COVID-19, Job Loss, and Intimate Partner Violence in Peru*

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Abstract

A large literature has explored the effect of the COVID-19 pandemic on intimate partner violence (IPV) worldwide. However, few studies provide clear evidence on the mechanisms through which the pandemic exacerbated violence and many rely on hotline or police report data, which confounds changes in reporting behavior. Our paper addresses this issue by conducting a large nationwide survey in Peru, a country that has been hit particularly hard by COVID-19. We isolate pandemic-related economic shocks based on geographic variation in the industry composition of employment shocks, and find a sizable and sustained increase in IPV, which aligns with the patterns found in helpline calls. Households most likely to lose a job experienced the largest increases in IPV. These patterns indicate that economic losses were an integral causal mechanism through which COVID-19 increased IPV.

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1 Introduction

Worldwide, the COVID-19 pandemic has generated great concern over its economic and social effects. An increase in intimate partner violence (IPV) is one of the most critical worries. Early in the pandemic, global stakeholders raised alarm bells by predicting an additional 31 million cases of gender-based violence worldwide (UNFPA, 2020). A growing number of papers has documented increases in IPV in both developed and developing countries during the pandemic (Bourgault et al., 2021).

However, two important questions remain unanswered in order to understand the relationship between the pandemic and the incidence of IPV. First, much of the empirical work on the topic has studied trends in the frequency of helpline calls or police reports to evaluate pandemic-driven changes in IPV (e.g., Leslie and Wilson, 2020; Bullinger et al., 2020; Perez-Vincent et al., 2020; Agüero, 2021). Yet, because access to in-person services, including social support networks, were restricted during the lockdown, it is unclear to what extent increases in IPV-related emergency calls reflect a substitution away from traditional sources of victim support. Second, to the extent that helpline trends reflect higher rates of violence, little is still understood about the particular mechanisms through which the pandemic has exacerbated IPV, and, specifically, what role did pandemic-related economic shocks play in increasing IPV versus more generalized social unrest or anxiety. This distinction is important because it influences both policy prescriptions and projections of future trends in IPV when disease shocks arise; predicting who will be most at risk of IPV going forward and extrapolating to other settings requires disentangling the specific mechanisms at play.

The goal of this paper is to address the previous two issues. First, to disentangle substitution towards phone-based helplines versus a generalized increase in IPV, we partnered with the Peruvian Ministry of Women and Vulnerable Populations (MIMP) to conduct a phone-based survey of 1077 urban women located in cities across the country. Along with demographic data on the household, the survey collected data on the incidence of physical and psychological IPV, as well as changes in economic circumstances, including income and employment, at three points in time before and during the pandemic. We are therefore able to measure IPV before and after the pandemic's onset.

Second, in order to firmly attribute time trends to a causal impact of economic shocks on IPV our analysis makes use of industry-level variation in the degree of economic contraction experienced as a result of the pandemic. We find a large increase in the rate of IPV during the pandemic, on the order of 53% relative to 2019. Households most at risk of employment shocks experience significantly larger increases in IPV compared to pre-pandemic levels. A 10 percentage point increase in job loss in the household head's primary economic sector, which corresponds to a 2.8% decrease in income, is associated with a 9% increase in the likelihood of any physical and sexual IPV and a 4.3% increase in psychological IPV in July-August of 2020. These effects are sizeable considering that the job loss rate in the primary economic sector for the median household was 58%. Meanwhile, we find no link between IPV and local cases of COVID-19. Together, these patterns imply that economic stress resulting from household-level shocks rather than disease anxiety explain the recent surge in IPV in our setting.

We focus on the case of Peru, a country that has been hit particularly hard by COVID-19 in the worst-hit region of the world, despite a rapid and strict policy response by the government. In mid-March 2020, the Peruvian government imposed a broad and early lockdown throughout the country to stop the spread of the virus, and in April they issued an extension of the confinement. Despite these efforts, by 2021 Peru was only second to Brazil in the number of cases and deaths in the number of cases and deaths from coronavirus in Latin America, despite the large population differences. As of September 2021, Peru confirmed 2.17 million cases and nearly 200,000 deaths related to coronavirus. In addition, the economy experienced an 11% decline in GDP and unemployment more than doubled in 2020 compared to 2019. Moreover, both the incidence of new cases and deaths from COVID-19 continued to rise.

The potential consequences of these economic and disease shocks on IPV are particularly concerning in Peru, a country that was already suffering from high and growing rates of gender-based violence pre-pandemic. Data from the *Demographic and Health Survey* (DHS) in 2017 indicate that more than one-third of Peruvian women have experienced physical or sexual violence from an intimate partner during their lifetime (INEI, 2017).[1] Between 2017 and 2019, the rate of femicides in Peru increased by more than 10 percent (Comité Estadístico Interinstitucional de la Criminalidad, 2021). While the reason for the increase is still debated, the upward trends suggest that gender-based violence was unresponsive to the substantial policy effort put forth to tackle the issue and the social protests and media coverage that drew attention to the crisis (Pan American Health Organization, 2019). Indeed, data from the national victims helpline *Línea 100*, reveal that calls increased by 48 percent between April and July 2020, indicating that the pandemic has been yet another factor exacerbating

[1] Proportion of ever-partnered women aged 15-49 years experiencing intimate partner physical or sexual violence at least once in their lifetime.

violence against women in Peru (Agüero, 2021). Calderon-Anyosa et al. (2021) find a similar increase in call volume. However, given that Peru closed all in-person domestic violence services the moment the lockdown started, including hundreds of government shelters for abused women, it is unclear how much of the increase reflects a surge in pandemic-related cases versus substitution across reporting platforms.

Our results are related to prior work exploring the role of income on IPV, especially to work on cash transfers in developing countries prior to the pandemic (e.g., Hidrobo and Fernald, 2013; Hidrobo et al., 2016; Haushofer and Shapiro, 2016; Heath et al., 2020; Díaz and Saldarriaga, 2022). Consistent with these studies, we show that a critical mechanism for the increase in violence during the pandemic is via household-level income shocks. In that regard, our paper is also related to work that attempts to identify the role of the U.S. CARES Act and similar policies applied in other countries (Chetty et al., 2020; Erten et al., 2021). Our findings suggest that such transfers, by reducing the economic hardship of families, have the potential to minimize the impact of economic contraction on violence against women.

