DO INCOME SHOCKS AFFECT DOMESTIC VIOLENCE?

Jillian B. Carr [*]   Analisa Packham[†]

October 24, 2018

Abstract

This project examines how the timing of nutritional assistance payments affects instances of domestic violence, using a policy change in Illinois that altered SNAP benefit issuance timing to evaluate two arguments: (i) parents or partners use violence as a way to express frustration near the end of the month when resources are lean, and/or (ii) parents or partners use violence as a way to control the allocation of household resources shortly after an influx of benefits. Using crime-level data, we find that distributing SNAP benefits later in the month increases domestic abuse by 6.7% and child maltreatment by 32.5%.

JEL Classification: I38, I18, J18, K42

Keywords: SNAP Benefits, Domestic Violence, Income Shocks, Consumption Smoothing

1   Introduction

In 2015 over 680,000 children were victims of child maltreatment in the United States, and nearly 25% of these victims lived in households with reports of physical violence by an intimate partner (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 (Fletcher, 2009; Currie and Tekin, 2012; Currie and Spatz Widom, 2010). 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, Liu, Davies, Boykin, Alder, Harris, Som, McPherson, and Dees, 2009; U.S. Department of Justice, 2014).

In this paper, we investigate the impact of income shocks on domestic violence. Despite the well-established link between poverty and domestic violence, the theoretical relationship between *changes* in income 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 and Brown, 1980; McElroy and 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 (Pollak, 2005; Farmer and Tiefenthaler, 1997).[1] Moreover, if intimate partner violence is used as a way to relieve stress, and enters into an abuser's utility function directly, income receipt by resource-constrained households leads 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 and 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 and Rao, 2002; Bobonis, Gonzalez-Brenes, and Castro, 2013).

Depending on the underlying cause of domestic abuse, positive income shocks have the ability to increase or decrease violence. More specifically, changes in the timing of income receipt has the potential to affect domestic violence if benefits help to alleviate stress (as in the case of expressive violence) or if changing the

---

[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 contact curve has a positive slope, increases in relative income lead to fewer instances of violence.

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

This paper builds on a larger literature on income and domestic violence. Recent studies have documented that poor economic conditions and resource constraints play a large role in the incidence of family violence, likely due to economic stress. Negative income shocks, such as job loss, are associated with increases in child abuse, neglect, and domestic homicide (Lindo, Hansen, and Schaller, 2013; Brown and de Cao, 2017; Dugan, Nagin, and Rosenfeld, 1999). Similarly, economic downturns, like the Great Recession, result in greater instances of child trauma and abuse (Stephens-Davidowitz, 2013; Huang, O'Riordan, Fitzenrider, McDavid, Cohen, and Robinson, 2011; Wood, Medina, Feudtner, Luan, Localio, Fieldston, and Rubin, 2012).

And while there are many studies showing that resource constraints lead to more violence, evidence on the effects of positive income shocks on violence is mixed. For example, Aizer (2010) documents that historical reductions in the gender wage gap were responsible for a 9 percent decline in domestic violence from 1990-2003. Alternatively, Hsu (2016) finds that monthly cash transfers, such as Temporary Assistance for Needy Families (TANF) benefits, increase household violence right after receipt, driven by alcohol-related purchases.

Although it is well-established that nutrtional assistance timing has effects on consumption, a relatively small literature addresses these timing effects on other outcomes, including crime (Foley, 2011; Hsu, 2016; Carr and Packham, 2018) and test scores (Cotti, Gordanier, and Ozturk, 2017). This paper expands on this literature by analyzing the monthly cyclicality of abuse within households and measuring the effects of in-kind transfers on domestic violence.

The goal of this paper is to separate the effects of benefit receipt from other factors to measure the causal impact of nutritional assistance benefit transfers on domestic violence. To estimate these effects, we use detailed crime data from the city of Chicago containing cases of child maltreatment and domestic violence between partners 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, Heise, Pena, Agurto, and Winkvist, 2001). Nearly all of the recent studies on domestic violence rely on ex-post survey data, which include only self-reported incidents of abuse and grossly underestimate the severity of maltreatment (U.S. Department of Justice, 2005; Bondurant, 2018; Cicchetti and Carlson, 1989; Waldfogel, 1998; Swahn, Whitaker, Pippen, Leeb, Teplin, Abram, and McClelland, 2006; McMillan, Jamieson, and Walsh, 2003). 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.

In this paper, we exploit variation in nutritional benefit assistance timing to measure the causal effect of in-kind income shocks on household violence. Specifically, in 2010, Illinois changed its Supplemental Nutrition Assistance Program (SNAP) benefit issuance from a primarily first-of-the-month distribution to a staggered distribution over twelve 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 (Carr and Packham, 2018).[2] We take this as evidence that families do respond to the policy change, and in this work we determine whether there are signs that violent criminal activity *within* households responds to the change as well. Strikingly, we find that distributing benefits later in the month increases domestic abuse by 6.7 percent, and increases child maltreatment by 32.5 percent primarily by shifting more violent behavior to the middle of the month. Effects are largely driven by more serious types of abuse and violence committed outside the home.

This paper contributes to a growing literature on income shocks and domestic violence in five main ways. First, we conclude that an influx of benefits in the middle of the month increases violence between partners, and that this conflict induces negative spillovers to children. Second, 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. 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 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

---

[2]In Carr and Packham (2018) we use the maximum bandwidth available - 3 years. For a replication of those results using the smaller bandwidth of 12 months used in this paper, see Table A1. Estimates indicate that distributing SNAP benefits later in the month reduced theft by 10.3 percent, which is statistically similar to the 10.5 percent effect reported in Carr and Packham (2018).

on the advantages and disadvantages of in-kind transfers and on the efficacy of distributing SNAP benefits only once per month.

2 Background on Illinois SNAP Policy

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 receipt 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.

Previous work has documented that SNAP recipients view benefits as fungible, and issuance dates serve as a household income shock.[3] In particular, 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. Many families are forced to make difficult choices about how to keep food available and, given that over half of beneficiaries exhaust all benefits before the end of the month, many find themselves going without food (Wilde and Ranney, 2000; Shapiro, 2005; Castner and Henke, 2011; Hamrick and Andrews, 2016; Bruich, 2014; Hastings and Washington, 2010; Goldin, Homonoff, and Meckel, 2016; Kuhn, 2018). This consumption cycle has been shown to affect incentives for criminal behavior in low-income communities, which could extend to violence within the household (Carr and Packham, 2018; Foley, 2011).

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 days after issuance.[4] 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, Children (WIC) program. Therefore, by distributing

---

[3] Fraud presents an opportunity for families to convert their benefits to cash, but in 2010 fraud only accounted for 0.4% 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.

[4] See Figure A1, 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%.

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 1st 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.

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 income shocks.[5] On the other hand, if benefit issuance constitutes a household income shock, staggering benefits could create an extra day of potential struggle, as partners divy up the additional resources, which could increase instances of abuse.

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% of benefits were distributed on the 1st, while the remaining 30% 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.[6,7] To minimize the impact of moving benefit dates, the change occurred over a three month period, starting in February.[8] The Illinois Department of Human Services announced the change to the public 13 days before the policy change.

After February 2010, no recipient 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 monthly changes in SNAP distribution timing.

---

[5]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.

[6]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.

[7]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 5-10 percent of caseloads. See Goldin, Homonoff, and Meckel (2016) for more information on Illinois SNAP issuance dates.

[8]To account for this phase-in period, we estimate some specifications which drop these three months.

3 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.[9] 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-January 31, 2011, which contains data for one year before and one year after the SNAP policy change.[10,11] For placebo tests and bandwidth sensitivity tests, we expand our sample to include data from January 2007-June 2013. (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.[12]

One of the primary advantages of these data is the ability to pinpoint the location, date and time that the crime was reported.[13] For our main analyses, we use coordinates to geocode the location of each crime and create a Census Tract-by-day panel.

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 observational access, 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. Therefore, we supplement our analysis with data from the National Child Abuse and Neglect Data System

---

[9]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).

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

[11]Specifically, all MSERD-optimal bandwidths range from 87-323 days.

[12]In particular, estimates from our baseline model indicate that the SNAP policy change increased domestic abuse by 17.1 percent, which is statistically similar to the 23.9 percent estimate from Table 2 Column 1.

[13]While the National Child Abuse and Neglect 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.

(NCANDS), which contains detailed information on child maltreatment reports for all counties in Illinois, to test whether we observe changes in child maltreatment across many types of reporting channels.

Finally, we use annual, county-level data from the FBI Uniform Crime Reports, which contain counts of violence against families and children, as reported by local agencies. These data allow us to capture changes in crimes across all Illinois counties and compare these trends to other counties in the US in an effort to account for trends in domestic violence over time. By comparing areas both within and outside of Chicago to other areas across the country, we are able to analyze the effects of changing SNAP issuance schedules more broadly and observe their persistence.

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, and extend this analysis to separately estimate effects for domestic battery, assault, property damage, or threats made. In our definition of child maltreatment, we include cases of child abuse and neglect. 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, and/or contributing to criminal delinquency of a child/juvenile.[14]

To estimate differential effects of the policy change across Census Tracts, we additionally use data from the American Community Survey (ACS) on Census Tract-level SNAP participation and income for 2010. We use this year of data for two main reasons. First, since 2010 is the year of the policy change, we are able to use a snapshot of program participation the year the policy was enacted, which is the most relevant year for our analysis. Second, by excluding years after 2010, we eliminate the possibility that any findings are driven by differential Census Tract-level program take-up due to staggering SNAP benefits.

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 as well as labor market data from the Bureau of Labor Statistics.

Table 1 contains summary statistics on the Census Tract-by-day level for these crime data and a measure of SNAP participation. 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 2 incidents per year.[15]

---

[14]We do not consider domestic sexual offenses in these analyses, largely because of sample size and because we do not expect such crimes to respond as directly to changes in income disbursement.

[15]Child abuse is the most common child-specific offense, with each Census Tract experiencing 0.003 per day on average (or about 1 per year), or approximately 896 annual cases of physical child abuse city-wide.

