Do income transfer distribution schedules affect intimate partner violence? Evidence from the SNAP program

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Abstract

The Supplemental Nutrition Assistance Program (SNAP) is one of the largest federal welfare programs in the United States, providing funds to poor households to spend on food. While there is plentiful research on the effects (intended and otherwise) of SNAP funding, there is little research on how changes to the distribution of these cash-equivalent transfers affect household dynamics. We assess how changes in the SNAP distribution schedule (specifically, changes in the number of days between the first and last monthly distribution of benefits, or "stagger") affect intimate partner violence (IPV). We develop a theoretical framework for understanding the different effects SNAP stagger changes may have on IPV through stress frequency and stress intensity. In general, we find that increases to the length of the distribution stagger decrease non-lethal assault. However, we find little effect of changes to the SNAP stagger distribution length on IPV homicide. Our results suggest that policymakers would do well to consider the potential effects of changes to the SNAP distribution stagger on household violence before adjusting the length of the SNAP distribution. Further, policymakers should broadly consider how to alleviate the barriers to consumption smoothing that affect behavioral and expressive violence.

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Key Words: domestic violence; SNAP; welfare programs; family welfare

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1 Introduction

Domestic violence (DV), and intimate partner violence (IPV) specifically, is a pervasive problem affecting one in three women and one in four men (National Coalition Against Domestic Violence, 2020). From 1980 to 2008 one in every five homicides involved an intimate partner (Cooper and Smith, 2013). Despite extensive research into the causes and effects of IPV[1], prevention remains a seemingly insurmountable policy challenge. Identified risk factors include low income, unemployment, and salient emotional cues like stress (Costa et al., 2015). IPV disproportionately occurs in marginalized communities, including among/against people in poverty and those who are likely to draw on social services such as food assistance (Currie et al., 2020; Smith et al., 2017). As a result, it is of potential policy effectiveness to understand the ways in which modifications to social service provision, such as the timing of SNAP benefit distribution, influences IPV.

In this paper, we study how distributional changes to a particular type of social services, the Supplemental Nutrition Assistance Program (SNAP), affect the incidence of IPV. SNAP is the largest food assistance program in the United States, benefiting over 35 million low-income Americans in 2019.[2] In 2013, SNAP enrollment reached a historical peak enrollment of roughly 47.6 million people. SNAP participation has dropped in recent years, due both to the alleviation of poverty and to changes in eligibility requirements, though it has surged following shut-down orders related to the COVID-19 pandemic (Center on Budget and Policy Priorities, 2021).

We leverage data on changes to the SNAP distribution schedules across the United States to assess how changes in benefits timing affect family violence using a difference-in-differences identification strategy. We match data on homicide and non-lethal IPV to state-determined SNAP distribution schedules (and schedule changes) to identify the effects of changes on multiple dimensions of partner violence. Data on distribution schedules comes from the Environmental Research Service of the US Department of Agriculture (ERS). We use both the FBI's Uniform Crime Reporting Supplemental Homicide Reports (UCR-SHR) and the National Incident-Based Reporting System (NIBRS) for data on IPV-related crimes. The UCR-SHR allows us to identify all homicides perpetrated by individuals currently/formerly involved in intimate relationships (IPV). This represents the most conservative data approach as homicide is the least subject to measurement error of the forms of family violence (DeLeon-Granados and Wells, 2003). However, because homicide is an extreme, edge case of IPV, we also use NIBRS data to look at how these changes affect reports of non-lethal violence such as assault, stalking, and intimidation. We discuss how we overcome this measurement error

[1] In this paper, we focus specifically on violence between current and former intimate partners (IPV), which is a subset of DV. We are able to measure reported physical violence including homicide and assault, though IPV also includes non-physical forms of abuse such as financial or emotional abuse. For definitions of DV and IPV and a discussion of their overlap, see Breiding et al. (2015).

[2] Data source: USDA, Food and Nutrition Service. Available from: https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap.

and limitations in sections 6 and 8.

We use both difference-in-differences and event-study designs to estimate the relationship between the stagger length, changes to the distribution schedule, and lethal and non-lethal IPV. We find distinct effects of increases and decreases to the SNAP distribution stagger length on non-lethal IPV. Stagger increases decrease the rates of non-lethal IPV assault significantly, while stagger decreases increase the rate of non-lethal assault. We show our results are robust to exclusion of certain states and sample restrictions based on reporting frequency. We find little evidence of a change to IPV homicide, and we discuss hypotheses for why this may be the case.

There is an extensive body of literature on the effects of SNAP distributions on beneficiaries and their families (Goldin et al., 2019; Hastings and Washington, 2010). There is a smaller strand of literature that uses exogenous changes to SNAP distribution schedules to explore the effects of distribution timing on a number of outcomes. Prior work has found that across three US welfare programs, staggered frequent payments disincentivize crime by smoothing consumption for recipients (Foley, 2011). Evidence from Illinois and Indiana has shown that staggered SNAP disbursement reduced violent crime as well as theft, specifically at grocery stores (Carr and Packham, 2019).

We contribute to the literature on social service provision and IPV in three main ways. First, we provide the first wide-scale analysis of the effect of changes in SNAP distribution timing on IPV across the United States, whereas the current literature mainly focuses on evidence from only one or two states (As an exception, see (Xu, 2020)). Second, we identify the different effects of increases and decreases to stagger length, whereas the closest papers in the literature focus on unidirectional changes. Finally, we pair our empirical analysis with a novel theoretical framework that distinguishes between the effects of increased frequency of salient negative emotional cues and increased intensity of these cues.

This work has direct policy implications both for violence prevention policy and unintended consequences of changes to welfare distribution. Based on our finding, increasing the length of the distribution stagger significantly decreases the incidence of IPV, whereas decreasing the length of the stagger significantly increases rates of non-lethal IPV. The administrative costs of changing the SNAP distribution schedule may be substantially smaller than other methods of reducing IPV, making longer SNAP distributions a cost-effective method of IPV prevention or reduction. Meanwhile, our findings suggest that policymakers should use caution before reducing the length of the stagger to avoid unintended consequences of increased violence against poor individuals. Finally, policymakers should consider how changes to distribution schedules may affect the ability of already constrained households to consumption smooth across the month.

The rest of the paper proceeds as follows: section 2 provides an overview of the SNAP program, while 3 reviews the existing literature on SNAP distribution schedules and crime, particularly domestic violence.

Section 4 describes the theoretical framework we develop to disentangle the effects of changes in distribution patterns and the hypotheses it generates. Section 5 provides information on the data used in this paper, while section 6 describes our empirical estimation strategies. Results are presented in section 7, and section 8 concludes with a discussion of limitations and policy implications.

2 Policy context

The earliest iteration of SNAP began in 1939 as the Food Stamp Program, which allowed recipients to purchase surplus foods using government-issued stamps as a way of coping with national surpluses of certain food items. Over the four years that this early program operated, nearly 20 million people benefited from food stamps. In 1961, a pilot Food Stamp Program similar to the program we see today paved the way for the 1964 Food Stamp Act. This federal program quickly grew to enroll over 15 million people by 1974. The program's name changed to SNAP with the 2008 Farm Bill, and the federal government increased its commitment to the program by over $10B (Food and Nutrition Service, 2021a). As of September 2020, over 22 million households received SNAP benefits, comprised of over 42 million individuals receiving $7.9B in benefits (Food and Nutrition Service, 2021b).

SNAP benefits are issued at the household level rather than to individuals. Households are eligible when gross income is less than 130% of the federal poverty line, *and* net income is less than 100% of the poverty line for that household size[3]. In general, SNAP beneficiaries must provide proof of work or participate in work training programs, though this requirement is waived for some groups and was lifted during the COVID-19 pandemic. As of 2008, documented non-citizens became eligible for SNAP benefits (Food and Nutrition Service, 2021c). Further eligibility requirements may be set by the state of residence, including limits to total household assets.

Currently, benefits are disbursed using an electronic benefits transfer (EBT) card that operates like a debit card. Benefits are issued once per month, though the day of disbursement is set by the state.[4] The amount of the SNAP transfer (called the "allotment") is determined by household size, with maximum allotments for individuals of $234 and a family of four of $782. Benefits can only be spent at qualifying grocery stores, determined by either the inventory or sales of staple food items. Benefits may only be used to buy food items, though the definition of qualifying food items is limited. Benefits cannot be used to purchase liquor, cigarettes, supplements, hot foods, prepared foods ready for consumption, or other "non-food" items (Food and Nutrition Service, 2021c).

[3]Gross and net income caps are higher in Alaska and Hawaii.
[4]See table 1 for state-by-state details.

3 Literature review

Researchers have long been interested in the effects of policy mechanisms on beneficiaries and their communities. Much of this work has explored the effects of conditional and unconditional cash transfers, especially in development economics (Buller et al., 2016; Litwin et al., 2019). There has been some work on cash transfers and tax credits in the US, specifically using data from the Temporary Assistance for Needy Families (TANF) and Earned Income Tax Credit (EITC) programs (Averett and Wang, 2013, 2018).