Our results also relate to the many articles exploring IPV in Latin America. Hernández et al. (2019) document the importance of Peru's national victims helpline and the rise in demand even before the pandemic. Bardales Mendoza et al. (2022) find that femicides in Peru increased during the pandemic, however Aebi et al. (2021) do not find an increase in femicides when examining more countries in the region. Perez-Vincent and Carreras (2022) examine changes in reporting behaviors during the pandemic. Similar to our findings, Porter et al. (2021) use a list experiment and find that IPV increased during the pandemic in Peru. Using police reports, Hoehn-Velasco et al. (2021) find that most crimes against women decreased during the pandemic in Mexico, while Valdez-Santiago et al. (2021) document an increase in violence using household survey data. We complement these papers by documenting the importance of job loss during the pandemic as a driver of violence and by collecting original survey data on IPV and household employment sector at a national level in order to establish a causal link between job loss and the increase in IPV that occurred during the pandemic in Peru.

The paper is organized as follows: Section 2 describes Peru's policies during the pandemic. Section 3 describes our data and survey. Section 4 explains our empirical strategy. Section 5 documents our results, and section 6 concludes and discusses avenues for future research.

2 Peru's lockdown measures

Peru adopted one of the earliest and most severe lockdowns in Latin America. The first case of COVID-19 was confirmed on March 6th of 2020. Ten days later, on March 16th, the government enacted a nation-wide lockdown through a National State of Emergency (Decreto Supremo 044-2020-PCM). The first COVID-19 death was confirmed on March 19th, after the lockdown had been enacted. The National State of Emergency suspended several constitutional rights, including freedom of movement and transit, as well as the right to gather.

The severe lockdown lasted for over three months, with a localized lockdown approach starting on June 26th of 2020. As in most countries, a State of Emergency was enacted at first for 15 days, but the Peruvian government extended it many times in an effort to lower the transmission of COVID-19. The economic reactivation plan, which allowed people working in specific sectors to commute and work outside their homes, was organized in four phases and started in May of 2020. For instance, restaurants were only allowed to start offering food delivery services in May of 2020. The fourth and final phase started in October of 2020.

Overall lockdown policies were most severe during the first few months, and were progressively loosened until October 2020. This helps contextualize our results. As such, we asked our respondents about their experiences during April-May 2020, and July-August-2020, in order to understand the dynamic effects of the COVID-19 pandemic as restrictions were gradually lifted.

3 Data

The main dataset is a socioeconomic phone survey we conducted between September and November 2020, sampled using random digit dialling (RDD) to reach cellphone numbers throughout the country. Women reached by phone were included in the sample if they were between the ages of 18 and 49 and self-reported to be in a domestic partnership in April 2020. More details of the phone survey are in Appendix A.1. We complemented this sample of RDD respondents with another sample of urban women that were surveyed in 2019 as part of a baseline for an impact evaluation of an intervention of the Peruvian Ministry of Women that was put on hold due to the pandemic. An important advantage of this sample is that respondents were interviewed pre-pandemic allowing us to construct a true panel of household-level behavior and outcomes. The final sample size includes 1077 respondents, 794 from the RDD and 283 from the panel sample.[2] Panel data from these latter respondents allow us to assess the quality of retrospective data on IPV, which we do in Appendix Section A.3.

3.1 Measures of IPV

The survey was retrospective and focused on three recall time periods in collecting information on IPV. First, we asked about the prevalence of violence in all of 2019, prior to the pandemic. We then asked about IPV at two distinct points *during* the pandemic: April-May 2020 and July-August 2020. The latter was the most recent point in time prior to the launch of the survey, while April and May were chosen because they reflect the two months at the very start of the pandemic, in which the strictest lockdown measures were enacted throughout the country. In contrast, by July and August, the lockdown was much less strict and varied greatly across municipalities.[3]

To inquire about IPV, we reproduced the standard set of IPV-related questions used in the Peruvian Demographic and Health Survey (ENDES), which asks respondents to report on six dimensions of IPV.[4] We categorize the frequency of occurrence for each type of violence in each of the three time periods.[5] To estimate the frequency of occurrence, respondents were asked whether each type of IPV occurred "Never", "One Time", "Sometimes", or "Many Times". We coded the option "Never" as 0, the option "One Time" as 1, and both "Sometimes" and "Many Times" as 2.[6] We then added all sub-questions related to physical and/or sexual IPV to form an estimated count of physical and/or sexual IPV, and added all sub-questions related to psychological IPV to create an estimated count of psychological IPV. In the main text, we focus on an indicator variable for any incidence of physical or psychological IPV for each time period. Our results therefore focus on the extensive margin.

Our phone survey mainly captured urban areas: 92.9% of our respondents live in an urban district.[7] In Table 1, we compare our survey with the urban sub-samples from the

[2]The latter sample was not restricted to be younger than 49 years old. However, only 70 respondents were older than 49 and we included them in the analysis.

[3]The *Decreto Supremo N° 116-2020-PCM* established a targeted lockdown starting on July 1st, 2020.

[4]The survey strategy and ethical protocols are described in Appendix A.1.

[5]The 6 questions we use are listed in Appendix A.1 alongside detailed explanations of our variable construction. The original survey in Spanish can be found in the Online Appendix

[6]We group "Sometimes" and "Many Times" together into 2, since we can deduce at least 2 events happened from these questions. However we cannot deduce how many more events happened. We code as 2 as a conservative estimate.

[7]An urban district is a district with at least 50% of its inhabitants living in an urban town, based on the

ENAHO and ENDES datasets. On average, our sample is more educated at baseline than the average obtained from these samples. This is expected given that our phone survey required people to have a cellphone as opposed to in-person surveys that do not have that condition as part of their interview requirements.

