SNAP Schedules and Domestic Violence

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Abstract

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

JEL Classification: I38, I18, J18, K42

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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 intimate partner violence (U.S. Department of Health and Human Services, 2017). Domestic violence, including child abuse, child neglect, and intimate partner violence, has large social and economic implications. Mistreated children are more likely to have poor physical and mental health, experience behavioral problems, and have worse economic outcomes in adulthood (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 household resource shocks on domestic violence and analyze this relationship in the context of in-kind, government transfers. Despite the well-established link between poverty and domestic violence, the theoretical relationship between *changes* in resources and violence is ambiguous, and conclusions from economic models largely depend on how the intent or expression of violence is modeled. Standard models of economic theory, for example, predict that within-household violence can be mitigated by the relaxation of liquidity constraints. In these classic household bargaining models, violence is limited by threat points, such as divorce (Manser 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 (Farmer and Tiefenthaler, 1997; Pollak, 2005).[1] Moreover, if intimate partner violence is used as a way to relieve stress, and enters into an abuser's utility function directly, income or benefit receipt by resource-constrained households would lead to fewer instances of violence due to a reduction in financial anxiety.

Other types of signaling models suggest that an influx of resources perpetuates domestic violence. This is particularly relevant when an abuser attempts to exert control over the victim's behavior or the allocation of scarce household resources, known as instrumental violence (Anderberg 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).

These existing studies document that, depending on the underlying cause of domestic abuse, positive resource shocks, including government transfers, have the ability to increase or decrease violence. More

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

specifically, changes in the timing of benefit receipt have the potential to affect domestic violence if resources help to alleviate stress (as in the case of expressive violence) or if changing the transfer date creates temporary financial disruptions and/or a new opportunity for potential conflict (as in the case of instrumental violence).

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

This paper builds on a larger literature on household resources and domestic violence. Recent studies have documented that poor economic conditions and liquidity play a large role in the incidence of family violence, likely due to economic stress. Negative, unexpected changes in income, such as job loss, are associated with increases in child abuse, neglect, and domestic homicide (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), while positive shocks can accordingly reduce domestic violence in some settings. 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, suggesting that changes in the labor market can improve women's outside options and provide a more credible threat point.

Alternatively, existing evidence suggests that income from a large cash transfer program, Temporary Assistance for Needy Families (TANF), increases household violence right after receipt, driven by alcohol-related purchases (Hsu, 2016). These findings imply that monthly government benefits may affect within-household bargaining decisions differently than changes in wages, and may increase purchases of goods that are complements to crime. In this paper, we estimate to what extent this behavior extends beyond cash by focusing on the distribution schedules of in-kind transfers.

This paper contributes to this important, policy-relevant discussion by exploring the monthly cyclicality of intimate partner violence and child maltreatment and analyzing how changes in the timing of benefits affect the levels and timing of abuse. In particular, our findings fit into a larger literature suggesting that such benefits are considered fungible, and that the timing effects of government transfers affect other outcomes,

including crime (Foley, 2011; Hsu, 2016; Carr and Packham, 2019) and test scores (Cotti, Gordanier, and Ozturk, 2017).[2] We note that any perceived fungibility can contribute to the effects we find, especially if it influences purchases of alcohol or drugs which are correlates of domestic violence.[3] Moreover, we note that domestic violence, unlike other types of crime, like property crime, is often motivated less by criminal opportunity payoffs than financial strain and power inequality. Therefore, by focusing on domestic abuse and child maltreatment, our analysis can build on and contribute to the body of work discussing the effectiveness of legal and household interventions on reducing the direct and external costs of within-household violence (Dugan, Nagin, and Rosenfeld, 1999, 2003; Stevenson and Wolfers, 2006).

To measure the impact of in-kind benefit transfers on domestic violence, we exploit variation in SNAP timing to measure the effect of household resource shocks on household violence. Specifically, in 2010, Illinois changed its 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, and take this as evidence that families do respond to the policy change (Carr and Packham, 2019).[4] In this study, we leverage additional granular data on child maltreatment as well as domestic violence and family violence data for areas outside of Chicago to answer several new questions, including whether violent criminal activity *within* households responds to the change in SNAP timing, whether there is any evidence of impacts on child health, how these effects vary across neighborhoods, how short-lived these effects are, and what could be driving any observed changes in criminal activity. In doing so, we address how household shocks affect a fundamentally different type of crime—domestic violence—which is motivated more by within-household conflict, psychological and financial control, and strain, than opportunities for financial gain.

We answer these questions using administrative datasets on domestic violence crimes. We estimate the effects on daily domestic violence incidents at the Census Tract-level using both regression discontinuity and difference-in-regression discontinuity (D-i-RD) approaches. Because domestic violence rates have fallen nearly continuously over the past three decades, such approaches allow us to separate out the effects of the change in SNAP disbursement dates from other factors. Notably, we do not observe which households are SNAP recipients. Therefore, all estimates will represent intent-to-treat effects.

Our estimates are based on detailed crime data from the city of Chicago from February 2009 to February

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

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

[4] In Carr and Packham (2019) 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.

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 studies on domestic violence to date rely on ex-post survey data, which include only self-reported incidents of abuse and grossly underestimate the severity of maltreatment (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. Using these data, estimates based on our primary difference-in-RD approach indicate that distributing nutritional assistance benefits later in the month increases domestic abuse by 6.9 percent, and increases child maltreatment by 30.0 percent. Effects are largely driven by more serious types of abuse (domestic battery). We note that this change in violent family crimes across the course of the benefit month similarly tracks increases in drug-related crime, suggesting that in-kind benefits constitute enough of a household resource shock to alter drug-related behavior. We find that effects are short-lived; estimates indicate that increases in violence last more than a few months but less than 1 year, indicating that changing benefit cycles may cause short-run disruptions in household planning and cause undue financial stress.

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

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

2   Background on Illinois SNAP Policy

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

2.1   The Supplemental Nutrition Assistance Program (SNAP)

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

While we recognize that there is recent evidence that many SNAP families mentally earmark SNAP funds to be spent on food (Hastings and Shapiro, 2018), previous work has also documented that SNAP recipients view benefits as fungible, displacing cash expenditures on food such that more of a family's budget is available for non-food purchases. Therefore, SNAP issuance serves as a general household income shock.[5] Nevertheless, participants increase food consumption right after benefit receipt, and subsequently reduce consumption throughout the month. Families that receive SNAP often face substantial resource limitations at the end of the month, just before they receive their next benefit payment, and many families 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; Gregory and Smith, 2019).

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

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[5]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.

in low-income communities. This argument is especially compelling given the evidence that recipients spend a majority of their benefits in the first two weeks after issuance.[6] Second, recipients often receive other sources of income at the beginning of the month, from employment or other programs, such as Temporary Assistance for Needy Families (TANF) or the Women, Infants, and Children (WIC) program. Therefore, by distributing SNAP benefits later in the month, states can spread out administrative costs.

One potential demand-side advantage of staggered issuance policies is that receiving benefits later in the calendar month could assist families with consumption smoothing or maintaining stable levels of food availability. A majority of SNAP recipients also earn wages or other income, which are typically distributed, at least partially, on the 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 resource shocks.[7] On the other hand, if benefit issuance constitutes a household income shock, and individuals view benefits as fungible, staggering benefits could create an extra day of potential struggle, as partners divy up the additional resources, or could create another opportunity for alcohol or drug use.

2.2 The 2010 Illinois SNAP Policy Change

On February 16, 2010, as a way to reduce crowding in grocery stores, the State of Illinois enacted a staggered benefit issuance schedule. Prior to the policy change, 70% 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.[8,9] To minimize the impact of moving benefit dates, the change occurred over a three-month period, starting in February.[10] The Illinois Department of Human Services announced the change to the public 13

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[6]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%.

[7]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.

[8]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.

[9]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.

[10]To account for this phase-in period and/or any potential announcement effects, we estimate the policy change cutoff at February 1, 2010, although we also estimate some specifications which drop these three months.

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days before the policy change.

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

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.[11] 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, containing offenses occurring one year before and one year after the SNAP policy change.[12,13] For placebo tests and bandwidth sensitivity tests, we expand our sample to include data from January 2007-June 2013.[14]

One of the primary advantages of these data is the ability to pinpoint the location, date, and time that the crime was reported.[15] 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 frequency of interaction with mandatory reporters, as cases do not depend on individuals being legally required to report an incident. Therefore, our reports contain records of abuse and maltreatment for any first- or third-party observation that was reported

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[11]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).

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

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

[14]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.

[15]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.

to police. This is an important feature, especially when analyzing effects on child maltreatment, because if family resources affect the likelihood that a child interacts with responsible adults (such as teachers or community workers) who are obligated to report signs of abuse, it could be difficult for institutional reporting systems to disentangle reporting effects from actual changes in abuse patterns.

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

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

In our main analysis, we select the set of crimes used in each category to reflect different mechanisms that can affect family violence. For domestic abuse we consider all reports that are flagged for or indicate a case of domestic violence between adults, and extend this analysis to separately estimate effects for domestic battery, assault, property damage, or threats made. Battery, the most serious offense listed above, includes unlawful physical contact with the intent to cause injury, while assault reflects the presence or threat of harm to another individual. In our definition of child maltreatment, we include cases of child abuse and neglect.[16] 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. Lastly, to show that other factors related to the outcomes of interest are not driving our results, we use daily weather data on wind speed, temperature, and precipitation from the Global Historical Climatology Network measured at O'Hare Airport.

Table 1 contains summary statistics on the Census Tract-by-day level for these crime data and a measure

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[16]Child physical abuse is defined as non-accidental injury to a child inflicted by a parent or caregiver. Child neglect includes the failure of a parent or caregiver to provide adequate supervision, medical care, or other necessities, and includes cases of child abandonment and endangerment.

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.[17]

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[18]:

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

where $crime_{it}$ represents the count of various domestic violence crimes in Census Tract i on date t, $SNAP\ staggered_t$ 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.[19] 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.[20]

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

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[17]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.

