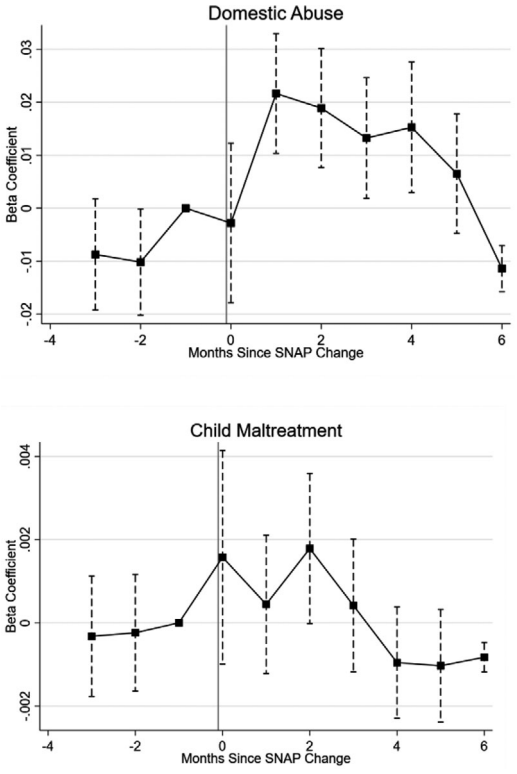
Notes: Each figure plots month-level means of residuals (after differencing out Census Tract, day-of-week, and day-of-month fixed effects) of each of the crimes listed. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. Crime data are from the city of Chicago from February 1, 2009 to January 31, 2011.

Figure A2. Effect of Illinois SNAP Disbursement Change on Domestic Violence.



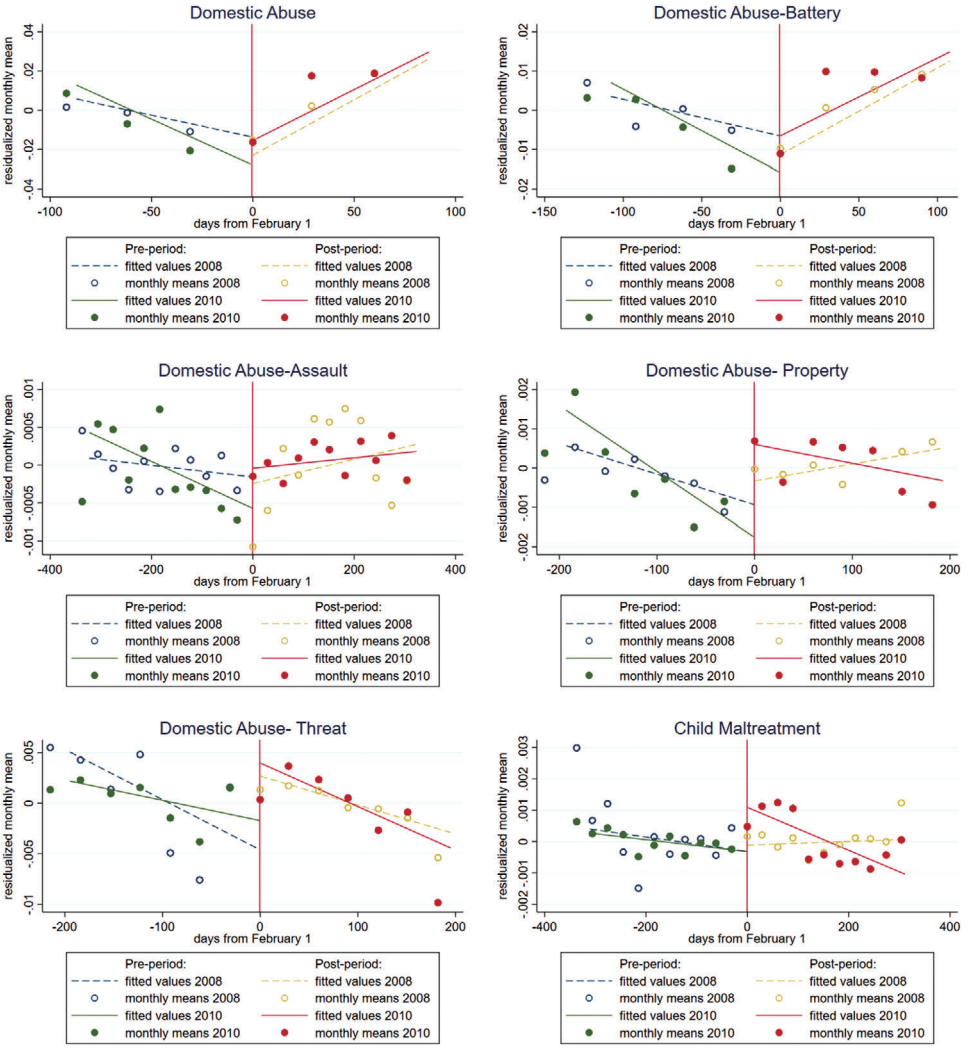
Notes: Each figure plots coefficients from equation (1) using three-day bins for each of the outcomes listed and using MSERD-optimal bandwidths. Crime data are from the city of Chicago from February 1, 2009 to January 31, 2011.

