Do Increased Unemployment Insurance Payments Increase Violence Against Women?

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Abstract We examine the relationship between unemployment insurance generosity and reported intimate partner violence against women in the U.S. using detailed nationwide police report data and the staggered adoption of the Federal Pandemic Unemployment Insurance program in the Spring of 2020. Using a difference-in-differences approach, we compare local governments that started paying an extra $600 per week of unemployment benefits to governments which had not yet done so. We find that implementing this program increased reported intimate partner violence offenses against women by 11%, driven by increases in aggravated assaults and sexual assaults. This is consistent with prior evidence on changes in government transfers and intimate partner violence. We do not detect an effect among black women, but the effect is significant for other racial categories. We also find an 11% decrease in reports of intimate partner violence against men, but this is driven by reductions in less serious offenses such as simple assault and intimidation, while we find increases in greater offenses such as aggravated assault and sexual assault.

Keywords Unemployment Insurance, CARES Act, Intimate Partner Violence, FPUC

JEL Codes J12 · J65 · D19

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Highlights

Research question

We estimate the effect of the Federal Pandemic Unemployment Compensation (FPUC) program on police reports of Intimate Partner Violence (IPV) in the U.S. from April 5 to May 2, 2020.

Methods

While the FPUC program was signed into law as part of the federal Coronavirus Aid, Relief, and Economics Security (CARES) Act on March 27, 2020, states took different amounts of time to implement the policy. We leverage this staggered adoption to employ a difference-in-differences (DID) research design. Assuming that there was no anticipation of FPUC implementation, and that, conditional on controls, trends in IPV reports would have been the same in "treated" and "untreated" areas would have been the same in the absence of treatment, we can estimate the effect of FPUC implementation on the margin of police agencies in our sample.

Additionally, to avoid issues of heterogeneous treatment effects biasing our estimates (Goodman-Bacon 2021), we use the efficient and robust estimator developed by Borusyak et al (2021). This estimator, and best practices (Borusyak et al 2021), require us to drop all dates after the last agencies in our sample implement FPUC, which is May 2, 2020.

Data

- We collect data on IPV reports from the National Incident-Based Reporting System (NIBRS), part of the Federal Bureau of Investigation's Uniform Crime Report.
- We hand-collect FPUC implementation dates as reported by state-level agencies in charge of disbursing unemployment insurance (UI) benefits.
- The county-level maximum temperature data are from the Global Summary of the Day (GSOD), which is compiled by the National Climactic Data Center at the National Oceanic and Atmospheric Administration.
- We estimate the fraction of people at home all day using mobile device data from the SafeGraph Data Consortium.
- Weekly state-level data on UI claims are from the U.S. Department of Labor. This includes standard UI claims as well as extended claims from the Pandemic Emergency Unemployment Compensation (PEUC) program and newly applicable claims from the Pandemic Unemployment Assistance (PUA) program.
- City-month-level unemployment data are from the Local Area Unemployment Statistics program, part of the U.S. Bureau of Labor Statistics.

Results and Discussion

We estimate that the implementation of FPUC increased reported IPV against women by 11%, driven by increases in aggravated assault and sexual assault. We also find that total reported IPV against men decreased, but more serious reported offenses such as aggravated assault and sexual assault increased.

Previous research found substantial decreases in overall reported crime caused by the FPUC implementation in 17 cities (Henke and Hsu 2022). However, the effect of increased cash transfers on IPV is more ambiguous than its effect on overall crime, since an abusive partner might be incentivized to use violence to control how the benefit is spent (Hsu 2017; Carr and Packham 2021).

We estimate a short-term effect of this policy during the COVID-19 pandemic (April 5 to May 2, 2020). It is plausible that the effect of the policy changes over time, and that it is different if there is no pandemic.

Introduction

Violence against women, especially intimate partner violence (IPV), is an important global phenomenon with substantial costs. For instance, researchers have found that IPV increases the rate of fetal death when the victim is pregnant (Aizer, 2011); abusers may actively sabotage their partners' ability to earn labor income through arguments and violence (Anderberg and Rainer, 2013); and children of abusive households act out in school, creating a negative externality by lowering educational attainment for other students (Carrell and Hoekstra, 2010). Peterson et al (2018) estimate that the lifetime costs of IPV in the US exceed $3 trillion.[1] It is therefore not surprising that the study of the causal determinants of IPV against women has received growing attention among economists (Hsu and Henke 2022).

This paper examines the effect of increasing the size of unemployment insurance (UI) transfers on IPV in the U.S. from April 5 to May 2, 2020. As part of the Coronavirus Aid, Relief, and Economic Security (CARES) Act signed on March 27th, 2020, the Federal Pandemic Unemployment Compensation (FPUC) program increased UI payments by $600 per week (Courtney, 2020). It took states different amounts of time to comply with the Department of Labor and program the payment increases into the system, which allows us to estimate the effect of implementing FPUC on IPV using a difference-in-differences (DID) research design and modern estimation techniques.

Using nationwide data on criminal incidents including IPV, we find that police agencies in states that implemented FPUC reported 11% more IPV against women than agencies in states that had not yet implemented FPUC. This is driven by increases in aggravated assaults and sexual assaults, and we do not detect a decrease in any category. We control for unemployment, UI recipiency, the fraction of people at home all day, and daily maximum temperature, and this finding is robust to several alternative specifications. We conduct subgroup analyses and find no significant effect for black women, but significant effects for women of other racial categories. We also find that IPV against men decreased by 11%. However, this is driven by a reduction in simple assaults and intimidation, while more serious cases such as aggravated assault and sexual assault increase.