[18]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.

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

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

Specifically, for our main RD analysis, we estimate the Equation 1 using ordinary least squares, allowing for a linear function of the running variable, although we additionally fit models where the running variable enters the equation quadratically, allowing it to vary across the treatment threshold. Moreover, while we use a bandwidth of two years (February 1, 2009-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, we consider the full month to be treated in the following analyses, and normalize our running variable to be equal to zero on February 1, 2010, which yields more conservative estimates than a mid-month treatment definition, as it accounts for any announcement effects of the policy change.[21] The fact that SNAP recipients cannot manipulate SNAP issuance timing alleviates potential selection concerns. Nonetheless, we consider whether there may be additional policy changes or general disruptions related to domestic violence that coincide with the change in SNAP issuance timing. We find no evidence of such changes, and provide support that weather, county-level employment rates, and the timing of other sources of income do not drive our findings.

That being said, it is well-known that crime levels follow recurring patterns based on seasonality. To account for this cyclicality even further, we use an alternative difference-in-regression discontinuity (D-i-RD) approach. To do so, we introduce observations with temporal variation to serve as comparison groups for our treated observations. This model includes all of variables in a traditional RD, but adds interactions of each with an indicator for whether the observation is from a treated or untreated period. Specifically, we estimate models of the following form:

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

where $treatedyear_t$ is an indicator variable equal to one for observations in 2010, the year of the policy

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[21]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, as recommended by Hausman and Rapson (2018). When dropping February from the analysis, estimates indicate an increase in domestic abuse and battery by 28.3 and 32.2 percent, respectively, and an increase in child maltreatment by 20.0 percent. When omitting February and March, our estimates are even larger. These estimates are all similar, albeit larger in magnitude, to the estimates shown in Column 2 of Table 2. Therefore, in subsequent analysis we include data from February 2010 and March 2010 in an effort to produce more conservative estimates.

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

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

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 the MSE-optimal bandwidth, and control for cyclical trends (year, day-of-week, and day-of-month fixed effects) and Census Tract fixed effects. The vertical line denotes the timing of the policy change. Overall, Figure 1 shows striking evidence of an increase in overall household violence after the policy change, including large effects on domestic battery and child maltreatment.[22]

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

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

as our preferred specification. We additionally provide the pre-period means for all estimates to inform the magnitude of the effects.

Estimates in Column 1 indicate that the change in SNAP disbursement dates increased domestic abuse by 23.9 percent with increases in domestic battery and assault of 28.9 percent and 26.7 percent, respectively. Effects for property crimes and 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.[23]

As shown in Columns 3–5, we find that cases of domestic battery decrease at the beginning of the month followed by a 15.3 percent increase spanning days 2–23.[24] Given that battery is the most serious domestic abuse offense in terms of physical harm, these findings suggest that when there is an influx of resources later in the month, households respond by initiating violence and/or increasing the intensity of attacks during these weeks. These findings are consistent with other evidence showing that intimate partner violence increases by between 73–210 percent for TANF recipients in the first four days following TANF disbursement (Hsu, 2016).

We find similar patterns in child maltreatment, although we do not find subsequent decreases at the beginning of the month. Estimates indicate that staggering SNAP benefits leads to a 47.5 percent increase in

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

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

child maltreatment crimes spanning days 2–23. Altogether, these results imply that when families experience resource shocks at different times, there are more opportunities for households to engage in bargaining, leading to more conflict. While some of these effects are mitigated by the reduction of income and violence on the first of the month, these effects are not fully offset and overall levels of violence increase. Below, we provide some insight as to possible mechanisms that could explain these findings.

Given that domestic crimes experience seasonality effects, we additionally present local average estimates from a difference-in-RD model, using years other than 2010 as controls.[25] This approach will allow us to estimate how much the SNAP timing change affected domestic crimes relative to years in which there was no policy change. Specifically, Table 3 Columns 1 and 2 contains D-i-RD results using 2008, 2009, 2011 and 2012 as control years, and the baseline results are consistent with the results in Table 2. In particular, we find that staggering SNAP benefits leads to a 6.9 percent increase in domestic abuse, driven by large increases in domestic battery. Estimates also indicate that changes in SNAP timing increase child maltreatment by 30.0 percent, suggesting that increases in domestic crimes in 2010 are larger than what would have been expected in the absence of the policy change.

5.2   Differential Effects by Location

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

Given that a large majority of domestic crimes occur at home, it is perhaps unsurprising that effects for domestic abuse are concentrated in residential locations. However, estimates for domestic battery are statistically significant both for residential and non-residential locations, and indicate effects of 8.4–11.2 percent, suggesting that domestic violence crimes after the policy change are more likely to occur both in public and at home. Estimates for child maltreatment indicate a statistically significant increase of 18 percent, although estimates are not statistically significant at conventional levels.

Similarly, in Table 4, we estimate effects for Census Tracts for high and low levels of SNAP participation,

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[25]For a graphical representation of our main difference-in-RD estimates, comparing pre-period crime levels in 2008 and 2010, respectively, to those in the following months, before and after February 1, see Figure A4. Overall, graphs for domestic battery, assault, and child maltreatment show that, prior to the policy change, crime levels decreased on February 1, whereas levels in 2010 increased at the threshold, indicating that such increases are not typical each year.

separately, based on data from the ACS. When we split the Census Tracts at the median SNAP enrollment percentage (24.8%), and repeat the methodology described in the previous section, we find that domestic crimes increase more in high-SNAP enrollment areas. In particular, we find that shifting benefits later in the month increases domestic abuse and domestic battery by 24.5 percent and 20.0 percent, respectively, in Census Tracts with relatively high SNAP participation rates.[26]

Alternatively, effects could suggest that after the change in SNAP timing such crimes are more likely to be reported.[27] This is especially relevant if changes in the frequency or severity of domestic violence increases the likelihood of reporting by a third party, and implies that our findings may represent a lower bound if victims are unlikely to report crimes that occur at home. In 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 potential confounders, discussed in the previous sections, are empirically relevant to our analysis, as well as provide evidence that our preferred model specifications are not yielding an anomalous result. A common concern in regression discontinuity-type models is that the results are a product of over- or underfitting the data or a consequence of bandwidth selection. To address these concerns, we explore various alternative RD specifications in this section and show that our average estimates are not sensitive to these other specifications.

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

Since the crime data are discrete, we estimate a corresponding Poisson model and display results in Column 5. Notably, some Census Tracts may have no reported cases of child maltreatment or domestic abuse. Therefore, a number of observations are dropped in this model. Poisson estimates for all outcomes are statistically significant and similar to the baseline results. Finally, in Columns 6 and 7, we test how sensitive

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[26] Estimates for child maltreatment are statistically significant only for low-SNAP areas, which may indicate that reporting of child maltreatment is more likely to be from a third party, rather than the victim. However, estimates when splitting our sample are less precise for this outcome, and we cannot reject large effects (up to 88.8 percent) in high-SNAP areas.

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

these estimates are to kernel selection by estimating the model using a triangular kernel, as compared to the uniform kernel, which we assume for our main results. Estimates using a triangular kernel are positive and nearly all estimates are statistically similar to our baseline estimates in Columns 1 and 2.

Second, to test how sensitive our main results are to bandwidth selection, we replicate our difference-in-RD and RD specifications under a range of bandwidths. We start by testing how robust our difference-in-RD estimates are to various bandwidths spanning 3 months on either side of the threshold up to 12 months, and show our results for a linear fit in Figure 2. Estimates for domestic abuse and child maltreatment are all positive and are statistically significant for bandwidths spanning 5–12 months (on each side); estimates using a one-sided bandwidth of 3 and 4 months are similar in sign and magnitude, but slightly less precise. All estimates for domestic battery are positive and statistically significant.

Because our difference-in-RD estimates are limited to this 12-month window, since other years are used as controls in that approach, we provide an additional test of our RD estimates using bandwidths spanning 3 to 39 months in Figure A5. Estimated effects for domestic abuse and domestic battery are positive and stable across all bandwidths, and estimates are always statistically significant at the 5% level. Similarly, effects on child maltreatment are positive and nearly all estimates are statistically significant.

Third, although our preferred differences-in-RD specification accounts for the recurring monthly fluctuations in crime, we perform additional checks to test to what extent any RD estimates are driven by existing crime cyclicality. In Table 6, we test whether the discontinuity observed in crime levels after the policy change is a season regularity, or "February effect." Specifically, we estimate Equation 1, assigning February 2008, 2009, 2011, and 2012 separately as treatment cutoff dates. None of the estimates in Columns 2–5 indicate a statistically significant increase in domestic abuse or child maltreatment, which implies that our findings are not a result of typical monthly fluctuations in crime rates.[28],[29]

Furthermore, we conduct permutation inference using placebo RD estimates from pre-period crime data to provide more evidence that the discontinuity observed in Chicago is a result of the SNAP policy change and not an artifact of the data. To do so, we randomly select a date from 2007–2010, and assign it as a treatment cutoff date, without replacement.[30] 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

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[28]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.

[29]Similarly, when we include month fixed effects into our main RD specification, estimates indicate a statistically significant increase in domestic abuse by 36.4 percent and an increase in child maltreatment by 17.5 percent. We do not control for month fixed effects in our main results, given that the MSE-optimal bandwidths are less than one year for all outcomes and relatively small for some outcomes (e.g. 45 days for domestic abuse). Therefore many samples do not contain more than 2 months, and controlling for month fixed effects in this context would be yield estimates from likely misspecified models.

[30]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.

with Table 2, to determine what percent of the simulated estimates from 1,000 random draws are greater than the estimate reported in Column 2 of Table 2. The distributions of t-statistics, based on placebo estimates for domestic abuse and child maltreatment, are shown in Figure 3. Based on these placebo distributions, 4.4 percent and 0.2 percent of t-statistics are 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 these 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. In Figures A6 and A7, we test these outcomes formally, using weather data on precipitation, temperature and wind from the Global Historical Climatology Network and unemployment rate data from the Bureau of Labor Statistics, respectively. All weather variables and unemployment rates are smooth across the treatment threshold when tested at conventional levels of statistical significance. Visually, however, because the amount of snowfall in inches increased in February 2010 from 0.2–0.4 inches in December and January to approximately 0.6 inches in February, we have alternatively estimated models which add weather controls to account for any possible weather effects. When we do, estimates for domestic abuse and child maltreatment are statistically similar at the 1% level to our preferred estimates in Table 2 and Table 3.