We find higher incidence of violence in our phone survey relative to ENDES. Several factors could explain the differences between our survey and the ENDES findings. First, our questions are retrospective, and we interviewed respondents during the height of the pandemic. Studies have found that current mood states, in particular poor mental health and recent experience of violence, correlate with higher reporting of past trauma and adverse events (e.g. Monroe and Harkness, 2005; Pachana et al., 2011). Second, the ENDES surveys were all carried out in person, while our survey was conducted over the phone. Greater anonymity provided by phone may have increased respondents' willingness to report on potentially stigmatizing events (Aguero and Frisancho, 2021; Bulte and Lensink, 2019; Cullen, 2020; Joseph et al., 2017; Peterman et al., 2018).[8]

3.2 Income and Mobility Measures

Our survey also asks about income and employment in each time period. Specifically, respondents are asked to report both employment status and average monthly income for themselves and their spouses, excluding pandemic-related government transfers. We combine these two income sources to obtain a measure of the household's earned income. Additionally, we ask about the primary earner's economic sector before the pandemic. We use this income and employment information to characterize patterns of income shocks across sectors that coincide with the pandemic.

We also asked about the number of days individuals left the house, as the mobility restrictions alone could influence domestic conflict. For each time period, we asked households how many days per week on average they left their home to socialize or to make purchases (e.g., buy groceries). Responses to these two questions were added to create a "total days out" variable.

2017 Census.

[8]In related work, Agüero et al. (2021) show that providing greater anonymity during surveys can increase physical and sexual IPV reporting by up to 6 percentage points. These reasons help explain the discrepancy between the 2019 ENDES results and the results from our survey.

Table 1: Descriptive Statistics

Variable	Nationally Representative Survey	Phone Survey (2020)	Difference
A: Demographics			
Age (women)	35.804	35.597	0.206
Age (male partner)	40.098	37.937	2.161***
Household size	4.401	5.020	-0.619***
% of women w/complete secondary	0.661	0.745	-0.084***
Number of children	1.860	1.732	0.128***
Household income, Soles (2019)	1522.047	1482.020	40.027
Household income, Soles (2020Q2)	791.505	781.481	10.024
Household income, Soles (2020Q3)	1069.353	966.604	102.749
B: IPV			
Psychological IPV	0.102	0.273	-0.171***
Physical and/or sexual IPV	0.105	0.163	-0.058***
Any IPV	0.152	0.305	-0.153***

Notes. Descriptive statistics comparing our 2020 phone survey to the urban sub-samples from two nationally representative surveys conducted in 2019 and 2020 (ENAHO and ENDES). In practice, our 2020 phone survey skews urban, hence we only compare our survey to urban sub-samples. % of women w/ complete secondary refers to fraction of women that completed secondary education. Age refers to year of age. Household size refers number of people living in the household. Household income refers to the total income earned by all members in the household, measured in Peruvian Soles. Unless otherwise noted, all questions for the Nationally Representative Survey refer to 2019 values. Panel A compares our 2020 survey to ENAHO.

Panel B compares the ENDES survey to our measures of IPV. These refer to the fraction of women that have had an IPV event during 2019, as reported by the ENDES and our survey. Both surveys use 2019 as the reference period.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.3 Mental Health Measures

Our survey asks a series of mental health questions. These questions are of the form "In the months of April and May 2020, on average, did you feel more, same, or less [mental health issue] than an average month in 2019." In particular, we asked about anxiety, moodiness, loneliness, rage, urges to raise your voice and urges to act violently. These variables are coded as follows:

$$\Delta M_i = \begin{cases} 1, & \text{if the individual felt more [mental health issue]} \\ 0, & \text{if the individual felt no change in [mental health issue]} \\ -1 & \text{if the individual felt less [mental health issue]} \end{cases}$$

For example, when asking about anxiety, decreases in ΔM_i translate to reductions in anxiety and improvement in mental health.

3.4 Nationally Representative Employment and IPV Surveys

We employ two additional datasets, the National Household Survey (ENAHO) and the Peruvian Demographic and Health Survey (ENDES). The former is released on a quarterly basis and provides information on occupation, earnings, and employment status from a nationally-representative sample of respondents. We use the 2019 ENAHO survey to measure employment outcomes prior to the pandemic, while data from the second quarter of 2020 enable us to measure employment changes during the pandemic. The latter, the ENDES, is a nationally representative survey that provides measures of IPV prevalence, conducted annually. We use the ENDES samples from 2011 to 2019, prior to the COVID-19 pandemic, to construct a placebo test to validate our main empirical specification.

4 Empirical Strategy

Our analysis leverages substantial heterogeneity in the unanticipated employment shocks experienced across economic sectors in Peru. This allows us to investigate whether those most impacted by the pandemic in economic terms experienced higher increases in violence. Fundamentally, we argue that the differential decreases in employment across sectors are unrelated to IPV trends prior to the pandemic. Using panel data on self-reported violence, our main estimates employ a standard difference-in-difference strategy to evaluate the effect of economic shocks on IPV.

To construct a measure of unanticipated employment shocks we classify economic sectors according to employment losses at the pandemic's onset. Using the nationally-representative ENAHO dataset, we calculate the percentage changes in employment in each of the 22 two-digit sector codes between 2019 and the second quarter of 2020. This captures the employment losses that occurred soon after the pandemic started. Each sector and their associated job loss is shown in Appendix Table A1.

We then match the sectoral job losses to households with the main breadwinner's occupation in 2019. While both partners may have been working in 2019, our survey only asks the economic sector for the main breadwinner, i.e. the person identified as the top-earner in the household in 2019 by our survey respondents. [9] Let $s(i)$ be the sector of the economy where the main earner of household i worked at baseline. The shock in each sector, g^{sector}, is given by

$$g_i^{\text{sector}} = \frac{L_{s(i)1} - L_{s(i)0}}{L_{s(i)0}} \times 100 \tag{1}$$

where $L_{s(i)t}$ is the total employment count in sector $s(i)$ at time t. For this measure, $t = 0$ refers to the 2019 average employment count given by ENAHO, while $t = 1$ refers to the second quarter of 2020.[10] As such, standard errors are clustered at the economic-sector level. Note that a larger g_i^{sector} indicates increases in employment, or equivalently decreases in job losses during the pandemic. We therefore expect g_i^{sector} to be negatively correlated with IPV, since with increased employment (or decreased job losses) we expect there to be less IPV risk.

