The impact of the COVID-19 pandemic on treatment for domestic violence injuries: evidence from medical claims

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Abstract

Previous studies have observed heterogeneous changes in domestic violence-related 911 calls, police incident reports, and arrests at the onset of the COVID-19 pandemic. In this study, we use a large-scale medical claims database with coverage of over 100 million patients to examine the impact on domestic violence victims' use of emergency department care for their injuries in the early weeks of the pandemic compared to the corresponding weeks in previous years. We find a 35% decrease in utilization of emergency medical services by domestic violence victims between March and June of 2020. Based on this finding, it is essential to use caution when using medical claims to measure domestic violence in future research covering this time period. Decreases in care utilization also have important implications for the detection, screening, and treatment of domestic violence injuries during future public health crises.

1 Introduction

At the onset of the COVID-19 pandemic, news reports and victim advocacy organizations raised concerns about potential increases in domestic violence due to increased time at home, widespread job losses and furloughs, and increased stress - all known risk factors for violence in the home. Furthermore, concerns about COVID-19 infection risk and strain on the healthcare system reduced overall healthcare utilization in the early weeks of the pandemic. The combination of these factors suggests that domestic violence victims may have been less willing and able to access medical care and other supportive services in the early weeks of the pandemic. In this paper, we use a medical claims database covering over 100 million patients in 48 states to investigate the ways that the onset of the pandemic and its associated behavioral and policy responses affected victims' access to emergency medical care for domestic violence injuries as well as any additional service that may result from emergency department domestic violence screening.

Much of the existing economic literature on domestic violence during the COVID-19 pandemic (Bullinger, Carr, and Packham 2021; Hsu and Henke 2021; Leslie and Wilson 2020; McCrary and Sanga 2021; Miller, Segal, and Spencer 2020, 2022) measures incidence of domestic violence by using police data measures, such as 911 calls for service or police incident reports for domestic violence offenses. These studies find that domestic violence 911 calls increased, while arrests and criminal incidents decreased. Miller et al. (2020) find that reports of severe domestic violence offenses like aggravated assaults and homicides decreased while 911 calls increased. Ivandic et al. (2020) link the rise in calls to increased reporting by both victims and third parties such as neighbors and note increases in violence by current partners and decreases in violence by ex-partners. Bullinger et al. (2021) find a similar pattern of rising calls for service, noting that the drop in arrests and incident reports for domestic violence offenses mimics that of other crimes, suggesting that the decline in reports may be driven not by declines in domestic violence but by changes in police behavior. In fact, some police agencies issued explicit directives to alter or reduce public contact (Jennings and Perez 2020). Taken together, simultaneously changing victim and police behavior result in heterogeneous effects depending on the police data measures used.

In addition to the substantial work focused on assessing domestic violence using law enforcement data, economic literature has examined incidence of domestic violence using alternative data sources which may be less subject to shifts in both victim reporting and police behavior. A survey undertaken by Arenas-Arroyo et al (2021) in Spain finds that the pandemic increased verbal and emotional abuse rather than physical abuse, a result that is consistent among other studies employing survey measures of verbal and emotional abuse (Drotning et al. 2023). Research using calls to domestic violence hotlines (Perez-Vincent and Carreras 2022; Silverio-Murillo, de, and Hoehn-Velasco 2023) and internet search intensity for domestic violence (Berniell and Facchini 2021) find significant increases during the initial period of lockdowns. These findings are consistent with the observed increases in 911 calls coupled with decreasing police reports or arrests observed in the U.S. and international contexts (Hoehn-Velasco, Silverio-Murillo, and de la Miyar 2021; Ravindran and Shah 2020).

While these studies provide important evidence about changing incidence of domestic violence, we focus on the substantial impacts on medical treatment for domestic violence injuries during the COVID-19 pandemic. To this end, we use detailed medical claims data spanning over 100 million patients in 48 states with tens of millions of visits to healthcare providers. Existing studies have used medical records to study domestic violence during thepandemic within a single medical facility or geographic area and find a range of differing impacts. Several studies find increases to domestic violence medical visits (Pallansch et al. 2022; Rahman et al. 2022; H. X. Rhodes et al. 2020; Smith et al. 2022); in contrast, others find decreases in medical visits (Muldoon et al. 2021). This study fills an important research gap by analyzing domestic violence medical visits with extensive coverage across varied providers and geographies.

Overall, we find that visits for domestic violence injuries declined considerably in the early phase of the pandemic from March to June of 2020. Visits recovered in the later months of the year, but remained lower than prior years. However, this decline was smaller in percentage terms than the corresponding decline in accidental injury visits, which is suggestive that it is likely driven by the broader decline in healthcare utilization rather than declines in the actual incidence of domestic violence injuries. Reinforcing this conclusion, the decline is driven almost entirely by less severe injuries; visits for severe or life-threatening injuries make up a small portion of the decline in assault injury visits. This pattern is consistent with forgoing or delaying care as victims face tradeoffs between untreated domestic violence injuries and COVID-19 infection risk.

To ensure our results are not driven by increased unemployment and insurance loss related to the pandemic (Albanesi and Kim 2021; Bundorf, Gupta, and Kim 2021; Cajner et al. 2020), we examine trends in domestic violence visits separately among public and private insurance payers. It is unlikely that our results are driven by insurance loss, as trends in assault claims are similar across both private insurance payers administering group plans that would be more affected by pandemic-related job loss and government and exchange payers that primarily administer individual plans that are less tied to specific jobs. The totality of these results leads us to conclude that while domestic violence injuries were likely

increasing or stable in the early weeks of social distancing behavior, victims were less likely to receive emergency medical care for their injuries.

Our main finding - that victims were less likely to access emergency medical care for domestic violence during the most acute phase of the pandemic - has implications for measurement of domestic violence in future empirical work. Aizer (2010) used emergency department visits by women for assault injuries as a measure of domestic violence that is unaffected by victims' self-selection into reporting their abuse to law enforcement. This is a sensible proxy, as victims often do not report their abuse to law enforcement, due to shame, fear of retaliatory violence, or a desire to keep their families intact (Felson et al. 2002; Felson and Paré 2005; Griffing et al. 2002). While those that require medical care often seek it, many do not admit to the cause of their injuries or hesitate to seek additional services for victims (Kothari and Rhodes 2006). However, our results imply that future analyses whose time horizons cover the COVID-19 pandemic (or other public health crises that reduce medical care utilization) should not rely solely on medical visits as a measure of domestic violence.

Our findings also have implications for service provision for domestic violence victims during times of widespread distress, as healthcare providers have increasingly adopted screening to identify social determinants of health. Since 2012, the American College of Obstetricians and Gynecologists has recommended regular intimate partner violence screening during pregnancy and postpartum and in 2018, the U.S. Preventive Services Task Force adopted a recommendation for universal violence screening among reproductive-aged individuals (US Preventive Services Task Force 2018). In a large sample of healthcare providers, nearly 60% of physician practices and U.S. hospitals report screening for interpersonal violence (Fraze et al. 2019). Although research examining the effectiveness of both screening and resulting domestic violence interventions is limited (Ghidei et al. 2023; Seff et al. 2021), recent evidence suggests that access to digital safety planning tools (Ford-Gilboe et al. 2020; Hegarty et al. 2019; Bloom et al. 2014) and virtual therapies (Ragavan et al. 2020; Gray et al. 2015) may result in improved safety-seeking behaviors and mental health outcomes for survivors of domestic violence. In light of this, policy responses to any future public health crises that may reduce in-person medical care utilization should account for reduced ability to screen for domestic violence.