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

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

16

are able to capture changes in crimes at a broader level and compare these changes to other urban counties in the US in an effort to account for trends in domestic violence over time. In focusing on the entirety of Cook County, we are able to get a more comprehensive picture of the effects of such policy changes.[31]

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

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

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

where DV_{ct} is the logged domestic violence crime rate in a county c in year t, $SNAP\ staggered_{c,t-k}$ is an indicator variable that takes a value of one for Cook County k years after 2009 and zero otherwise, 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. Rates are constructed per 100,000 population. All analyses allow errors to be correlated within counties over time when constructing standard-error estimates.

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

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

Difference-in-differences estimates are shown in Table 7. Estimates in Columns 1–3 indicate domestic

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[31] Ideally, we would be able to track crimes across all Illinois counties; however, Illinois incident data on family crimes is available only for Cook County and Winnebago County. Illinois NIBRS data is available only for Rockford County.

violence crimes in Cook County did not increase, on average, relative to other counties in the three years after the policy change. However, effects for the year of the policy change are positive and statistically significant and indicate an increase in domestic violence crimes of 9.7–13.6 percent, which is similar to our baseline results of 6.9 percent.[32] Moreover, estimates for a one-year leading indicator variable are statistically insignificant, providing additional support for the identification assumption. Estimates in Columns 2 and 3 measuring effects 2–3 years after the policy change are statistically insignificant at the 5% level. Overall, these findings suggest that the policy change had immediate effects that phased out over the following years.

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

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 8.[33] 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 8, we display estimates from Equation 2, accounting for SNAP distribution on paydays and weekends, respectively. Specifically, Column 1 replicates our main difference-in-RD baseline estimates, while Column 2 includes an indicator for common paydays falling on a Friday or Saturday, and Column 3 instead includes an indicator if any SNAP issuance date corresponds to a Friday or Saturday. When controlling for weekend income or benefit receipt, models yield similar findings to our main results; namely, that shifting

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

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

SNAP benefits later in the month results in an increase in household violence, on average, and this uptick in violence is not driven by weekend SNAP transfers or paydays. Results for child maltreatment are similar to those in Tables 2 and 3, and indicate that weekend benefit issuance does not play a substantial role in affecting a recipient's interactions with children. Notably, the coefficient on the interacted term in Column 2 suggests that weekend paydays are correlated with higher levels of intimate partner violence, which may be explained both by the notion that partners spend more time together on the weekends and/or also by recent work showing that even SNAP disbursement dates drive alcohol purchases (Castellari, Cotti, Gordanier, and Ozturk, 2017).

These findings are consistent with recent work showing that the timing of non-SNAP income streams do not mitigate or exacerbate the SNAP cycle (Beatty, Bitler, Cheng, and van der Werf, 2019). In particular, they support the above conclusions that issuing benefits later in the month provide more opportunities for abusive spouses or live-in partners to use violence as a bargaining mechanism, regardless of 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. Recent work suggests that SNAP issuance is linked to alcohol purchases and drinking behavior (Castellari, Cotti, Gordanier, and Ozturk, 2017; Cotti, Gordanier, and Ozturk, 2015), as well as drug misuse (Allen, Atwood, Young, Pauly, and Harrington, 2019).[34] While highly relevant, we are unable to observe alcohol-related crimes in our data. Therefore, we focus solely on drug crimes for this analysis, while acknowledging that these products may indeed be complements, and that violence may be fueled by either drugs, alcohol, or a combination of both.

In Table A2 we replicate our main difference-in-RD results for crimes indicating drug possession, selling or manufacturing. We find that staggered SNAP policies increase overall drug crimes by 8.3 percent, or approximately 9 more drug crimes per day, and that effects are concentrated in non-residential areas. Moreover, we find that these crimes not only shift to different parts of the month, but increase on net, which could suggest that allowing families to better consumption smooth could lead to individuals to engage in more risky behavior and could be one driver of the net increase in violence.

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[34]Specifically Allen, Atwood, Young, Pauly, and Harrington (2019) finds that staggering welfare benefits reduces the likelihood of substance use events by 13 percent.

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

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

Estimates in Column 2 indicate a 7.0 percent increase in domestic abuse crimes ending in arrest. These effects for domestic abuse are driven entirely by increases in arrests for domestic battery, which lends support to the notion that giving benefits later in the month may both increase seriousness and detectability of domestic violence crimes. In particular, out of the 9 additional domestic abuse crimes across the city of Chicago per day, estimates indicate approximately 3 more arrests for domestic abuse, which represents not only an increase in total arrests, but also in the proportion of domesic crimes ending in arrest.[36]

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

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

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

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

offender.

Taken with our findings in Figure A2, these results suggest something else: staggered SNAP policies could increase tension in the household, which leads to more violent behavior both in and outside of the home in the short run. This increase in violence leads to more visibility and seriousness of domestic violence, which increases the likelihood of reporting by both the victim and a third party. Despite the increase in violence, victims are less likely to press charges, indicating that estimated effects are likely due to increases in violence, and not simply a result of increases in victim reporting.

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

In Figure 5, we test if either reports of child maltreatment, or the number of child victims in Illinois increased as a result of the SNAP policy change.[37] Across all crime types, including abuse and neglect, we estimate large effects on child maltreatment for both reports and adjudicated crimes. These findings provide additional evidence that the effects of the state-level policy were not concentrated on Chicago alone, but impacted the entire state of Illinois.

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

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[37]"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 6.9 percent and an increase in child maltreatment by 30.0 percent, driven by increases in crimes in the last three weeks of the month. These estimates correspond to 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 transfers are a within-household motivator of domestic violence, and monthly disbursement of electronic benefits can have large, unintended consequences. Despite the fact that our findings speak to violence in the context of Chicago, domestic violence remains a prevalent problem that is relevant for all major cities, and our findings suggest that more research is needed to better understand the effects of policies on the prevalence of domestic violence.

These findings may be surprising, given that in previous work we find that changes in SNAP benefit timing have economically meaningful reductions on theft, and these changes are largest for older, female individuals (Carr and Packham, 2019). 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). However, our findings combined with recent work on staggered TANF policies (e.g. Hsu (2016)) suggests that although staggering SNAP benefits can allow families to better consumption smooth, this reduction in scarcity at the end of the month does not lead to less conflict between partners. To the extent that staggered SNAP issuance is less likely to be aligned with disbursement of other types of income, such as paychecks or other government transfers, the schedule change may create more opportunities for conflict due to potential resource struggles.

While we are unable to speak to psychological motivators in this paper, other studies have suggested that a partner's household finances do serve as an economically significant incentive for domestic violence (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 alcohol or drug consumption, which may depend on the cyclicality of household finances. Previous studies have shown that SNAP receipt affects alcohol purchases and drunk driving accidents, suggesting that consumption patterns of social drinkers are tied to

benefit timing (Cotti, Gordanier, and Ozturk, 2015; Castellari, Cotti, Gordanier, and Ozturk, 2017). Indeed, although we are unable to directly study the effects on alcohol crimes, we do present some evidence that the policy change increased drug crimes, which suggests that this is one potential channel through which domestic abuse is increasing.

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

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Table 1: Summary Statistics

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

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

Table 2: The Effect of Staggering SNAP Benefits on Domestic Violence, Regression Discontinuity Estimates

| | | | | Day of Month Range | |
	Average Effect	Average Effect	1st of Month	Days 2-23	Days 24-31
Domestic Abuse					
SNAP Staggered	0.0461***	0.0114**	-0.2829***	0.0144**	0.0221*
	(0.0036)	(0.0048)	(0.0672)	(0.0058)	(0.0116)
Pre-Period Mean	0.193	0.170	0.237	0.169	0.168
N	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) for the relevant bandwidth.

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

Table 3: The Effect of Staggering SNAP Benefits on Domestic Violence by Location Type, Difference-in-RD Estimates

| | | | Location | |
	Average Effect	Average Effect	Non-Residence	Residence
Domestic Abuse				
Staggered*Treated Year	0.0146***	0.0117**	0.0038	0.0079*
	(0.0036)	(0.0052)	(0.0026)	(0.0044)
Pre-Period Mean	0.192	0.170	0.065	0.169
N	2981642	715757	715757	715757
Battery				
Staggered*Treated Year	0.0120***	0.0110***	0.0088**	0.0179**
	(0.0027)	(0.0035)	(0.0043)	(0.0075)
Pre-Period Mean	0.102	0.092	0.078	0.213
N	2981642	887540	269072	269072
Assault				
Staggered*Treated Year	0.0003	0.0002	-0.0004	0.0005
	(0.0004)	(0.0004)	(0.0006)	(0.0010)
Pre-Period Mean	0.003	0.003	0.004	0.006
N	2981642	2646258	835120	835120
Property				
Staggered*Treated Year	-0.0012	0.0006	0.0010	-0.0001
	(0.0007)	(0.0008)	(0.0011)	(0.0017)
Pre-Period Mean	0.008	0.008	0.008	0.016
N	2981642	1582846	497945	497945
Threat				
Staggered*Treated Year	0.0008	-0.0011	-0.0034	-0.0013
	(0.0019)	(0.0020)	(0.0022)	(0.0050)
Pre-Period Mean	0.054	0.051	0.016	0.112
N	2981642	1599206	503442	503442
Child Maltreatment				
Staggered*Treated Year	0.0013**	0.0012**	0.0002	0.0009
	(0.0006)	(0.0006)	(0.0003)	(0.0005)
Pre-Period Mean	0.004	0.004	0.001	0.005
N	2981642	2548097	2548097	2548097
One-Sided Bandwidth	1 Year	Optimal	Optimal	Optimal