Given its scope, there is a considerable body of research on specifically the effects of SNAP. Researchers have found that SNAP benefits do have the intended effect of increasing food expenditure and calorie consumption (Wilde and Ranney, 2000). However, the benefits of SNAP extend far beyond daily food consumption, including reducing overall food insecurity (Ratcliffe et al., 2011), improving child health outcomes (Kreider et al., 2012), decreasing mortality (Heflin et al., 2019), and reducing parenting stress (Wang et al., 2021). Other work is interested in the ways that outside institutions respond to SNAP cycles. As one example, Goldin et al. (2019) do not find evidence that grocery retailers adjust pricing to respond to SNAP-adjusted patterns of customer demand.

One reason why researchers are so interested in SNAP is that beneficiaries respond to the transfers in unique ways compared to other policy mechanisms. Researchers across disciplines have identified what is known as the "SNAP benefit cycle," wherein beneficiaries spend more of their benefits early in the month, leading to reduced consumption (both in terms of dollar value and in terms of caloric intake) toward the end of the month (for some examples, see Cotti et al. (2020); Hastings and Washington (2010); Kinsey et al. (2019)). Dorfman et al. (2019) and Shapiro (2005) find that a fraction of SNAP beneficiaries spend the majority of their benefits within four days of receipt. Those authors suggest policymakers adopt multiple disbursement days for a single recipient within a given month as a way of enforcing consumption smoothing. Economic theory is also evolving to adapt commonly used economic principles to the special case of SNAP beneficiaries. Why do beneficiaries spend their benefits so quickly? Smith et al. (2016) suggest that recipient households are characterized by short-run impatience and see SNAP benefits as fungible income. Wilde and Ranney (2000) find that SNAP beneficiaries have hyperbolic discounting preferences, rather than consumption smoothing over the month/lifetime.

There is a growing literature on the behavioral effects of SNAP on crime specifically. Tuttle (2019) finds that a policy ban preventing drug traffickers from accessing food stamps increases criminal recidivism in financially motivated crimes like theft after release. Xu (2020) uses data on SNAP staggers across 36 states and the District of Columbia to find that longer staggers (the same mechanism of action we study in this paper) decrease overall crime rates, particularly thefts, with effects driven by states with staggers of more than 15 days. Carr and Packham (2019) find that SNAP benefits staggering in Illinois decreased crime, specifically theft, in the weeks following the transfers. Those authors find evidence that theft rates swing back (and indeed increase) in the week preceding the upcoming transfer, likely as a result of resource constraints.

The work that ties most closely into our research is Carr and Packham (2020), which uses data from Illinois to show that a change to the SNAP distribution schedule in 2010 increased DV by 6.9% and child maltreatment by 30.0%.[5] We expand on the work in this paper in three substantial ways. First, we expand both spatial and temporal coverage by using a panel data set of IPV incidence covering 37 states over twenty years. Second, we examine the effects of both increases *and* decreases to the SNAP stagger length. There is reason to believe that the effect of an increase in the length of the distribution schedule would be of a different magnitude to moving to a single-day distribution. By leveraging policy changes across 37 states, we are able to distinguish between these effects.

Researchers across disciplines have spent recent decades attempting to understand risk factors for IPV. Identified risks include (but in no way are limited to) limited access to social networks (Dutton et al., 2004; Kirst et al., 2015; Lanier and Maume, 2009), access to and use of firearms and controlled substances (Campbell et al., 2003), histories of prior victimization (Costa et al., 2015; Dutton et al., 2006; Morrissey, 2003), fertility (Anderberg et al., 2018), and attempted separation (Morrissey, 2003). Unemployment is a widely studied predictor of IPV and DV, where male unemployment decreases the risk for IPV and female unemployment increases the same risk (Anderberg et al., 2016). The different directions of these effects are due to changes in relative power dynamics and reduced outside options for victims.

In this paper, we are particularly concerned about the effects of income and emotional cues on the likelihood of violence. Lower income has been consistently shown to increase DV prevalence (Anderberg et al., 2016; Benson et al., 2003; Carr and Packham, 2019; Fagan and Browne, 1994). Income shocks (specifically, cash transfers), on the other hand, have an ambiguous effect on family violence, with some researchers finding large and significant positive effects and others finding the opposite or no effect (Bobonis et al., 2013; Buller et al., 2016). Hidrobo and Fernald (2013) find that the direction of the effect of an unconditional cash transfer to mothers depends on the education differential between the recipient and their partner. Litwin et al. (2019) finds evidence that a transfer program in Brazil increased the likelihood that women separated from their partners, suggesting that income transfers can reduce violence by increasing

[5]Carr and Packham (2019) use an earlier legislation change in Illinois that happened in February 2010, which adjusted the proportion of cases that are processed on the first and other days of a month. As the authors describe, "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." Later, in July 2014, Illinois adopted a slightly less dispersed schedule that ranges from the 1st to the 20th. Therefore, in this study, we consider Illinois as a "stagger decrease" state.

agency. These results draw on the idea that violence is a tool of resource extraction.

Negative, salient emotional cues have been consistently found to increase the incidence of violence (Benson et al., 2003; Card and Dahl, 2011; Gibson et al., 2001). This is consistent with the aggression-frustration hypothesis, whereby poverty or income-related stress trigger aggression, including violence (Barlett and Anderson, 2013). In these cases, violence is considered "expressive" or a way to "relieve" the stress of the situation.

Researchers have identified other links between emotional or financial states and the likelihood of committing crime more broadly. Access to resources has been found to be negatively associated with crime, especially theft, using variation in EITC benefits levels (Lenhart, 2020), SNAP benefits (Carr and Packham, 2019), and variation from the US foreclosure crisis (Jones and Pridemore, 2012). Intuitively, these papers suggest that resource extraction (the ability to gain financial benefit from a crime) is a significant motive behind theft.

4 Conceptual framework

Next, we outline the conceptual framework used to develop hypotheses in this paper. We consider a household composed of an abuser and the abused partner where income and transfers are jointly received by the household. However, a full formal model is beyond the scope of this paper and should be an avenue for future theoretical work. Instead, we propose some possible channels through which changes to SNAP schedule can affect IPV incidence.

4.1 Congestion effects

If all beneficiaries in the community receive benefits on the same day, we might expect there to be congestion effects at grocery stores. In fact, Food Research and Action Center (2019) specifically recommends states adopt staggered distribution schedules to reduce this congestion. We can simply operationalize this congestion effect as reducing utility by decreasing total consumption (a beneficiary is unable to purchase everything they would like).

If we propose that congestion affects violence via reduced consumption (either because the abuser is using violence to extract resources from the victim or to express stress caused by congestion), by removing congestion effects (or simply reducing them), the average daily level of violence will also decrease. The level of violence that would "smooth" consumption in this case is, therefore, lower, leading us to the following hypothesis:

Hypothesis 1 (H1): *Lengthening the distribution schedule stagger will decrease the incidence of IPV by reducing congestion effects.*

This hypothesis should hold both for cases where there is a shift from a single-day distribution to a staggered distribution as well as when the length of the stagger increases.

4.2 Increased stress cycle frequency

As discussed in section 3, the SNAP benefits cycle has been well-documented. When benefits are distributed at the same time as income, the SNAP benefits cycle leads to one (large) pre-benefits period over which the household has to consumption-smooth. Because beneficiaries tend to spend benefits early in the cycle and are resource-constrained, often having expenses in excess of received income and transfers, this results in increased stress[6] later in the month.

We argue that changes to the distribution schedule should affect how the SNAP benefits cycle occurs, especially as it relates to other income cycles. Specifically, if SNAP distribution shifts away from paycheck distribution, then there would be two (smaller) cycles throughout the month. In the simplest case, if a beneficiary receives income on the first of the month and SNAP benefits on the fifteenth, then there are now two pre-payment periods over which the household has to smooth consumption. This should increase the incidence of stress during a representative month.

This increased frequency of stress leads us to our second hypothesis:

Hypothesis 2 (H2): *Shifting from a single-day (first of month) distribution to a multi-day distribution will increase IPV by increasing the frequency of pre-payment, high-stress periods.*

We test this hypothesis using the sub-sample of counties where benefits shifted from a single-day distribution to a staggered distribution. We also use an event-study estimation strategy to show how patterns of IPV change across the month. This prediction would be muddied by high rates of bimonthly pay periods. This should attenuate our effect, however, by increasing mid-cycle income.

4.3 Reduced maximum stress level

However, it is possible that the shift to a staggered distribution with more frequent cycles could have the opposite effect. Indeed, it is possible that this shift to fewer, less intense cycles spreads the experienced stress over multiple periods. This could reduce the maximum experienced negative emotional cue experienced

[6]Though in this section we refer to the outcome of the cycle as "stress" broadly, this can also be conceived of as a food insecurity effect.

during the month. If this is the case, we would expect a shift to a staggered distribution to reduce IPV. This leads us to the following hypothesis:

Hypothesis 3 (H3): *Shifting from a single-day distribution to a multi-day distribution will decrease IPV by decreasing the maximum negative emotional cue during high-stress periods.*

We test this hypothesis using the same estimation strategy as hypothesis 2. If we assume that the emotional cue threshold necessary for an abuser to commit lethal violence is higher than the necessary cue for non-lethal violence, then differences in the effect of changes on lethal and non-lethal violence may tell us about which mechanism is dominating. As we will discuss in section 5, we are able to separately analyze the effects of SNAP distribution schedules on lethal and non-lethal forms of domestic abuse. If our results are significant for IPV homicide but not non-lethal IPV outcomes, this would suggest that the maximum stress hypothesis is true. If non-lethal IPV outcomes are significant but homicide is not, then this suggests that more periods of stress are the dominant mechanism.