2 Data

2.1 Medical Claims: COVID-19 Research Database

We use data from the COVID-19 Research Database, a collaboration between industry leaders and researchers with the objective of providing medical data for pandemic-related research.[4] The COVID-19 Research Database is a collection of health databases including multiple medical claims data sources. We use de-identified medical claims data from a claims data aggregator that facilitates reimbursement for submitted claims between payers and billing providers. This aggregator spans over 100 million patients in 48 states with tens of millions of visits to healthcare providers. We construct a balanced panel of emergency department visits to healthcare providers that consistently appear in the dataset from

2017-2020.[5]

Figure 1 shows the geographic coverage of our sample. The finest geographic designation available in our dataset is the three-digit zip code, which is often roughly city-sized in urban and suburban areas and often roughly county-sized in rural areas. The sample is not nationally representative, as coverage differs across states, and we do not observe uninsured patients. However, even after dropping zip codes with apparent reporting issues that are difficult to resolve at the billing provider level (described below) and areas in which no emergency department billing providers report any assault visits during the panel, our sample contains billing providers from emergency departments in 449 different three-digit zip codes, resulting in at least partial coverage of about 50% of regular residential three-digit zips in the

United States.[6] This large coverage area should provide sufficient information to identify impacts on medical treatment in emergency department claims for domestic violence injuries at the onset of the pandemic compared to the usual seasonal pattern.

2.2 Identifying Domestic Violence Visits

We construct counts of emergency department claims for visits to treat domestic violence injuries as outcomes. We use two different definitions of domestic violence visits constructed from medical billing record use of International Classification of Disease (ICD-10) coding. We use a feature of ICD coding known as external cause of injury codes, or "E-Codes." E-codes track injuries by intent and mechanism of injury, and include designations for assault injuries generally as well as injuries believed to be caused by domestic abuse specifically. While E-codes are not a billable medical code, their use is standard practice in medical coding such that over ninety percent of emergency department injury visits include an E-code (Barret, M 2016). We use claims submitted for reimbursement for professional services[7], identifying emergency department services from the place of service listed on the professional claim.[8] We identify a visit as a unique patient treated by physicians in an emergency department on a single day of service and we classify each visit according to the associated E-codes. We de-duplicate claims that are likely to reflect billing errors or changes by excluding duplicate claims for the same patient for the same diagnosis codes, billing provider NPI and service dates.

We use E-codes to classify narrow and broad definitions of domestic violence visits in accordance with the Health Resources and Services Administration Uniform Data System (UDS) Reporting Requirements (Rebbe et al. 2022; Uniform Data System 2021 Health Center Data Reporting Requirements 2021). Our most narrow visit outcome is abuse, ICD-10 codes T74 or T76, denoting suspected or confirmed adult and child abuse, neglect and other maltreatment. This coding choice includes few false positives, where a visit for assault injuries caused by a party outside the household is erroneously counted as domestic violence (Brown et al. 2023; Rasooly et al. 2023). However, this outcome severely undercounts the number of domestic violence cases, since victims often do not share with medical providers the source of their assault injuries (K. V. Rhodes et al. 2011). For this outcome, we include both female and male patients.

We also include a broader outcome of "female assault" to capture female victims who are not identified by their provider as abuse victims but are nonetheless victims of domestic violence. About 75% of assaults of women are domestic violence (Tjaden and Thoennes 1998), and an influential paper by Aizer (2010) studying the effect of the gender wage gap on domestic violence uses the same proxy. We cannot measure visits for male victims of domestic violence using this particular outcome because the majority of assaults of men are not related to domestic violence (Lauritsen and Heimer 2012). This outcome measure does introduce some false positives, as female assault visits will include assaults perpetrated by outside parties as well as those perpetrated by partners. However, it omits fewer true domestic violence cases than the more strictly defined "abuse" outcome because it includes domestic violence victims who do not tell their medical providers the source of their injuries. Because most assaults against women are by intimate partners and our results are similar across abuse and female assault visits, it is unlikely that the estimated changes in female assault visits are driven by perpetrators outside the home.

In addition to the measures of domestic violence, we also include outcome variables for accidental injuries (falls, cuts, strikes) and firearm assaults in order to more fully characterize care-seeking behaviors during the COVID-19 pandemic. Accidental injuries are a useful benchmark outcome for which the underlying incidence of injuries does not depend on police reporting or interactions, and our estimated changes in accidental injury visits are similar in magnitude to documented decreases in preventative care visits as well as national trends (Cantor et al. 2022; Law et al. 2022). Accidental injuries are an important comparison to characterize changes in emergency department claims for external injuries, as increases in violence, decreases in the likelihood of seeking emergency medical care due to perceived risk or strain on the healthcare system, and decreases in claims due to changes in insurance status may have offsetting effects. We omit injuries resulting from motor vehicle accidents to avoid measuring decreases in injury driven by decreasing vehicle use. We also limit the analysis to adults, which prevents this benchmark from being biased by the increase in children's accidental injury visits due to supervisory neglect during the pandemic (Bullinger et al. 2021).

2.3 Panel Construction

We construct a balanced panel of billing providers that consistently submit claims for reimbursement to the COVID-19 Research Database claims data aggregator. We construct a sample of "good reporters" from billing provider unique identifiers to consistently track weekly emergency department visit volume without bias from idiosyncratic data reporting behavior at the provider level as billing providers may enter or exit the full sample.[9] We require that each billing provider, which generally measures a physician or physicians network, to submit at least one claim in a given state in at least 50 weeks of each year in our sample period.. Any provider that does not meet this condition is dropped entirely in order to ensure that results are not driven by changes in sample composition over time. This approach is especially important to appropriately use the COVID-19 Research Database. Payers may change claims clearinghouse services, triggering changes to where billing providers submit claims. Billing provider inclusion in the research database varies in terms of how far back in time records are included. Our approach avoids

false zeros in the panel, or instances where a provider does not report claims despite actually seeing patients for assault injuries because it does not appear in the dataset that week.

We compare seasonal patterns in 2017–2018, before the pandemic, to those in 2020. We exclude 2019 from our sample due to a known data reporting issue identified by the data provider that results in an artificial dip in claims in April through July of 2019.[10] After filtering out billing providers with irregular reporting behavior, claims are aggregated to the zip-week level.