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

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

Table 4: The Effect of Staggering SNAP Benefits on Domestic Violence, Difference-in-RD Estimates

	Baseline	High-SNAP	Low-SNAP
Domestic Abuse			
Staggered*Treated Year	0.0117**	0.0186***	0.0047
	(0.0052)	(0.0049)	(0.0091)
Pre-Period Mean	0.170	0.076	0.263
N	715757	357875	357882
Battery			
Staggered*Treated Year	0.0110***	0.0076**	0.0145**
	(0.0035)	(0.0031)	(0.0062)
Pre-Period Mean	0.092	0.038	0.146
N	887540	443765	443775
Assault			
Staggered*Treated Year	0.0002	0.0001	0.0003
	(0.0004)	(0.0004)	(0.0008)
Pre-Period Mean	0.003	0.001	0.005
N	2646258	1323115	1323143
Property			
Staggered*Treated Year	0.0006	-0.0003	0.0015
	(0.0008)	(0.0008)	(0.0013)
Pre-Period Mean	0.008	0.003	0.013
N	1582846	791415	791431
Threat			
Staggered*Treated Year	-0.0011	-0.0039	0.0017
	(0.0020)	(0.0024)	(0.0033)
Pre-Period Mean	0.051	0.037	0.064
N	1599206	799595	799611
Child Maltreatment			
Staggered*Treated Year	0.0012**	0.0006	0.0018*
	(0.0006)	(0.0006)	(0.0010)
Pre-Period Mean	0.004	0.002	0.006
N	2548097	1274035	1274062
One-Sided Bandwidth	Optimal	Optimal	Optimal

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

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

Table 5: Robustness Checks, RD Specification

	Optimal BW	Full BW	Quad Fit	Poisson	Triangular Kernel MSERD BW	Full BW
Domestic Abuse						
SNAP Staggered	0.0114**	0.0461***	-0.0090	0.0720***	0.0020	0.0363***
	(0.0048)	(0.0036)	(0.0086)	(0.0277)	(0.0050)	(0.0026)
N	143150	597140	143150	138250	597140	597140
Domestic Abuse- Battery						
SNAP Staggered	0.0101***	0.0295***	-0.0041	0.1130***	0.0230***	0.0040
	(0.0031)	(0.0026)	(0.0054)	(0.0336)	(0.0018)	(0.0033)
N	177506	597140	177506	170128	597140	597140
Domesic Abuse- Assault						
SNAP Staggered	0.0007*	0.0008**	0.0008*	0.2807*	0.0007**	0.0007**
	(0.0004)	(0.0004)	(0.0005)	(0.1546)	(0.0003)	(0.0003)
N	529246	597140	529246	340322	597140	597140
Domesic Abuse- Property						
SNAP Staggered	0.0018**	0.0014**	0.0009	0.2389**	0.0014***	0.0015**
	(0.0007)	(0.0006)	(0.0009)	(0.0940)	(0.0005)	(0.0007)
N	316566	597140	316566	220203	597140	597140
Domesic Abuse- Threat						
SNAP Staggered	0.0029	0.0030*	0.0011	0.0549	0.0028**	0.0019
	(0.0019)	(0.0017)	(0.0025)	(0.0356)	(0.0013)	(0.0017)
N	319838	597140	319838	310454	597140	597140
Any Child Maltreatment						
SNAP Staggered	0.0013***	0.0015***	0.0015**	0.2929***	0.0014***	0.0014***
	(0.0005)	(0.0005)	(0.0006)	(0.1128)	(0.0004)	(0.0004)
N	509614	597140	509614	371931	597140	597140

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. Column 3 allows for the days from the cutoff to vary quadratically (in addition to varying on either side of the threshold). Column 4 reports Poisson coefficients. Columns 5 and 6 fit the model using a triangular kernel instead of uniform kernel. In particular, Column 5 uses a MSE-driven bandwidth, while Column 6 reports estimates from the full sample. One-sided MSE-optimal bandwidths for domestic abuse, battery, assault, property crimes, threats, and child maltreatment when using a triangular kernel are 88, 105, 373, 190, 209, and 315 days, respectively. Crime data are from the city of Chicago.

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

Table 6: February Placebo RD Estimates

	February 2010 (actual)	February 2008	February 2009	February 2011	February 2012
Domestic Abuse					
SNAP Staggered	0.0114**	-0.0118**	0.0008	-0.0056	0.0045
	(0.0036)	(0.0049)	(0.0029)	(0.0060)	(0.0046)
N	143150	143150	143150	143150	143150
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)
N	509614	509614	509614	509614	509614
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 from Table 2 Column 2 for comparison, using February 1, 2010 as the treatment date. Columns 2, 3, 4, and 5 reassign the treatment cutoff to February 1, 2008, 2009, 2011, and 2012, respectively. Crime data from are from the city of Chicago.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 7: Difference-in-Differences Estimates of the Effect of the 2010 Illinois SNAP Policy Change on Logged Domestic Violence Crime Rates in Urban Counties

	(1)	(2)	(3)
Effect in 2010	0.109***	0.097**	0.136**
	(0.035)	(0.046)	(0.064)
Effect in 2011	0.085**	0.063	0.102
	(0.042)	(0.062)	(0.079)
Effect in 2012	-0.088*	-0.120	-0.081
	(0.048)	(0.075)	(0.091)
One-Year Lead			0.078
			(0.050)
Average Effect	0.04	0.01	0.05
P-Value (test average effect = 0)	0.33	0.81	0.48
Mean	2.86	2.86	2.86
Observations	1776	1776	1776
County Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Controls	No	Yes	Yes

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

Table 8: The Effect of Staggering SNAP Benefits on Household Violence, Controlling for Weekend Income, Difference-in-RD Estimates

	Average Effect	Weekend Payday	Weekend SNAP
Domestic Abuse			
Staggered*Treated Year	0.0117**	0.0124**	0.0115**
	(0.0052)	(0.0052)	(0.0052)
Weekend Payday		0.0118**	
		(0.0053)	
Weekend SNAP			-0.0017
			(0.0023)
Pre-Period Mean	0.170	0.170	0.170
N	715757	715757	715757
Child Maltreatment			
Staggered*Treated Year	0.0012**	0.0011**	0.0012**
	(0.0006)	(0.0006)	(0.0006)
Weekend Payday		-0.0007	
		(0.0005)	
Weekend SNAP			-0.0002
			(0.0002)
Pre-Period Mean	0.004	0.004	0.004
N	2548097	2548097	2548097
One-Sided Bandwidth	Optimal	Optimal	Optimal

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

Table 9: The Effect of Staggering SNAP Benefits on Domestic Violence by Arrest Indicator, Difference-in-RD Estimates

	Average Effect	Arrest	No Arrest
Domestic Abuse			
Staggered*Treated Year	0.0117**	0.0040*	0.0077*
	(0.0052)	(0.0023)	(0.0045)
Pre-Period Mean	0.170	0.057	0.175
N	715757	715757	715757
Battery			
Staggered*Treated Year	0.0110***	0.0025*	0.0085***
	(0.0035)	(0.0015)	(0.0030)
Pre-Period Mean	0.092	0.035	0.095
N	887540	887540	887540
Assault			
Staggered*Treated Year	0.0002	-0.0003	0.0005
	(0.0004)	(0.0002)	(0.0004)
Pre-Period Mean	0.003	0.002	0.003
N	2646258	2646258	2646258
Property			
Staggered*Treated Year	0.0006	-0.0001	0.0007
	(0.0008)	(0.0002)	(0.0007)
Pre-Period Mean	0.008	0.001	0.010
N	1582846	1582846	1582846
Threat			
Staggered*Treated Year	-0.0011	-0.0005*	-0.0006
	(0.0020)	(0.0003)	(0.0020)
Pre-Period Mean	0.051	0.001	0.057
N	1599206	1599206	1599206
Child Maltreatment			
Staggered*Treated Year	0.0012**	0.0001	0.0010*
	(0.0006)	(0.0003)	(0.0005)
Pre-Period Mean	0.004	0.002	0.004
N	2548097	2548097	2548097
One-Sided Bandwidth	Optimal	Optimal	Optimal