4.4 Resource extraction

As discussed in our review of the literature, there is evidence that some IPV is committed as a means of extracting resources from the abused person. One relevant example may be an abuser extracting some SNAP dollars for their own use, or (given the fungibility of SNAP benefits) extracting non-SNAP cash from the abused person. In this case, we would expect that changing the distribution of benefits will change the timing of abuse, not the incidence of it, leading to the following hypothesis:

Hypothesis 4 (H4): *If the dominant mechanism is resource extraction, changes to the SNAP distribution schedule will have no effect on IPV incidence once aggregated to the month or year.*

5 Data

5.1 SNAP distribution schedules

The main independent variable of interest is the timing of SNAP distributions, available from the SNAP policy database published by the USDA Economic Research Service. Changes to the distribution schedule, which is our treatment level of interest, happen at the state level. During our study period (2000-2017), three states (Montana, Oklahoma, and Virginia) changed from a non-staggered distribution to a staggered distribution schedule. Two states (Arkansas and Illinois) reduced the length of their stagger. Twelve states increased the length of their stagger. Three states (Idaho, Maryland, and Pennsylvania) experienced multiple changes during our observation period. For most analyses, we eliminate these three states. The remaining states had no changes in SNAP distribution, which are categorized simply as being staggered or non-staggered. The policy changes we use in this analysis are outlined in table 1.

5.2 IPV incidence data

We use two sources of data on IPV incidence to explore how changes in distribution scheduling affect family violence. The first comes from the FBI's Uniform Crime Reporting Supplemental Homicide Reports (UCR-SHR), which provides detailed information on homicides since 1984. The UCR-SHR defines the victim-perpetrator relationship, allowing us to categorize homicides as IPV (among individuals who are or have been involved in an intimate relationship including dating or marriage). However, IPV homicide is an extreme form of abuse. How do SNAP distribution schedule changes affect non-lethal forms of IPV? To assess this, we use data on IPV incidents reported by law enforcement to the National Incident-Based Reporting System (NIBRS). NIBRS data include information on a variety of non-lethal IPV incidents, including stalking, assault, sexual assault, and intimidation. It also disaggregates incidents by relationship between victim and offender, again allowing us to identify IPV cases.

We specifically use three different crime categories from NIBRS: assault, intimidation, and sexual violence. Intimidation includes threats as well as stalking. For the NIBRS data, to better identify the victim-offender relationship, we restrict our study sample to crime incidents with one victim and one offender only. Furthermore, we focus on incidents where the victim and the offender are in an intimate partner relationship, which includes current or former dating partners, common-law spouses, and current or former spouses.

For both the UCR-SHR and NIBRS data, we aggregate incident reports to county-month observations. We pair these incident-level data with county- and state-level control variables available from the US Census Bureau, the University of Kentucky Center for Poverty Research, and the Bureau of Labor Statistics. State-level controls include per-capita gross state product, real minimum wage, unemployment rate, Medicaid eligibility thresholds, SNAP and Medicaid recipients as a percentage of total state population, and an indicator for whether the state governor is a Democrat, available from the University of Kentucky Center for Poverty Research. We further include county-level SNAP benefit totals, available from the Food and Nutrition Service. We also control for agency-level population.

5.3 Summary statistics

Table 2 provides summary statistics for the different outcome variables used in our analysis by whether the state was a control state (no change), had an increase to the stagger length, or had a decrease to the stagger length. All three categories are roughly similar on the homicide outcome variables (Panel A), but counties in states that changed their stagger length tended to have higher non-lethal IPV overall than control states. Table 3 does the same comparison of means for included control variables. Control counties have larger population and have larger average weekly wages. Control counties also tend to be in states with higher state GDP, lower state-level poverty, and larger state populations.

6 Methodology

6.1 Event-study specifications

Before estimating a difference-in-differences regression, we check for pre-trends using event-study specifications. We do this to rule out the possibility of reverse causality, whereby counties in states that made changes to their distribution schedule were statistically significantly different from counties in states that made no change to their distribution.

We plot a series of event studies of IPV homicide and assault over time, measured in six-month bins before and after the observed change to the stagger distribution (figure 1). We find that the event studies pass the pre-trends test as we do not find any significant relationship between the IPV crime rate and the time bins prior to the change in the distribution schedule. We show this both for the entire sample of distribution changes and for only stagger distribution increases, to avoid concern that the effects in stagger-increase states are washed out by stagger-decrease states. Our results are robust to this division.

6.2 Linear specifications

We first estimate the relationship between distribution length (and changes to the distribution) and IPV using continuous difference-in-differences framework:

$$\begin{aligned} \mathrm{Y}_{c(i),t} = \alpha &+ \beta_1\{\text{SNAP Days}_{s,t}\} + \beta_2\text{Post}_{s,t} + \beta_3\{\text{SNAP Days}_{s,t}\} * \text{Post}_{s,t} \\ &+ \rho X_s + \delta Z_c + \gamma_{c(i)} + \lambda_t + \epsilon_{c(i),t}. \end{aligned} \tag{1}$$

Here $\mathrm{Y}_{c(i),t}$ is either lethal homicides or non-lethal IPV assaults per 100,000 population. SNAP $\text{Days}_{s,t}$ is the length of the stagger (intensity of treatment) subtracted by one day, meaning it takes a value of 0 for non-staggered month-years. $\text{Post}_{s,t}$ is an indicator variable equal to one if the observation is after the change from a single-day distribution to a staggered distribution. The coefficient of interest is therefore β_3. We include a series of state- and county-level control variables (X_s and Z_c), as well as county or agency fixed effect ($\gamma_{c(i)}$) and month-year fixed effect (λ_t). Following Abadie et al. (2017), we cluster standard errors at the level of treatment, the state. Because we have relatively few clusters, we follow Cameron et al. (2008) and bootstrap the standard errors. In this specification, we drop observations where the state distribution stagger length decreases.

6.3 Nonlinear specifications

It is reasonable to believe that the effect of changing from a single day distribution to a staggered distribution is different than increasing the length of an already a staggered distribution. As a result, we estimate a nonlinear specification, allowing for different trends depending on change type. We again interact these indicators for the type of change with an indicator for whether the observation is after the change. The estimation strategy is as follows:

$$\begin{aligned} Y_{c(i),t} = \alpha &+ \beta_0 \text{SNAP Days}_{s,t} + \beta_1 \mathbb{1}\{\text{Stagger Increase}_{s,t}\} * \text{Post}_{s,t} + \beta_4 \mathbb{1}\{\text{Stagger Decrease}_{s,t}\} * \text{Post}_{s,t} \\ &+ \rho X_s + \delta Z_c + \gamma_c + \lambda_t + \epsilon_{c(i),t}. \end{aligned} \tag{2}$$

Here, $\text{Y}_{c(i),t}$ is the IPV incident rate, which can be lethal homicides or non-lethal cases per 100,000 population. $\text{Post}_{s,t}$ is an indicator variable equal to one if the observation is after the change in SNAP distribution, and 0 always for states which did not have any changes to their distribution. The two indicator variables indicate whether the observed change represents a lengthening of the stagger or a shortening of the stagger. The reference group here are single-day distributions. We interact these dummies with the SNAP policy change indicator. SNAP $\text{Days}_{s,t}$ is the length of the stagger (intensity of treatment), which takes a value of 0 for non-staggered month-years. We include the variable for the number of days over which benefits are distributed to differentiate between short and long staggers (e.g., 5 versus 20 days). Additionally, we include a series of state- and county-level control variables (X_s and Z_c), as well as county (γ_c) and month-year (λ_t) fixed effects. The coefficients of interest are β_1 through β_5.

We drop states that experienced multiple changes in the distribution schedule (Idaho, Maryland, and Pennsylvania). As a robustness test, we include Idaho and Maryland but limit their pre- and post-periods to five years before and after their changes in distribution schedules to allow for separate trends for each change. We always exclude Pennsylvania due to the repeated changes in distribution scheduling that make observations from Pennsylvania distinctly incomparable to those in other states.

We use the total number of days over which the distribution is staggered. Other research on the behavioral effects of SNAP distribution length have instead used the median date of the distribution (Cotti et al., 2021). Those authors are particularly interested in the likelihood that SNAP benefits are distributed closely to the first of the month, when SNAP transfers are received. Using the median day allows them to consider whether recipients are consumption smoothing across more or less lumpy income distribution timing.

In this paper, we are interested in both the *spread* of the distribution and its relationship to "typical" pay periods. There is strong (but imperfect) correlation between stagger length and median distribution day. For these reasons, we focus on the stagger length rather than an alternative measure.