Despite filtering out individual billing providers based on inconsistent reporting behavior, some reporting anomalies remain at the three-digit zip level. Specifically, areas that report very few assault or accidental injury claims will infrequently report disproportionately high visit counts. To mitigate this concern, each three-digit zip area a is assigned a percentile $Percentile_{ay}$ of assault claims and accidental injury claims reported in the first ten weeks of each year y of the panel. Each zip area's percentile range $PctRange_a$ is calculated as:

$$PctRange_a = max_z\left(Percentile_{ay}\right) - min_z\left(Percentile_{ay}\right)$$

1

Areas above the 95th percentile of $PctRange_a$in either assault claims or accidental injury claims are dropped from the panel, as their extreme movement in percentiles across years in the weeks of the year prior to the onset of the pandemic is likely to be the result of changes in reporting behavior rather than a measure of true changes in injury visit rates.

The final panel includes professional claims for assault and abuse injuries across 449 three-digit zip areas over three years. Many of these areas rarely report assaults in our sample, and may not report assaults every year. Results are robust to limiting the sample to areas that do report assaults every year, as these areas provide most of the variation in the panel (See Fig. 7).

2.4 Potential Sources of Heterogeneity in Care-Seeking Behavior

We also explore domestic violence visits unlikely to be deterred by COVID-19 risk by examining severe types of injuries likely to require medical attention. We focus on gun assaults - including rifles, handguns, and unknown firearm types.[11] Gun assaults are the rarest outcome in the data and analyses of gun assaults disaggregated by gender are underpowered. Nonetheless, gun assaults are a measure of extremely severe instances of assault in which patients are very unlikely to forgo medical care due to other concerns. Patients treated in emergency departments for assault by gunshot wound suffer injuries so severe that the majority are admitted for further treatment in an inpatient setting (Fowler et al. 2015). Even with inpatient treatment, about one in six gun assaults are fatal (Barber, Cook, and Parker 2022; Braga and Cook 2018). Furthermore, firearms are used in the majority of intimate partner homicides (Gold 2020), so female gun assault visits are likely to primarily reflect extremely severe domestic violence incidents.

To further capture differences in seriousness of injury, we calculate an Injury Severity Score (ISS) for all assault injuries.[12] The ISS score classifies injury severity according to risk of injury mortality, morbidity, and inpatient hospital admission (Petridou and Antonopoulos 2017), enabling us to differentiate between severe injuries likely to require immediate medical attention and more minor physical injury. To calculate the ISS score, each of a patient's affected anatomical injury area is scored from 1 to 6 where a score of 1 constitutes a "minor" injury while 6 denotes an "untreatable" injury (Clark et al. 2018). Each anatomical injury area is aggregated to a total score, which is binned into four final severity categories ranging from category 1 for minor injuries to category 4 for severe life-threatening injuries. We therefore interpret scores in category 1 to reflect injuries that are not life threatening, and scores in categories 2–4 to reflect injuries that could result in threats to the patient's life or long-term health if not treated. Table 2 in Appendix A presents descriptive statistics for visits by injury severity score; we see on average 1.18 assault visits and 22.52 accident visits per zip-week in category 1, and 0.11 assault visits and 1.84 visits per zip-week in categories 2–4 combined, suggesting that the vast majority of visits of both types are for injuries that are not life-threatening.

2.5 Interpretation of Outcomes

Because each outcome - assault, accidental injury, or gun assault visits - is the number of patient visits submitted to the claims aggregator for a given injury type in three-digit zip area *a* at time *t*, it is reflective of several factors including patients' decisions to seek medical care if injured $P(Care|Injury)_{at}$,the insurance status of injured patients$P(Insurance|Injury)_{at}$, and injury status of persons in the area $(TotalInjuries)_{at}$. Visits counts can therefore be conceptualized as the following composite of the three latent variables:

$$VisitClaims_{at} = P(Care|Injury)_{at} * P(Insurance|Injury)_{at} * (TotalInjuries)_{at}$$

2

For domestic violence injuries, all three latent variables are likely to be affected by the pandemic. Total injuries likely increased or remained stable given the documented increases in domestic violence 911 calls discussed in Section 1. However, many victims may have foregone or delayed emergency medical care due to COVID-19 risk, strain on the healthcare system, or increased partner control. Finally, some victims may have become uninsured due pandemic-related job loss, causing their visits to be excluded from the sample, which we discuss in Section 4.2.

Table 1 presents summary statistics for main outcome variables. Female assault visits are about ten times as common as abuse visits in the sample.

Table 1

Summary Statistics.

	Mean	Std. Dev.	Min	Max	Count
Abuse Visits	0.06	0.40	0	13	70044
Female Assault Visits	0.64	2.38	0	43	70044
Accident Visits	11.09	34.85	0	377	70044
Gun Assault Visits	0.04	0.29	0	10	70044

All outcomes are counts at the zip-week level. Authors' calculations using the COVID-19 Research Database.

2.6 Benchmark Measures

We first benchmark our estimates of changes in emergency department domestic violence injury visits against ED visits for accidental injuries, excluding car accidents due to changes in driving behavior. Because we find that accidental injury visits decreased by roughly the same percentage as the decrease in preventative care visits estimated by Cantor et al. (2022), we use these estimates as a benchmark for the likely change in emergency department utilization for physical injuries in the absence of large expected changes in the underlying need for care.

3 Methodology

To investigate whether the pandemic and associated behavioral and policy responses changed emergency room visits for abuse and assault, we use a strategy akin to a difference-in-differences design. We contrast emergency room visits in 2020 to the usual seasonal pattern of visits in 2017 and 2018 before and after the onset of the pandemic in March.[13] Prior year comparison groups are essential to capture the true effect of pandemic behavior change, as many in areas and time periods not under stay-at-home orders nonetheless significantly reduced their mobility (Goolsbee and Syverson 2021) and past work documents significant seasonal variation in domestic violence (Einiö 2019; Farrell and Pease 1994). This design is similar to that used by Leslie and Wilson (2020), the first to document the increase in domestic violence 911 calls at the beginning of the pandemic. Although this is not a canonical difference-in-differences design with a cross-sectional comparison group, it still identifies the causal effect of the pandemic on domestic violence-related emergency department visits provided that seasonal trends in visits would have been similar in 2020 to those in 2017 and 2018 if the pandemic had not happened.

3.1 Event Study

To assess how domestic violence-related visits differed in 2020 from the usual seasonal pattern, we use an event study regression comparing a given two-week period t in 2020 to the same period in 2017 and

2018. Each observation $AssaultClaims_{awy}$ is at the week level but coefficients are pooled at the two-week period beginning in week τ of the year, or in other words where

$$t = w \forall oddw$$

$$t = w - 1 \forall evenw$$

,
as assaults are a relatively rare outcome in some zips and are somewhat noisy at the weekly level.

We estimate:

$$AssaultClaims_{awy} = \sum_{t=1,3,5\ldots}^{51} \beta_t \left(Periodt_w * Year2020_y\right) + Year2020_y + \theta_t + \theta_a + \epsilon_{awy}$$

3
with the two-week period τ beginning week 9 of the year as the reference period. β_t is interpreted as the year-over-year change in assault injury visits in period t of 2020 over the average in the same period in 2017 and 2018. θ_t is a two-week period fixed effect, and θ_a is a three-digit zip area fixed effect. Standard errors are clustered at the three-digit zip area level. Results from a similar specification omitting these fixed effects to allow for pandemic-induced migration are presented in Fig. 7 and are similar to the main estimates.