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

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

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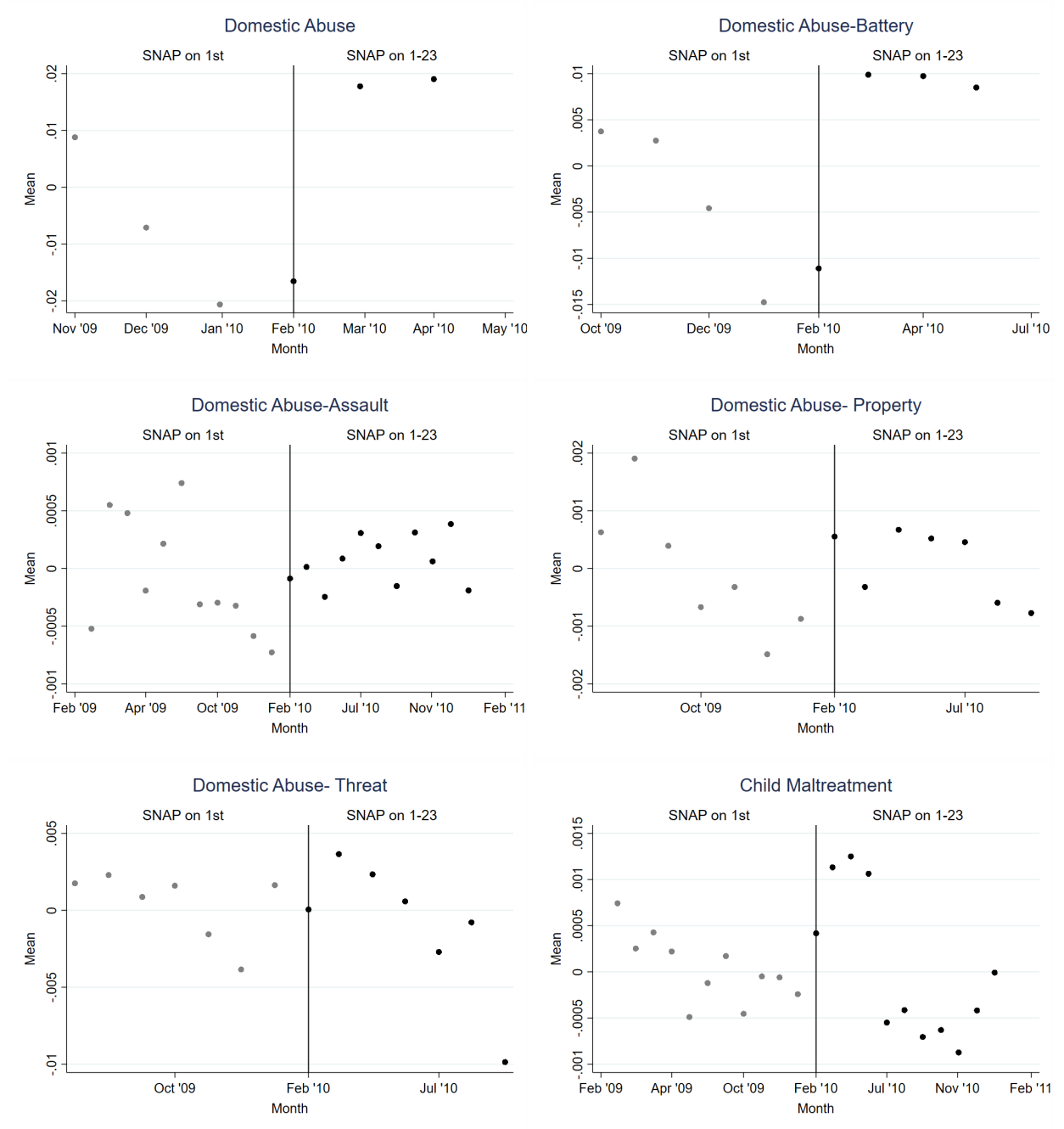
Notes: Each figure plots month-level means of residuals (after differencing out Census Tract, year, day-of-week and day-of-month fixed effects) of each of the crimes listed, using MSERD-optimal bandwidths. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month,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

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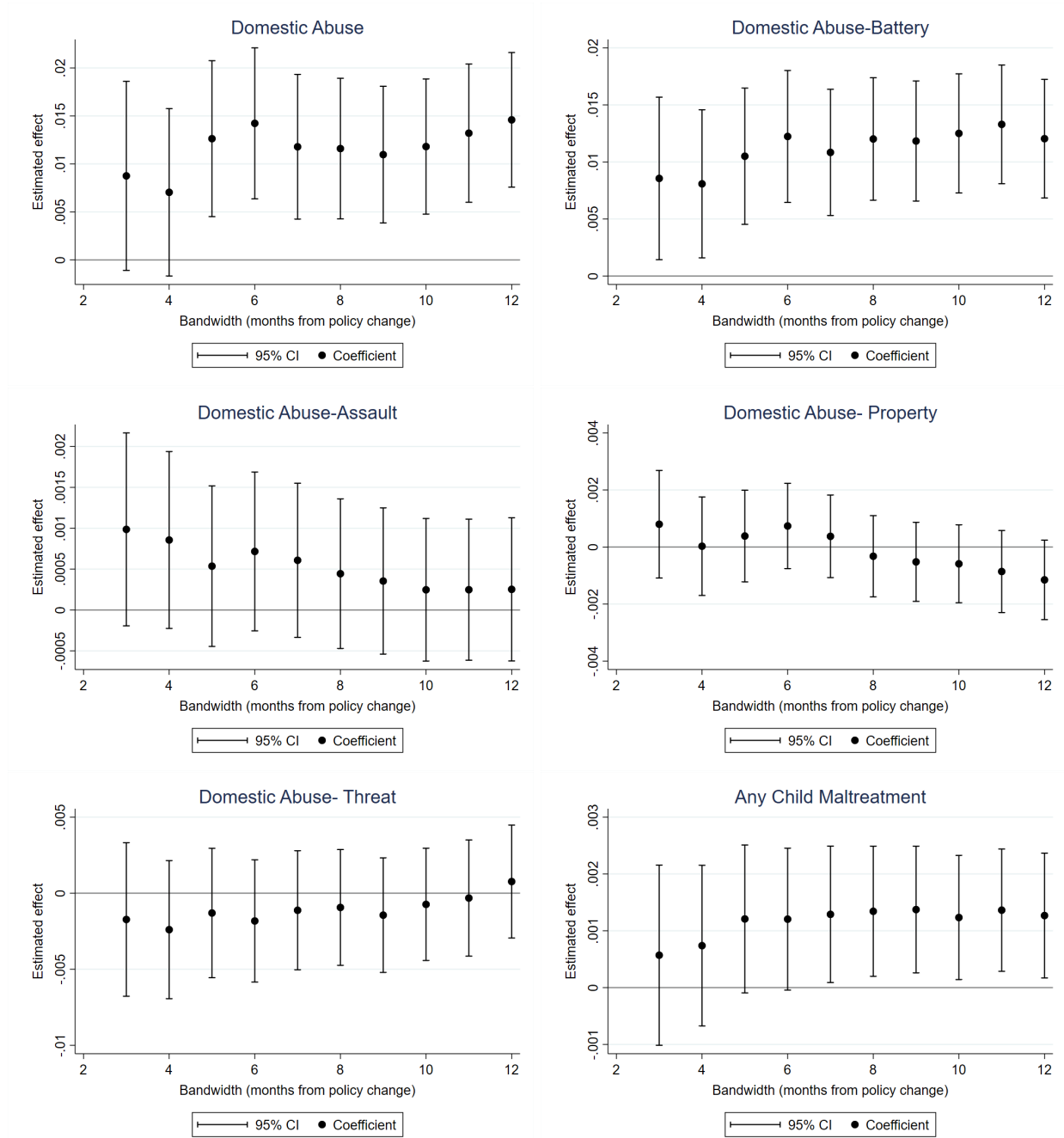
Notes: Each dot represents the coefficient of interest generated by a separate regression as specified by Equation 2. The various bandwidths on which these regressions were performed are represented on the x-axis. We also report the 95% confidence interval of the coefficient. Reported crime data are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

Figure 3: Empirical Distribution of Placebo Estimates

Domestic Abuse

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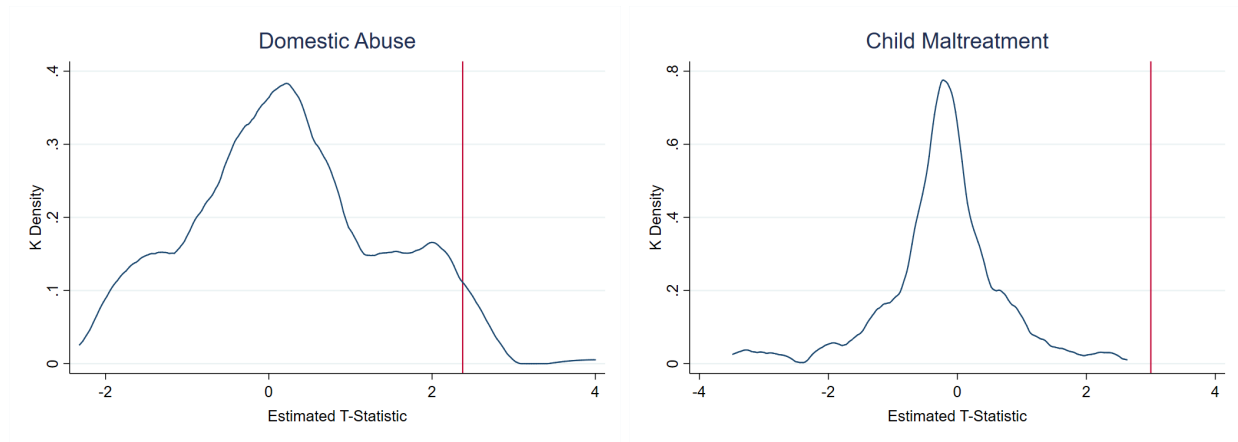
Notes: Each figure plots the distribution of 1,000 t-scores from placebo regressions of the regression discontinuity specification (Equation 1) using randomly drawn discontinuities and pre-period crime data from 2007-2010. For domestic abuse and child maltreatment, 4.4 percent and 0.2 percent of t-statistics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