4 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. In doing so, we operationalize a regression discontinuity model of the following form[16]:

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

where $crime_{it}$ represents that count of various domestic violence crimes in Census Tract i on date t, $SNAP\,staggered_t$ represents a dummy variable equal to one for dates 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 $f(days\,from\,cutoff_t)$ 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 cylicality in crime: π_d is a set of day-of-week fixed effects, γ_m is day-of-month fixed effects, ψ_y is year fixed effects. To account for variation across neighborhoods, we also control for Census Tract fixed effects, λ_i. 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.[17] For our preferred specifications, we estimate the above equation using ordinary least squares and allowing for a linear function of the running variable, although we additionally fit models where the running variable enters the equation quadratically and cubically, always allowing it to vary across the treatment threshold. Moreover, as mentioned in the previous section, while we use a bandwidth of two years (February 1, 2009-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 MSERD-optimal bandwidth, as suggested by Calonico, Cattaneo, Farrell, and Titiunik (2016), 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, where individuals received some benefits on their old disbursement date and some on their new disbursement date, we consider the full month to be treated in the following analyses, and normalize our running variable to be equal to zero

---

[16]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.

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

on February 1, 2010, which yeilds more conservative estimates than a mid-month treatment definition.[18] 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. 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.

5 Results

5.1 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 a two-year sample period, and control for cyclical trends (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.[19]

In Table 2 we formalize the relationships presented in Figure 1. 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 estimates from our full sample, February 1, 2009-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.

---

[18]We have also considered a model which drops February 2010 entirely as well as a model which drops February 2010 and March 2010 to account for phase-in. 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 our main results table. Therefore, in subsequent analysis we include data from February 2010 and March 2010 in an effort to produce more conservative estimates.

[19]We additionally provide figures showing plots for each outcomes based on the MSERD optimal bandwidth in Figure A2.

Effects for property crimes and 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 9 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-31st remained untreated, we separately examine effects on these date ranges. Columns 3-5 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.

As shown in Columns 3-5, we find that cases of domestic abuse fall by over 100 percent on the first of the month, but increase by 8.5 percent and 13.2 percent in days 2-23 and 24-31, respectively. Although the first-of-the-month decrease is relatively large, it is smaller than the total increase in crimes on days later in the month. In particular, in a given year, our estimates indicate a reduction in 231 first-of-the-month domestic violence crimes, but an increase in approximately 400 crimes committed on later dates. Estimates for domestic battery follow a similar trend, with large decreases at the beginning of the month followed by a 15.3 percent increase spanning days 2-23. Given that battery is the most serious domestic abuse offenses 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 and/or increasing the intensity of attacks during these weeks.

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 2-23. Altogether, these results imply that when families experience income 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 on the first of the month by the reduction of income on that date, these effects are not fully offset and overall levels of violence increase.

5.2 Differential Effects by Geography

Although our main results suggest that staggering SNAP benefits leads to changes in domestic violence, on average, these results could mask heterogeneous effects across households. Since changes to the SNAP program affect low-income families, for example, we may expect to see larger effects in lower income Census Tracts, or, similarly, in Census Tracts in which a larger percentage of the households are enrolled in the program. To explore this possibility, in Table 3 we split the Census Tracts at the median SNAP enrollment percentage (24.8%) and by income, and repeat the methodology described in the previous section.

In Table 3 Column 1 we report the results from the preferred specification based on the optimal bandwidth found in Column 2 of Table 2 for comparison. Across Columns 2-3, we replicate results from Column 1 (including all days of the month), limiting our samples to Census Tracts with a percentage of households enrolled in SNAP that is below the median, and those with a percentage of participants above the median, respectively. For nearly all outcomes, we find results similar to those found in Table 2.

As shown in Table 3, overall effects on domestic abuse are more concentrated in Census Tracts with a low proportion of SNAP enrollees, driven by cases of battery in these low enrollment areas. However, effects for all other domestic crimes are driven by an increase in crimes in high SNAP enrollment areas. In particular, we find that shifting benefits later in the month increases domestic battery by 9.9 percent in Census Tracts with high SNAP usage, as compared to a 15.0 percent increase in areas with low enrollment. Effects on child maltreatment are also concentrated in Census Tracts with more SNAP recipients; estimates indicate a 31.7 percent increase in child maltreatment in these areas.

Additionally, in Table 3 we show estimates for domestic violence across high- and low-income Census Tracts, as defined by the USDA.[20] Estimates for overall domestic abuse are positive and statistically significant only for the high-income Census Tracts and indicate that staggered SNAP policies increase family violence by 21.5 percent. These estimates are driven by similarly large increases in domestic battery (22.3 percent).[21] For other types domestic crimes, such as assault, property crime, theft, and child maltreatment, effects are concentrated in low income areas.

It is worthwhile to note that the locations in the dataset are those at which the crime is reported, not necessarily the addresses of the involved parties. It is possible that instances of physical violence are mostly reported by a third party in a location outside of the couple's home, and, when reported, partners are in public places. In Table 4, we explore this possibility by estimating effects by location type to address

---

[20]Specifically, the criteria for identifying a census tract as low income are from the Department of Treasury's New Markets Tax Credit (NMTC) program. This program defines a low-income census tract as any tract where: The tract's poverty rate is 20 percent or greater; or The tract's median family income is less than or equal to 80 percent of the state-wide median family income; or the tract is in a metropolitan area and has a median family income less than or equal to 80 percent of the metropolitan area's median family income.

[21]For a visual representation of the incidence of domestic abuse crimes by Census Tract income level, see Figure A3.

the idea that the increase in crimes is driven by reports outside of the home. In particular, in Columns 2 and 3 we separately show the effects of staggered SNAP policies on residential violence, which includes crimes occurring in a house, apartment, college dorm room, or government housing, and non-residential crimes, which include all other locations. Effects for all crime types are statistically significant only for non-residential locations, suggesting that either domestic violence crimes are more likely to be committed in public after the policy change, or that domestic violence crimes after the change are more noticeable, and are more likely to be reported by a third party. This could be especially relevant if the intensity or seriousness of the violence increases, or if the increase in frequency increases the likelihood of reporting by a first or third party outside of the home.[22] This implies that our findings may represent a lower bound if partners or spouses are unlikely to report crimes that occur at home.

Alternatively, these findings might suggest that staggered SNAP policies systematically lead to more reporting of crimes in public. In Section 6, we address to what degree our results could be driven by changes in monitoring behavior and provide additional explanations for these findings.

5.3   Robustness

In this section, we consider the extent to which the 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 product of bandwidth selection. To address these concerns, we explore various alternative specifications in this section and show that our average estimates are robust 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 Columns 3 and 4, we fit the days from the policy change (the running variable) quadratically and cubically, while still allowing the fit to vary on either side of the cutoff. Estimates in Column 3 indicate that shifting SNAP benefits later in the month increases domestic abuse by 10.4 percent, with a 12.5 percent increase in domestic battery. Estimates for child maltreatment are positive and statistically significant and indicate that the SNAP policy change led to large increases of 35.0 percent. All estimates but one in Column 4 are statistically insignificant and relatively imprecise, indicating that models using a cubic polynomial may overfit the data.

Second, since the crime data are discrete, we estimate a corresponding Poisson model and display results in Column 5. Notably, some Census Tracts may have no reported cases of child maltreatment or domestic

---

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

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 6 and 7, 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.

To test how sensitive our main results are to bandwidth selection, we replicate our preferred specification under a range of bandwidths. We test bandwidths from 3 months on either side to 39 months, and show our results for a linear fit in Figure 2. For all outcomes of domestic violence and child maltreatment, the estimated coefficient is stable across the different bandwidths and is nearly always statistically significant on the 5% level.

Given the cycliality in crime, in Table 6, we additionally test whether the discontinuity observed in crime levels after the policy change is a season regularity, or "February effect". To do so, we estimate Equation 1, assigning February 2008, 2009, 2011, and 2012 separately as treatment cutoff dates. None of the estimates in Columns 2-5 indicate a statistically significant increase in domestic abuse or child maltreatment, which implies that our findings are not a result of monthly fluctuations in crime rates.[23],[24] Similarly, when we include month fixed effects into our main specification, estimates indicate a statistically significant increase in domestic abuse by 36.4 percent and an increase in child maltreatment by 17.5 percent.[25]

Furthermore, we conduct permutation inference using placebo 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 2007-2010, and assign it as a treatment cutoff date, without replacement.[26] 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

---

[23]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 and/or reporting of domestic abuse during the Great Recession.

[24]An alternative approach to performing this test would be to perform difference-in-regression discontinuity (D-i-RD) analysis. In this methodology, the researcher introduces observations for untreated units (often geographic or temporal) for which the running variable (or an analog) is observed. This model includes all of variables in a traditional RD, but adds interactions of each with an indicator for whether the observation is from a treated or untreated unit. Appendix Table A2 contains D-i-RD results using 2008, 2009, 2011 and 2012 as control years, and the results are consistent with the results in Table 6. It also contains a difference-in-difference-in-regression discontinuity model where an additional geographic difference is also introduced. These results generally do not indicate that there are differences between these more-treated geographies and less-treated ones. This is unsurprising given the scope for geographic spillovers discussed in the previous section. For more details on these models, including the full lists of variables used in an example, see Carr and Doleac (2017).

[25]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 2 months, and controlling for month fixed effects in this context would be inappropriate.

[26]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.

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 less than the reported estimates for domestic abuse and child maltreatment, respectively, which provides additional support for the idea that the policy change is driving the reported results.

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.

We note that, as shown in Figure A4, all of these weather variables are smooth across the treatment threshold. Alternatively, we could add weather controls to our preferred models. When we do, estimates for domestic abuse and child maltreatment indicate an effect of 4.7 percent and 30.0 percent, respectively, and are statistically similar at the 99% level to our preferred estimates in Table 2 Column 2. We additionally show that our results are not being driven by sharp changes in labor market conditions, including unemployment rates, using data from the Bureau of Labor Statistics in Figure A5.

5.4   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 couples 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. As shown in Figure 1, 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 A6 and Table A3 we use annual, county-level data from the FBI Uniform Crime Reports (UCR) to analyze the changes in domestic abuse crimes in Illinois as compared to other states. UCR data classifies domestic violence crimes as "offenses against family and children." The advantage of these data is that we are able to capture changes in crimes across all Illinois counties and compare these trends to other counties in the US in an effort to account for trends in domestic violence over time. In focusing on cities outside of Chicago, we are able to get a more comprehensive picture of the effects of such policy changes.

Figure A6 shows a striking trend in Illinois domestic violence crimes over time; in particular, crimes increased in 2010 then fell to 2009 levels before decreasing even more dramatically in the following years. In

14

comparison, domestic violence crimes across the US remained relatively unchanged.