3.2 Pooled Difference-in-Differences

We pool different phases of the pandemic together based on mobility patterns: the first phase will be the early weeks of the pandemic in which mobility patterns were changing rapidly, and the second phase will be the later weeks of 2020 in which mobility, though lower than before the pandemic, had stabilized considerably. In analyses where several weeks are pooled together, we designate the beginning of the “first phase” of the pandemic from March to June beginning in Week 10, the week mobility patterns began to significantly change (Elarde et al. 2021; Kellermann et al. 2022; Lee et al. 2020). We designate Week 26 at the beginning of the “second phase” from July to December, where some social distancing behavior remained but mobility patterns were more stable (with the exception of holidays).

We estimate the following pooled difference-in-differences specification to obtain an overall estimate of how the number of assault and abuse claims in each phase of the pandemic differed from the usual seasonal average:

$$AssaultClaims_{awy} = \beta_0 + \beta_1 Year2020_y + \beta_2 \left(Mar - Jun\right) + \beta_3 \left(Jul - Dec\right) +$$

$$\beta_4 Year2020_y * \left(Mar - Jun\right) + \beta_5 Year2020_y * \left(Jul - Dec\right) + \theta_a + \epsilon_{awy}$$

4
where (Mar-Jun) and (Jul-Dec) are indicators for the first and second phase of the pandemic, and the coefficients on the interaction terms β_4 and β_5 can be interpreted as the change in visits in each phase of

the pandemic in 2020 compared to the usual seasonal pattern of claims in those weeks in 2017 and 2018.θ_a is a three-digit-zip fixed effect and standard errors are clustered at the three-digit-zip level. It should be noted that, based on the pattern of effects in the event studies, the effect appears to be concentrated in the early months of the pandemic. Splitting the pandemic into these two phases in the pooled regression allows us to capture the possibility that some patients who were injured in the first phase of the pandemic may have delayed care or faced different factors determining the decision to seek medical care than in later weeks of the year.

4 Results

4.1 Main Results

Our first main finding is that the pandemic and associated shutdowns social distancing measures are associated with decreases in abuse, accident, and female assault visits treated in emergency departments. Figure 2 shows event studies in the number of visits per zip-week, with event study coefficients pooled into two week periods (see Eq. 3) and overall difference-in-difference coefficients pooled into the early and later phases of the pandemic (see Eq. 4). The top left panel contains results for abuse visits, where the provider knows or suspects that the injuries are a result of domestic violence, showing an average decrease of about 0.02 visits per zip-week which translates to a 35% reduction relative to the prior years' average, between March and June of 2020. The largest decrease, about .04 visits per zip-week (68% reduction), occurs in weeks 13–18. Likewise, the top right panel shows results for all female assault visits, showing an average decrease of 0.2 visits per zip-week (30% decrease) over the same period, with the largest decrease (about .35 visits per zip-week or about 52%) also occurring in weeks 13–18. Accident visits decreased by 4.48 visits per zip-week (38%) in the early phase, with even larger decreases of about 7 visits per zip-week (roughly 58% decrease) occurring in weeks 13–16. The magnitudes of the declines in accidental visits are in line with those estimated by Cantor et al. (2022) for decreases in preventive care visits for colonoscopies (34% relative reduction) and mammograms (38% relative reduction) immediately after the implementation of shelter-in-place policies, suggesting that the change is likely to be driven by care utilization rather than the number of injuries for which patients would normally seek emergency care.[14] Lastly, unlike accidental injury and overall assault visits, gun assault visits increased in the early (March-June) phase of the pandemic by about .02 visits per zip-week (45%). This contrasting result would be consistent with some victims forgoing care unless they believe their injuries will be more dangerous than any COVID-19 infection risk or retaliatory violence that may result from accessing emergency medical care.

In the later phase (July to December), accident visits treated in emergency departments reverted towards baseline levels but remained approximately 25% below prior-year averages. In contrast, female assault visits recovered to 16% below prior-year averages. Abuse visits remained 10% lower than prior-year averages, but this difference was not statistically significant as these visits are relatively rare compared to the other visit categories. Gun assault visits remained 47% higher than prior years during this later phase.

The differences between these effects are consistent with decreases in care utilization rather than decreases in underlying victimization. This accords with other studies about domestic violence during the pandemic: Erten et al. (2021) find that 911 calls increased in the early weeks of the pandemic and decreased after the CARES Act economic impact payments were disbursed to eligible households. Miller et al. (2022) show that increases in 911 calls were concentrated prior to the implementation of stay-at-home orders. Because we find more pronounced decreases in assault compared to accident visits during the early period when 911 calls were increasing, it is unlikely that these decreases in visits coincide with decreases in the underlying number of domestic violence injuries that would normally cause victims to seek emergency medical care. Rather, victims likely declined to seek care for other reasons such as COVID-19 risk or increased partner control, which we will explore in Section 4.2. We also compare our main results to those limiting the sample to geographic areas included in Leslie and Wilson (2020), Erten et. al. (2021), and Miller et. al. (2022), in Fig. 9 in Appendix B; our main results, while underpowered, are qualitatively similar in those samples.

4.2 Mechanisms

We explore the severity of victim injuries to examine how victims with severe injuries sought medical care for their injuries. Figure 3 illustrates that decreases in female assault visits are concentrated in minor injuries (Injury Severity Score Category 1) while severe or life-threatening emergency department visits for female assaults (Injury Severity Score Categories 2–4) remained more stable. Female assault visits for minor injuries declined by 0.50 visits per zip-week (a 32% decrease relative to prior year averages) in the early phase of the pandemic and 0.28 visits per zip-week (18% decrease) in the later phase compared to prior years. By contrast, visits for severe assaults (Injury Severity Score Categories 2–4) were not statistically different from prior years across early and late periods.

The left panel of Fig. 3 shows accident visits by injury severity, indicating that minor injury visits declined by 12.25 visits per zip-week, or 42% of the past years' average, in the early pandemic months (May-June). Late pandemic minor accidental injury visits declined less relative to prior years, decreasing by 8.61 visits per zip-week in July to December (30% relative decrease). Contrary to severe female assaults which remained stable relative to prior-year averages, severe accidental injuries decreased by almost one visit per zip-week (32% decrease) in the early pandemic months, followed by a decrease of 0.25 visits, or 11%, in later pandemic months.

We supplement this analysis using reports of domestic and intimate partner assaults reported to police during the same timeframe (see Appendix C, Fig. 10) and find similar heterogeneity across injury severity stemming from reported crimes. Domestic violence assaults resulting in major injuries declined by at most 3–4%, suggesting that the large declines in emergency department visits that we find in our sample are unlikely to be driven by decreases in the incidence of domestic violence injuries. Furthermore, changes in reported domestic violence crimes resulting in major injuries are similar whether 2019 is included in or

excluded from the comparison time period, suggesting that our choice to exclude it due to poor medical claims data reporting is unlikely to affect our results.