7 Results

7.1 Lethal violence

When we consider treatment to be any change in the stagger (regardless of the direction of the change), we find that there are no effects of changes to the stagger length on the overall homicide rate or IPV homicide rate, both against victims of all genders and against women specifically (table A). This is intuitive if the effects of increases and decreases go in opposite directions, meaning any effect is washed out when we aggregate changes in all directions.

As a result, we estimate a specification including separate dummies for stagger length increase and decrease (table 5). When we do so, we find little to no effect of stagger changes on homicide (all homicide and IPV homicide). Stagger decreases are associated with a statistically significant decrease in IPV homicide of women on the order of 0.02-0.03 homicides per 100,000 population. Notably, stagger decreases increase overall homicide by roughly 20% (0.09-0.118 homicides per 100,000 population compared an outcome mean of 0.573). We take this as suggestive evidence that SNAP stagger decreases are operating through some channel that affects overall crime but not IPV, specifically.

7.2 Non-lethal violence

We find that changes in any direction to the stagger length have a small effect non-lethal violence, reducing the assault rate by between 0.01 and 0.08 events per 100,000 population (table 7). While any decrease in violence is important, these are small changes relative to the mean. However, when we do split the stagger length change variables into increases and decreases (table 6), we find distinct effects of increases and decreases on different types of non-lethal physical violence.

Stagger length increases overall reduce violent assaults, with a marginal effect of roughly 1 fewer assault per 100,000 population, an 11% decrease. The effects are larger for aggravated assault, with a reduction of 1 event compared to 1.3 average events per 100,000 population per month. Effects are comparable for victims of all genders as for female victims. Meanwhile, stagger rate decreases increase the rate of overall assault and simple assault, with respective effect sizes of 1.76 and 2.28 more incidents per 100,000 population. Here, we find that the effects on assault against women are larger than for the general population. Stagger decreases increase the overall assault rate against women by 19.5% and the simple assault rate by 31.9%.

We do find a positive and significant effect of stagger increases on sexual violence, but these are of a similar magnitude as the effect of any change to the distribution schedule regardless of direction, and therefore we do not interpret the results as significant.

7.3 Heterogeneity by gender

We further test for heterogeneity by gender of the victim. Norms and stigma around violence and victimization are highly gendered, which may result in differences in how Panel B in our main results table reports the relationship between the distribution schedule and IPV for women specifically. The majority of cases of reported non-lethal violence are against women, while fewer IPV homicides are against women. Our results do not change substantially when we limit results to violence against women.

7.4 Stagger decrease states

Only two states (Arkansas and Illinois) decreased their stagger length during the period of study. However, only one agency reported in the state of Illinois during the period of study. On the other hand, a total of 282 agencies in the state of Arkansas participated in NIBRS between 2000 and 2017. Not surprisingly, we find that our results are robust to excluding Illinois (table B7). Results dropping Arkansas (table B6) are not robust, but we are likely dramatically under-powered to identify an effect because this leaves a small number of reporting municipalities in the remaining stagger decrease state (Illinois). For a more detailed analysis of the effects of SNAP stagger in Illinois, see Carr and Packham (2019) and Carr and Packham (2020).

7.5 Selection

NIBRS data is voluntarily reported monthly by agencies, but some agencies report less than the full twelve months in a year. This leads to reasonable concerns about selection if agencies that report all twelve months are significantly different from agencies reporting less than the full twelve months. Indeed, agencies that report all twelve months are significantly different from those that report less than twelve months in the year.

We test for robustness based on agency reporting frequency. NIBRS data is voluntarily reported at the month level, and as a result most agencies do not report all twelve months of the year. There may be concern that agency reporting is endogenous to something else that would correlate with IPV incidence (e.g., if reporting is costly in terms of labor hours and there has been a local uptick in violence, then an agency may not prioritize reporting). We take the most conservative approach and limit analyses to those agencies that always reported in all twelve months of the year ("always-always reporters"). Results for the linear specification are provided in table B4, and results for the nonlinear specification are in table B5. We again find a small and negative effect of any stagger period change on assault, both in general and specifically against female victims. Results on the non-linear specification are robust and actually increase in magnitude. This follows as the dependent variable mean is larger in always-reporting agencies.

7.6 Alternative non-linear specification

It is possible that there is a non-linear relationship between the distribution length and crime outcomes. Specifically, if there is a "ideal" distribution length after which increases to the distribution length have decreasing or no further effects on the crime rate, then we would expect that estimating our regressions using a linear relationship between the stagger length and crime rate would miss key features of this relationship. To address this, we estimate a model where the independent variable of interest is a series of dummy variables indicating how many weeks the distribution is staggered over. Results are reported in appendix table 7. We find suggestive evidence that longer staggers have larger (negative) effects on the overall assault rate, relative to single-day distribution. However, because we only have representation from a few states in each of the bins, we do not expect to have sufficient statistical power to distinguish between the magnitudes.

7.7 Threats to identification

A reader may be concerned that other state and local policies aimed at reducing poverty and IPV may interact with our results. However, because we focus on changes after the year 2000, we are observing a period after many of these changes took place. Specifically, our sample occurs after many cities adopted "no-drop" orders preventing victims form withdrawing IPV charges once filed (Aizer and Dal Bó, 2009) and mandatory arrest laws for individuals accused of IPV offenses (Sims, 2021).

Additionally, it is important that we account for the role of IPV services, such as emergency shelters, in changing patterns of violence.[7] These shelters and support services may be seen as an "outside option" for victims fleeing violence rather than staying in a dangerous situation. So long as changes in the availability of the outside option does not correlate with changes in the SNAP distribution schedule, then this should not bias our results.

Finally, it is possible that state distributions changed before the period of study. If we treat all states as

[7] For further detail on these shelters, see Sims (2021).

having not changed their distribution schedule before 1998 (when our sample begins), then we are assuming that states that did have stagger changes before 1998 are actually untreated. If these changes before our study period occurred *and* these pre-1998 changes cause effects during our sample window, this will likely bias our results down, making our estimates a lower bound.

8 Conclusion and discussion

Cash transfer programs have the propensity to affect behavior, including family violence, via congestion effects, changes to the frequency and intensity of poverty-induced stress, and violence used to extract resources (Buller et al., 2016; Litwin et al., 2019; Averett and Wang, 2013, 2018). In this study, we contributes to the literature on the behavioral effects of SNAP, one influential food assistance program, on crime (Carr and Packham, 2019, 2020; Xu, 2020). We test whether changes to the length of the SNAP stagger have an effect on the incidence of lethal and non-lethal IPV. We find a substantial effect of changes to the SNAP distribution schedule on violent, non-lethal intimate partner violence. Increasing the length of the stagger reduced the incidence of IPV assault by up to 1.2 events per 100,000 population, representing a 13% decrease in the IPV assault rate. We find a comparable and positive effect of stagger decreases on IPV assault. We do not find evidence for a significant effect of SNAP distribution schedule changes to the IPV homicide rate. Our results are robust to different inclusion and exclusion criteria.

8.1 Contributions

This paper contributes to the literature in several ways. First, we present a novel theoretical framework that distinguishes between the effects of increased frequency of salient negative emotional cues and increased intensity of these cues. Empirically, we provide a major improvement on coverage for research of the implications of SNAP distribution schedule changes on crime, specifically IPV, compared with existing studies (with Xu (2020) as one exception). Using both the NIBRS and the UCR-SHR data, we are able to speak to how multiple types of distribution changes affect violence, and we distinguish between the unique effects of lethal and non-lethal IPV.

Moreover, rather than focusing on the unidirectional effect of any change to SNAP stagger length, we identify the different effects of increases and decreases to stagger length. This has direct policy implications both for social service provision and supporting victim/survivors of IPV. Changing the length of the stagger in SNAP benefit issuance is a relatively low-cost approach to reducing IPV. We estimate that increasing the length of the stagger reduced the incidence of IPV assault by up to 1.2 events per 100,000 population, or a 13% decrease in the IPV assault rate. Policymakers face the challenge of considering unintended consequences of program implementation on the individuals they serve, especially multiply-marginalized communities. This work shows that the effect of program implementation on individuals experiencing violence should also be considered. Additionally, support services for victim/survivors, including IPV/DV hotlines, emergency shelters (Sims, 2021), and housing assistance programs, should consider scheduling support to account for how victimization may cycle with distribution schedules.

8.2 Limitations

Measurement error is a major concern in all research on IPV. There are many reasons for the significant under-reporting of violence, including stigma about victimization, desire to protect the abuser or children, and views about what does and does not constitute abuse. As a result, the non-lethal incidents we observe in the NIBRS data are likely a select sample of IPV incidents. To address this, we use the UCR-SHR data as homicide is less subject to measurement error (DeLeon-Granados and Wells, 2003). However, misidentification of the relationship between victim and offender can lead to measurement error even in homicide data. Further, both the NIBRS and UCR-SHR data are an unbalanced panel.