Our research design has some limitations. First, we can only estimate the effect of IPV reported to the police, rather than the entire universe of IPV. We must therefore assume that there is no systematic effect on (under)reporting behavior; and even if that is true, we may underestimate the true effect of the policy on all IPV. Second, our research design relies on the assumption that, conditional on our controls, trends in "treated" and "untreated" police agencies would have been the same but for the treatment. We therefore rely on our controls to proxy for differences in pandemic-related trends also occurring in April 2020. Third, even if our estimate is unbiased, we can only estimate the effect of the policy in April 2020. The medium- and long-

[1] Also see Hsu and Henke (2022) for a review of IPV and its costs.

run effects of increased UI generosity, as well as the effects outside the context of the beginning of the pandemic, may be importantly different.

To our knowledge, this is the first article to estimate the effect of the FPUC program on IPV. Henke and Hsu (2022) find that the FPUC program reduced overall property and violent crime in 17 cities with publicly available police incident data. Beach and Lopresti (2019) also find that increased UI generosity decreases crime. By contrast, our focus is on the effect of violent crimes between intimate partners. We use a detailed national data set that identifies IPV.

Another strand of literature close to this research examines the effect of the COVID-19 pandemic on IPV. Leslie and Wilson (2020) effectively define the "event" of the pandemic as the beginning of official stay-at-home orders in the U.S. and find that IPV calls for service increased dramatically in cities with open police report data following the implementation of stay-at-home orders. However, using similar open police report data in cities that report crime, Bullinger et al (2021) and Miller et al (2022) find that there were fewer reported IPV crimes and arrests after these orders. Alternative IPV estimates, such as internet searches related to IPV, also suggest an increase in IPV after stay-at-home orders (Berniell and Facchini, 2021). Research attempting to disentangle the overall effect of the pandemic found that staying at home and being unemployed increase IPV (Henke and Hsu, 2022; Hsu and Henke, 2021a, 2021b). We add to this research by examining a policy channel for changes in IPV, specifically increased unemployment insurance payments.

This paper contributes to the literature examining the effect of social insurance programs and transfers on IPV. Welfare payments in the form of small cash transfers (Hsu 2017) and food (Carr and Packham, 2021) present an opportunity for an abusive partner to use violence to take control of government transfers. This is consistent with instrumental violence theory, which predicts that abusers will use violence as a tool to control household resources (Tauchen et al., 1991). That said, increased cash transfers, especially those directed at women, can also shield them from violence (Buller et al., 2018; Hidrobo et al., 2016). This is consistent with a household bargaining model which predicts that economic empowerment of victims protects them from violence (Aizer, 2010; Henke and Hsu, 2020). Other studies find nuanced results of cash transfers containing some of both theories (Angelucci, 2008; Bobonis et al., 2013); cash transfers may protect more powerful women from violence, but they also may invite instrumental violence on less powerful women.

Our work also aligns with scholarship on the effects of unemployment and unemployment insurance on IPV. Anderberg et al (2016) find that male unemployment reduces IPV and female unemployment increases IPV in the United Kingdom. By contrast, Bhalotra et al (2021) find that job losses for both men and women increase domestic violence in Brazil. Fewer studies examine the impact of unemployment insurance on domestic violence. Bhalotra et al (2021) also find that men receiving unemployment benefits (as opposed to being unemployed and not receiving benefits) reduces domestic violence.

Our research on the FPUC program builds on these strands of the literature and, to our knowledge, answers a new question: How does a large *increase* in the size of unemployment insurance payments affect IPV? Part of the novelty of this research arises from the fact that the $600-per-week increase in UI payments from the CARES Act was unprecedentedly large (Ganong et al., 2020), dwarfing similar programs like the Supplemental Nutritional Assistance Program (Parolin et al., 2020). Since UI recipients automatically received the extra cash, it effectively acted as a substantial cash transfer experiment. While Londoño-Vélez and Querubin (2022) find that unconditional cash transfers to poor families in Colombia substantially improve their wellbeing, they do not find effects on IPV. By contrast, we find an increase in IPV against women in the U.S. context.

Data

All data cover dates from January 1, 2020 to December 31, 2020. Due to our methodology, the analytic sample ends on May 2, 2020—i.e., we drop all data points after this date. This means that anything occurring after May 2, 2020 does not affect this analysis.

Intimate Partner Violence

For the purposes of this study, we define intimate partner violence (IPV) as reports of assault, sexual assault, verbal assault (intimidation), or homicide against an intimate partner. We derive data on reports of Intimate Partner Violence (IPV) from the National Incident-Based Reporting System (NIBRS) compiled by the Federal Bureau of Investigations as part of the Uniform Crime Report (UCR).[2] The NIBRS records every criminal incident brought to the attention of a participating law enforcement agency. In 2020, 9,991 police agencies covering 53% of the U.S. population participated in the NIBRS.

The NIBRS has several important advantages. First, it is a daily data set, which allows us to exploit our observed policy variation. Second, it has detailed information on victims and offenders, including their relationship, gender, and racial category.[3] Third, it links incidents to police agencies, which can then be linked to specific locations.