Taken together, heterogeneity by injury severity is suggestive of a decrease in care utilization rather than changes in levels of domestic violence. Severe female assault visits to the emergency department declined alongside severe accidents in the early months of the pandemic. But relative to severe accidents and abuse, severe female assaults returned to prior-year levels in the later phase (July-December) of the pandemic. These estimates would be consistent with either severe injuries increasing during the early phase of the pandemic and victims delaying or forgoing care, or severe injuries remaining stable during the early phase and becoming more likely during the later phase. Either scenario is plausible given that victims' response to the COVID-19 pandemic may have softened over time leading them to eventually seek care, or victims experiencing domestic violence incidents during the early phase that did not result in significant outside intervention (crime incident report, arrest, or medical care) may have experienced subsequent escalations in violence within the relationship resulting in more severe injuries later in the year.

Because the underlying dataset we use to construct our panel consists of insurance claims, a primary concern is that pandemic-related job loss resulted in artificial decreases in claims in our data if patients were no longer eligible to appear in the sample due to loss of insurance but were still visiting emergency departments. To mitigate this concern, we show trends in abuse and female assault visits by insurance payer type in Fig. 4. As described in Section 2, "low loss" payers in Panel A are those associated with government-sponsored insurance programs such as Medicaid, Medicare, and ACA exchange plans, so patients on these plans are less likely to lose their health insurance after a pandemic-related job loss. "Regular commercial" payers in Panel B are other payers that are likely to contain primarily employer-based plans.[15]

Figure 4 displays similar trends in female assault visits across low-loss payers and regular commercial payers, suggesting that pandemic-induced decreases in visits are unlikely to be driven by patients' exit from the sample due to insurance loss and likely reflect actual decreases in emergency medical care utilization by domestic violence victims. Relative to the full sample (Fig. 2), where abuse visits fell in the early stages of the pandemic by approximately 31%, visits from patients insured by "low-loss" payers declined by 40% and commercial payers declined by 31%. Similarly, relative to the 30% reduction in assault visits during the early stage of the pandemic, among "low-loss" payers, assault visits dropped by 30% and visits from patients with commercial insurance decreased by 28%.

Finally, we explore how victims may have elected to forgo care for pandemic-related reasons such as increased partner control due to increased time at home together. We examine the timing of domestic violence visits to examine if changes in time spent at home during lockdowns altered medical care timing. Prior to the pandemic, victims may have been more able to seek care on weekdays while their partners were at work. Detailed in Fig. 7 in Appendix B, we do not find strong evidence that the pandemic

shifted the timing of visits within the week in response to changes in victims' or abusive partners' work schedules, but cannot fully rule out a partner control mechanism given the nature of our data.

4.4 Robustness

Figure 5 shows the robustness of the main results to two alternative specifications. Overall, results are qualitatively similar across specifications and suggest a decline in emergency medical care utilization for domestic violence injuries during the early weeks of the pandemic when mobility was lower. The first alternative specification drops "sparse zips" – those where assault and abuse visits are the most rare due to having few billing providers in our sample. We classify a zip as sparse if there is at least one year in the panel where providers in that three-digit zip do not report any assault or abuse visits. The left side of Panel A shows results for abuse visits; in the early phase of the pandemic, abuse visits decline by about 32% of the zip-week mean in the main sample and about 70% of the zip-week mean in this sample where sparse zips are dropped; this is unsurprising, as the three-digit zips with more assault visits provide most of the variation. We see a similar pattern for female assault visits in Panel B: in the early phase, female assault visits in the sample dropping sparse zips decline by about 67% (compared to 30% in the main sample) and by about 37% in the later phase (compared to 17% in the main sample).

The next alternative specification removes three-digit zip fixed effects to allow for the possibility of pandemic-induced migration. The right side of Panel A shows results for abuse visits, finding a decline of 34% with a return to prior-year averages in the later part of the pandemic. Female assaults dropped 29% in the early part of the pandemic and 16% in the later part of the pandemic, nearly identical to the main sample decline, suggesting that results are not affected by migration across three-digit zip areas.

5 Conclusion

Overall, we find that visits for domestic violence injuries declined considerably in the early phase of the pandemic from March to June of 2020. Visits recovered in the later months of the year, but remained lower than prior years. We find that the decline is driven almost entirely by less severe injuries; visits for severe or life-threatening injuries remain stable in the early phase of the pandemic and increase in the later phase. The decline in assault visits is also smaller in magnitude than both our estimated decrease in accidental injury visits and previously documented decreases in preventative care visits, suggesting that decreases in visits are driven by declines in emergency department use rather than declines in actual injuries. Furthermore, trends in assault claims are similar across insurance types, confirming that the decline in claim counts is likely to be driven by actual declines in the number of visits rather than a mechanical effect of patients exiting the sample due to insurance loss. The totality of our results, in combination with the findings in the literature documenting an increase in domestic violence 911 calls and a decrease in medical visits at the onset of the pandemic, leads us to conclude that while domestic violence injuries were likely increasing in the early weeks of social distancing behavior, victims were less likely to receive emergency medical care for their injuries. Our findings underline that policy responses to

future public health crises that reduce emergency medical care utilization should include alternative strategies for outreach to victims of domestic violence.

Declarations

Conflicts of Interest

We declare that the authors have no competing interests as defined by Springer, or other interests that might be perceived to influence the results and/or discussion reported in this paper.

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Footnotes

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3. lauren.schechter@nyu.edu, Social Science Research Council and New York University Public Safety Lab
4. We considered including urgent care providers, but they comprised only 4% of billing providers who 1) consistently appeared in our dataset and 2) reported at least one assault in the data, so we elected to drop them.
5. Our data use agreement does not permit us to disclose the name of the claims aggregator that provides the data to the COVID-19 Research Database. Several aggregators provide datasets to the database, and researchers are discouraged from requesting access to multiple datasets, so we limit our analysis to this single claims dataset.
6. We define “regular residential” three-digit zips as those that are still in use and are not designated for a special purpose such as government, IRS, military, and parcel return. By this definition there are 895 regular residential three-digit zips.
7. While institutional or hospital bills have valuable information and may capture a somewhat larger proportion of the universe of emergency department claims than a provider-based definition of emergency department services (Venkatesh et al. 2016), in practice, the claims data aggregator masks fields that would enable identification of inpatient vs. outpatient visits on institutional claims.
8. We considered including urgent care providers, but they comprised only 4% of billing providers who 1) consistently appeared in our dataset and 2) reported at least one assault in the data, so we elected to drop them.
9. The claims aggregator informed us that claims are reported to the dataset by billing providers, not by insurance payers, which informed this choice. They were unable to provide further information about how claims are selected into the dataset, which led us to develop this approach to ensure a consistent sample.