We formalize this relationship using a difference-in-differences approach to compare crimes in Illinois counties and other US counties over time, and present these estimates in Table A3. Specifically, we estimate the following model:

$$DV_{ct} = \beta_0 + \beta_1 SNAP\ staggered_c * post_t + \Theta X_{ct} + \lambda_c + \alpha_t + u_{ct} \tag{2}$$

where DV_{ct} is the count of domestic violence crimes in a county c in year t, $SNAP\ staggered_c * post_t$ is an indicator variable that takes a value of one for all Illinois counties after 2009 and zero otherwise, λ_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. All analyses allow errors to be correlated within counties over time when constructing standard-error estimates.

Difference-in-differences estimates are shown in Table A3. Estimates in Columns 1-3 indicate domestic violence crimes in Illinois counties did not increase, on average, relative to other counties in the four years after the policy change. To more directly estimate how these effects vary differentially over time, Columns 4-6 display estimates for lagged indicator variables for each year after the policy change. Column 7 replicates Column 5 additionally controlling for a leading indicator variable to provide additional support that the trends in crimes in Illinois counties and other US counties were not diverging prior to the Illinois policy change. Mirroring findings in our main analysis, effects for the first year of the policy change are positive and statistically significant and indicate an increase in domestic violence crimes of 7.0 percent, with larger effects for urban counties (7.9 percent).[27] Estimates 1-3 years after the policy change are statistically insignificant at the 95% level, although we do estimate similar increases in domestic violence 2 years after the policy change at the 90% level. Overall, these findings show that the policy change had immediate effects that phased out 2-4 years later.

This evidence points to two arguments explaining why domestic violence crimes spike after initiating a staggered SNAP policy but fall sharply 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.

---

[27]Our findings are qualitatively similar when calculating rates of domestic violence, accounting for county population.

6 Alternative Explanations

6.1 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 7.[28] 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 (like 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 7, we display estimates from Equation 1, accounting for SNAP distribution on Fridays and Saturdays. When controlling for weekend 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. Increases in domestic abuse in the latter part of the month are partially offset by decreases on the first, and this uptick in violence is not driven by weekend SNAP transfers. Results for child maltreatment are similar to Table 2, and indicate that weekend benefit issuance does not play a substantial role in affecting a recipient's interactions with children. These findings support the 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 if households receive benefits on the weekend.

6.2 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. 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 A4 we replicate our main results for crimes indicating drug possession, selling or manufacturing. Similar to Table 2, we find that staggered SNAP policies increase overall drug crimes by 21.5 percent, or

---

[28]We use these dates due to the fact that over 36 percent of American businesses (and 72.9 percent of businesses with over 1,000 employees) have a biweekly pay schedule (Burgess, 2014).

61 more drug crimes per day, with effects concentrated in the middle of the month. Therefore, there is some evidence to support the idea that on days when recipients receive benefits, they engage in more risky behavior, which could lead to more violence.

6.3   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. 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 8, we show effects for domestic violence crimes by arrest indicator. For domestic abuse, effects are stronger for crimes in which no arrest is made at the time of report. Estimates also indicate that staggered SNAP policies increase domestic abuse crimes that end in arrest by 9.5 percent; however, these effects are not statistically significant at conventional levels (p-value = 0.108).

Effects on crimes ending in arrest are driven almost entirely by cases of 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 8 additional battery crimes across the city of Chicago per day, estimates indicate 2.3 more arrests for domestic battery, which represents not only an increase in total arrests, but also in the proportion of battery crimes ending in arrest.[29]

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 5.9 percent, likely due to the fact that these crime types most frequently end without an arrest. This is true for both less physical crimes like threats and property crimes (6.4 and 24.3 percent, respectively), but also for more serious crimes like assault and battery (6.4 and 50.0 percent, respectively). Overall, results in Tables 4 and 8 imply that changing a recipient's benefit date increases domestic violence crimes, and these crimes are more likely to be noticed and reported outside the home. However, results indicate that the effects are not entirely driven by third-party reporting, since arrests for domestic battery also increase after the policy change, which implies a subsequent increase in

---

[29]On average, around 25 percent of domestic battery crimes end in arrest.

victims choosing to press charges against a violent offender.

Taken with our findings in Figure 1, 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 are likely due to increases in violence, and not increases in reporting by the victim.

6.4 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. Or, it could be the case that our average effects are disguising changes in maltreatment 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, neglect, and sexual abuse, including 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 4, 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.[30] Across all crime types, including abuse and neglect, we estimate large effects on child maltreatment.

In Table A5 we extend this analysis to separately examine effects by victim and perpetrator character-istics. 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. Despite the fact that we find no effects on the number of adjudicated cases ($p = 0.14$), we note that the increase in the number of victims as a proportion of all reports (19.2%) after the policy change is larger than the mean prior to the change (16.3%), which provides some additional evidence that our main results are not driven solely by increases in reporting.

---

[30]"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.

7   Discussion

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 6.7-23.9 percent and an increase in child maltreatment by 37.5-50.0 percent, driven by increases in crimes in the last three weeks of the month. These estimates correspond to 9 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 income shocks are a within-household motivator of domestic violence, and monthly disbursement of electronic benefits can have large, unintended consequences.

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, female individuals (Carr and Packham, 2018). Moreover, theoretical models of household bargaining as well as models that predict first-of-the-month effects for crime that 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 and Rao, 2002; Foley, 2011; Hsu, 2016). Contrary to standard economic theory, we find 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 partners. Therefore, our results inform how policy can affect within-household decision-making.

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 as resource struggles could occur more frequently. 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 (Bloch and Rao, 2002; Hsu, 2016; Aizer, 2010; McMillan and Gartner, 1999). Additionally, individuals may face internal social norms in which they do not see their behavior as abnormal, but simply 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 dependence on alcohol or drugs, 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 (Cotti, Gordanier, and Ozturk, 2015; Castellari, Cotti, Gordanier, and Ozturk, 2016). 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 by 21.5 percent, which suggests that this is one potential channel through which domestic abuse is increasing.

An alternative explanation for why we estimate local increases in domestic violence is that families were already consumption smoothing well and not experiencing scarcity; in this scenario, the introduction of a staggered disbursement only temporarily changes budgeting calculations, which may lead to additional frustration in the short run, while long run levels of domestic violence remain unchanged, or even decrease. Given that we find large increases in battery in the few months following the policy change, with crime levels decreasing 1-2 years later, we find some evidence to support this hypothesis. Since staggering SNAP benefits not only increases reports, but also arrests for domestic battery, it could be the case that shifting income shocks later in the month leads to incapacitation of more violent offenders in the short run. Therefore, it's possible that the long-term benefits of staggered SNAP policies outweigh short-run spikes in domestic violence. Given that the costs of violent crime are approximately $87,000 per serious assault (Heaton, 2010), these tradeoffs are important to consider in future policy discussions of the timing of government transfers

One important implication of our findings is that domestic violence is as responsive to in-kind transfers as other methods of income. New research suggests that this may not be the case for low-income individuals, who instead mentally earmark these funds for groceries (Hastings and Shapiro, 2017). However, our findings suggest that households do consider these benefits as fungible. Combining these findings with those that document that staggering benefits reduces theft, 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.

References

Aizer, A. (2010): "The Gender Wage Gap and Domestic Violence," *American Economic Review*, 100(4), 1847–1859.

Anderberg, D., and H. Rainer (2011): "Domestic Abuse: Instrumental Violence and Economic Incentives," Working Paper 3673, CESifo.

Bloch, F., and V. Rao (2002): "Terror as a Bargaining Instrument: A Case Study of Dowry Violence in Rural India," *The American Economic Review*, 92(4), 1029–1043.

Bobonis, G., M. Gonzalez-Brenes, and R. Castro (2013): "Public Transfers and Domestic Violence: The Roles of Private Information and Spousal Control," *American Economic Journal: Economic Policy*, 5(1), 179–205.

Bondurant, S. R. (2018): "The Role of Educators in the Reporting of Child Maltreatment: Causal Effects of School Attendance," *Mimeo*.

Brown, D., and E. de Cao (2017): "The Impact of Unemployment on Child Maltreatment in the United States," *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," *Harvard University. Mimeograph, September*.

Burgess, M. (2014): "How Frequently Do Private Businesses Pay Workers?," *Beyond the Numbers: Pay & Benefits*, 3(11).

Calonico, S., M. D. Cattaneo, M. H. Farrell, and R. Titiunik (2016): "rdrobust: Software for Regression Discontinuity Designs," Discussion paper, working paper, University of Michigan.

Carr, J. B., and J. L. Doleac (2017): "Keep the Kids Inside? Juvenile Curfews and Urban Gun Violence," *Working Paper*.

Carr, J. B., and A. Packham (2018): "SNAP Benefits and Crime: Evidence from Changing Disbursement Schedules," *forthcoming in Review of Economics and Statistics*.

Castellari, E., C. Cotti, J. M. Gordanier, and O. D. Ozturk (2016): "Does the Timing of Food Stamp Distribution Matter? A Panel-Data Analysis of Monthly Purchasing Patterns of US Households," *Forthcoming, Health Economics*.

Castner, L., and J. Henke (2011): "Benefit Redemption Patterns in the Supplemental Nutrition Assistance Program," Discussion paper, Mathematica Policy Research.

Cicchetti, D., and V. Carlson (1989): *Child Maltreatment: Theory and Research on the Causes and Consequences of Child Abuse and Neglect*. New York: Cambridge University Press.

Cotti, C., J. M. Gordanier, and O. D. Ozturk (2015): "Eat (and Drink) Better Tonight: Food Stamp Benefit Timing and Drunk Driving Fatalities," *Available at SSRN 2589553*.

——— (2017): "When Does it Count? The Timing of Food Stamp Receipt and Educational Performance," *Available at SSRN 2992390*.

Currie, J., and C. Spatz Widom (2010): "Long-Term Consequences of Child Abuse and Neglect on Adult Economic Well-Being," *Child Maltreatment*, 15(2), 111–120.

Currie, J., and E. Tekin (2012): "Understanding the Cycle: Childhood Maltreatment and Future Crime," *Journal of Human Resources*, 47(2), 509–549.

Dugan, L., D. Nagin, and R. Rosenfeld (1999): "Explaining the Decline in Intimate Partner Homicide: The Effect of Changing Domesticity, Women's Status and Domestic Violence Resources," *Homicide Studies*, 3(3), 1–16.

Ellsberg, M., L. Heise, R. Pena, S. Agurto, and A. Winkvist (2001): "Researching Domestic Violence against Women: Methodological and Ethical Considerations," *Studies in Family Planning*, 32(1), 1–16.

Farmer, A., and J. Tiefenthaler (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(1), 799–806.