There are also some concerns about the quality of the NIBRS data: Not all agencies report their statistics to the FBI, especially during the pandemic; Agencies may have entered data incorrectly; Relatedly, some agencies' reporting is spotty (Kaplan, 2021). For this reason, our analytic sample only includes agencies with a listed population of 100,000 or higher who reported at least one crime every month of 2020.[4] While the NIBRS sample is more limited than the UCR, the UCR is monthly and lacks details necessary for our analysis. The NIBRS is the most

[2] https://www.openicpsr.org/openicpsr/project/118281/version/V6/view?flag=follow&pageSize=50&sortOrder=(?title)&sortAsc=true
[3] It also has information about ethnicity, but this is much more sparsely reported, so we do not use it.
[4] See Table A1 in the Appendix for a list of cities in the analytic sample.

extensive U.S. crime data set which offers the level of detail required for this analysis.[5] We also drop agencies that do not report at least once a month in 2020. Our sample covers 236 police agencies in 215 cities.

To construct our dependent variable, we count the number of offenses in each agency-day where the offender and victim are listed as (ex-)spouses, common-law spouses, "homosexual relationship," and (ex-)boyfriend-girlfriend. In our main specification, we focus on violence against women—i.e., we count offenses where the victim is a woman. Hereafter, we refer to this main count as IPV. We also count IPV against men and consider it in an alternative specification. We count separately aggravated assault, simple assault, intimidation, sexual assault, homicide, and the sum of these counts is the total count of IPV on that day in that agency.[6] The NIBRS also provides information about the victim's race, so we construct IPV counts for women categorized as white, black, and all others, which includes when the victim's race is unknown. To construct rates of IPV, we divide the count by the agency's population, which is provided by the NIBRS, and multiply by 1,000,000.

Our primary data unit is agency-day. Other variables are less granular. For instance, FPUC Implementation varies by state-day. Thus, agencies in the same agency will have the same treatment.

FPUC Implementation

To find when states implemented the FPUC program, we manually searched for official government statements online and recorded reported implementation dates. See Table 1 for a list of states included in the sample and the FPUC implementation date.

Maximum temperature

Weather is a good control because high temperatures induce IPV (Henke and Hsu, 2020), and clearly it is not caused by short-term policy or IPV. This means it improves the efficiency of our estimates without introducing bias. The county-day-level maximum temperature data are from the Global Summary of the Day (GSOD), which is compiled by the National Climactic Data Center at the National Oceanic and Atmospheric Administration.[7] When there are multiple weather stations, we use the daily average maximum temperature in that county. When a county lacks temperature information, we use the maximum temperatures from adjacent counties.

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[5] Related studies use open daily police report data from large cities. There are three main advantages of this data relative to the NIBRS: It is released sooner; it is easier to use; and it sometimes has more detailed data such as granular location identifiers. The two most important advantages of the NIBRS relative to open police data is that it has much more detail on victims and offenders, including the type of domestic violence incident, and it covers more police agencies in more states.

[6] Sexual assaults include fondling, rape, sexual assault with an object, sodomy, and statutory rape.

[7] https://www.ncei.noaa.gov/access/metadata/landing-page/bin/iso?id=gov.noaa.ncdc:C00516

Other data sources

Staying at home with an abuser was a key driver of IPV during the COVID-19 pandemic (Hsu and Henke, 2021a, 2021b). To control for this effect, we estimate the fraction of people at home all day using mobile device data from the SafeGraph Data Consortium.[8] SafeGraph pings 45 million anonymized mobile devices in the U.S. per day and tracks where they are at different times of day. We count the number of pinged devices that never left their designated home area in a 24-hour period and divide by the total number of sampled devices in that county.

Weekly state-level data on UI claims are from the U.S. Department of Labor. This includes standard UI claims as well as extended claims from the Pandemic Emergency Unemployment Compensation (PEUC) program and newly applicable claims from the Pandemic Unemployment Assistance (PUA) program. City-month-level unemployment data are from the Local Area Unemployment Statistics program, part of the U.S. Bureau of Labor Statistics. To create a per-capita crime rate, we use city population estimates from the U.S. Census Bureau in 2019.

Summary Statistics

Table 2 shows that, on average, agencies in the sample report 9.50 IPV offenses against women per million people per day and 2.88 offenses against men at the same rate.

Methods

Individual states took different amounts of time to implement the FPUC program, but individual UI recipients generally could not influence this process. This resembles a natural experiment varying the size of the UI transfer for recipients over time, which allows us to take a DID approach. Ideally, we could observe the counterfactual trend of treated groups had they not been treated and directly estimate the effect of a treatment. Since this is not possible, DID estimates the counterfactual trend using data from untreated groups (Goodman-Bacon 2021). Since all recipients eventually received payments, there is no “never treated” group that never implemented FPUC; instead, we compare data from the “treated” agencies in states where FPUC has already been implemented on that day with “not yet treated” agencies in states where FPUC has not yet been implemented. Since we can only make meaningful comparisons when some groups are treated and some groups are not, the key period of comparison in our analytic sample is from April 5, 2020, to May 2, 2020. The primary outcome of interest is total IPV against women; we also consider IPV against women by incident type and by racial category.