Figure A3. Effect of Illinois SNAP Disbursement Change on Domestic Violence, by Every Three Days Since Issuance.



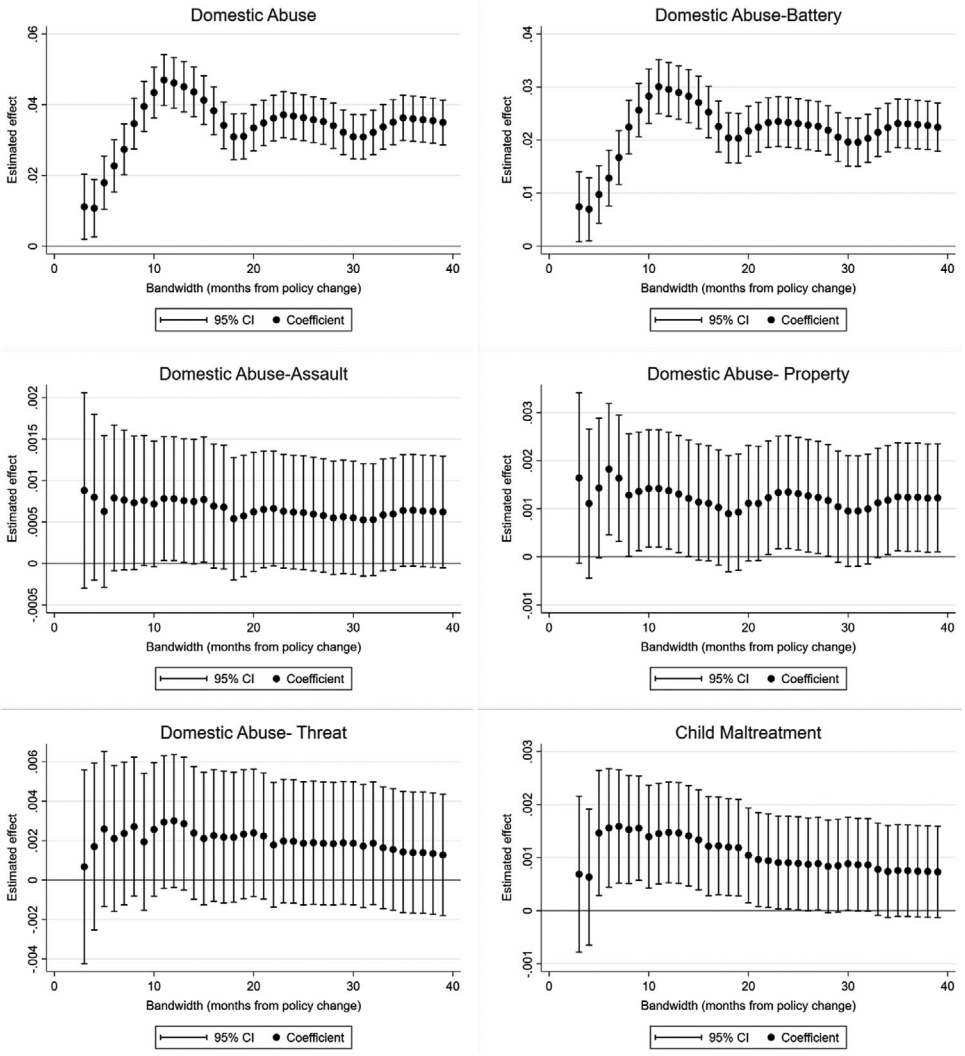
Notes: Each figure plots monthly event-study coefficients based on the equation: $y_{it} = \beta_0 + \beta_1 SNAPChange{*}HighSNAP_{it} + \gamma_i + \theta_y + \pi_d + \tau_m + \alpha_w + \eta_{it}$, where we analyze child maltreatment and domestic abuse incidents for a Census Tract i on date t. *SNAPChange* is a variable indicating whether a date falls after the February 2010 policy change. *HighSNAP* is an indicator variable taking the value of one if a Census Tract is in the top quartile of SNAP participation, according to the ACS. We drop Census Tracts in the 2nd and 3rd quartiles of the SNAP participation distribution so as to compare the highest and lowest SNAP-affected areas. We include year, day-of-week, day-of-month, month and Census Tract fixed effects γ_i, θ_y, π_d, τ_m, and α_w, respectively. Standard errors are clustered at the Census Tract-level. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. Crime data are from the city of Chicago from February 1, 2009 to January 31, 2011.