The main estimating equation is:

$$Y_{it} = \sum_{j=1,2} \gamma_j g_i^{\text{sector}} \times 1[t = j] + \alpha_i + \tau_t + u_{it} \tag{2}$$

where Y_{it} is an outcome for person i measured in period t, and α_i and τ_t capture individual and time fixed effects respectively. g_i^{sector} is our measure of an individual exposure to COVID-related employment shocks, based on the main breadwinner's pre-pandemic occupation. Therefore, the coefficients γ_1 and γ_2 can be interpreted as the effect of a one percentage

[9] Our survey directly asked respondents which member of the household earned the most income during 2019. Only 16% of our survey respondents have a woman as the top-earner.

[10] Note that our phone survey asks about IPV during April-May 2020, while we use ENAHO data from the second quarter which is April-June because the ENAHO survey is collected on a quarterly basis. With a slight abuse of notation, we are letting $t = 1$ denote April and May for our outcome variable Y_{it}, while $t = 1$ denotes April-June for the shock variable $g_i^{\text{sector}} = \frac{L_{s(i)1} - L_{s(i)0}}{L_{s(i)0}} \times 100$.

point decrease in pandemic-related job loss. As a result, we expect γ_1 and γ_2 to be negative, since we expect decreases in job loss to reduce IPV risk. For ease of notation, we use $t = 0$ to denote the calendar year 2019, $t = 1$ for the months of April and May of 2020, and $t = 2$ for the months of July and August of 2020. For the IPV outcomes, we use a dummy indicating any amount of violence and estimate the model with OLS.

Figure A1 shows no evidence of differences in pre-existing IPV trends predicted by our measure of exposure. Using repeated cross sectional data from the ENDES surveys from 2011 to 2019 and our own survey, we run a linear probability model similar to Equation (2), where instead of household fixed effects we include district fixed effects, as shown below:

$$Y_{it} = \sum_{j=2012}^{2019} \delta_j g_i^{\text{sector}} \times 1[t = j] + \phi_d + \tau_t + u_{it} \tag{3}$$

where ϕ_d are district fixed-effects, and this regression excludes 2011 as the reference year.[11] There do not seem to be any significant pre-trends, and the estimated effects for physical and psychological IPV during the pandemic are much larger than anything we detect before the pandemic.

4.1 Estimating Time Trends in IPV

While the main estimates focus on the effects of sectoral job loss on IPV, our first analysis documents the time trends in IPV. To do so, we estimate the following regression:

$$Y_{it} = \alpha_i + \tau_t + u_{it} \tag{4}$$

where the coefficients of interests are τ_t while α_i are individual fixed effects. To account for the different exposure lengths, since we compare all of 2019 with April-May 2020 (or with July-August 2020), we use a Poisson regression and use counts of IPV cases per time period as the outcome variable. [12] All the other regressions in the paper use an OLS linear probability model.

[11] Note that we cannot include household fixed effects because the ENDES is not an individual level panel, but rather repeated cross sections.

[12] Simple time dummies are not interpretable in a regular OLS model in our setting. Since we are comparing all of 2019 with 2 month periods in 2020 (April-May and July-August), the mean rate of IPV is naturally lower in 2020. In contrast Poisson models can easily accommodate for these differences in exposure by explicitly adding the exposure length to the likelihood formula. Note that while OLS doesn't estimate meaningful time effects in our setting, the γ_j coefficients of equation 2 are still consistently estimated, since the differences in exposure is controlled for with the τ_t dummies.

4.2 Estimating Effects on Mental Health

The mental health questions in our survey do not allow us to construct a retrospective survey, as explained in Section 3.3. Since the mental health questions are about the changes instead of asking for levels pre- and during the pandemic, we modify the empirical strategy to be a first difference strategy of the type:

$$\Delta M_i = \alpha + \delta g_i^{sector} + u_i \tag{5}$$

where g_i^k are the measured shocks and $DeltaM_i$ are reported changes in mental health measures. Recall that the g_i^{sector} shocks essentially capture changes in economic conditions between April-May 2020 and 2019, hence this strategy is akin to a conventional first difference strategy controlling for individual fixed effects.

5 Results

5.1 Trends in IPV during the pandemic

We start by examining time trends from our survey data in order to explore whether our IPV estimates follow the time trend that has been documented over the period using hotline data from Peru (Agüero, 2021). Table 2 shows estimates of equation 4 on time dummies for April-May and July-August of 2020, relative to 2019. Columns 1 and 2 report estimates for psychological violence and physical violence (which includes sexual violence), respectively; column 3 reports estimates for any violence.

Overall, the estimates indicate a substantial increase in self-reported cases of IPV during the pandemic, including large and statistically significant increases in both time periods and for most types of violence. Both, physical and psychological violence, increased during at the onset of the pandemic (April-May 2020), when the most strict mobility restrictions were in place. In April/May, physical violence (column 2) increased relative to 2019 levels by 48.9%, while psychological violence increased by 56.3% (column 1). The incidence rate for *any* type of violence increased by 53.4% during April-May 2020 (column 3).

Higher rates of IPV persist even as mobility restrictions became less severe during July-August 2020, although the rates fall from those of April-May 2020. Psychological violence increased by 27.2% and physical by 39.4% (columns 1 and 2, respectively). In July-August 2020, the rate of any violence was 32.4% higher than 2019 levels, which is significantly lower

Table 2: IPV Time Trends 2019-2020

	(1) Psychological	(2) Physical/Sexual	(3) Any Violence
April-May (2020)	0.563***	0.489***	0.534***
	(0.0883)	(0.113)	(0.0767)
July-August (2020)	0.272***	0.394**	0.324***
	(0.0896)	(0.160)	(0.104)
Outcome Mean (2019)	1.985	2.223	3.042
Observations	981	606	1083

Notes. Results of a Poisson regression, where we control for different lengths of exposure between our 3 reference periods (all of 2019, April-May (2020) and July-August (2020)). The results use the sum of IPV related events for each time period, not the binary indicator. Each column refers to a different measure of intimate partner violence from our survey. "Psychological" refers to psychological violence. "Physical/Sexual" refers to acts of physical or sexual violence. "Any" refers to any type of violence, which is the sum of "Physical/Sexual" and "Psychological".