Figure 4: Effect of Illinois SNAP Disbursement Change on Domestic Violence Arrests, Difference-in-Differences using FBI Uniform Crime Reports
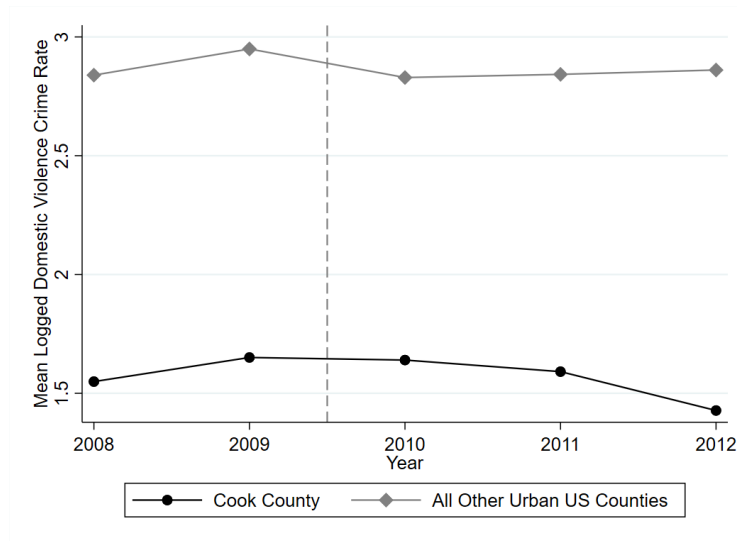
Figure 5: Child Maltreatment Reports and Victims (NCANDS)
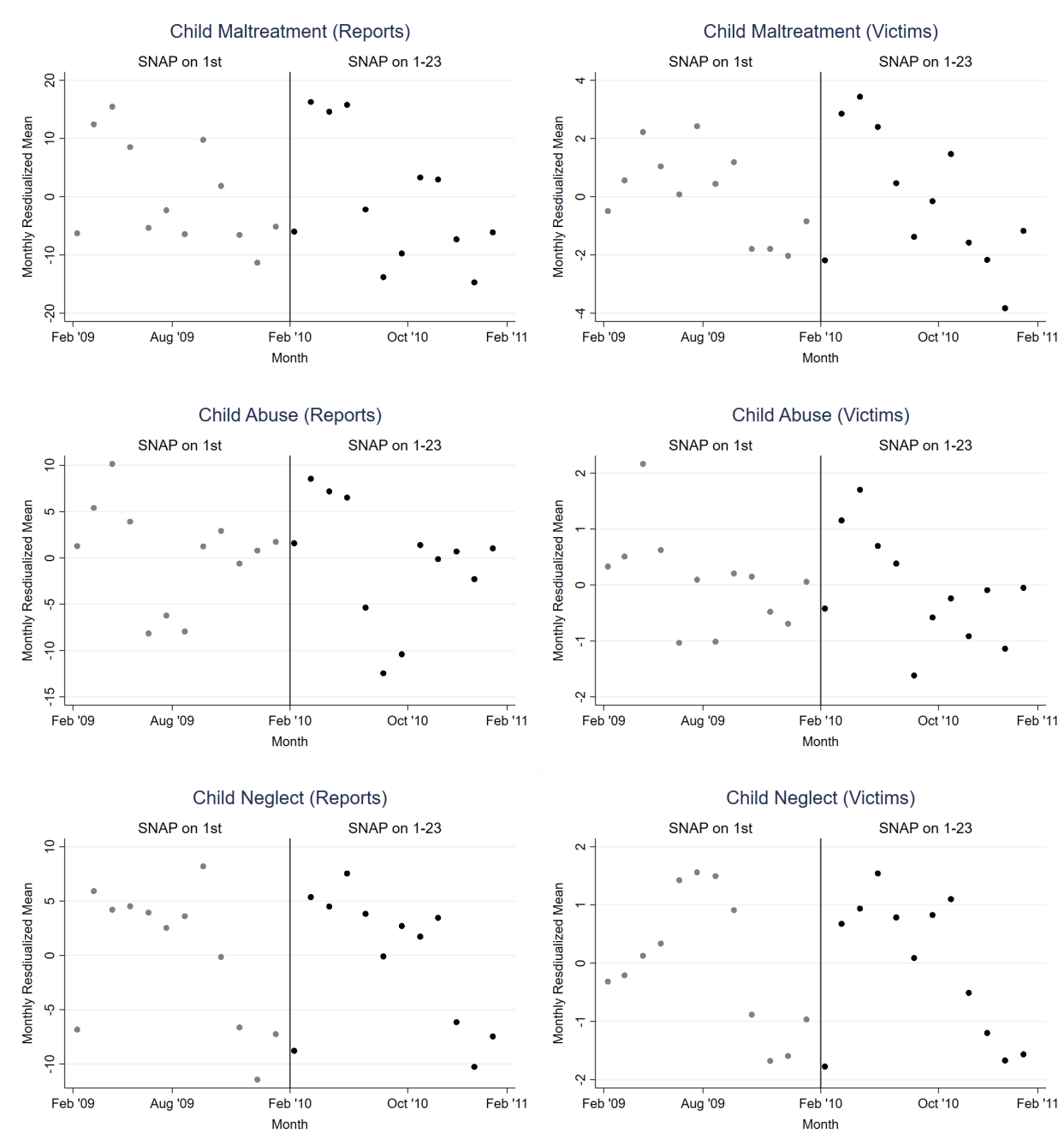
Notes: Each figure plots month-level residualized means (accounting for bimonthly and county fixed effects) of each of the crimes listed. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. 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 1, 2009 - January 31, 2011 for the state of Illinois are from the National Child Abuse and Neglect Data System.

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: The Effect of Staggering SNAP Benefits on Drug Crimes by Location Type, Difference-in-RD Estimates

	Average Effect	Average Effect	Location Non-Residence	Residence
Staggered*Treated Year	0.0113***	0.0094	0.0349**	-0.0065
	(0.0040)	(0.0075)	(0.0172)	(0.0066)
Pre-Period Mean	0.137	0.132	0.443	0.038
N	2981642	404914	151845	151845
One-Sided Bandwidth	1 Year	Optimal	Optimal	Optimal

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

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

Table A3: 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 1, 2009 - January 31, 2011. Each regression includes Census Tract, year, and bimonthly fixed effects. Standard errors are clustered on the county level and reported in parentheses. We also report the mean of each outcome for the period before the policy change.

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

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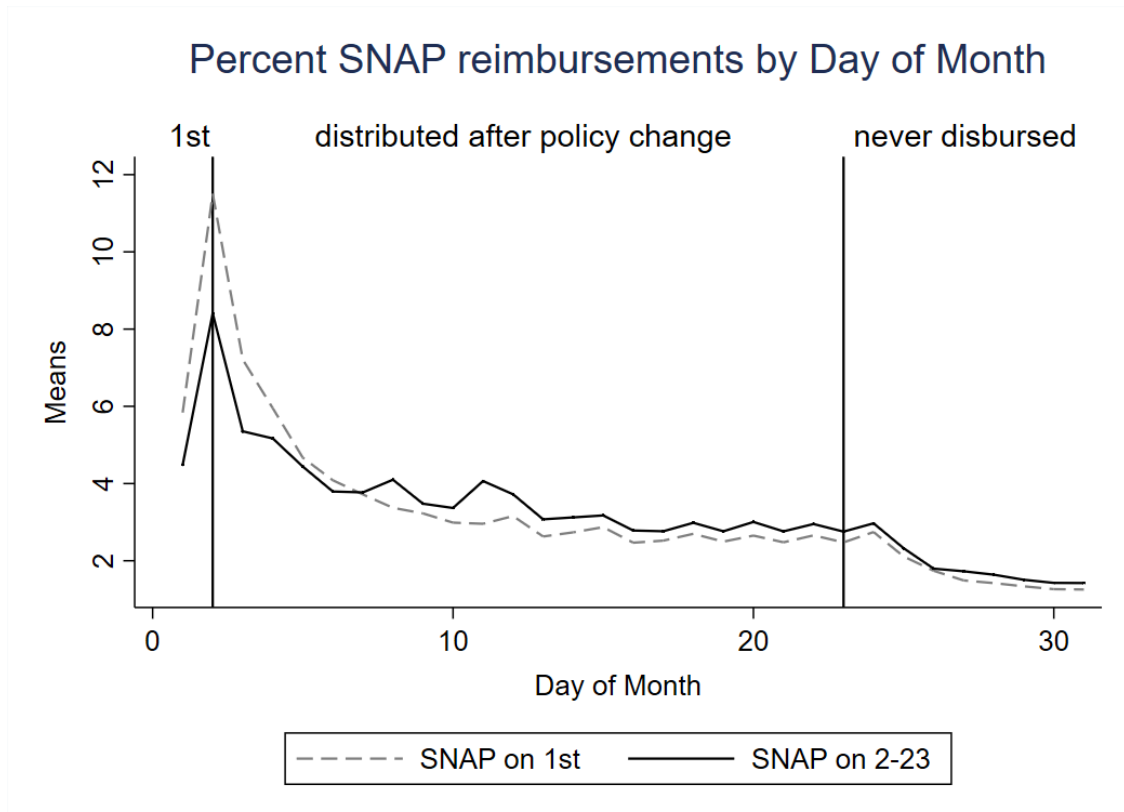

Notes: Authors' calculation based on daily SNAP redemptions data from the Illinois Department of Health and Human Services. The dotted line is calculated for February 2009 - January 2010. The solid line, indicating the post-period after the policy change, is calculated for February 2010 - February 2011.