In addition to possible measurement errors on IPV incidence, the effect of changes to the state-level SNAP disbursement schedule on IPV incidence may be confounded by many local, state, and federal laws. Such policies include mandatory arrest laws for individuals accused of violence and required firearms surrender for anyone under a temporary protection order.[8] In this work, we do not separately analyze the effects of these co-occurring laws. Federal laws (such as the 1996 Lautenberg Amendment to the Federal Gun Control Act of 1968, which prevents individuals convicted of misdemeanor domestic violence from owning or purchasing firearms) will be subsumed in our month-year fixed effects. Because our variation of interest occurs at the state-year level (the same level as state laws relating to IPV), we are unable to control for these changes using fixed effects. Future work should look explicitly into how state policy regarding SNAP and IPV interact.

8.3 Future research

We develop a framework that distinguishes between the effects of reduced congestion, increased stress, and increased frequency of stress on IPV incidence. While we use this to motivate the hypotheses we develop for how changes to the SNAP distribution stagger may affect IPV, this framework disentangles many effects that are broadly addressed in the economics of crime literature. Future work should develop this framework into a formal model to demonstrate how a game theoretic model of a non-cooperative household that operationalizes IPV/DV mechanisms known in other literatures compare to traditional household bargaining models of

[8] For an overview of policy changes relevant to IPV and DV incidence, including mandatory versus discretionary arrest laws, firearms restrictions, and emergency shelter policy, see Sims (2021).

violence.

Additionally, we provide a broad overview of the effect of SNAP timing changes on IPV. We anticipate a role for deep, case-specific and qualitative work to understand how individuals facing income constraints who receive SNAP benefits understand the relationship between distribution timing and timing of other forms of income. This would enable further refinement of the conceptual framework we pose in this paper and drive application of the framework to other forms of anti-poverty aid such as the Temporary Assistance for Needy Families (TANF) and Women, Infants, and Children (WIC) programs.

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A Tables and figures

Table 1: Summary of SNAP issuance policy changes by state (1998-2018)

Categories	Empirical sample	States
Non-staggered to staggered	Treatment group	MT, OK, VA
Stagger length increase	Treatment group	AL, DE, FL, GA, IN, KY, MI, MS, NC, OH, SC, TN
Stagger length decrease	Treatment group	AR, IL
Multiple changes	Dropped	ID, MD, PA
No change & non-staggered	Control group	AK, NV, NH, ND, RI, SD, VT
No change & staggered	Control group	AZ, CA, CO, CT, DC, HI, IA, KS, LA, ME, MA, MN, MO, NE, NJ, NM, NY, OR, TX, UT, WA, WV, WI, WY

Source: SNAP policy database, published online by USDA, Economic Research Services. (1) ID: staggering period first decreased from 5 days to 1 day / non-staggered (August 2009), then increased to 10 days (July 2016) (2) MD: staggering period first increased from 5 to 10 days (December 2003), then increased to 15 days (September 2015), then increased to 20 days (October 2015) (3) PA: staggering mode changes almost every month starting from February 1999, but always staggered across over 10 days.

Table 2: Summary statistics of crime incidence by state-level SNAP distribution schedule

	(1) No-change states	(2) Stagger-increase states	(3) Stagger-decrease states
Panel A: County-level homicide incidence			
Overall homicide rate	0.556	0.600	0.739
	(2.697)	(1.885)	(2.172)
IPV-related homicide rate	0.099	0.095	0.086
	(1.237)	(0.778)	(0.778)
Overall homicide rate with female victims	0.173	0.172	0.201
	(1.565)	(1.007)	(1.160)
IPV-related homicide rate with female victims	0.073	0.067	0.061
	(1.059)	(0.621)	(0.625)
Observations	165,720	154,272	11,652
Panel B: Agency-level non-lethal crime rate			
IPV assault rate	6.588	12.046	12.726
	(10.003)	(14.429)	(14.062)
IPV simple assault rate	5.589	10.459	10.219
	(8.916)	(12.837)	(12.579)
IPV aggravated assault rate	0.999	1.587	2.507
	(2.556)	(3.408)	(3.793)
IPV intimidation rate	0.794	1.958	3.611
	(2.244)	(4.099)	(5.604)
IPV sexual assault rate	0.169	0.188	0.166
	(0.713)	(0.800)	(0.631)
IPV assault rate with female victimss	4.520	8.331	9.757
	(7.893)	(11.718)	(12.083)
IPV simple assault rate on female victims	3.834	7.295	7.941
	(7.046)	(10.503)	(10.912)
IPV aggravated assault rate with female victims	0.687	1.035	1.815
	(2.067)	(2.697)	(3.207)
IPV intimidation rate with female victims	0.575	1.422	2.802
	(1.803)	(3.320)	(4.787)
IPV sexual assault rate with female victims	0.138	0.157	0.148
	(0.629)	(0.747)	(0.606)
Observations	133,948	149,239	8,725

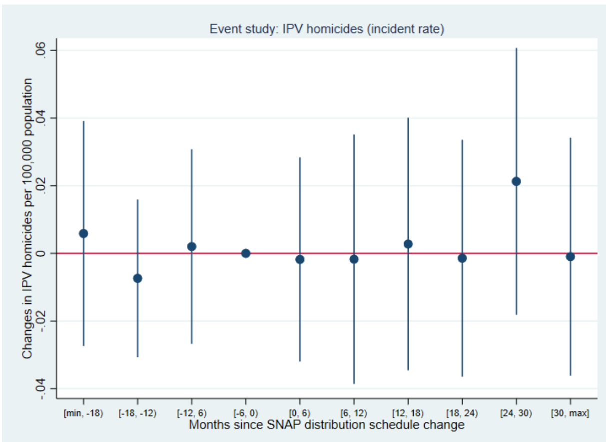
Note: Crime rate is defined as crime incident count per 100,000 population, where population is at the agency level. Standard errors in parentheses, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level.

Table 3: Summary statistics of county- and state-level controls by SNAP distribution schedule (UCR-SHR)

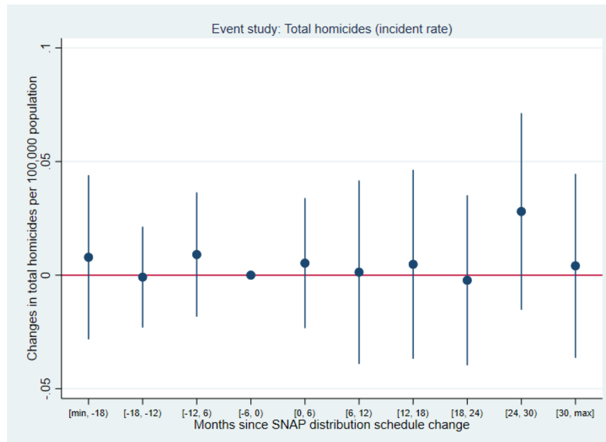
	(1) No-change states	(2) Stagger-increase states	(3) Stagger-decrease states
County unemployment rate	6.045	6.684	6.349
	(2.588)	(2.833)	(2.196)
County average weekly wage	689.674	635.197	616.592
	(212.018)	(156.647)	(175.178)
County population (in 100 thousands)	2.196	1.095	1.843
	(5.525)	(1.815)	(7.588)
Gross State Product per capita	47.384	41.202	37.398
	(11.925)	(8.408)	(11.338)
State minimum wage	6.563	6.047	6.402
	(1.592)	(1.177)	(1.260)
State unemployment rate	5.744	6.151	5.826
	(1.756)	(2.134)	(1.379)
State poverty rate	13.490	14.405	16.654
	(3.336)	(3.001)	(2.257)
Share of TANF enrollment in state population	1.446	1.158	0.659
	(1.054)	(0.735)	(0.362)
Share of SNAP enrollment in state population	10.604	11.967	13.291
	(4.373)	(4.263)	(2.453)
Share of Medicaid enrollment in state population	16.672	16.545	22.312
	(6.258)	(4.907)	(4.848)
Medicaid expansion (1=yes)	0.123	0.064	0.290
	(0.329)	(0.245)	(0.454)
State population (in 100 thousands)	112.806	70.951	41.735
	(111.185)	(27.221)	(33.957)
Demographic governor (1=yes)	0.417	0.425	0.333
	(0.493)	(0.494)	(0.471)
Observations	165,720	154,272	11,652

Note: Crime rate is defined as crime incident count per 100,000 population, where population is at the agency level. Standard errors in parentheses, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level.