April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.

Standard errors are clustered by the main breadwinner's economic sector.

than the estimated increase immediately after the pandemic.

In Table 3 we examine whether the timing of increases in violence coincided with changes in household income and physical mobility, for different measures of household income. These outcomes are estimated with an OLS regression, since the outcomes are unaffected by the different exposure lengths.[13] Column 1 shows that, in April-May 2020, households experienced an average income loss of 704.4 Nuevos Soles (S/.). Given that average income is S/.1,081.3, this is an extremely large income shock, amounting to a 65% loss in earned income for the average household in our sample.[14] Columns 2 and 3, respectively, show the changes in income for each spouse. Husbands lost more money (S/.409) relative to wives (S/.334). In July/August the decline in income for both persist but with a lower magnitude.

In column 4, we examine the time trends in physical mobility, which we proxy with the reported number of days per week respondents left their home during a typical week in each

[13] We ask income for an average month in either 2019, April-May 2020 or July-August 2020. Wife's days out is asked relative to an average week in the time period.

[14] This relationship is not driven by extreme values. In results available upon request, we show the estimated effect on log income, which implies a 52.1% decline. Also, the probability of having non-zero income decreases by 30.2 percentage points. As an additional check, we use the inverse hyperbolic sine transformation to smooth out extreme values without dropping observations with zero. The results are consistent with our previous estimates, yielding an estimated loss in earned income of 42.2%.

Table 3: Income Time Trends 2019-2020

	(1) Household's Income	(2) Wife's Income	(3) Husband's Income	(4) Wife's Days Out
April-May (2020)	-704.4***	-333.7***	-408.6***	-3.510***
	(65.16)	(41.60)	(50.33)	(0.225)
July-August (2020)	-516.0***	-255.5***	-274.8***	-2.813***
	(45.83)	(28.61)	(31.81)	(0.173)
Outcome Mean (2019)	1488.1	632.8	939.3	5.252
Observations	3231	3167	2922	3231

Notes. Results of an OLS regression. For each partner, we ask what average monthly earnings were during 2019, April-May 2020 and July-August 2020. We then add the earnings of each partner together to calculate household income in Peruvian Soles. Wife's Days Out is the average number of days in a week the wife left to socialize or shop for groceries.
April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.
Standard errors are clustered by the main breadwinner's economic sector.

period. On average, households left home 3.5 fewer days per week in April-May 2020 relative to 2019. As with income and IPV, by July-August 2020, mobility restrictions have become less severe.

5.2 IPV Trends by Economic Sector

The previous estimates document nothing short of economic disaster experienced by the average household in Peru over the first half of 2020. As abrupt economic stress is a risk-factor associated with IPV (e.g. Arenas-Arroyo et al., 2021; Schneider et al., 2016), we should expect to see corresponding patterns of change in the incidence of violence due to dramatic increases in financial insecurity. Hence, to establish a causal relationship between pandemic-related economic contraction and IPV, we exploit sector-level and spatial variation in employment. Table A1 shows that, while most sectors witnessed important employment losses, not all sectors were equally affected by the crisis. For instance, while employment in hotel and food services dropped by 80%, employment in agriculture *increased* by 13%.

Table 4 shows the estimates for Equation (2) using sector-level variation. As mentioned before, g_i^{sector} is the percentage change in employment that took place to the sector where household i worked before the pandemic. A negative (positive) coefficient implies that a one percentage point *increase* in g_i^{sector} decreases (increases) violence. Hence the estimated coefficients refer to occupations that gained jobs, or lost relatively fewer jobs during the pandemic. [15]

Results from these estimates indicate that increases in IPV correspond to the pattern of employment losses brought about by the pandemic. In particular, we find statistically significant and large increases in violence in both periods, April-May and July-August, and across types of violence. For a drop in employment in the median sector (58.2%), the rate of any violence increased 7 percentage points (pp) in April-May ($-58.2 \times -0.00121 = 0.070$) and 8.3 pp in July-August (column 3). These effects are driven by both psychological and physical violence (columns 1 and 2, respectively). Moreover, these increases are massive relative to the 2019 means. For example, for the median sector, psychological violence increased 25.2%, physical violence increased 52.5%, and any violence increased 27.1% in July-August of 2020. Although we cannot conclude that the estimates for April-May are different from those for July-August, as indicated by tests of equality of coefficients, the point estimates are more moderate for the early period in the pandemic, if anything. For the median sector, psychological violence increased 21.7%, although not statistically significant, physical violence increased 36.3%, and any violence increased 23.1% in April-May of 2020.

Interestingly, we find that the point estimates for April-May are less precisely estimated. This would be consistent with increased residual variance of the outcome (and weaker explanatory power of job losses) in the early period of the pandemic. At least two (non-mutually exclusive) hypotheses could help explain this pattern. First, the pandemic was sharpest in the early period and resulted not only in job and income losses but also in a multiplicity of other channels likely affecting IPV, such as the continued extensions of the lockdowns increasing the number of days at home, health-related stress, lack of availability of services for violence victims, among others. Consistent with this the evidence on time trends in Table 2 suggests that IPV was higher in April-May than in July-August. By July 2020, as the lockdowns eased and the patterns of economic impact were revealed, it is likely that the IPV responses converged to follow more closely patterns of economic impact. That is, those most heavily hit by income losses continued to experience stress and household conflict, while those who were spared significant economic loss improved in terms of anxiety-induced conflict.