Foley, C. (2011): "Welfare Payments and Crime," *Review of Economics and Statistics*, 93(1), 97–112.

Goldin, J., T. Homonoff, and K. Meckel (2016): "Is there an Nth of the Month Effect? The Timing of SNAP Issuance, Food Expenditures, and Grocery Prices," *Working Paper*.

Hamrick, K. S., and M. Andrews (2016): "SNAP Participants' Eating Patterns over the Benefit Month: A Time Use Perspective," *PloS one*, 11(7), e0158422.

Hastings, J., and E. Washington (2010): "The First of the Month Effect: Consumer Behavior and Store Responses," *American Economic Journal: Economic Policy*, 2(2), 142–162.

Hastings, J. S., and J. M. Shapiro (2017): "How Are SNAP Benefits Spent? Evidence from a Retail Panel," Working Paper 23112, National Bureau of Economic Research.

Heaton, P. (2010): "Hidden in Plain Sight: What Cost-of-Crime Research Can Tell Us About Investing in Police," Accessed 27-November-2017 at `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(2), 1017–1031.

Huang, M., M. O'Riordan, E. Fitzenrider, L. McDavid, A. Cohen, and S. Robinson (2011): "Increased Incidence of Nonaccidental Head Trauma in Infants Associated with the Economic Recession," *Journal of Neurosurgery: Pediatrics*, 8(2), 171–176.

Illinois Attorney General (2018): "Violence Against Women: Illinois Domestic Violence Act," Accessed 11-March-2018 at `http://www.illinoisattorneygeneral.gov/women/idva.html`.

Kuhn, M. (2018): "Cyclical Food Insecurity and Electronic Benefit Transfer," *Mimeo*.

Lindo, J. M., B. Hansen, and J. Schaller (2013): "Caution! Men Not at Work: Gender-Specific Labor Market Conditions and Child Maltreatment," Working Paper 18994, National Bureau of Economic Research.

Manser, M., and M. Brown (1980): "Marriage and Household Decision-Making: A Bargaining Analysis," *International Economic Review*, 21(1), 31–44.

McElroy, M., and M. Horney (1981): "Nash-Bargained Household Decisions: Toward a Generalization of the Theory of Demand," *International Economic Review*, 22(2), 333–349.

McMillan, H., E. Jamieson, and C. Walsh (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., and R. Gartner (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(4), 947.

Pollak, R. A. (2005): "Bargaining Power in Marriage: Earnings, Wage Rates and Household Production," *NBER Working Paper No. 11239*.

Shapiro, J. M. (2005): "Is There a Daily Discount Rate? Evidence from the Food Stamp Nutrition Cycle," *Journal of Public Economics*, 89(2), 303–325.

Stephens-Davidowitz, S. (2013): "Unreported Victims of an Economic Downturn," *Mimeo*.

Stith, S., T. Liu, L. Davies, E. Boykin, M. Alder, J. Harris, A. Som, M. McPherson, and J. Dees (2009): "Risk Factors in Child Maltreatment: A Meta-Analytic Review of the Literature," *Aggression and Violent Behavior*, 14(1), 13–29.

Swahn, M. H., D. J. Whitaker, C. B. Pippen, R. T. Leeb, L. A. Teplin, K. M. Abram, and G. M. McClelland (2006): "Concordance between Self-reported Maltreatment and Court Records of Abuse or Neglect Among High-risk Youths," *American Journal of Public Health*, 96(10), 1849–1853.

U.S. Department of Agriculture (2017): "Supplemental Nutrition Assistance Program State Activity Report Fiscal Year 2016," Accessed 26-November-2018 at 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 25-October-2017 at https://www.acf.hhs.gov/sites/default/files/cb/cm2015.pdf.

U.S. Department of Justice (2005): "Family Violence Statistics," Accessed 23-February-2018 at https://www.bjs.gov/content/pub/pdf/fvs02.pdf.

——— (2014): "Household Poverty and Nonfatal Violent Victimization, 2008-2012," Accessed 30-October-2017 at 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., M. Guettabi, and M. Reimer (2018): "Universal Cash and Crime," *Mimeo*.

Wilde, P. E., and C. K. Ranney (2000): "The Monthly Food Stamp Cycle: Shooping Frequency and Food Intake Decisions in an Endogenous Switching Regression Framework," *American Journal of Agricultural Economics*, 82(1), 200–213.

Wood, J. N., S. P. Medina, C. Feudtner, X. Luan, R. Localio, E. S. Fieldston, and D. M. Rubin (2012): "Local Macroeconomic Trends and Hospital Admissions for Child Abuse, 2000-2009," *Pediatrics*, 130(2), e358–e364.

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
Percent Household on SNAP (2010)	0.171	0.143

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 2009- February 2011. SNAP enrollment data are from the American Communities Survey.

Table 2: The Effect of Staggering SNAP Benefits on Domestic Violence

| | Average Effect | Average Effect | Day of Month Range | | |
			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	597140	143150	4090	104704	34356
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	597140	177506	5726	130062	41718
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	597140	529246	17178	383642	128426
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	597140	316566	10634	226586	79346
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	597140	319838	10634	229858	79346
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	597140	509614	17178	366464	125972
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) within the bandwidth.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 3: Neighborhood Subgroups

		SNAP Enrollment		Income	
	Average Effect	Low	High	Low	High
Domestic Abuse					
SNAP Staggered	0.0114**	0.0142***	0.0087	0.0090	0.0157***
	(0.0048)	(0.0049)	(0.0083)	(0.0069)	(0.0054)
Pre-Period Mean	0.170	0.076	0.263	0.226	0.073
N	143150	71575	71575	90825	52325
Battery					
SNAP Staggered	0.0101***	0.0057**	0.0144**	0.0114**	0.0078**
	(0.0031)	(0.0028)	(0.0056)	(0.0046)	(0.0033)
Pre-Period Mean	0.092	0.038	0.146	0.125	0.035
N	177506	88753	88753	112623	64883
Assault					
SNAP Staggered	0.0007*	0.0002	0.0013*	0.0013**	-0.0001
	(0.0004)	(0.0003)	(0.0007)	(0.0006)	(0.0003)
Pre-Period Mean	0.003	0.001	0.005	0.004	0.001
N	529246	264623	264623	335793	193453
Property					
SNAP Staggered	0.0018**	-0.0002	0.0038***	0.0026***	0.0003
	(0.0007)	(0.0006)	(0.0012)	(0.0010)	(0.0007)
Pre-Period Mean	0.008	0.003	0.013	0.011	0.002
N	316566	158283	158283	200853	115713
Threat					
SNAP Staggered	0.0029	-0.0014	0.0073**	0.0059**	-0.0021
	(0.0019)	(0.0022)	(0.0030)	(0.0025)	(0.0027)
Pre-Period Mean	0.051	0.037	0.064	0.058	0.038
N	319838	159919	159919	202929	116909
Child Maltreatment					
SNAP Staggered	0.0013***	0.0008	0.0019**	0.0018**	0.0005
	(0.0005)	(0.0005)	(0.0009)	(0.0007)	(0.0005)
Pre-Period Mean	0.004	0.002	0.006	0.005	0.002
One-Sided Bandwidth	Optimal	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 Census Tracts (Column 1) or the Census Tracts described at the top of each column. "Low" SNAP Enrollment refers to Census Tracts with SNAP enrollment below the city median (24.8%). "High" SNAP Enrollment refers to Census Tracts above the city median enrollment. The criteria for identifying a census tract as low income are from the Department of Treasury's New Markets Tax Credit (NMTC) program. 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 ten, five, and one percent levels, respectively.

Table 4: Effects by Location Type

	Average Effect	Location	
		Non-Residence	Residence
Domestic Abuse			
SNAP Staggered	0.0114**	0.0095***	0.0019
	(0.0048)	(0.0025)	(0.0041)
Pre-Period Mean	0.170	0.043	0.127
N	143150	143150	143150
Domestic Battery			
SNAP Staggered	0.0101***	0.0400***	-0.0097
	(0.0031)	(0.0081)	(0.0130)
Pre-Period Mean	0.092	0.139	0.401
N	177506	31538	31538
Domestic Assault			
SNAP Staggered	0.0007*	0.0018	-0.0006
	(0.0004)	(0.0012)	(0.0018)
Pre-Period Mean	0.003	0.006	0.011
N	529246	97300	97300
Domestic Property Crime			
SNAP Staggered	0.0018**	0.0041*	0.0017
	(0.0007)	(0.0021)	(0.0033)
Pre-Period Mean	0.008	0.014	0.030
N	316566	58131	58131
Domestic Threat			
SNAP Staggered	0.0029	-0.0033	-0.0016
	(0.0019)	(0.0041)	(0.0089)
Pre-Period Mean	0.051	0.045	0.239
N	319838	58788	58788
Child Maltreatment			
SNAP Staggered	0.0013***	0.0005*	0.0003
	(0.0005)	(0.0002)	(0.0005)
Pre-Period Mean	0.004	0.001	0.004
N	509614	509614	509614
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 1 using the listed crime type as the dependent variable and using data from all Census Tracts (Column 1) or the Census Tracts described at the top of each column. "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 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 ten, five, and one percent levels, respectively.

Table 5: Robustness Checks

| | Optimal BW | Full BW | Quad Fit | Cubic Fit | Poisson | Triangular Kernel | |
						MSERD BW	Full BW
Domestic Abuse							
SNAP Staggered	0.0114**	0.0461***	0.0200***	-0.0000	0.2513***	0.0020	0.0363***
	(0.0048)	(0.0036)	(0.0039)	(0.0045)	(0.0183)	(0.0050)	(0.0026)
N	143152	597148	597148	597148	583270	143968	597148
Domestic Abuse- Battery							
SNAP Staggered	0.0101***	0.0295***	0.0128***	-0.0023	0.2997***	0.0230***	0.0040
	(0.0031)	(0.0026)	(0.0028)	(0.0033)	(0.0254)	(0.0018)	(0.0033)
N	177508	597148	597148	597148	582540	171780	597148
Domesic Abuse- Assault							
SNAP Staggered	0.0007*	0.0008**	0.0008	0.0009	0.2917*	0.0007**	0.0007**
	(0.0004)	(0.0004)	(0.0005)	(0.0006)	(0.1535)	(0.0003)	(0.0003)
N	529253	597148	597148	597148	395660	597148	597148
Domesic Abuse- Property							
SNAP Staggered	0.0018**	0.0014**	0.0016**	0.0019**	0.1885**	0.0014***	0.0015**
	(0.0007)	(0.0006)	(0.0007)	(0.0009)	(0.0870)	(0.0005)	(0.0007)
N	316569	597148	597148	597148	488370	310840	597148
Domesic Abuse- Threat							
SNAP Staggered	0.0029	0.0030*	0.0013	0.0014	0.0599*	0.0028**	0.0019
	(0.0019)	(0.0017)	(0.0019)	(0.0025)	(0.0332)	(0.0013)	(0.0017)
N	319841	597148	597148	597148	581080	341924	597148
Any Child Maltreatment							
SNAP Staggered	0.0013***	0.0015***	0.0014**	0.0008	0.3289***	0.0014***	0.0014***
	(0.0005)	(0.0005)	(0.0006)	(0.0007)	(0.1111)	(0.0004)	(0.0004)
N	509620	597148	597148	597148	454060	515340	597148

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 for comparison. Columns 3 and 4 allow for the days from the cutoff to vary quadratically and cubically (in addition to varying on either side of the threshold) respectively. Column 5 reports Poisson coefficients. Columns 6 and 7 fit the model using a triangular kernel instead of uniform kernel. In particular, Column 6 uses a MSE-driven bandwidth, while Column 7 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 ten, five, and one percent levels, respectively.