We assume parallel trends conditional on controlling for the unemployment rate, the standard UI take-up rate, the UI take-up rate due to the PEUC and PUA programs in the CARES Act, the

[8] SafeGraph is a data company that aggregates anonymized location data from numerous applications to provide insights about physical places. To enhance privacy, SafeGraph excludes census block group information if fewer than five devices visited an establishment in a month from a given census block group.

daily maximum temperature, and the fraction of people who stay home all day by county-day.[9] We argue that we effectively control for other policies in the CARES Act. Some policies, such as loans to small businesses, may impact trends in IPV by changing unemployment (Autor et al., 2022); since we control for unemployment directly, we argue we capture the differential trends caused by this policy. Other policies, such as the Economic Impact Payments (EIP), were distributed at the federal level (Courtney, 2020) and therefore did not have a staggered implementation by state.

This research design also assumes no anticipation of the policy before its implementation. The law was passed on March 27, 2020, and even if individuals had to wait to get extra payments, some fraction of recipients understood they were going to receive those payments. Prospective offenders often face financial constraints which do not permit them to leverage the fact that money is coming soon (Carr and Packham, 2019; Foley, 2011). Insofar as anticipation is an issue, we argue that the not-yet-treated group should act more like a partially treated group, meaning our estimates may be a lower bound of the true estimate—i.e., our estimated effects may be biased towards zero.

We analyze the effect of FPUC implementation over time with an event study plot, and then we estimate the overall effect with a DID analysis. For the event study plot, we aggregate all data to the week-level so that the figure is easier to read.[10] A typical two-way fixed-effects estimator will be biased in the presence of heterogeneous treatment effects (Goodman-Bacon, 2021), which we cannot rule out. Therefore, we use the robust and efficient estimator developed by Borusyak et al (2021) to conduct our event study and DID analyses.[11] This estimator drops all observations after the last unit is treated (May 2, 2020) because it requires an untreated group to impute a counterfactual trend. Finally, all errors are clustered at the state level.[12]

Results

Figure 1 shows the event study plot for IPV aggregated to weekly estimates.[13] There do not appear to be any significant pre trends—i.e., we do not detect effects occurring prior to the treatment, which may suggest the parallel trends assumption is violated. The effect of FPUC implementation on IPV reports is positive and growing over time. One possible explanation is that more people applied for and received UI assistance in April 2020.

Table 3 displays our main results. Panel A considers total IPV and then disaggregates by offenses against female victims categorized as white, black, and all others. Panel B considers intimate

[9] We include additional controls in alternative specifications.

[10] Specifically, we count all cases of IPV occurring that week, and we average all the values of the daily control variables (e.g., maximum temperature) for that week.

[11] In Stata, the codes for event study and DID are *event_plot* and *did_imputation*, respectively.

[12] See Borusyak et al (2021) for details of the conservative inference performed by their estimator.

[13] All daily data are aggregated to the week-level by averaging the days of that week.

partner aggravated assault, simple assault, intimidation, sexual assault, and homicide against women respectively.

We find that the implementation of FPUC increased reported IPV offenses by 1.071 per million people per day, or 11% relative to the sample mean.[14] This is driven primarily by a 21% increase in aggravated assaults and also an 85% increase in sexual assaults. We then consider the incidence of this effect by race. We do not detect a statistically significant effect on black women, but we find a 13% increase in IPV against white women and a 46% increase in IPV against women of other racial categories. Among the "other" race category, 19% are American Indian or Alaska natives, 29% are Asian or Pacific Islanders, 10% are Hawaiian or other Pacific Islanders, and 42% are unknown. One reason black women may be less affected by FPUC implementation is that unemployed black workers are less likely to receive UI payments (Kuka and Stuart, 2021), especially during the pandemic (Mar et al., 2022).

Discussion

The motivations for and causes of IPV are nuanced, and so it was unclear ex ante what the effect of the FPUC program would be on reported IPV. Instrumental violence theory suggests that increased UI transfers would increase the motivation to use violence to control household resources (Hsu 2017). On the other hand, household bargaining theory suggests that more money increases the cost of violence by making it easier for victims to leave an abusive relationship (Hsu and Henke 2022). Our results are consistent with an instrumental violence effect dominating for the first four weeks.

Our research design can only estimate effects of implementing FPUC from April 5 to May 2, 2020, or roughly four weeks. After this, all agencies in the sample are treated. It is possible that medium- and long-term effects of cash transfers differ from the short-term effects. For instance, the aforementioned instrumental violence effects would stop once the FPUC program ended at the end of June 2020.[15] By contrast, any effect of increased cash assistance on economic bargaining power for victims may be more durable, decreasing IPV in the long run, and we would not detect this. Understanding short-run (4-week) effects of large ($600/week) cash transfers to UI recipients on IPV is important by itself.

Alternative Specifications

Table 4 presents our results for IPV reports against men. We find that FPUC implementation decreases overall reports of IPV against men. However, this is driven by decreases in simple assault and intimidation, while sexual assaults and aggravated assaults increase.

We present our alternative specifications to our main specification on total IPV in Table 4. First, we control for the moving average of that agency's crime rate one year prior to the date. Second,

[14] Percentages are calculated by taking the estimated effect and dividing by the sample mean.
[15] The FPUC program re-started with smaller payments in December 2020 and ended in September 2021.

we account for potential differential impact of the Economic Impact Payments (EIP) in the CARES Act. Third, we control for the COVID-19 confirmed case rate by county-day. Fourth, we include all three additional controls. Fifth, we account for potential anticipation. Finally, we estimate the effect of FPUC implementation on IPV against men.