Another possible factor giving rise to this pattern is the use of savings. Households may have relied on their savings at the beginning of the pandemic to insulate them from

[15] The sample sizes differ from Table 2 due to the Poisson regression. Since we are using individual fixed effects, the Poisson regression drops all individuals for which there is no change in the outcome variables Correia et al. (2021).

negative employment shocks. As savings were depleted, households became more exposed to employment shocks. As result, job losses would not have the same predicting power on IPV in both periods, and it would be arguably weaker while households could buffer income losses, at the beginning of the pandemic. Unfortunately, we do not have panel data on savings in our survey. However, we do ask about savings usage during April and May 2020. As suggestive evidence, we correlate the use of savings during April and May 2020 with the employment shocks in Appendix Table A2. We find that positive employment shocks correlate with lower savings use in April and May. This is suggestive evidence that more protected households indeed used less of their savings, and this could account for the lack of precision by sector in the early stages of the pandemic.

Table 5 investigates the correspondence between patterns of household income and physical mobility as they relate to sector-level employment losses (g^{sector}). The estimates indicate that aggregate job losses indeed correspond to reported household income. Interestingly, employment shocks also track closely with restrictions on physical mobility even in July-August 2020. This is likely explained by the fact that physical mobility restrictions are followed more closely when household members are unemployed and have lower household income, as well as possible reverse causality (mobility restrictions led to job loss in certain sectors). Unfortunately, the correlation between the two mechanisms makes it difficult to empirically isolate physical mobility impacts on IPV from income effects on IPV using this approach.

5.3 Controlling for COVID

A possible challenge to our interpretation of the role of economic shocks is the extent to which these shocks are correlated with the likelihood of being infected with COVID-19. For instance, workers in the service sector may be more likely to be infected, and in turn these infections have impacts on IPV. As a result, the mechanism we are capturing may instead be related to disease anxiety and not income. To examine this concern we augment our regression to control for COVID-19 risk during April and May 2020 using administrative data on COVID-related deaths to proxy for *community level* risk. To do so, we calculate the district-level COVID-19 mortality rate for June of 2020.[16] We focus on death statistics, which do not depend on the district's testing capacity. The results can be seen in Table 6. The top panel reproduces the results from Table 4 to ease comparability, while the bottom panel

[16] We chose COVID-19 deaths in June because deaths trail infections. Hence the death rate in June would be indicative of infection risk in April or May.

Table 4: IPV by Employment Shocks

	(1) Psychological	(2) Physical/Sexual	(3) Any
April-May (2020)	-0.203***	-0.145***	-0.226***
	(0.0339)	(0.0273)	(0.0316)
July-August (2020)	-0.246***	-0.181***	-0.275***
	(0.0289)	(0.0191)	(0.0231)
(δ_1) April-May (2020)$\times g_i^{\text{sector}}$	-0.00102	-0.00101*	-0.00121*
	(0.000663)	(0.000508)	(0.000619)
(δ_2) July-August (2020)$\times g_i^{\text{sector}}$	-0.00118*	-0.00146***	-0.00142***
	(0.000603)	(0.000345)	(0.000460)
Outcome Mean (2019)	0.273	0.162	0.305
P-Value $\delta_1 = \delta_2$	0.237	0.0810	0.433
Observations	3231	3231	3231

Notes. Results of an OLS regression, with an indicator for any amount of violence on the left hand-side. The reference periods are all of 2019, April-May (2020) and July-August (2020). Each column refers to a different measure of intimate partner violence from our survey. "Psychological" refers to psychological violence. "Physical/Sexual" refers to acts of physical or sexual violence. "Any" refers to any type of violence, either "Physical/Sexual" or "Psychological".

g_i^{sector} measures decreases in pandemic related job loss. It is the percentage employment change in the main breadwinner's economic sector between the second quarter of 2020 and 2019, using the ENAHO labor force survey. For each economic sector we estimate the national employment counts using the provided survey weights, which we use to calculate the percentage change in employment between 2020 and 2019.

April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020. These coefficients are different than in table 2 since this table uses an OLS instead of a Poisson model.

P-Value $\delta_1 = \delta_2$ shows the p-value for the test of equality of coefficients.

The samples sizes differ with Table 2 because Table 2 uses a Poisson model for estimation. The individual fixed effects therefore drop all observations with no change on IPV outcomes Correia et al. (2021). This table uses an OLS and keeps observations with no change in IPV.

Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Income by Employment Shocks

	(1) Household's Income	(2) Wife's Income	(3) Husband's Income	(4) Wife's Days Out
April-May (2020)	-381.9***	-192.6***	-210.2***	-2.714***
	(46.68)	(28.91)	(41.78)	(0.290)
July-August (2020)	-312.7***	-157.2***	-156.0***	-2.256***
	(46.82)	(32.77)	(31.71)	(0.209)
(δ_1) April-May (2020)$\times g_i^{\text{sector}}$	6.685***	2.930***	4.132***	0.0165***
	(1.001)	(0.915)	(0.953)	(0.00554)
(δ_2) July-August (2020)$\times g_i^{\text{sector}}$	4.214***	2.042***	2.470***	0.0116***
	(0.957)	(0.701)	(0.732)	(0.00393)
Outcome Mean (2019)	1488.1	632.8	939.3	5.252
P-Value $\delta_1 = \delta_2$	0.0000	0.0522	0.0000	0.0133
Observations	3231	3167	2922	3231

Notes. Results of an OLS regression. For each partner, we ask what average monthly earnings were during 2019, April-May 2020 and July-August 2020. We then add the earnings of each partner together to calculate household income in Peruvian Soles. Wife's Days Out is the average number of days in a week the wife left to socialize or shop for groceries.

g_i^{sector} refers to the percentage employment change in the main breadwinner's economic sector between the second quarter of 2020 and 2019, using the ENAHO labor force survey. For each economic sector we estimate the national employment counts using the provided survey weights, which we use to calculate the percentage change in employment for each sector.

April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.

Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

shows estimates with the COVID-19 death rate control. The main estimates are virtually unchanged, while the June death rate coefficients are all statistically insignificant. We take this as evidence that anxiety due to COVID-19 is not the principal mechanism explaining our results.