Table 6: February Placebo 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)
Any Child Maltreatment					
SNAP Staggered	0.0013**	-0.0004	-0.0002	0.0007	0.0006
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0004)
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, using the listed crime type as the dependent variable. Column 1 replicates the baseline results for comparison, using February 1, 2010 as the treatment date. Columns 2, 3, 4, and 5 reassign the treatment cutoff to February 2008, 2009, 2011, and 2012, respectively. Crime data from are from the city of Chicago.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 7: The Effect of Staggering SNAP Benefits on Household Violence, Controlling for Weekend Income

				Day of Month Range	
	Average Effect	Average Effect	1st of Month	Days 2-23	Days 24-31
Weekend SNAP					
Domestic Abuse					
SNAP Staggered	0.0461***	0.0122**	-0.2829***	0.0144**	0.0221*
	(0.0036)	(0.0048)	(0.0672)	(0.0058)	(0.0116)
Weekend SNAP	0.0036	0.0090	0.0000	0.0002	0.0000
	(0.0027)	(0.0057)	(.)	(0.0066)	(.)
Pre-Period Mean	0.193	0.170	0.237	0.169	0.168
N	597140	143150	4090	104704	34356
Child Maltreatment					
SNAP Staggered	0.0015***	0.0013***	-0.0016	0.0019***	0.0002
	(0.0005)	(0.0005)	(0.0039)	(0.0006)	(0.0010)
Weekend SNAP	-0.0000	-0.0000	0.0040	-0.0003	0.0000
	(0.0004)	(0.0004)	(0.0029)	(0.0005)	(.)
Pre-Period Mean	0.004	0.004	0.007	0.004	0.004
N	597140	509614	17178	366464	125972
Weekend Payday					
Domestic Abuse					
SNAP Staggered	0.0477***	0.0149***	-0.2829***	0.0149**	0.0221*
	(0.0037)	(0.0049)	(0.0672)	(0.0059)	(0.0116)
Weekend Payday	0.0288***	0.0592***	0.0000	0.0112	0.0000
	(0.0058)	(0.0147)	(.)	(0.0179)	(.)
Pre-Period Mean	0.193	0.170	0.237	0.169	0.168
N	597140	143150	4090	104704	34356
Child Maltreatment					
SNAP Staggered	0.0015***	0.0013***	-0.0016	0.0018***	0.0002
	(0.0005)	(0.0005)	(0.0039)	(0.0006)	(0.0010)
Weekend Payday	0.0003	0.0001	0.0040	-0.0014	0.0000
	(0.0009)	(0.0010)	(0.0029)	(0.0012)	(.)
Pre-Period Mean	0.004	0.004	0.007	0.004	0.004
N	597140	509614	17178	366464	125972
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. "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 ten, five, and one percent levels, respectively.

Table 8: Effects by Arrest Indicator

	Average Effect	Arrest	No Arrest
Domestic Abuse			
SNAP Staggered	0.0114**	0.0036	0.0078*
	(0.0048)	(0.0022)	(0.0043)
Pre-Period Mean	0.170	0.038	0.132
N	143150	143150	143150
Battery			
SNAP Staggered	0.0101***	0.0028*	0.0072***
	(0.0031)	(0.0015)	(0.0027)
Pre-Period Mean	0.092	0.023	0.069
N	177506	177506	177506
Assault			
SNAP Staggered	0.0007*	-0.0002	0.0010***
	(0.0004)	(0.0002)	(0.0003)
Pre-Period Mean	0.003	0.001	0.002
N	529246	529246	529246
Threat			
SNAP Staggered	0.0029	-0.0003	0.0032*
	(0.0019)	(0.0003)	(0.0018)
Pre-Period Mean	0.051	0.001	0.050
N	319838	319838	319838
Property			
SNAP Staggered	0.0018**	0.0001	0.0017**
	(0.0007)	(0.0002)	(0.0007)
Pre-Period Mean	0.008	0.001	0.007
N	316566	316566	316566
Child Maltreatment			
SNAP Staggered	0.0013***	0.0004	0.0004
	(0.0005)	(0.0003)	(0.0005)
Pre-Period Mean	0.004	0.001	0.003
N	509614	509614	509614
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 1 using the listed crime type as the dependent variable and using data from all Census Tracts (Column 1) or the Census Tracts described at the top of each column. "No Arrest" refers to the subset of crimes in which a victim chooses not to press charges. "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 ten, five, and one percent levels, respectively.

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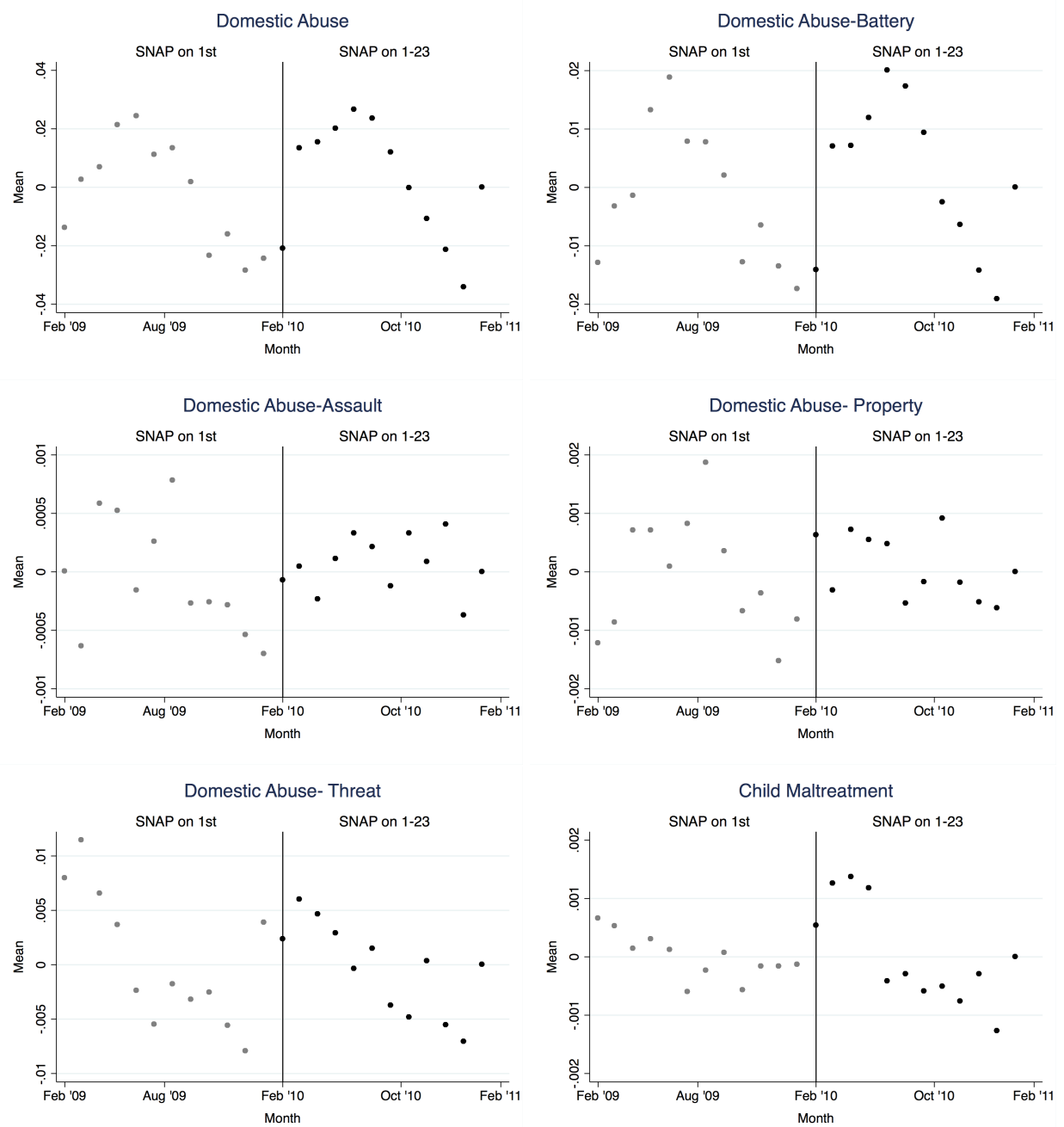
Notes: Each figure plots month-level means of residuals (after differencing out Census Tract, day-of-week, and day-of-month fixed effects) of each of the crimes listed. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed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