Agency-specific IPV report seasonality

To control for agency-specific seasonal time trends in IPV reports not captured by time dummies, we include a seven-day moving average of the IPV rate one year prior to the day in question.[16] The drawback is that we lose 22 agencies that did not report in 2019. In column 1 of Table 4 we see that our results remain significant at the 10% level.

Economic impact payments

The CARES Act also included federal Economic Impact Payments (EIP) which were mostly distributed from April 10th to April 15th, 2020 (Parker et al., 2022). Any uniform impact of EIP is effectively captured by time dummies, but a differential impact of EIP by state may cause a parallel trends violation. The Internal Revenue Service provides data on the total money provided to individuals through this round of EIP by state. We create a new variable by interacting this data with an indicator between April 10th to April 15th, 2020. In column 2 of Table 4, we find that our results remain significant at the 1% level.

COVID-19 case rate

Policies in the CARES Act were enacted during the COVID-19 pandemic and related shutdowns. Two substantial effects of the pandemic related to IPV were unemployment and staying at home with a potentially abusive partner (Henke and Hsu, 2022; Hsu and Henke, 2021b). We proxy for these effects with unemployment data and detailed mobile device data in the main specification. That said, the spread of the disease itself may affect IPV.

In this alternative specification, we collect daily county-level COVID-19 confirmed case data from USAFacts.[17] USAFacts is a non-profit civic initiative that provides comprehensive publicly available government data. The key advantage of this data is its broad coverage and variation by day and county. The data include both confirmed COVID-19 cases and deaths, but the total number of tests by county and day are not available. We calculate the 7-day moving average for newly confirmed COVID-19 cases to avoid day-of-week reporting effects. Another limitation is that the data set starts on January 22, 2020, before which we backfill zeroes—i.e., the case rate from January 1 to January 21, 2020, is zero in all locations. In column 3 of Table 4, we find that our results remain significant at the 1% level.

[16] For instance, for the IPV rate in Omaha on April 10, 2020, the control would be the average Omaha IPV rate over seven days centered around April 10, 2019.

[17] https://usafacts.org/visualizations/coronavirus-covid-19-spread-map

All additional controls

In column 4 of Table 4, we add all three of these additional controls – previous year IPV report rate, Economic Impact Payments, and the COVID-19 case rate –and our results remain significant at the 10% level.

Accounting for one week of anticipation

As noted, if UI recipients anticipated receiving payments and adjusted their behavior accordingly, then they are effectively partially treated, meaning our main results may represent a lower bound. We account for potential anticipation by setting the event time back uniformly by seven days—i.e., by setting the date of FPUC implementation back seven days for all states. We see in column 5 of Table 4 that our results remain significant at the 1% level.

Conclusion and future work

We examine the effect of the FPUC program on IPV. We estimate that FPUC implementation increases IPV against women by 11%, driven by increases in aggravated assault and sexual assault. We do not find an effect on IPV against black women, but we do find significant effects for other women. We also find that the program decreases IPV for men, but this is driven by a reduction in less serious offenses such as simple assault and intimidation, and that more serious offenses such as aggravated assault and sexual assault increase.

Gaps in knowledge

Our work addresses an important gap in the literature by estimating the effect of UI generosity on reported IPV rates in the short run (four weeks). Estimating longer-run effects of the CARES Act and related policies represents an important open question for future scholarship. This requires a new empirical strategy and potentially new data that can both capture variables which exogenously shift generosity and track outcomes over the long run. Any estimate of longer-run effects is also affected by a number of important related questions: Did unemployment spells last longer during the pandemic? If so, how much was caused by the pandemic shutdowns, health risks, FPUC, or expanded qualification into UI? Was there a loss in career earnings due to pandemic-related unemployment? Was there a gendered component of unemployment and lost career earnings, and how did childcare arrangements influence this effect?

Data improvements and funding

There are various ways new, improved, and more public-facing data sources could open up new lines of research and improve ongoing projects. First, the time it takes for individual states to comply with the DOL when there are changes to the UI system (e.g., the FPUC program) would provide a better understanding of the source of variation in implementation. If the primary issue is compliance, what can individual states and the DOL do to improve that? If the issue is programming, what can states do to improve their sometimes quite old systems that maintain the UI system?

Second, our analysis would be improved by more granular UI take-up data. Currently, we use overall UI take-up data by state-week. Data by county or Metropolitan Statistical Area (MSA), by gender, and by race would be highly useful as a control. Furthermore, such information could help inspire more direct lines of research related to race and gender.

Third, more detail related to FPUC specifically would greatly help analyses of FPUC. Specifically, how many UI recipients received FPUC payments each week? Given that individuals received pro-rated payments to cover extra payments not received, how many individuals received these payments, when, and how much? An ideal data set may link victim households to FPUC payments, but due to victim privacy concerns this may not be possible.

In each of these questions, there is a follow-up question related to the quality of the data during the pandemic. How can we know that state agencies accurately report when people began to qualify for and receive UI payments? Is there a way for the DOL to validate data? We hope to help answer these questions with follow-up research.