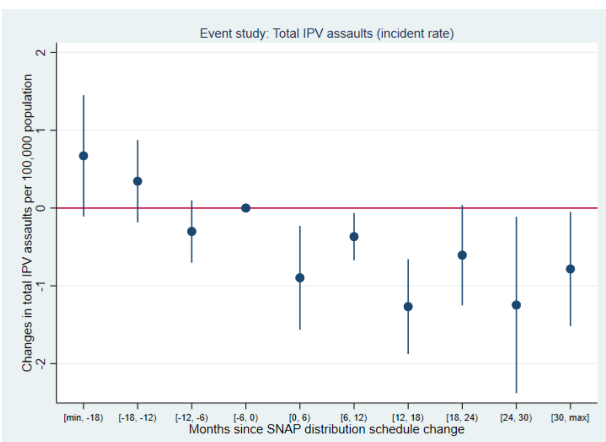
Figure 1: Event study of IPV homicide and assault before and after the change to the distribution stagger



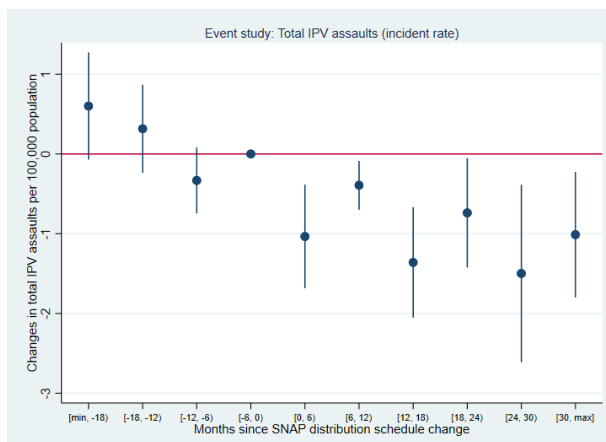
(a) IPV homicides (all states)



(b) IPV homicides (stagger increase states)



(c) IPV assault (all states)



(d) IPV assault (stagger increase states)

Notes: We define the six-month period prior to the announcement of a change in SNAP distribution schedule ($-6 \leq t < 0$) as the reference group.

Table 4: SNAP benefit staggering and intimate partner homicide (incident rate)

	Overall homicide rate			*IPV-related homicide rate*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All victims						
SNAP staggering period	-0.0022	-0.0028	-0.0034	0.0005	0.0003	0.0005
	(0.0070)	(0.0155)	(0.0068)	(0.0039)	(0.0033)	(0.0040)
post-schedule change	0.0231	0.0228	0.0073	0.0190	0.0216	0.0146
	(0.1102)	(0.2370)	(0.1212)	(0.0537)	(0.0624)	(0.0672)
SNAP staggering period × post-schedule change	-0.0010	-0.0009	-0.0002	-0.0017	-0.0019	-0.0013
	(0.0072)	(0.0159)	(0.0089)	(0.0032)	(0.0039)	(0.0044)
Outcome mean	0.573	0.573	0.573	0.094	0.094	0.094
Panel B: Female victims						
SNAP staggering period	-0.0019	-0.0021	-0.0022	-0.0006	-0.0007	-0.0008
	(0.0039)	(0.0042)	(0.0058)	(0.0033)	(0.0026)	(0.0036)
post-schedule change	0.0049	0.0077	-0.0063	0.0185	0.0198	0.0102
	(0.0975)	(0.0769)	(0.1003)	(0.0530)	(0.0640)	(0.0583)
SNAP staggering period × post-schedule change	0.0003	0.0001	0.0011	-0.0011	-0.0012	-0.0004
	(0.0064)	(0.0050)	(0.0066)	(0.0035)	(0.0042)	(0.0036)
Outcome mean	0.171	0.171	0.171	0.068	0.068	0.068
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-level controls	No	Yes	Yes	No	Yes	Yes
State-level controls	No	No	Yes	No	No	Yes
Observations	301348	301084	284500	301348	301084	284500

The outcome variable of interest is the homicide rate, defined as number of incidents per 100,000 agency population. Standard errors reported in parentheses are clustered at the state level , where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes).

Table 5: SNAP benefit issuance policy change and intimate partner homicide (incident rate)

	Overall homicide rate			*IPV-related homicide rate*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All victims						
SNAP staggering period	-0.0002	-0.0008	-0.0015	-0.0005	-0.0009	-0.0005
	(0.0066)	(0.0069)	(0.0071)	(0.0062)	(0.0060)	(0.0060)
post × stagger increase	-0.0118	-0.0103	-0.0127	0.0022	0.0028	0.0036
	(0.0571)	(0.0605)	(0.0608)	(0.0549)	(0.0526)	(0.0525)
post × stagger decrease	0.1457***	0.1392***	0.0968***	-0.0133	-0.0169	-0.0238
	(0.0202)	(0.0218)	(0.0241)	(0.0140)	(0.0131)	(0.0164)
Outcome mean	0.573	0.573	0.573	0.094	0.094	0.094
Panel B: Female victims						
SNAP staggering period	-0.0010	-0.0014	-0.0012	-0.0013	-0.0015	-0.0015
	(0.0053)	(0.0066)	(0.0070)	(0.0057)	(0.0061)	(0.0076)
post × stagger increase	0.0016	0.0018	0.0012	0.0086	0.0090	0.0102
	(0.0466)	(0.0583)	(0.0631)	(0.0501)	(0.0529)	(0.0684)
post × stagger decrease	0.0496***	0.0455***	0.0350*	-0.0078	-0.0100	-0.0170
	(0.0150)	(0.0174)	(0.0204)	(0.0126)	(0.0148)	(0.0185)
Outcome mean	0.171	0.171	0.171	0.068	0.068	0.068
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-level controls	No	Yes	Yes	No	Yes	Yes
State-level controls	No	No	Yes	No	No	Yes
Observations	301348	301084	284500	301348	301084	284500

The outcome variable of interest is the crime rate, defined as number of incidents per 100,000 agency population. Standard errors are reported in parentheses and clustered at the state level, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes). ID, MD and PA agencies are excluded.

Table 6: SNAP benefit staggering period and non-lethal IPV assaults (NIBRS crime outcomes, incident rate)

	(1) overall assault	(2) simple assault	(3) aggravated assault	(4) intimidation	(5) sexual violence
Panel A: All victims					
SNAP staggering period	-0.0735***	-0.0818***	0.0083	0.0064	0.0011
	(0.0240)	(0.0252)	(0.0168)	(0.0107)	(0.0013)
post-schedule change	-2.1041***	-2.1928***	0.0887	0.2644	0.0055
	(0.7239)	(0.6899)	(0.3720)	(0.1714)	(0.0148)
SNAP staggering period × post-schedule change	0.0819	0.1108*	-0.0289	-0.0486***	-0.0006
	(0.0580)	(0.0547)	(0.0385)	(0.0141)	(0.0014)
Outcome mean	9.612	8.263	1.349	1.461	0.185
Panel B: Female victims					
SNAP staggering period	-0.0559***	-0.0665***	0.0106	0.0047	-0.0001
	(0.0202)	(0.0198)	(0.0122)	(0.0086)	(0.0011)
post-schedule change	-1.7515***	-1.8723***	0.1208	0.1821	-0.0047
	(0.5717)	(0.5129)	(0.2792)	(0.1438)	(0.0125)
SNAP staggering period × post-schedule change	0.0764	0.1019**	-0.0255	-0.0353**	0.0004
	(0.0517)	(0.0414)	(0.0297)	(0.0130)	(0.0012)
Outcome mean	6.660	5.756	0.904	1.067	0.154
Agency FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
County-level controls	Yes	Yes	Yes	Yes	Yes
State-level controls	Yes	Yes	Yes	Yes	Yes
Observations	248384	248384	248384	248384	248384

The outcome variable of interest is the crime rate, defined as number of incidents per 100,000 agency population. Standard errors are reported in parentheses and clustered at the state level, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes). ID, MD and PA agencies are excluded.

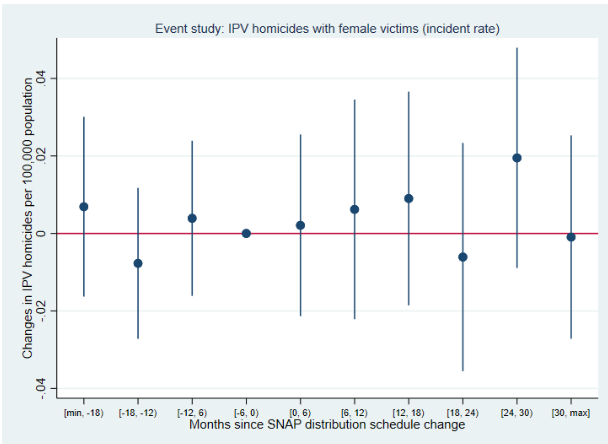
Table 7: SNAP benefit issuance policy change and non-lethal IPV assaults (NIBRS crime outcomes, incident rate)

	(1) overall assault	(2) simple assault	(3) aggravated assault	(4) intimidation	(5) sexual violence
Panel A: All victims					
SNAP staggering period	-0.0294	-0.0290	-0.0004	-0.0103	0.0007
	(0.0196)	(0.0201)	(0.0092)	(0.0068)	(0.0010)
post-schedule change × stagger increase	-1.2467***	-0.9547***	-0.2920***	-0.3525***	-0.0006
	(0.1604)	(0.1602)	(0.0615)	(0.0637)	(0.0078)
post-schedule change × stagger decrease	1.5915**	1.3245**	0.2670	0.1143	-0.0318
	(0.7732)	(0.6148)	(0.4008)	(0.6688)	(0.0288)
Outcome mean	9.612	8.263	1.349	1.461	0.185
Panel B: Female victims					
SNAP staggering period	-0.0175	-0.0194	0.0019	-0.0073	-0.0001
	(0.0142)	(0.0153)	(0.0071)	(0.0053)	(0.0009)
post-schedule change × stagger increase	-0.9207***	-0.7171***	-0.2036***	-0.2665***	0.0020
	(0.1334)	(0.1422)	(0.0478)	(0.0464)	(0.0073)
post-schedule change × stagger decrease	1.0959*	1.0324**	0.0635	0.0958	-0.0316
	(0.6647)	(0.5161)	(0.3562)	(0.5511)	(0.0294)
Outcome mean	6.660	5.756	0.904	1.067	0.154
Agency FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
County-level controls	Yes	Yes	Yes	Yes	Yes
State-level controls	Yes	Yes	Yes	Yes	Yes
Observations	248384	248384	248384	248384	248384