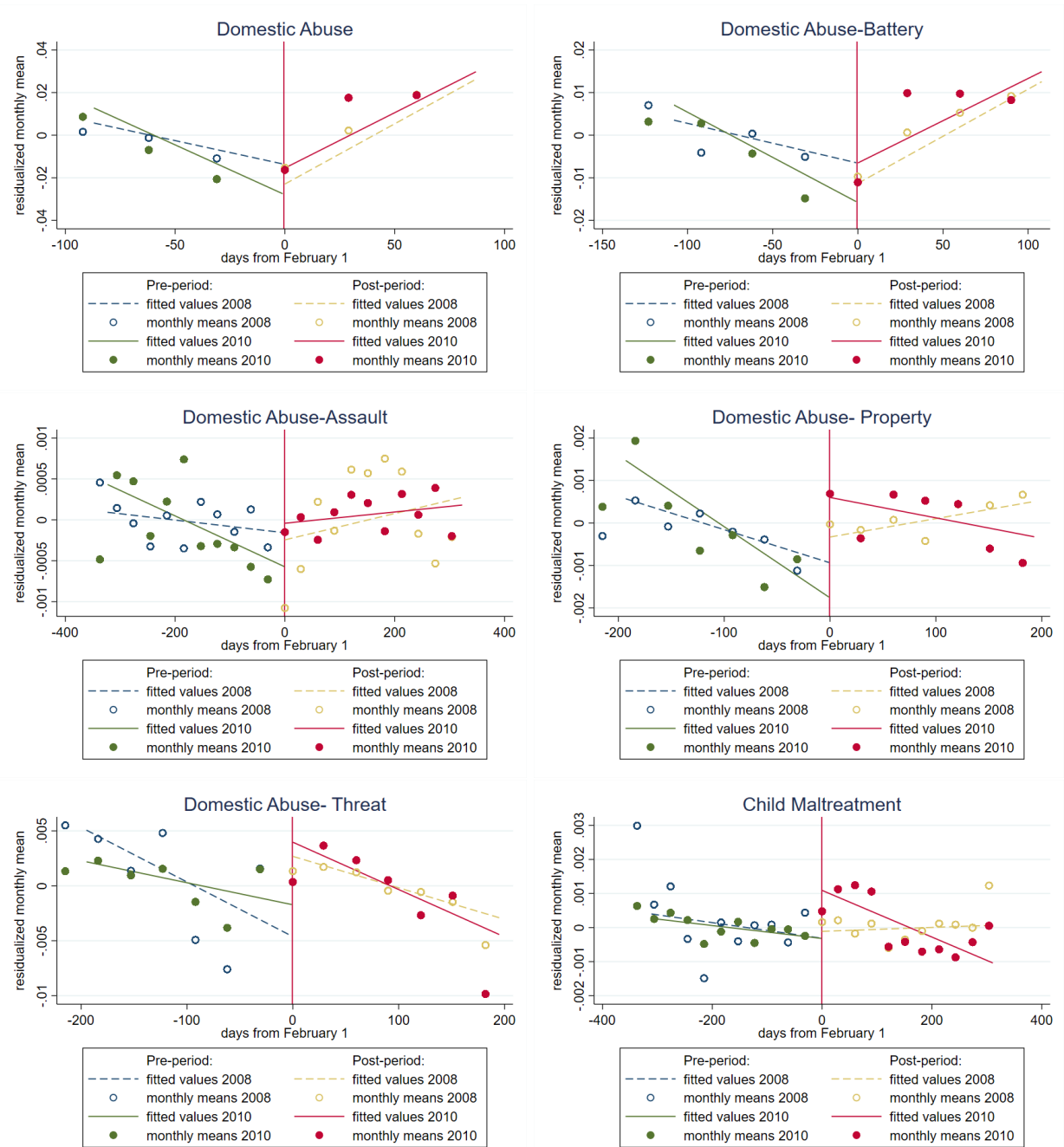Figure A2: Effect of Illinois SNAP Disbursement Change on Domestic Violence
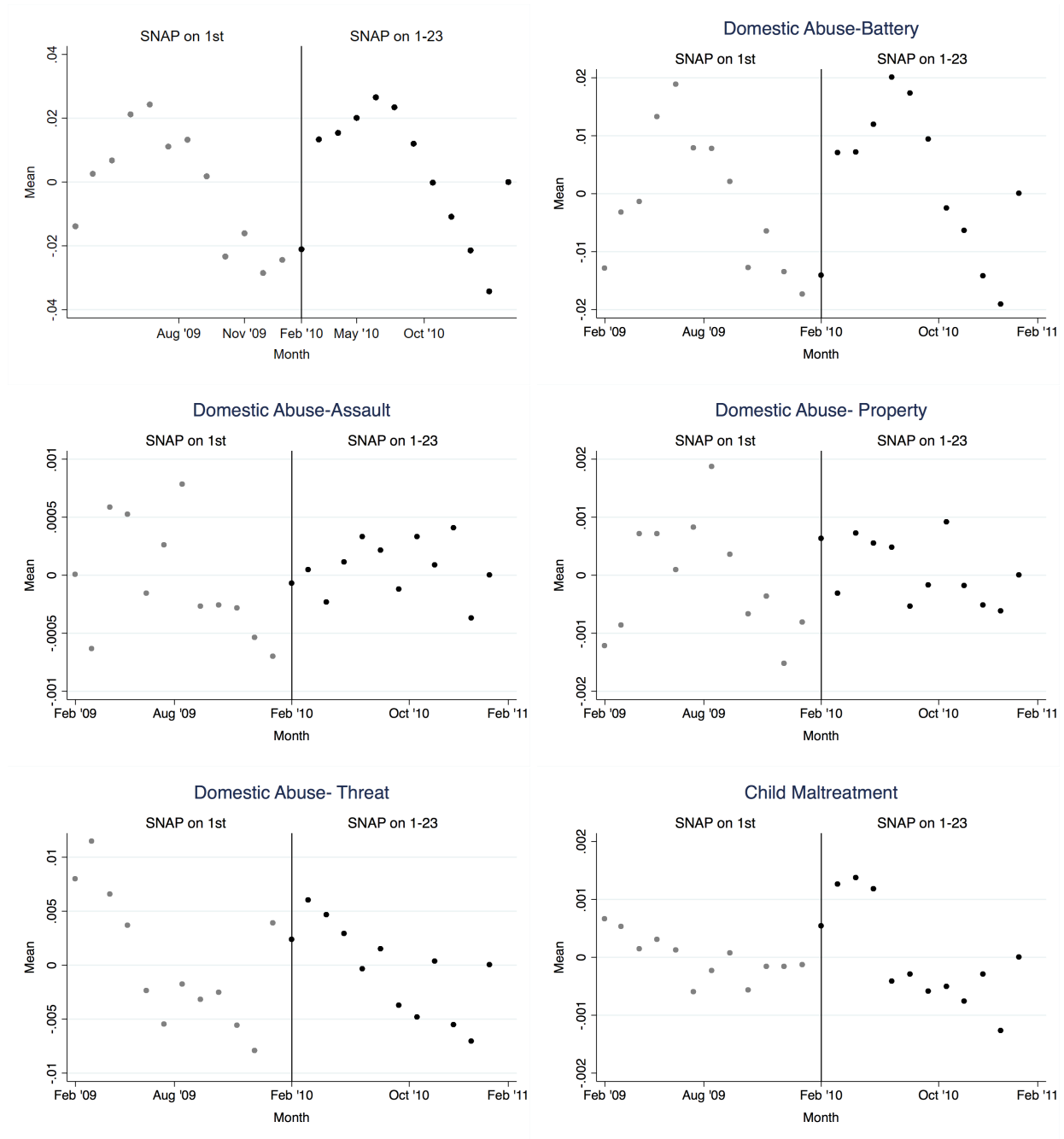
Notes: Each figure plots month-level means of residuals (after differencing out Census Tract, day-of-week, and day-of-month fixed effects) of each of the crimes listed. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. Crime data are from the city of Chicago from February 1, 2009 - January 31, 2011.

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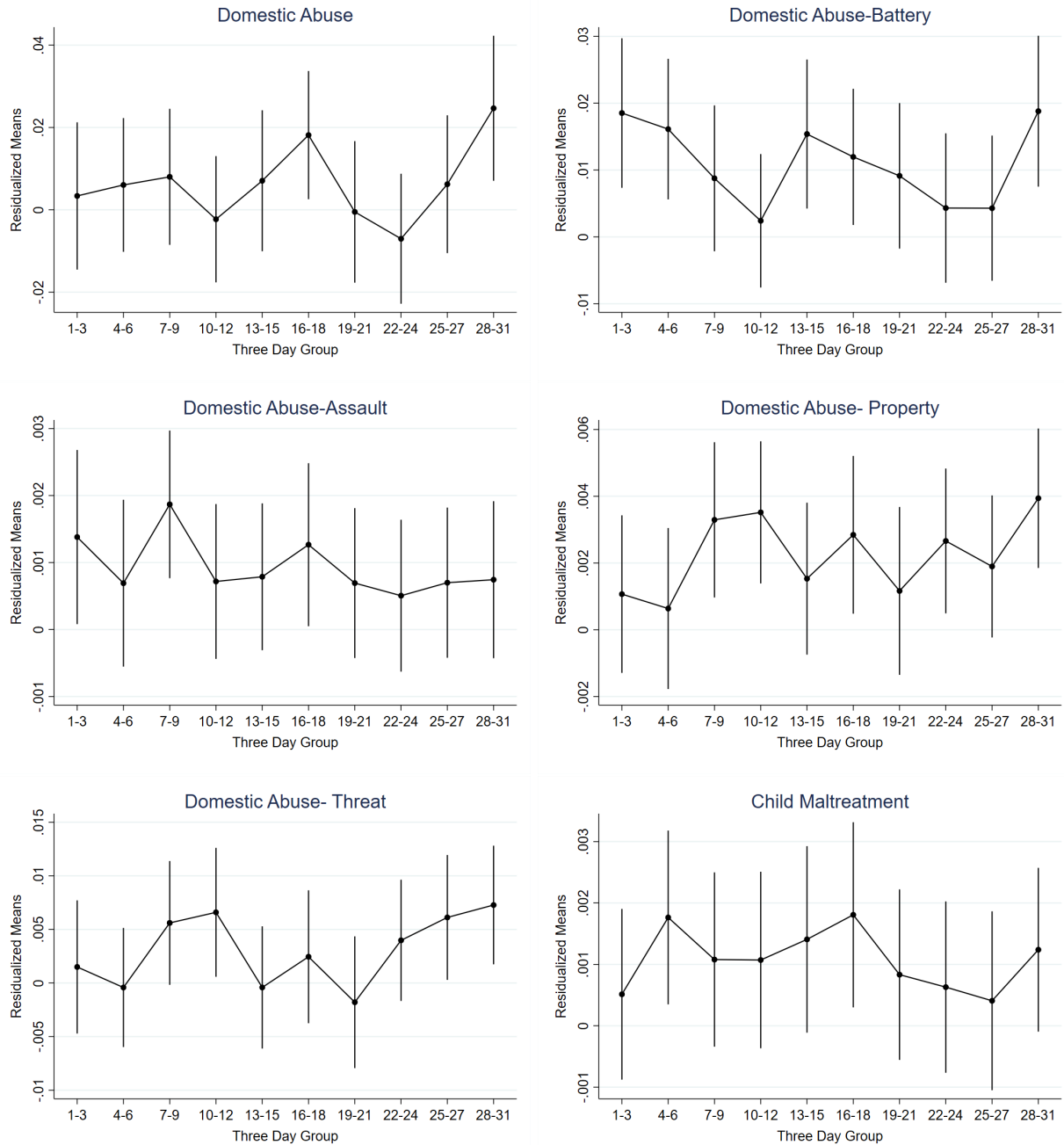

Notes: Each figure plots coefficients from Equation 1 using three day bins for each of the outcomes listed, using MSERD-optimal bandwidths. Crime data are from the city of Chicago from February 1, 2009 - January 31, 2011.