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Table 1: Federal Pandemic Unemployment Compensation Implementation by State in 2020

State	FPUC start date	State	FPUC start date
Arizona	13-Apr-20	Nevada	12-Apr-20
Arkansas	05-Apr-20	New Hampshire	18-Apr-20
Colorado	27-Apr-20	New Mexico	20-Apr-20
Connecticut	25-Apr-20	New York	10-Apr-20
Delaware	13-Apr-20	North Carolina	15-Apr-20
Georgia	13-Apr-20	North Dakota	12-Apr-20
Hawaii	17-Apr-20	Ohio	23-Apr-20
Idaho	24-Apr-20	Oklahoma	07-Apr-20
Illinois	06-Apr-20	Oregon	10-Apr-20
Indiana	17-Apr-20	Rhode Island	11-Apr-20
Iowa	17-Apr-20	South Carolina	18-Apr-20
Kansas	23-Apr-20	South Dakota	08-Apr-20
Kentucky	09-Apr-20	Tennessee	14-Apr-20
Louisiana	13-Apr-20	Texas	12-Apr-20
Maryland	17-Apr-20	Utah	08-Apr-20
Massachusetts	09-Apr-20	Virginia	13-Apr-20
Michigan	09-Apr-20	Washington	12-Apr-20
Minnesota	08-Apr-20	West Virginia	11-Apr-20
Missouri	12-Apr-20	Wisconsin	02-May-20
Montana	13-Apr-20		

Source: We manually searched for official government statements online and recorded reported implementation dates.

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Table 2: Summary Statistics of Reported Intimate Partner Violence (IPV), by race and type in 2020

	(1) N	(2) Mean	(3) Std. Dev.
Against Women			
Total Reported IPV	86,376	9.50	10.0
By race:			
White	86,376	5.63	7.0
Black	86,376	3.39	6.3
Other	86,376	0.48	1.9
By type:			
Aggravated assault	86,376	1.42	3.1
Simple assault	86,376	6.78	7.8
Intimidation	86,376	1.06	3.0
Sexual assault	86,376	0.22	1.2
Homicide	86,376	0.01	0.3
Against Men			
Total Reported IPV	86,376	2.88	4.6
By type:			
Aggravated assault	86,376	0.43	1.6
Simple assault	86,376	2.23	3.9
Intimidation	86,376	0.21	1.2
Sex assault	86,376	0.01	0.2
Homicide	86,376	0.00	0.2
PUA + PEUC claim	86,376	4.30	5.3
Insured unemployment rate (%)	86,376	6.22	4.9
Unemployment rate (%)	86,376	7.69	4.2
Fraction of people at home	86,376	0.31	0.1
Maximum temperature (°C)	86,376	20.19	10.14

Note: N is the size of the analytic sample by agency-day. IPV is defined as the rate of police reports of assaults, sexual assaults, intimidation, and homicide against an intimate partner per day per 1,000,000 people. The rates of regular insured unemployment, Pandemic Unemployment Assistance (PUA) claims, and Pandemic Emergency Unemployment Compensation (PEUC) claims are the number of claims per 100 people in the labor force (all people age 16 and older who are either employed and unemployed). The unemployment rate is the number of unemployed per 100 people in the labor force. The fraction of people at home is the fraction of mobile devices sampled by the SafeGraph data consortium that are at their home location all day. Maximum temperature is by county-day.

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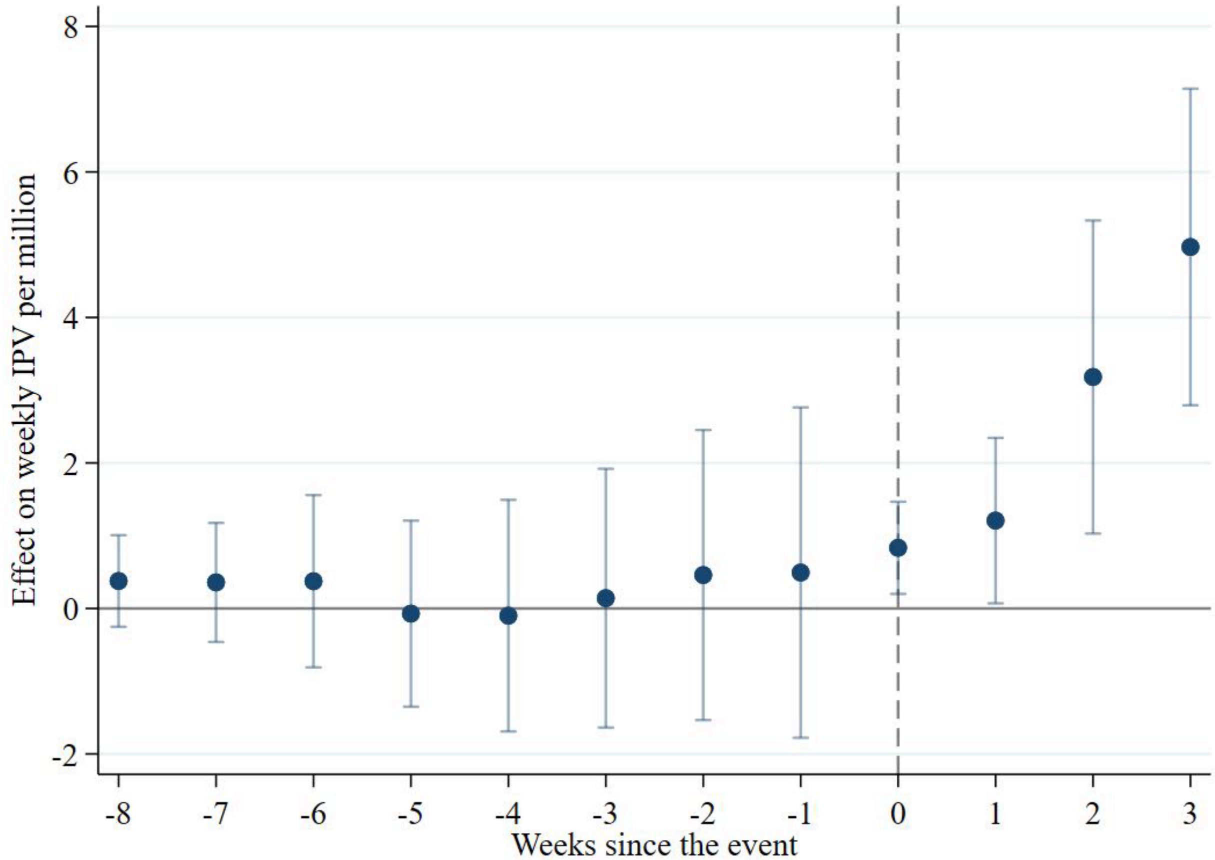