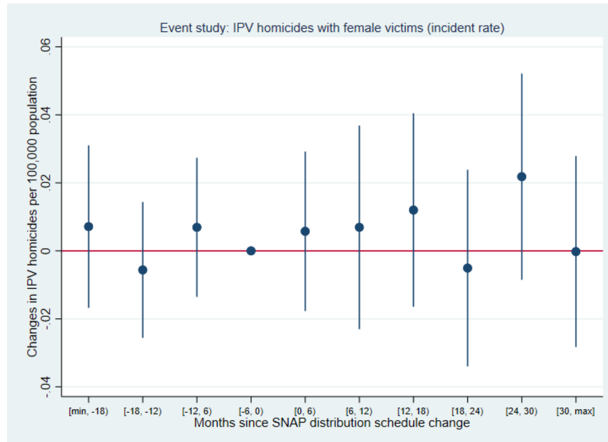
The outcome variable of interest is the crime rate, defined as number of incidents per 100,000 agency population. Standard errors are reported in parentheses and clustered at the state level, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes). ID, MD and PA agencies are excluded.

B Supplemental tables and figures

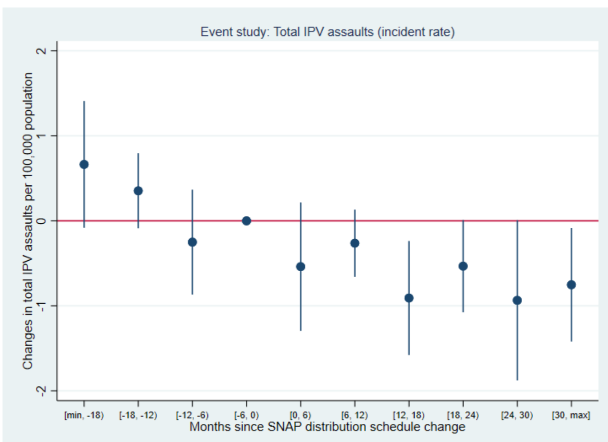
Figure B1: Event study of IPV homicide and assault before and after the change to the distribution stagger



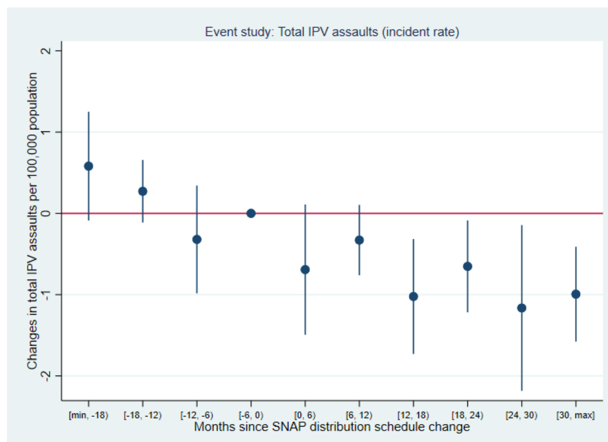
(a) IPV homicides against women (all states)



(b) IPV homicides against women (stagger increase states)



(c) IPV assault against women (all states)



(d) IPV assault against women (stagger increase states)

Notes: We define the six-month period prior to the announcement of a change in SNAP distribution schedule ($-6 \leq t < 0$) as the reference group.

Table B1: Summary statistics of county- and state-level controls by SNAP distribution schedule (NIBRS)

	(1) No-change states	(2) Stagger-increase states	(3) Stagger-decrease states
County unemployment rate	5.758	7.106	6.884
	(2.166)	(2.800)	(2.008)
County average weekly wage	795.889	706.305	659.401
	(223.534)	(163.557)	(126.234)
County population (in 100 thousands)	4.021	2.866	1.078
	(4.955)	(4.505)	(1.104)
Gross State Product per capita	51.446	43.012	36.689
	(11.875)	(7.178)	(4.207)
State minimum wage	7.141	6.577	6.522
	(1.618)	(1.195)	(0.989)
State unemployment rate	5.413	6.429	6.119
	(1.733)	(2.249)	(1.525)
State poverty rate	11.764	13.730	17.114
	(2.757)	(2.590)	(1.926)
Share of TANF enrollment in state population	1.235	1.407	0.553
	(0.629)	(0.722)	(0.178)
Share of SNAP enrollment in state population	10.397	12.836	14.785
	(4.249)	(4.224)	(1.735)
Share of Medicaid enrollment in state population	16.333	17.880	24.505
	(5.859)	(5.294)	(3.928)
Medicaid expansion (1=yes)	0.207	0.143	0.319
	(0.405)	(0.350)	(0.466)
State population (in 100 thousands)	54.360	79.843	30.774
	(56.435)	(27.890)	(12.630)
Demographic governor (1=yes)	0.496	0.432	0.584
	(0.500)	(0.495)	(0.493)
Observations	133,948	149,239	8,725

Note: Crime rate is defined as crime incident count per 100,000 population, where population is at the agency level. Standard errors in parentheses, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level.

Table B2: Summary statistics of non-lethal crime incidents by agency reporting frequency to NIBRS

	(1) Always-reporting agencies	(2) Not-always-reporting agencies	(3) Difference
IPV assault rate	15.775	7.775	8.000***
	(16.061)	(11.177)	(0.059)
IPV simple assault rate	13.605	6.670	6.935***
	(14.432)	(9.919)	(0.053)
IPV aggravated assault rate	2.170	1.105	1.065***
	(3.462)	(2.921)	(0.015)
IPV intimidation rate	2.121	1.289	0.832***
	(3.816)	(3.398)	(0.017)
IPV sexual assault rate	0.249	0.159	0.090***
	(0.664)	(0.782)	(0.004)
IPV assault rate with female victims	10.693	5.456	5.237***
	(13.127)	(9.058)	(0.048)
IPV simple assault rate with female victims	9.316	4.696	4.620***
	(11.871)	(8.072)	(0.043)
IPV aggravated assault rate with female victims	1.376	0.760	0.616***
	(2.689)	(2.372)	(0.012)
IPV intimidation rate with female victims	1.536	0.944	0.592***
	(3.107)	(2.752)	(0.014)
IPV sexual assault rate with female victims	0.201	0.133	0.068***
	(0.603)	(0.716)	(0.003)
Observations	66,060	225,852	291,912

Note: Crime rate is defined as crime incident count per 100,000 population, where population is at the agency level. Standard errors in parentheses, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level.

Table B3: SNAP staggering period (by weeks) and non-lethal IPV assaults (NIBRS crime outcomes, incident rate)

	(1) overall assault	(2) simple assault	(3) aggravated assault	(4) intimidation	(5) sexual violence
Panel A: All victims					
Staggered within 7 days	-1.0075	-0.7305	-0.2770	0.1122	0.0314
	(1.1278)	(1.0400)	(0.2169)	(0.2991)	(0.0395)
Staggered within 8-14 days	-1.3043**	-1.5299**	0.2256	0.2824**	0.0181
	(0.5959)	(0.6064)	(0.1563)	(0.1283)	(0.0340)
Staggered across 8-14 days	-1.4984**	-1.6277**	0.1293	0.1027	0.0251
	(0.6428)	(0.6625)	(0.2100)	(0.1562)	(0.0366)
Staggered across 22 or more days	-1.9427	-1.1858	-0.7569**	-0.1677	-0.0508
	(1.1912)	(1.4547)	(0.3752)	(0.1683)	(0.0386)
post-schedule change	-0.9886***	-0.6958***	-0.2928***	-0.3708***	-0.0040
	(0.1672)	(0.1803)	(0.0684)	(0.0785)	(0.0078)
Outcome mean	9.612	8.263	1.349	1.461	0.185
Panel B: Female victims					
Staggered within 7 days	-0.9185	-0.7575	-0.1610	0.0524	0.0126
	(0.8731)	(0.7991)	(0.1559)	(0.1860)	(0.0265)
Staggered across 8-14 days	-1.0499**	-1.2922***	0.2423**	0.2074**	0.0018
	(0.4681)	(0.4384)	(0.1100)	(0.0923)	(0.0254)
Staggered across 15-21 days	-1.1367**	-1.3250***	0.1883	0.0748	0.0024
	(0.5053)	(0.4738)	(0.1457)	(0.1131)	(0.0269)
Staggered across 22 or more days	-1.6538	-1.1026	-0.5512*	-0.2206	-0.0524
	(1.3237)	(1.5418)	(0.2880)	(0.1642)	(0.0341)
post-schedule change	-0.7215***	-0.5029***	-0.2186***	-0.2791***	-0.0001
	(0.1434)	(0.1349)	(0.0576)	(0.0596)	(0.0071)
Outcome mean	6.660	5.756	0.904	1.067	0.154
Agency FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
County-level controls	Yes	Yes	Yes	Yes	Yes
State-level controls	Yes	Yes	Yes	Yes	Yes
Observations	248384	248384	248384	248384	248384

The outcome variable of interest is the crime rate, defined as number of incidents per 100,000 agency population. Standard errors are reported in parentheses and clustered at the state level, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes). ID, MD and PA agencies are excluded.