10. Appendix C reports results using this same strategy to estimate changes in police reports for domestic violence using NIBRS data, and these results suggest that trends in domestic violence in 2019 were not meaningfully different from those in 2017–2018 and the choice to exclude 2019 from the comparison period should not meaningfully affect any estimated changes at the onset of the pandemic.
11. BB guns, paintball guns, and air guns are the only types of firearms excluded from firearm assaults.
12. We use the open source ICDpicr package for Injury Categorization in R.
13. We omit 2019 from the comparison group, as the claims aggregator has informed us that there was a claims reporting issue in the summer of 2019 that caused claim counts to be artificially lower during that time.
14. Cantor et al (2022) present various estimates controlling for differential COVID-19 risk conditions leading jurisdictions to implement shelter-in-place policies at different times. We focus on estimating the overall effect of the pandemic and associated behavior changes, so we opt to compare to the usual seasonal pattern from prior years and designate all areas as treated at the same time.
15. Appendix B Fig. 8 presents analogous results comparing Medicaid payers to non-Medicaid payers, but non-parallel pre-trends and limited statistical power among Medicaid payers warrant caution in interpreting these results.

Figures

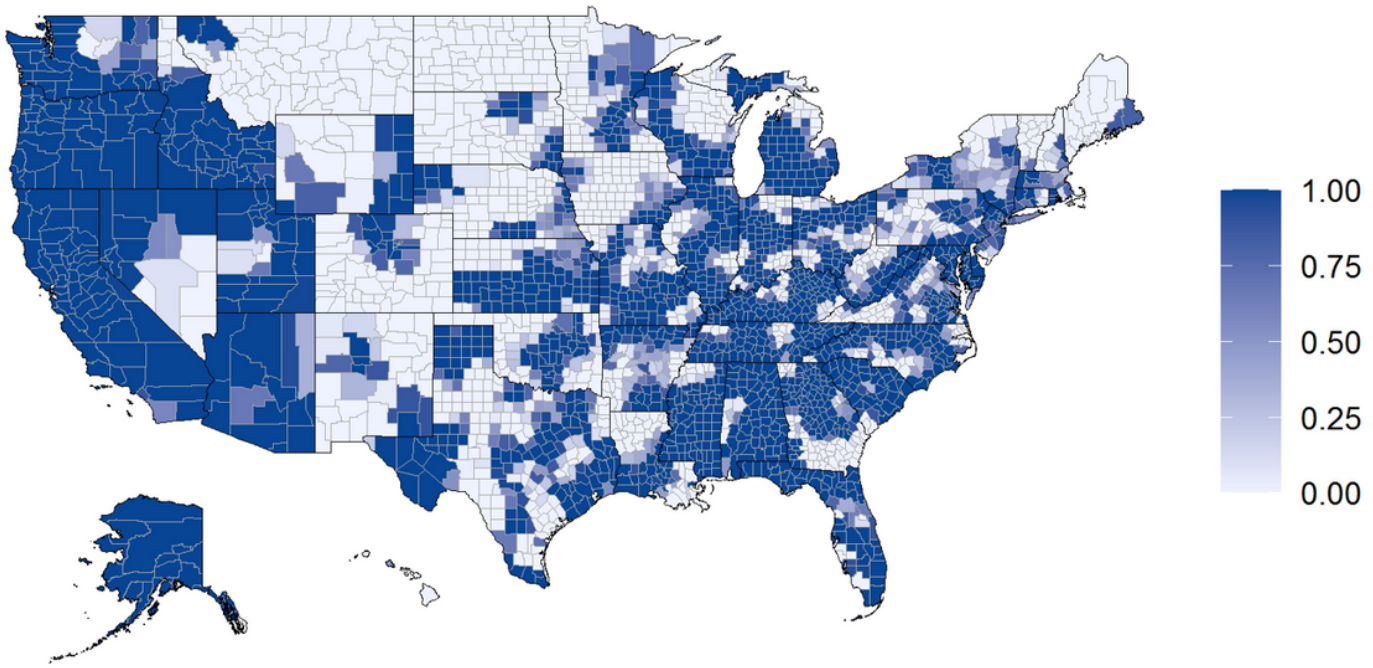

Figure 1

Sample Coverage Map. Map shading represents the portion of a county's population residing in a three-digit-ZIP area in our sample. Three-digit ZIP areas included in the sample are not fully covered, and their counts represent only the billing providers who 1) consistently appear in the dataset and 2) report at least one emergency department assault visit during our sample period.