Figure 1: Weekly Event Study Plot, FPUC Implementation and Reported Intimate Partner Violence (IPV) from 1/1 – 5/2/20

Note: Figure 1 displays the treatment effect estimates using the event study estimator developed by Borusyak et al (2021). N = 3,638 is the size of the analytic sample by agency-week. Unlike other estimates, daily data are aggregated to the week level. IPV is defined as the rate of police reports of assaults, sexual assaults, intimidation, and homicide against a female intimate partner per week per 1,000,000 people. FPUC is the Federal Pandemic Unemployment Compensation program, and week 0 represents the week FPUC is implemented at the state level. We control for rates of regular unemployment insurance (UI) claims, additional UI claims from the Pandemic Unemployment Assistance and Pandemic Emergency Unemployment Compensation programs, the unemployment rate, the fraction of people staying at home all day, maximum temperature, and day and agency fixed effects.

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Table 3: Estimated Effect of Federal Pandemic Unemployment Compensation (FPUC) Implementation on Reported Intimate Partner Violence (IPV)

Panel A: The effect of FPUC implementation on total reported IPV and reported IPV by race

	(1) Total IPV	(2) White	(3) Black	(4) Other
Treatment effect	1.071*** [0.315,1.827]	0.707*** [0.225,1.189]	0.143 [-0.207,0.492]	0.221*** [0.071,0.372]

Panel B: The effect of FPUC implementation on different types of reported IPV

	(1) Aggravated Assault	(2) Simple Assault	(3) Intimidation	(4) Sexual Assault	(5) Homicide
Treatment effect	0.302*** [0.115,0.490]	0.505 [-0.150,1.160]	0.069 [-0.183,0.322]	0.187*** [0.135,0.239]	0.007 [-0.007,0.021]

Note: Table 3 presents treatment effect estimates using the difference-in-differences (DID) estimator developed by Borusyak et al (2021). N = 28,792 is the size of the analytic sample by agency-day. IPV is defined as the rate of police reports of assaults, sexual assaults, intimidation, and homicide against a female intimate partner per week per 1,000,000 people. All estimations include controls for rates of regular unemployment insurance (UI) claims, additional UI claims from the Pandemic Unemployment Assistance and Pandemic Emergency Unemployment Compensation programs, the unemployment rate, the fraction of people staying at home all day, maximum temperature, and day and agency fixed effects. This table provides the average treatment coefficient and the associated 95% confidence intervals in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table 4: Alternative Specifications for the Estimated Effect of Federal Pandemic Unemployment Compensation (FPUC) Implementation on Reported Intimate Partner Violence (IPV)

Panel A: Reported IPV against women

	(1) Lag	(2) EIP	(3) COVID cases	(4) All controls	(5) Anticipation
Treatment effect	0.728* [-0.060,1.516]	1.078*** [0.321,1.835]	1.036*** [0.280,1.792]	0.755* [-0.029,1.538]	1.405*** [0.681,2.129]
Observations (N)	26108	28792	23836	21614	27140

Panel B: Reported IPV against men

	(1) Total IPV	(2) Aggravated Assault	(3) Simple Assault	(4) Intimidation	(5) Sexual Assault	(6) Homicide
Treatment effect	-0.329*** [-0.554,-0.104]	0.211*** [0.115,0.307]	-0.477*** [-0.702,-0.252]	-0.085** [-0.156,-0.013]	0.018** [0.000,0.035]	0.003* [-0.000,0.007]

Note: Table 3 presents treatment effect estimates using the difference-in-differences (DID) estimator developed by Borusyak et al (2021). N is the size of the analytic sample by agency-day, and it is 28,792 in Panel B. IPV is defined as the rate of police reports of assaults, sexual assaults, intimidation, and homicide against a female intimate partner per week per 1,000,000 people; in Panel B, it is defined as a male partner instead. All estimations include controls for rates of regular unemployment insurance (UI) claims, additional UI claims from the Pandemic Unemployment Assistance and Pandemic Emergency Unemployment Compensation programs, the unemployment rate, the fraction of people staying at home all day, maximum temperature, and day and agency fixed effects. Panel A column (1) also controls for the 7-day moving average IPV rate of the previous year (2019); column (2) controls for the interaction term between total economic impact payment per state population and mostly likely distributed period (April 10th to April 15th, 2020) column (3) controls for the county-level COVID-19 confirmed case in the sample rate as calculated by USAFacts (January 1st to 22nd, 2020 has no observations and therefore is imputed as zero); column (4) includes all previous additional controls; column (5) incorporates one week of anticipation by setting the time of implementation back seven days for all units. This table provides the average treatment coefficient and the associated 95% confidence intervals for the imputation estimator in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Appendix Table A1: Cities included in the analytic sample