Table B4: SNAP benefit staggering period and non-lethal IPV assaults (incident rate; robustness: always-reporting agencies)

	(1) overall assault	(2) simple assault	(3) aggravated assault	(4) intimidation	(5) sexual violence
Panel A: All victims					
SNAP staggering period	-0.0757	-0.0845	0.0088	-0.0102	-0.0013
	(0.8020)	(0.2306)	(0.0990)	(0.1632)	(0.0056)
post-schedule change	-3.0180	-3.1736	0.1556	0.2826	0.0017
	(8.9301)	(5.6570)	(2.0317)	(2.5518)	(0.1590)
SNAP staggering period × post-schedule change	0.1348	0.1644	-0.0296	-0.0469	-0.0018
	(0.8568)	(0.3505)	(0.1266)	(0.1571)	(0.0099)
Outcome mean	15.594	13.453	2.141	2.060	0.257
Panel B: Female victims					
SNAP staggering period	-0.0757	-0.0845	0.0088	-0.0102	-0.0013
	(0.8020)	(0.2306)	(0.0990)	(0.1632)	(0.0056)
post-schedule change	-3.0180	-3.1736	0.1556	0.2826	0.0017
	(8.9301)	(5.6570)	(2.0317)	(2.5518)	(0.1590)
SNAP staggering period × post-schedule change	0.1348	0.1644	-0.0296	-0.0469	-0.0018
	(0.8568)	(0.3505)	(0.1266)	(0.1571)	(0.0099)
Outcome mean	10.559	9.196	1.363	1.493	0.208
Agency FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
County-level controls	Yes	Yes	Yes	Yes	Yes
State-level controls	Yes	Yes	Yes	Yes	Yes
Observations	56808	56808	56808	56808	56808

The outcome variable of interest is the crime rate, defined as number of incidents per 100,000 agency population. Standard errors are reported in parentheses and clustered at the state level, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes). ID, MD and PA agencies are excluded.

Table B5: SNAP benefit issuance policy change and non-lethal IPV assaults (incident rate; always-reporting agencies only)

	(1)	(2)	(3)	(4)	(5)
	overall assault	simple assault	aggravated assault	intimidation	sexual violence
Panel A: All victims					
SNAP staggering period	-0.0376	-0.0417	0.0041	-0.0208	-0.0008
	(0.6862)	(0.5719)	(0.2313)	(0.2347)	(0.0141)
post-schedule change × stagger increase	-1.9719	-1.5764	-0.3955	-0.4919	-0.0213
	(6.2642)	(5.0611)	(2.1315)	(2.1596)	(0.1295)
post-schedule change × stagger decrease	7.3029	4.3124	2.9905***	4.8113	-0.0506
	(6.7800)	(5.9464)	(0.9321)	(3.2591)	(0.0957)
Outcome mean	15.594	13.453	2.141	2.060	0.257
Panel B: Female victims					
SNAP staggering period	-0.0234	-0.0263	0.0029	-0.0183	-0.0019
	(0.5921)	(0.2430)	(0.1616)	(0.1827)	(0.0111)
post-schedule change × stagger increase	-1.4298	-1.1823	-0.2474	-0.3662	-0.0198
	(5.2139)	(2.2184)	(1.4796)	(1.6680)	(0.1008)
post-schedule change × stagger decrease	4.0458	2.1341	1.9117***	3.7684	-0.0621
	(5.0045)	(4.7738)	(0.4161)	(2.5452)	(0.0770)
Outcome mean	10.559	9.196	1.363	1.493	0.208
Agency FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
County-level controls	Yes	Yes	Yes	Yes	Yes
State-level controls	Yes	Yes	Yes	Yes	Yes
Observations	56808	56808	56808	56808	56808

The outcome variable of interest is the crime rate, defined as number of incidents per 100,000 agency population. Standard errors are reported in parentheses and clustered at the state level, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes). ID, MD and PA agencies are excluded.

Table B6: SNAP benefit issuance policy change and non-lethal IPV assaults (incident rate; dropping AR agencies)

	(1) overall assault	(2) simple assault	(3) aggravated assault	(4) intimidation	(5) sexual violence
Panel A: All victims					
SNAP staggering period	-0.0294	-0.0306	0.0012	-0.0106	0.0007
	(0.0200)	(0.0214)	(0.0094)	(0.0066)	(0.0010)
post-schedule change × stagger increase	-1.2601***	-0.9589***	-0.3012***	-0.3557***	-0.0005
	(0.1588)	(0.1723)	(0.0593)	(0.0652)	(0.0082)
post-schedule change × stagger decrease	-3.9329***	-5.7106***	1.7776***	-0.4825***	0.0932***
	(0.2091)	(0.2177)	(0.0890)	(0.0745)	(0.0096)
Outcome mean	9.538	8.221	1.317	1.399	0.186
Panel B: Female victims					
SNAP staggering period	-0.0191	-0.0223	0.0032	-0.0088*	-0.0001
	(0.0148)	(0.0162)	(0.0072)	(0.0053)	(0.0009)
post-schedule change × stagger increase	-0.9303***	-0.7187***	-0.2115***	-0.2661***	0.0022
	(0.1374)	(0.1408)	(0.0496)	(0.0500)	(0.0074)
post-schedule change × stagger decrease	-4.1697***	-5.6769***	1.5071***	-0.3969***	0.0616***
	(0.1511)	(0.1669)	(0.0668)	(0.0548)	(0.0086)
Outcome mean	6.582	5.703	0.879	1.016	0.154
Agency FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
County-level controls	Yes	Yes	Yes	Yes	Yes
State-level controls	Yes	Yes	Yes	Yes	Yes
Observations	241257	241257	241257	241257	241257

The outcome variable of interest is the crime rate, defined as number of incidents per 100,000 agency population. The sample excludes Arkansas agencies. Standard errors are reported in parentheses and clustered at the state level, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes). ID, MD and PA agencies are excluded.

Table B7: SNAP benefit issuance policy change and non-lethal IPV assaults (incident rate; dropping IL agencies)

	(1) overall assault	(2) simple assault	(3) aggravated assault	(4) intimidation	(5) sexual violence
Panel A: All victims					
SNAP staggering period	-0.0300	-0.0298	-0.0002	-0.0103	0.0007
	(0.0207)	(0.0207)	(0.0095)	(0.0065)	(0.0010)
post-schedule change × stagger increase	-1.2470***	-0.9546***	-0.2924***	-0.3527***	-0.0006
	(0.1691)	(0.1656)	(0.0628)	(0.0692)	(0.0084)
post-schedule change × stagger decrease	1.7386**	1.5116***	0.2270	0.1300	-0.0352
	(0.7723)	(0.5717)	(0.4224)	(0.7129)	(0.0302)
Outcome mean	9.595	8.248	1.348	1.460	0.185
Panel B: Female victims					
SNAP staggering period	-0.0182	-0.0203	0.0021	-0.0074	-0.0001
	(0.0146)	(0.0159)	(0.0070)	(0.0053)	(0.0009)
post-schedule change × stagger increase	-0.9215***	-0.7175***	-0.2040***	-0.2667***	0.0020
	(0.1485)	(0.1437)	(0.0490)	(0.0443)	(0.0071)
post-schedule change × stagger decrease	1.2368*	1.2114**	0.0253	0.1083	-0.0342
	(0.6513)	(0.5012)	(0.3653)	(0.5323)	(0.0299)
Outcome mean	6.647	5.744	0.903	1.066	0.154
Agency FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
County-level controls	Yes	Yes	Yes	Yes	Yes
State-level controls	Yes	Yes	Yes	Yes	Yes
Observations	248264	248264	248264	248264	248264

The outcome variable of interest is the crime rate, defined as number of incidents per 100,000 agency population. The sample excludes Illinois agencies. Standard errors are reported in parentheses and clustered at the state level, where single asterisk (*) represents significance at the 10% level, double asterisk (**) significance at the 5% level, and triple asterisk (***) significance at the 1% level. County-level controls include unemployment rate, average weekly wage and population (in 100 thousands). State-level controls include: per capita Gross State Product, real minimum wage, unemployment rate, poverty rate, SNAP and Medicaid recipients as a percentage of total state population, Medicaid eligibility threshold (as a percentage of federal minimum wage), and an indicator for whether the state governor is a Democrat (1=yes). ID, MD and PA agencies are excluded.