Figure A4: Effect of 2010 Illinois SNAP Disbursement Change on Domestic Violence Compared to 2008

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

Figure A5: Effect of Varying Bandwidth on RD Estimates
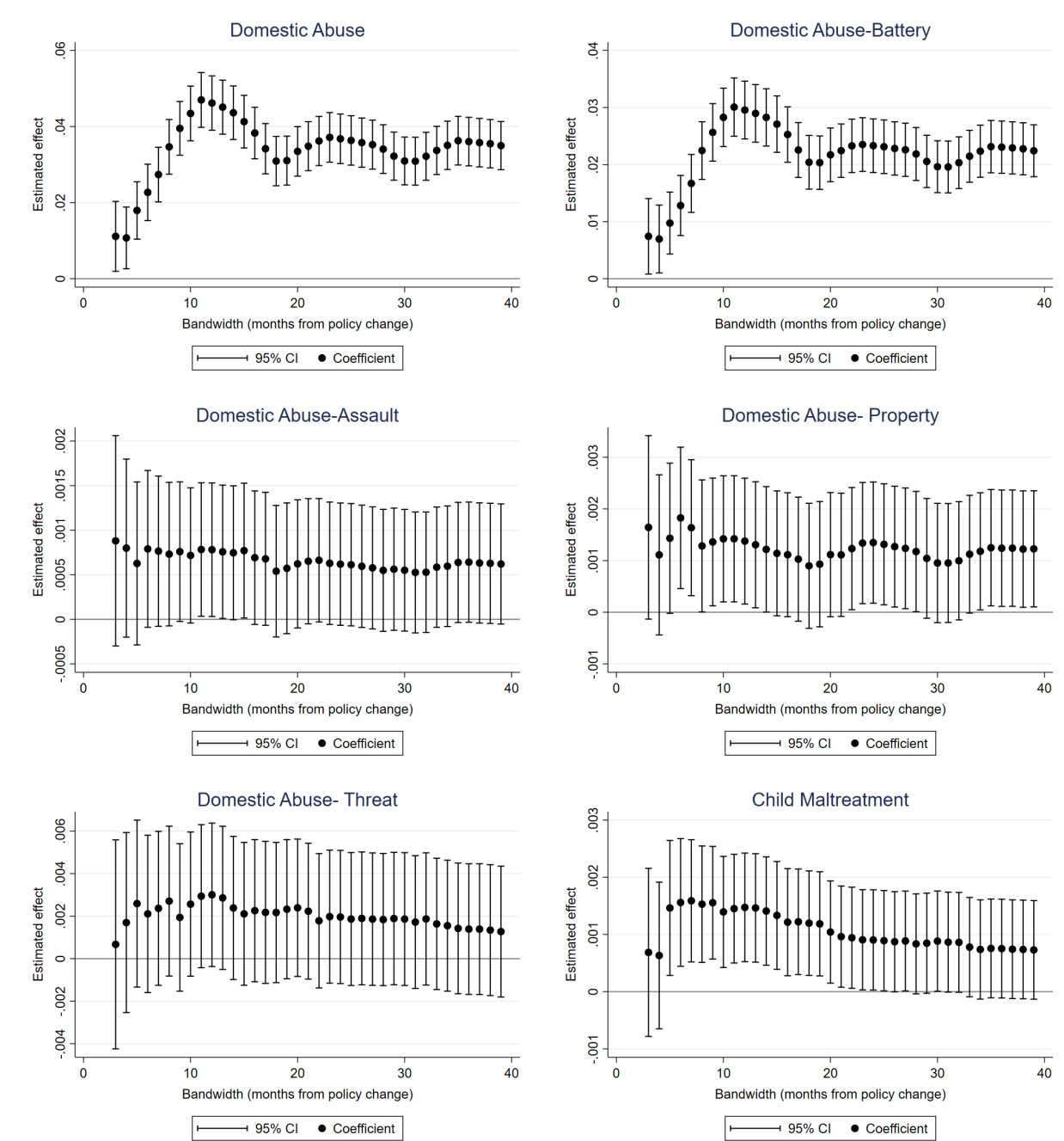
Notes: Each dot represents the coefficient of interest generated by a separate regression as specified by Equation 1. The various bandwidths on which these regressions were performed are represented on the x-axis. We also report the 95% confidence interval of the coefficient. Reported crime data are from the city of Chicago.

Figure A6: Effect of Illinois SNAP Disbursement Change on Weather
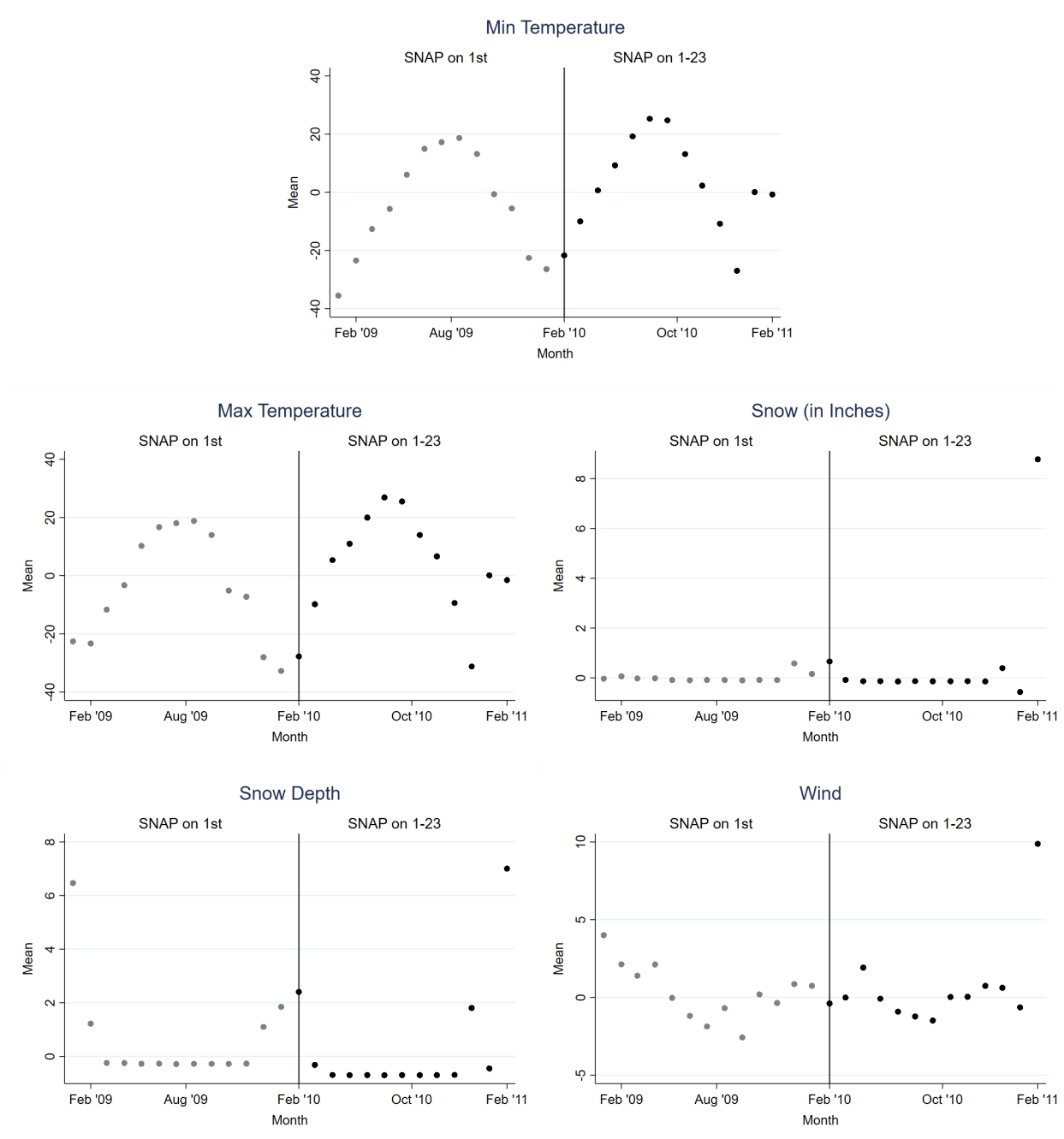
Notes: Each figure plots month-level means of daily weather patterns in the city of Chicago. Daily weather data for Chicago are from the Global Historical Climatology Network and are based on temperature, precipitation and average wind speeds from the Chicago O'Hare International Airport weather station.

Figure A7: Effect of Illinois SNAP Disbursement Change on Unemployment
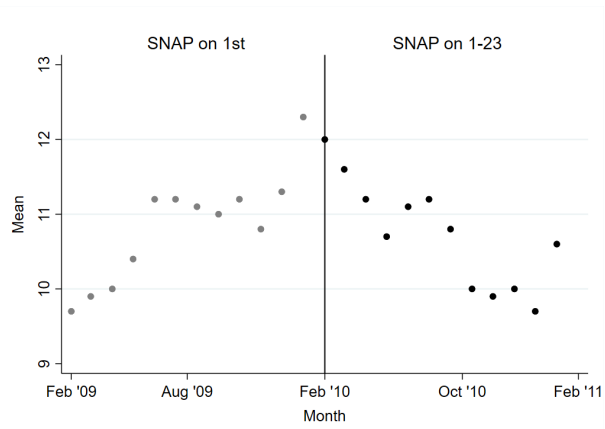
Notes: The figure plots month-level means of the monthly unemployment rate in Cook County. To the left of the vertical line, SNAP benefits were given out primarily on the 1st of the month, and to the right, they were distributed over the 1st to the 23rd. Monthly unemployment data are from the U.S. Bureau of Labor Statistics.