Figure 2: Effect of Varying Bandwidth on Estimates
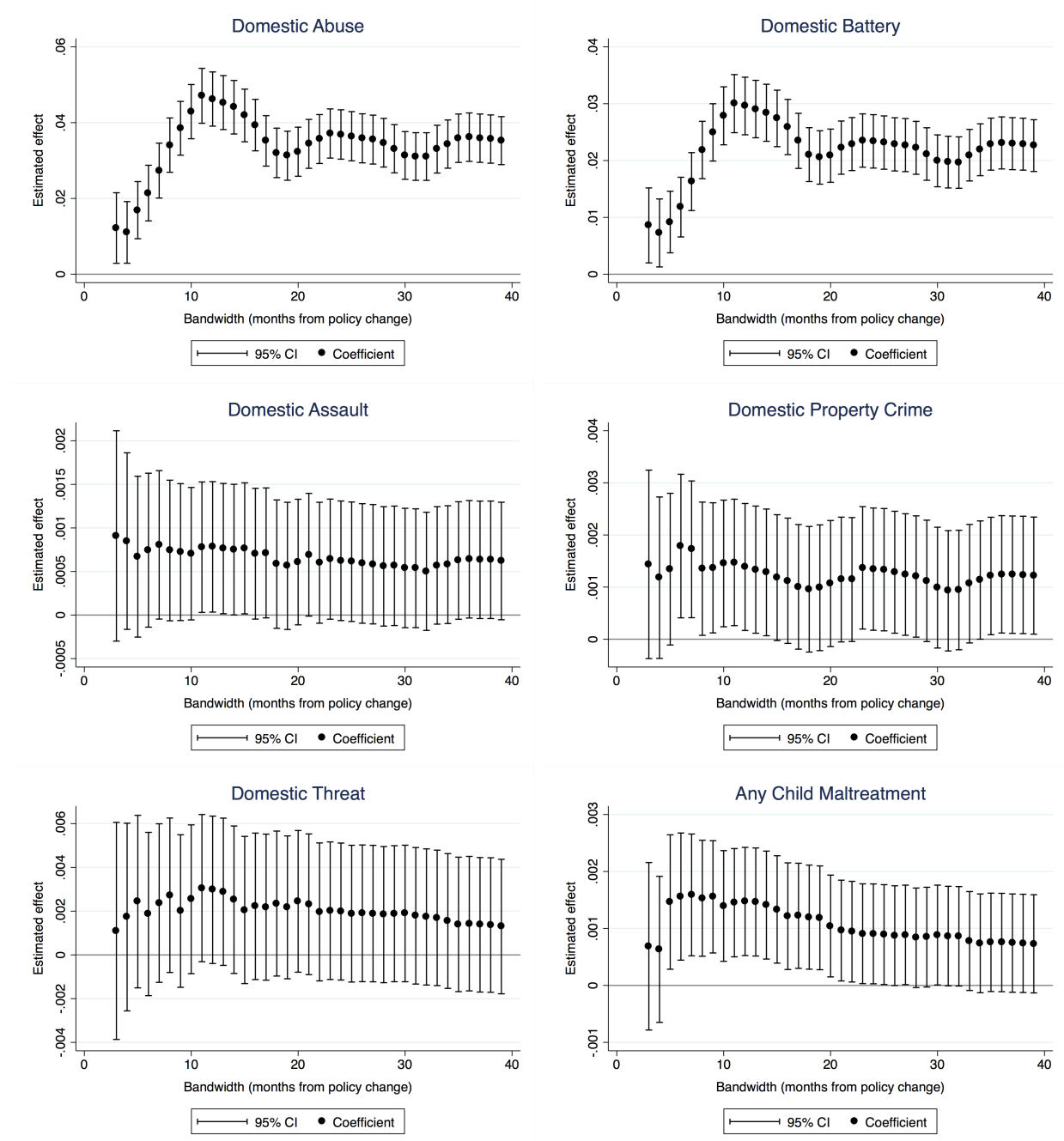
Notes: Each dot represents the coefficient of interest generated by a separate regression. The various bandwidths on which these regressions were performed are represented on the x-axis. We also report the 95% confidence interval of the coefficient. Reported crime data are from the city of Chicago.

Figure 3: Empirical Distribution of Placebo Estimates
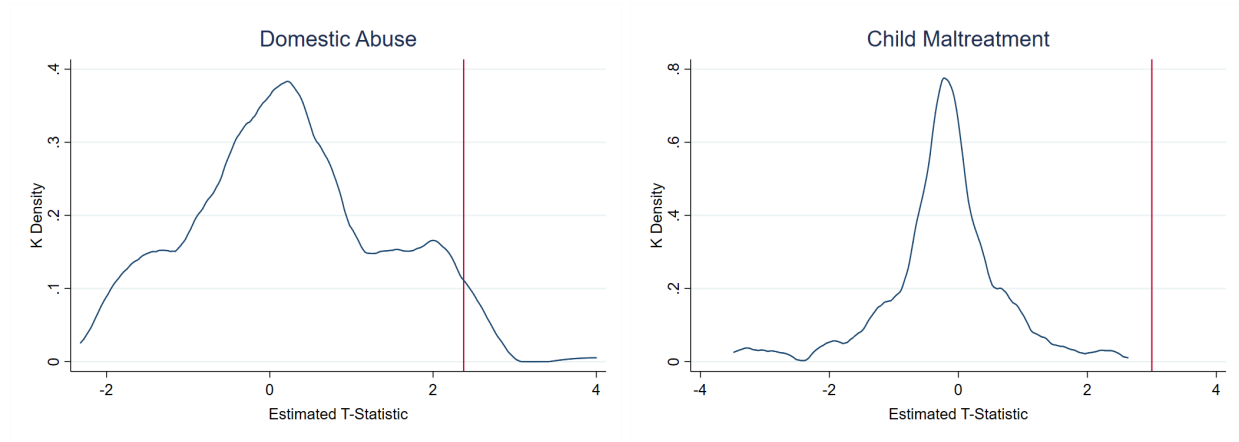
Notes: Each figure plots the distribution of 1,000 t-scores from placebo regressions of the regression discontinuity specification (Equation 1) using randomly drawn discontinuities and pre-period crime data from 2007-2010. For domestic abuse and child maltreatment, 4.4 percent and 0.2 percent of t-statistics (in absolute value) are larger than those reported in Table 2, respectively. Reported crime data from are from the city of Chicago.

Figure 4: Child Maltreatment Reports and Victims (NCANDS)
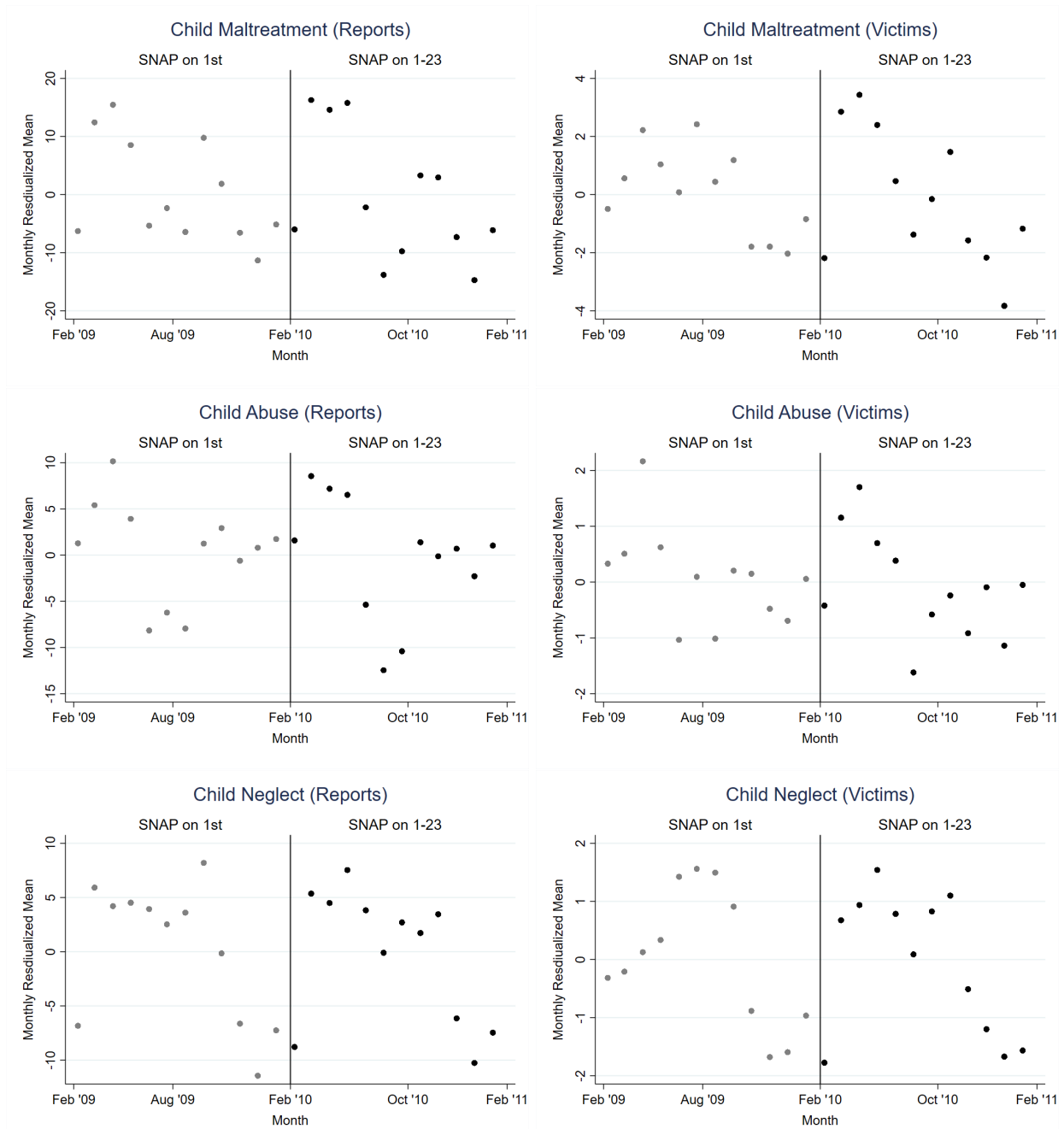
Notes: Each figure plots month-level residualized means (accounting for bimonthly and county fixed effects) of each of the crimes listed. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. 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. Bi-monthly county-level crime data from February 2009-February 2011 for the state of Illinois are from the National Child Abuse and Neglect Data System.

36

Appendix

For Online Publication

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	236463	38674	1800	28117	8757
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 ten, five, and one percent levels, respectively.