Table 6: IPV by Employment Shocks, with COVID-19 Controls

	(1) Psychological	(2) Physical/Sexual	(3) Any
April-May (2020)$\times g_i^{\text{sector}}$	-0.00102	-0.00101*	-0.00121*
	(0.000663)	(0.000508)	(0.000619)
July-August (2020)$\times g_i^{\text{sector}}$	-0.00118*	-0.00146***	-0.00142***
	(0.000603)	(0.000345)	(0.000460)
April-May (2020)$\times g_i^{\text{sector}}$	-0.000944	-0.000980*	-0.00115*
	(0.000642)	(0.000500)	(0.000610)
July-August (2020)$\times g_i^{\text{sector}}$	-0.00105*	-0.00144***	-0.00131***
	(0.000557)	(0.000332)	(0.000437)
April-May (2020)$\times$June Death Rate	0.000293**	0.000133	0.000219
	(0.000126)	(0.000153)	(0.000198)
July-August (2020)$\times$June Death Rate	0.000540**	0.000111	0.000451
	(0.000255)	(0.000177)	(0.000342)
Outcome Mean (2019)	0.273	0.162	0.305
Observations	3231	3231	3231

Notes. Results of an OLS regression, with an indicator for any amount of violence on the left hand-side. The reference periods are all of 2019, April-May (2020) and July-August (2020). Each column refers to a different measure of intimate partner violence from our survey. "Psychological" refers to psychological violence. "Physical/Sexual" refers to acts of physical or sexual violence. "Any" refers to any type of violence, either "Physical/Sexual" or "Psychological".
g_i^{sector} measures decreases in pandemic related job loss. It is the percentage employment change in the main breadwinner's economic sector between the second quarter of 2020 and 2019, using the ENAHO labor force survey. For each economic sector we estimate the national employment counts using the provided survey weights, which we use to calculate the percentage change in employment between 2020 and 2019.
April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.
June Death Rate refers to the death rate per 100000 inhabitants in June due to COVID-19 in the respondent's district. Inhabitant per district data comes from the 2017 Census, and COVID-19 deaths come from administrative data.
Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.4 Local labor market effects

Another potential concern is that household employment shocks are correlated with local labor market conditions instead of just sectors' conditions. A depressed local labor market may induce IPV by increasing economic anxiety. Our results may be biased by estimating the additional effect of a depressed local labor market. This is a salient concern if economic activity is specialized across geographic regions and if occupations cluster together.

To address this concern, we construct a high frequency measure of local labor markets based on a shift-share strategy, $g_i^{\text{shift-share}}$, as in (Bartik, 1992). We combine the occupational losses in the ENAHO between 2019 and Q2 2020 with district employment shares based on 2017 Population Census data. See Appendix A.2 for more details. This allows us to gauge employment levels at the district level, which is the smallest administrative unit in Peru. The results can be seen in Table A3.

Our main estimates of g_i^{sector} are still negative and retain their statistical significance. However, their estimated magnitudes decrease slightly. For instance the July-August coefficient for Physical violence shrinks from -0.00146 to -0.00126. The coefficients on the shift-share variable, $g_i^{\text{shift-share}}$, are negative as well, indicating that depressed local economic activity increases the risk of IPV. The shift-share effects are slightly nosier than the g_i^{sector}, which we would expect given that the g_i^{sector} are based directly on the household's occupation, instead of the more indirect effects captured by the shift-share variable. We take these results as evidence that our estimates are not capturing the effect of depressed local economic activity, and instead they reflect the effect of household economic conditions.

5.5 Effects on Mental Health

We estimate the equation described in Section 4.2. The results can be seen in Table 7. All the coefficients are negative, which indicate that the more economically protected households saw relative improvements (or less deterioration) in mental health. Because of the variable's coding, we can interpret these coefficients as reductions in the likelihood of experience negative mental health outcomes, akin to conventional panel estimates with binary outcomes. For instance, the median employment shock of -58.2 percentage points would yield a 0.172 increase in feelings of anxiety relative to 2019 ($= -58.2 \times -0.00296$). However, since we only asked for the change we are not able to compare this coefficient to the overall mean of the mental health outcomes in 2019.

In summary, we document that more economically protected households saw less deterioration in their mental health outcomes. The economic effects of the pandemic not only increased IPV risk for households, but additionally impacted their mental health.

Table 7: Mental Health Outcomes

	(1) Anxiety	(2) Moodiness	(3) Loneliness	(4) Rage	(5) Raise Voice	(6) Violence	(7) Any Mental Health
g_i^{sector}	-0.00296**	-0.000840	-0.000334	-0.00142	-0.00128	0.0000889	-0.000879**
	(0.00119)	(0.000794)	(0.000664)	(0.00103)	(0.000864)	(0.000478)	(0.000361)
Outcome Mean (2020)	0.644	0.669	0.286	-0.0160	-0.0197	-0.416	0.894
Observations	1066	1072	1062	1065	1065	1048	1076

Notes. This table shows OLS regression results for mental health outcomes. The results are from two different regressions. Standard errors are shown below the estimates. The g_i^{sector} coefficients are clustered by economic sector.
The survey questions are of the form "In the months of April and May 2020, on average, did you feel more, same, or less anxiety than an average month in 2019." The table headers show the type of mental health feeling we ask about. Anxiety refers to feelings of anxiety. Moodiness refers to feelings of moodiness. Loneliness refers to feelings of loneliness. Rage refers to ability to control the respondent's anger. Raise Voice refers to feeling urges to raise your voice. Violence refers to urges to act violently. Any Mental Health refers to any mental health deterioration as measured in columns 1-6. It is the maximums across columns 1-6.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.6 Robustness Checks

5.6.1 Recall Error

Our phone survey constructed a retrospective panel, so a primary concern is that our estimates are affected by recall bias. For example, if respondents were to systematically under-report (over-report) violence in 2019, this could lead to upward (downward) bias on the impacts of job losses on IPV. Several studies have analyzed the quality of reporting of trauma and life events, especially in psychology, in retrospective surveys. Although the evidence on IPV reporting is very limited (Abramsky et al., 2022; Loxton et al., 2019), studies suggest that IPV and trauma reporting might be subject to both fall-off (not reporting past events) and telescoping (reporting past events as more recent) (Abramsky et al., 2022; Langeland et al., 2015; Loxton et al., 2019; Pachana et al., 2011).