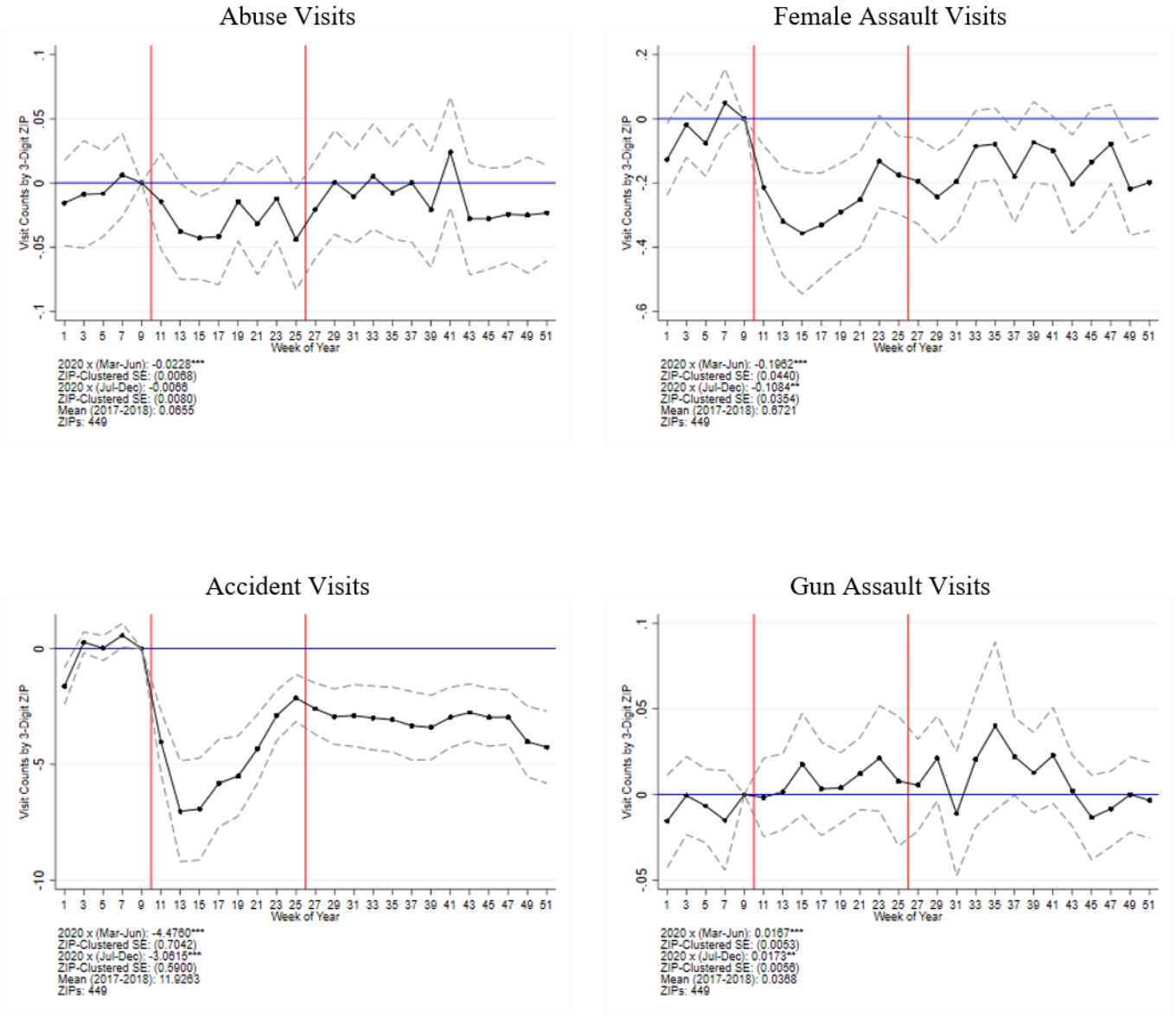

Figure 2

Abuse and Female Assault Visits. All event studies include three-digit zip and week-of-year fixed effects and compare zip-week visit counts in 2020 to the same zip-weeks in 2017 and 2018. All outcomes are visit counts at the zip-week level. Event study estimates are results from the specification in Equation 3 with dotted lines denoting 95% confidence intervals. Pooled difference-in-differences estimates reported below each event study are results from the specification in Equation 4. ***,**,*,+ denote $p < .001$, $p < .01$, $p < .05$, and $p < 0.1$, respectively.

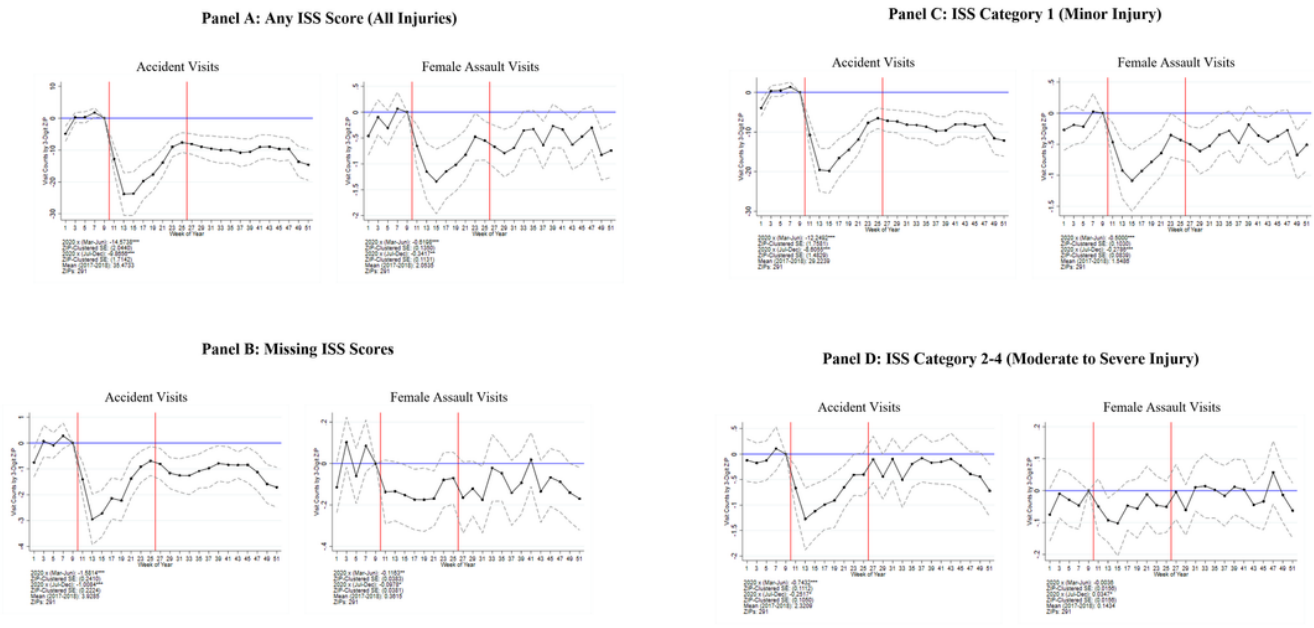

Figure 3

Accident, Female Assault Visits by Injury Severity. All event studies include three-digit zip and week-of-year fixed effects and compare zip-week visit counts in 2020 to the same zip-weeks in 2017 and 2018. All outcomes are visit counts at the zip-week level. Left panel includes accident visits by Injury Severity Score Category. The right panel includes female assault visits by Injury Severity Score Category. Category 1 denotes injuries that are not life threatening. Injury Severity Score Categories 2-4 denote injuries that could severely threaten the life or long-term health of the patient if not treated. These visits are the subset of the female assault visits in Figure 2 for which injury coding was sufficiently well-populated to calculate the injury severity score. As a result, a few sparse zips have been removed from this sample as providers in those zips did not enter this data. Event study estimates are results from the specification in Equation 3 with dotted lines denoting 95% confidence intervals. Pooled difference-in-differences estimates reported below each event study are results from the specification in Equation 4. ***,**,*,+ denote $p < .001$, $p < .01$, $p < .05$, and $p < 0.1$, respectively.