Table A2: Difference-in-RD and Difference-in-Difference-in-RD

	D-i-RD Across Time	D-i-D-i-RD Across Usage & Time	D-i-D-i-RD Across Income & Time
Domestic Abuse			
Staggered*Treated Year	0.0120**	0.0185***	0.0178***
	(0.0052)	(0.0049)	(0.0059)
Staggered*Treated Area and Year		-0.0130	-0.0091
		(0.0103)	(0.0094)
Pre-Period Mean	0.170	0.263	0.226
N	715758	715758	715758
Battery			
Staggered*Treated Year	0.0099***	0.0079***	0.0091**
	(0.0034)	(0.0031)	(0.0035)
Staggered*Treated Area and Year		0.0040	0.0014
		(0.0068)	(0.0061)
Pre-Period Mean	0.092	0.146	0.125
N	887541	887541	887541
Assault			
Staggered*Treated Year	0.0001	-0.0002	-0.0004
	(0.0003)	(0.0003)	(0.0003)
Staggered*Treated Area and Year		0.0004	0.0008
		(0.0007)	(0.0006)
Pre-Period Mean	0.003	0.005	0.004
N	2646259	2646259	2646259
Property			
Staggered*Treated Year	0.0008	-0.0004	-0.0002
	(0.0007)	(0.0006)	(0.0007)
Staggered*Treated Area and Year		0.0025*	0.0015
		(0.0013)	(0.0012)
Pre-Period Mean	0.008	0.013	0.011
N	1582847	1582847	1582847
Threat			
Staggered*Treated Year	-0.0024	-0.0053**	-0.0046*
	(0.0018)	(0.0021)	(0.0026)
Staggered*Treated Area and Year		0.0059*	0.0034
		(0.0035)	(0.0035)
Pre-Period Mean	0.051	0.064	0.058
N	1599207	1599207	1599207
Child Maltreatment			
Staggered*Treated Year	0.0011***	0.0006	0.0004
	(0.0004)	(0.0004)	(0.0004)
Staggered*Treated Area and Year		0.0010	0.0011
		(0.0008)	(0.0007)
Pre-Period Mean	0.004	0.006	0.005
N	2548098	2548098	2548098
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 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-D-in-RD results which add a geographic difference (high SNAP usage and low income) to model as well. 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 ten, five, and one percent levels, respectively.

Table A3: Difference-in-Differences Estimates of the Effect of the 2010 Illinois SNAP Policy Change on Domestic Violence Crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average Effect	1.791	1.783	5.664				
	(1.206)	(1.465)	(3.493)				
Effect in 2010				2.062***	2.019***	5.863**	
				(0.651)	(0.713)	(2.631)	
Effect in 2011				1.127	1.027	1.169	
				(0.940)	(1.132)	(4.483)	
Effect in 2012				1.993*	1.974	4.142	
				(1.156)	(1.233)	(4.780)	
Effect in 2013				1.983	2.113	11.393**	
				(3.568)	(3.798)	(5.018)	
One Year Lead							-1.783
							(1.465)
N	15660	15640	4040	15660	15640	4040	15640
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	Yes
Urban Counties Only	No	No	Yes	No	No	Yes	No

Notes: Domestic violence crimes are crimes reported as "offenses against family and children". Annual, county-level UCR data from 2009-2013 is from the FBI. County-level demographic and economic controls include percent black, percent Hispanic, unemployment rate and percent in poverty. Standard errors are clustered at the county level. The pre-period mean for domestic violence offenses is 30.9.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A4: The Effect of Staggering SNAP Benefits on Drug Crimes

| | Average Effect | Average Effect | Day of Month Range | | |
			1st of Month	Days 2-23	Days 24-31
SNAP Staggered	0.0174**	0.0751***	-0.5326*	0.0700***	0.0536**
	(0.0085)	(0.0113)	(0.2833)	(0.0126)	(0.0216)
Pre-Period Mean	0.342	0.349	0.256	0.357	0.338
N	236463	76609	2930	55242	18437
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 ten, five, and one percent levels, respectively.

Table A5: The Effect of Staggering SNAP Benefits on Child Maltreatment, by Victim and Perpetrator Characteristics

	All Reports	Adjudicated Victims	Victim Characteristics			Perp Characteristics	
			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	1304	1304	1304	1304	1304	1304	1304

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 2009-February 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 ten, five, and one percent levels, respectively.

Figure A1: Effect of Illinois SNAP Disbursement Change on SNAP Redemptions
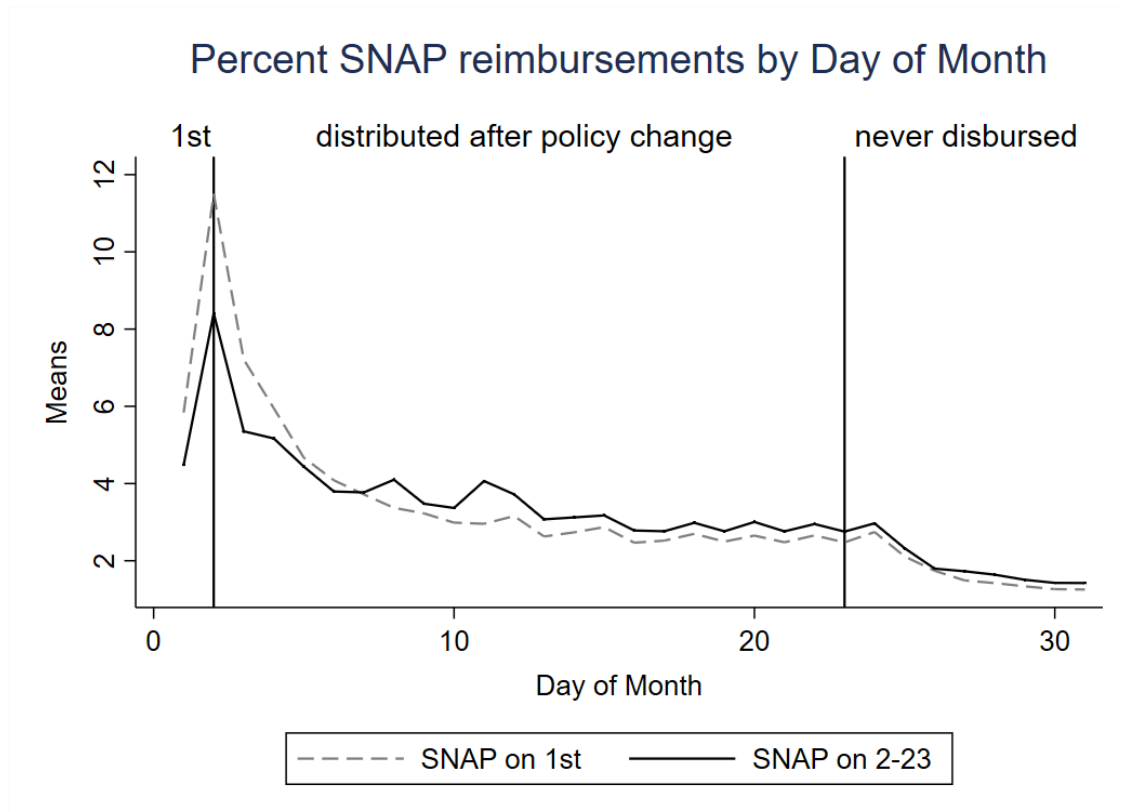
Notes: Authors' calculation based on daily SNAP redemptions data from the Illinois Department of Health and Human Services. The dotted line is calculated for February 2009 - January 2010. The solid line, indicating the post-period after the policy change, is calculated for February 2010 - February 2011.

Figure A2: Effect of Illinois SNAP Disbursement Change on Domestic Violence, Using Optimal Bandwidth
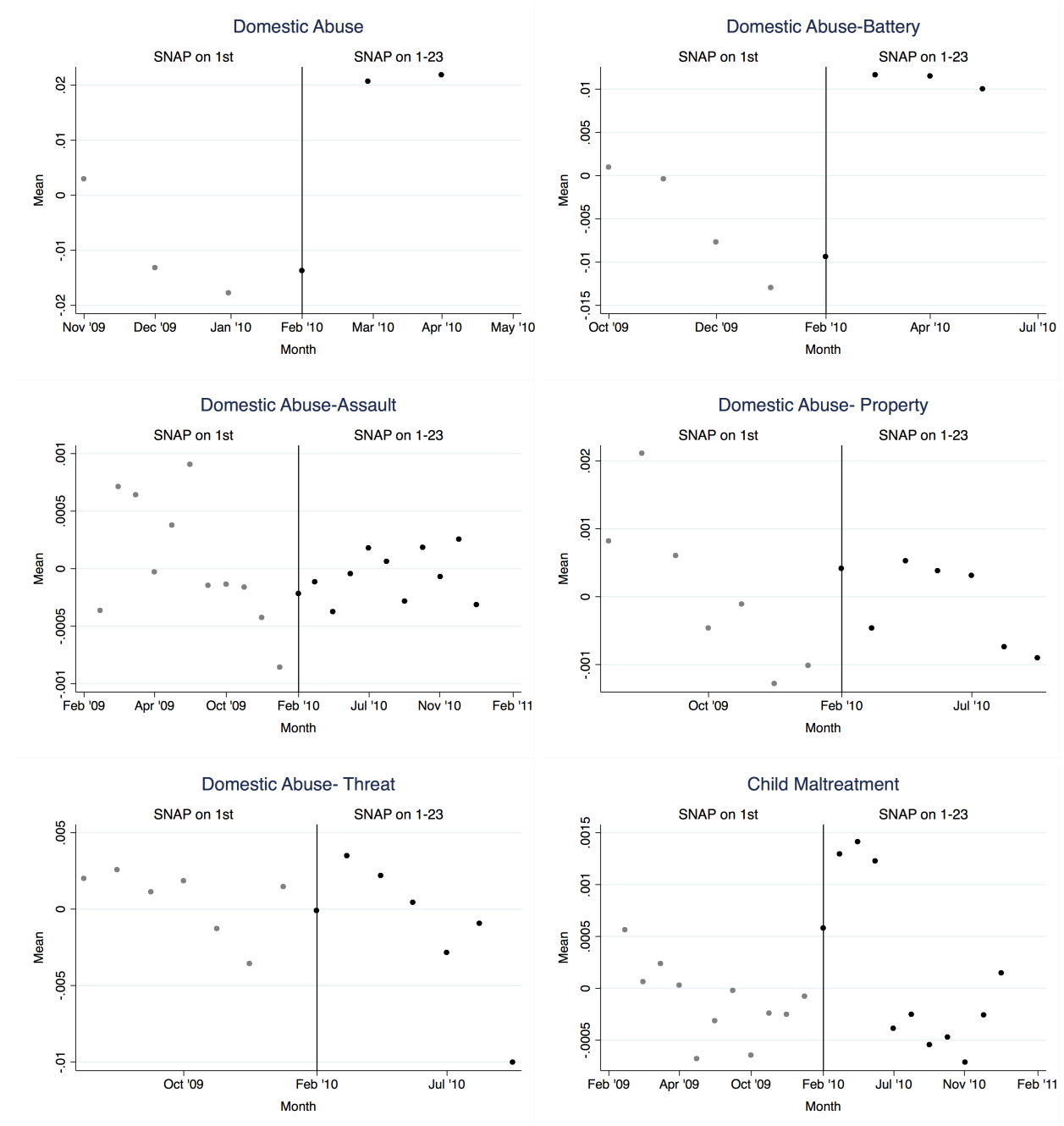
Notes: Each figure plots month-level means of residuals (after differencing out Census Tract, day-of-week and day-of-month fixed effects) of each of the crimes listed. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. Crime data are from the city of Chicago from February 2009-February 2011.