City	City	City
Abilene, TX	Cincinnati, OH	Grand Prairie, TX
Aiken, SC	Clarksville, TN	Grand Rapids, MI
Akron, OH	Cleveland, OH	Greeley, CO
Albuquerque, NM	Clinton Township, MI	Green Bay, WI
Alexandria, VA	College Station, TX	Greensboro, NC
Amarillo, TX	Colorado Springs, CO	Greenville, SC
Anderson, SC	Columbia, MO	Gresham, OR
Ann Arbor, MI	Columbia, SC	Hampton, VA
Arlington, TX	Columbus, OH	Hanover, VA
Arlington, VA	Conway, SC	Harvey, LA
Arvada, CO	Corpus Christi, TX	Henrico, VA
Asheville, NC	Cumming, GA	High Point, NC
Athens, GA	Dallas, GA	Hillsboro, OR
Aurora, CO	Dallas, TX	Honolulu, HI
Austin, TX	Davenport, IA	Houston, TX
Beaufort, SC	Dayton, OH	Howell, MI
Beaverton, OR	Denton, TX	Independence, MO
Bellevue, WA	Denver, CO	Indianapolis, IN
Bend, OR	Des Moines, IA	Irving, TX
Billings, MT	Detroit, MI	Jacksonville, NC
Boise, ID	Donaldsonville, LA	Jonesboro, GA
Boston, MA	Douglasville, GA	Kalamazoo, MI
Boulder, CO	Dover, DE	Kansas City, MO
Bridgeport, CT	Durham, NC	Kenosha, WI
Brighton, CO	Edinburg, TX	Kent, WA
Brownsville, TX	El Paso, TX	Knoxville, TN
Burlington, KY	Englewood, CO	Lakewood, CO
Cambridge, MA	Eugene, OR	Landover, MD
Canton, GA	Everett, WA	Lansing, MI
Canton, OH	Fairfax, VA	Laredo, TX
Cary, NC	Fargo, ND	Las Cruces, NM
Castle Hayne, NC	Fayetteville, NC	Las Vegas, NV
Castle Rock, CO	Fort Collins, CO	League City, TX
Cedar Rapids, IA	Fort Worth, TX	Leesburg, VA
Charleston, SC	Frisco, TX	Lewes, DE
Charlotte, NC	Gainesville, GA	Lewisville, TX
Charlottesville, VA	Garland, TX	Lexington, KY
Chattanooga, TN	Georgetown, TX	Lexington, NC
Chesapeake, VA	Gilbert, AZ	Lexington, SC
Chesterfield, VA	Golden, CO	Lillington, NC

Table A1 (continued)

City	City	City
Lowell, MA	Overland Park, KS	Spotsylvania, VA
Lubbock, TX	Pasadena, TX	Springfield, MA
Macon, GA	Pearland, TX	Springfield, MO
Madison, WI	Plano, TX	St Charles, MO
Manchester, NH	Port Orchard, WA	St Louis, MO
Marietta, GA	Portland, OR	Stafford, VA
Martinsburg, WV	Prince William, VA	Stamford, CT
McDonough, GA	Providence, RI	Statesville, NC
McKinney, TX	Pueblo, CO	Sterling Heights, MI
Memphis, TN	Raleigh, NC	Sugar Land, TX
Meridian, ID	Reno, NV	Surprise, AZ
Mesa, AZ	Renton, WA	Tacoma, WA
Mesquite, TX	Richardson, TX	Thornton, CO
Middletown, CT	Richmond, TX	Toledo, OH
Milwaukee, WI	Richmond, VA	Tucker, GA
Minneapolis, MN	Rochester, MN	Tucson, AZ
Moncks Corner, SC	Rochester, NY	Tyler, TX
Monroe, NC	Rockford, IL	Vancouver, WA
Mount Clemens, MI	Rockville, MD	Virginia Beach, VA
Murfreesboro, TN	Salem, OR	Waco, TX
Nampa, ID	Salt Lake City, UT	Warren, MI
Nashville, TN	San Angelo, TX	Waterbury, CT
New Castle, DE	Sandy Springs, GA	West Jordan, UT
New Haven, CT	Seattle, WA	West Olive, MI
Newport News, VA	Sioux Falls, SD	West Valley City, UT
Norfolk, VA	Smithfield, NC	Westminster, CO
Norman, OK	South Bend, IN	Wichita, KS
North Charleston, SC	South Fulton, GA	Wilmington, NC
Okla. City, OK	Sparks, NV	Winston Salem, NC
Olathe, KS	Spartanburg, SC	Worcester, MA
Olympia, WA	Spokane Valley, WA	York, SC
Oregon City, OR	Spokane, WA	

Note: We select the cities in the analytic sample, listed above, as follows: First, we must have all the analysis data in each city, including crime reports, FPUC implementation date, unemployment data, temperature data, and mobile device tracking data. Second, we eliminate all agencies who did not report a crime at least once per month and with populations under 100,000.

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