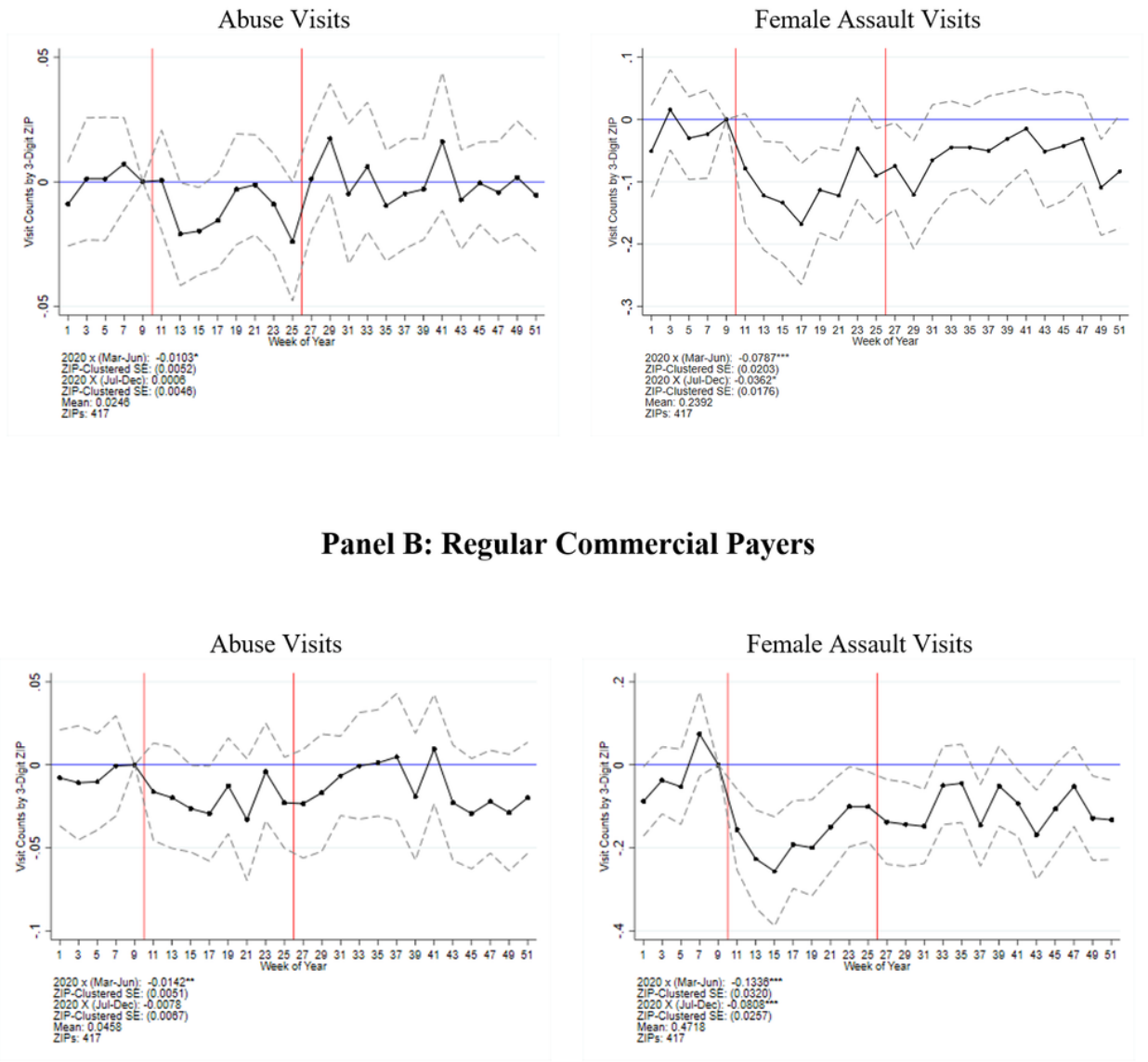

Figure 4

Abuse, Female Assault Visits Among Low Insurance Loss Groups. All event studies include three-digit zip and week-of-year fixed effects and compare zip-week visit counts in 2020 to the same zip-weeks in 2017 and 2018. All outcomes are visit counts at the zip-week level. Event study estimates are results from the specification in Equation 3 with dotted lines denoting 95% confidence intervals. Pooled difference-in-differences estimates reported below each event study are results from the specification in Equation 4. ***,**,*,+ denote p < .001, p < .01, p < .05, and p < 0.1, respectively.

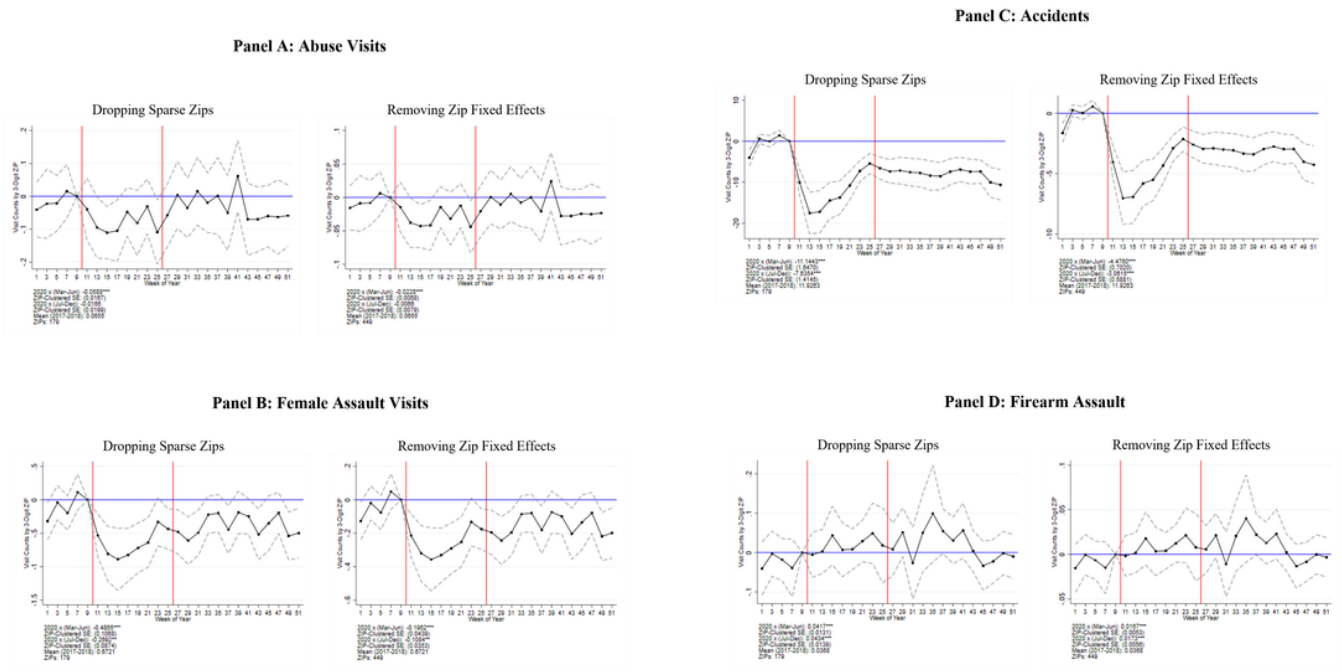

Figure 5

Main Outcome Visits (abuse, female assault, accident, gun assaults) Without Sparse Three-Digit Zips, Zip Fixed Effects. All event studies include three-digit zip and week-of-year fixed effects and compare zip-week visit counts in 2020 to the same zip-weeks in 2017 and 2018. All outcomes are visit counts at the zip-week level. Event study estimates are results from the specification in Equation 3 with dotted lines denoting 95% confidence intervals. Pooled difference-in-differences estimates reported below each event study are results from the specification in Equation 4. ***,**,*,+ denote p < .001, p < .01, p < .05, and p < 0.1, respectively.

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- AppendixREHO06202023.docx
- Fig10Arightno2019.png
- Fig10Bleft2019.png
- Fig10Brightno2019.png
- Fig10Cleft2019.png
- Fig10Crightno2019.png
- Fig10Dleft2019.png
- Fig10Eleft2019.png

- Fig10Erightno2019.png
- Fig6botleftgun.png
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- Fig6topleftabuse.png
- Fig6toprightfemassault.png
- Fig7Aleftweekend.png
- Fig7Arightweekday.png
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- Fig8leftnonmcd.png
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- Fig9Atoprightlw.png
- Fig9Bbotleftekp.png
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