Figure A3: Income and Domestic Abuse Crimes (2010)
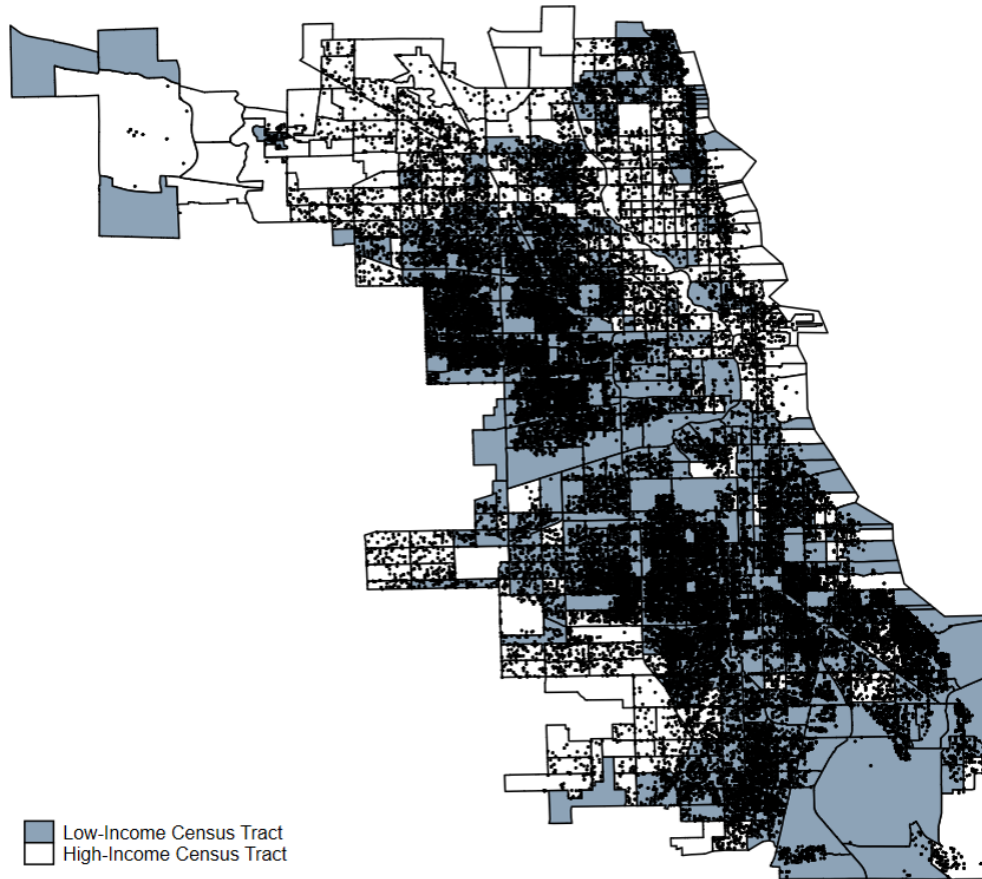
Notes: Census Tracts are shaded by income level, as defined by the USDA. Shaded areas represent low-income Census Tracts. Plotted points display all reported domestic abuse crimes for 2010. Crime data are from the City of Chicago and Census-Tract-level income data are from United States Department of Agriculture Food and Nutrition Service.

Figure A4: Effect of Illinois SNAP Disbursement Change on Weather
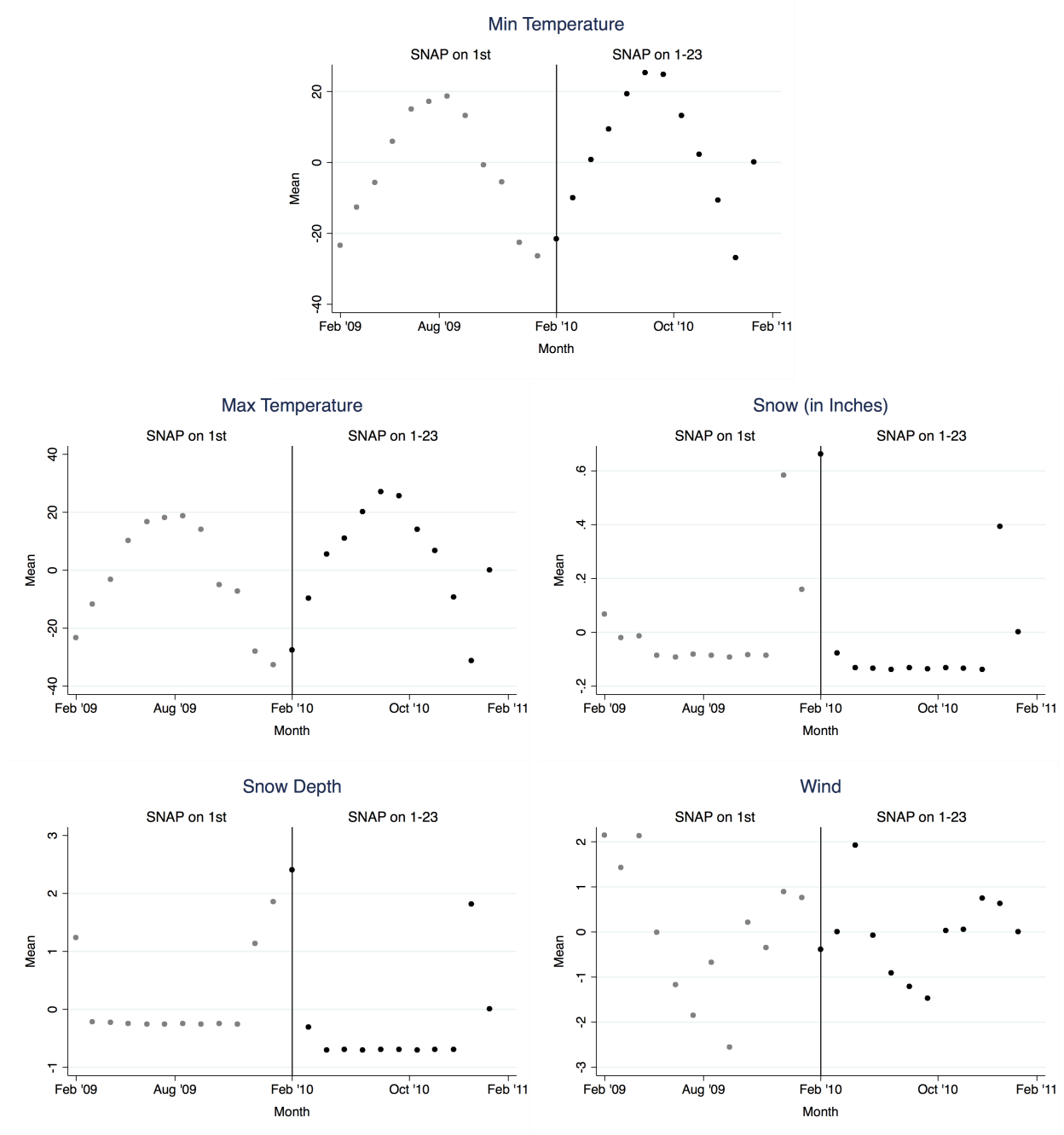
Notes: Each figure plots month-level means of daily weather pattens in the city of Chicago. Daily weather data for Chicago are from the Global Historical Climatology Network and are based on temperature, precipitation and average wind speeds from the Chicago O'Hare International Airport weather station.
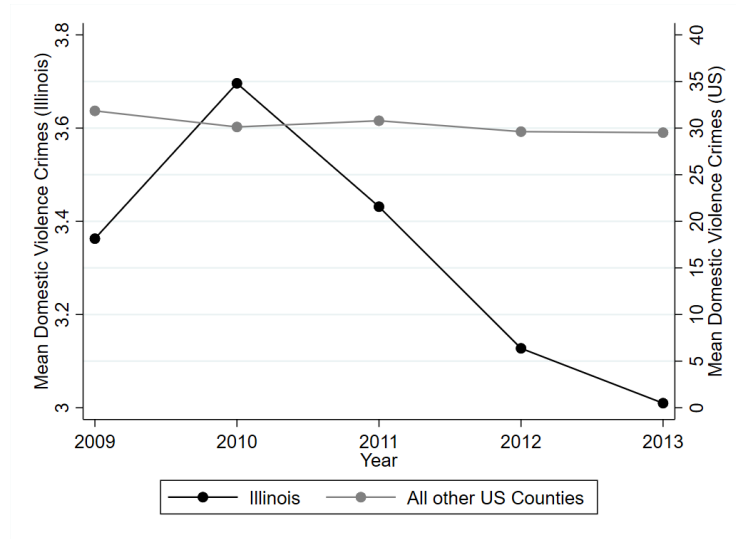
Figure A5: Effect of Illinois SNAP Disbursement Change on Unemployment
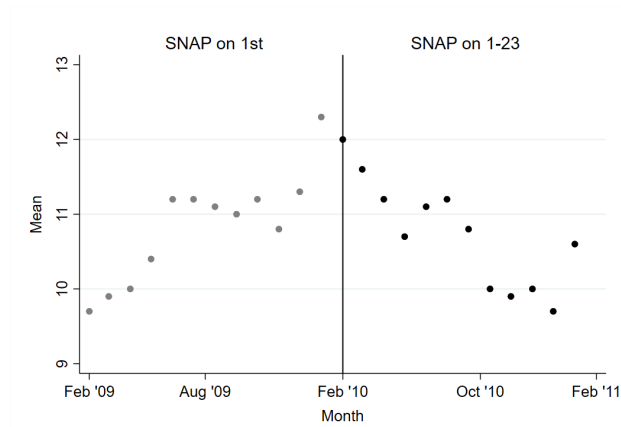
Notes: The figure plots month-level means of the monthly unemployment rate in Cook County. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. Monthly unemployment data are from the U.S. Bureau of Labor Statistics.

Figure A6: Effect of Illinois SNAP Disbursement Change on Domestic Violence Crimes, as Documented in the FBI Uniform Crime Reports

Notes: Domestic violence crimes are crimes reported as "offenses against family and children". Annual, county-level UCR data from 2009-2013 is from the FBI.