Figure A4. Effect of Illinois SNAP Disbursement Change on Domestic Violence, Difference-in-Differences Estimates Comparing Census Tracts by SNAP Participation Quartiles.



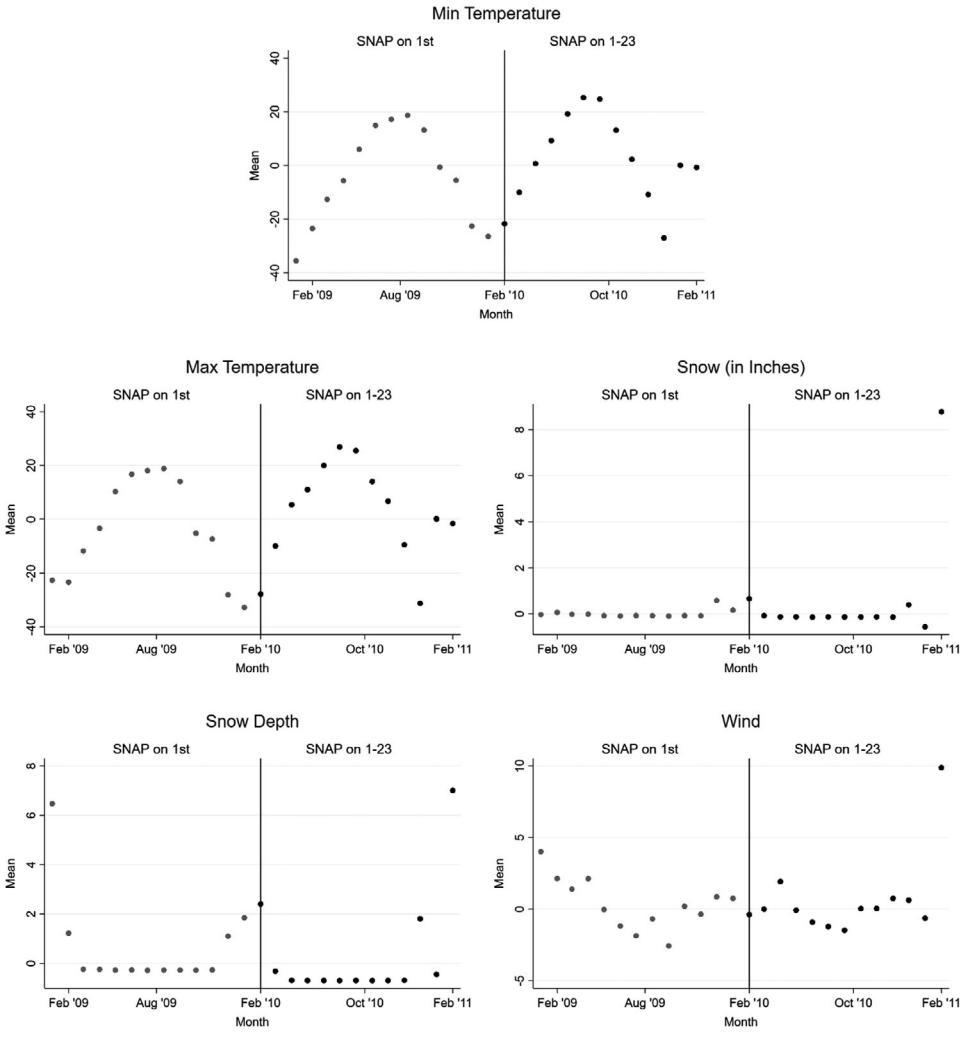
Notes: Each figure plots month-level means of residuals (after differencing out Census Tract, year, day-of-week, and day-of-month fixed effects) of each of the crimes listed for 2010 and control years, separately. The vertical line represents February 1. The dashed lines to the left and the right of the vertical line represent crime levels in the days leading up to and those after February 1, 2008, respectively. The solid lines represent crime levels in the days before and after February 1, 2010. Crime data are from the city of Chicago.

Figure A5. Effect of 2010 Illinois SNAP Disbursement Change on Domestic Violence Compared to 2008.
[Color figure can be viewed at wileyonlinelibrary.com]



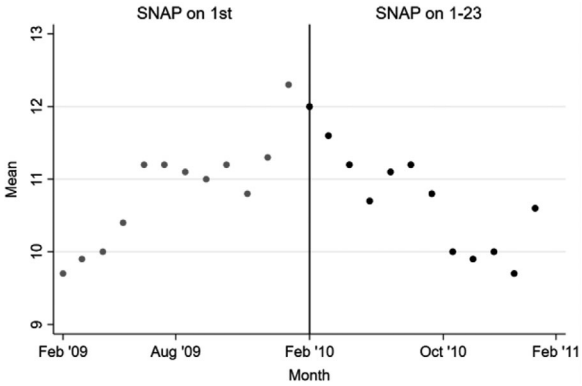
Notes: Each dot represents the coefficient of interest generated by a separate regression as specified by equation (1). The various bandwidths on which these regressions were performed are represented on the x-axis, such that the x-value indicates the bandwidth used on each side of the cutoff. We also report the 95 percent confidence interval of the coefficient. Reported crime data are from the city of Chicago.

Figure A6. Effect of Varying Bandwidth on RD Estimates.



Notes: Each figure plots month-level means of daily weather patterns in the city of Chicago. Daily weather data for Chicago are from the Global Historical Climatology Network and are based on temperature, precipitation, and average wind speeds from the Chicago O'Hare International Airport weather station.

Figure A7. Effect of Illinois SNAP Disbursement Change on Weather.



Notes: The figure plots month-level means of the monthly unemployment rate in Cook County. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. Monthly unemployment data are from the U.S. Bureau of Labor Statistics.

Figure A8. Effect of Illinois SNAP Disbursement Change on Unemployment.