Overall, it does not appear that these sources of bias drive our main results. As shown in Appendix Section A.3, we use a sub-sample of respondents we interviewed in person in 2019 and by phone in 2020. Although it is unclear to what extent repeated surveying might affect reporting and in what direction, consistent with longitudinal studies on IPV (e.g. Loxton et al., 2019), we find evidence of both over- and under- reporting when comparing 2019 answers with the recalled 2020 values. Recall error does not seem to be systematically biased in one direction. Importantly, however, we find no evidence that the recall error is correlated with our main regressor of interest as seen in Appendix Table A9.

5.6.2 Differences with respect to Nationally Representative Surveys

In Table 1, we document important differences between the phone sample survey and nationally representative surveys (the ENAHO and ENDES). In particular our phone survey reports much larger rates of IPV in 2019 relative to the 2019 ENDES, possibly due to changes in reporting behavior.

To evaluate the sensitivity of our results to changes in reporting behaviour, we conduct a weighting exercise in which we re-weight our phone survey sample to exactly match the 2019 ENDES rates using synthetic controls (Abadie et al., 2010; Robbins et al., 2017), and rerun our main analysis on the weighted sample. This procedure places more weight on women who reported on average more similarly to the 2019 ENDES respondents.

The results can be seen in Table B4, and a more detailed discussion is provided in Appendix B. The estimated directions as well as the magnitudes of our coefficients from the weighted sample are largely consistent with our main results. If anything, estimates from the reweighted sample are more statistically significant than those of our unweighted sample. Thus, our findings do not appear to by affected by the level differences between our sample and the 2019 IPV survey. We view this as suggestive evidence that the differences in reporting across our phone survey and the 2019 ENDES are not driving our results. This exercise does not rule out all potential concerns regarding retrospective error in our survey. However, we find it reassuring that our results are robust to matching 2019 rates from a non-retrospective survey.

5.6.3 Extensive vs Intensive margin

In Appendix Table A4, we use the total count of IPV events instead of the binary indicator as the outcome in equation 2. Given that the outcome variable is a count, we employ a Poisson model instead of an OLS regression. Our results are insensitive to using the count versus the binary indicator, suggesting that changes occur on both the extensive and intensive margins in the same direction.

6 Conclusion

We conducted a large household survey and document a substantial and sustained increase in IPV during the COVID-19 pandemic in Peru. These results complement and expand existing work showing similar increases in phone calls to victim helplines, indicating an enormous increase in IPV during the pandemic that was not simply a substitution away from in-person services and towards phone-based assistance. Moreover, we show that pandemic-related income shocks are strongly associated with changes in the incidence of IPV. While increases in IPV due to employment losses are statistically insignificant in the first two months of the pandemic, households most exposed to pandemic related job-losses suffered disproportionate and extremely large increases in physical IPV six months into the pandemic. This pattern is consistent with lockdown measures and uncertainty contributing to a general rise in IPV at the onset of the shock, while income losses experienced by a subset of the population led to sustained levels of IPV several months later.

These results provide important additional empirical evidence that economic crises, in this case generated by COVID-19, produce violence through both the stress of economic uncertainty as well as through material losses to individual households. Our results also provide indirect evidence on the benefits of cash transfers. Like many countries, Peru implemented a series of targeted cash transfers during the pandemic.[17] Our results suggest such payments likely helped reduce IPV, even months after the initial lockdown measures. However this interpretation requires caution, since our empirical strategy does not directly evaluate the benefits of these cash transfers.

Our study also suggests at least three avenues for future research. First, from a methodological perspective, more work is needed on the implications of retrospective and longitudinal surveying of IPV for reporting and assessing the impacts of policies and social phenomena. Second, our work suggests investigating the time horizon of the effects of the pandemic on IPV and assessing how lasting have been its effect. Third, governments responded to the pandemic with emergency programs that aim to protect households vulnerable to economic losses; future research could address which and how these programs might have averted further increases in IPV.

[17]Examples include: Bono *Yo me quedo en casa* , Bono *Independiente* , Bono *Rural*, Bono *Familiar Universal* and Bono *Universal*. These programs used administrative data to target poor households.

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A Appendix

A.1 Phone Survey, Ethical Protocols and IPV Variable Construction

We conducted the phone survey by randomly dialling cellphone numbers in Peru. The cellphone numbers were provided by Sample Solutions LLC, and are numbers that are believed to be active. Nonetheless, many numbers did not pick up, either because the person did not want to pick up or the number was inactive. Given the large amount of initial non response, we automated the first step in the survey. The survey would not have been feasible without automation. We automatically called the numbers provided and recorded the receivers' voice. We then went through the recordings to determine who was female. The final list for the surveying team was made of respondents that picked up the initial automated call and left a message with a female sounding voice. The surveying team used this final list to conduct the phone surveys, using the sample restrictions outlined in the main text. We limited the sample to women aged 18 to 49 who self reported to be in a domestic partnership in April 2020.

We follow best practices when asking about IPV. Our ethical protocol required interviewers to reschedule the interview if the respondent answered in a public space. If they were in a private space, we asked respondents if they were by themselves and if they could ask for privacy. If they were unable to, we rescheduled the interview. We also reminded respondents they were under no obligation to answer the IPV questions and could say "I don't know". Finally, we offered a safe word respondents could use at any time during the interview to signal they no longer felt safe and to skip the IPV module. We received IRB approval from the Duke Institutional Review Board with the protocol number 2020-0530.

We focus on 6 questions to construct the IPV measures. These 6 questions are:

1. With what frequency has your partner said or done things to humiliate you in front of others?

2. With what frequency has your partner insulted, yelled, broken your belongings, threatened to hit you or throw something to you?

3. With what frequency has your partner pushed, shook you or thrown something at you?

4. With what frequency has your partner slapped you or twisted your arm?

5. With what frequency has your partner hit you with their fist or something that could have hurt you?

6. With what frequency has your partner used physical strength to force you to have sexual relations, even if you didn't want to?

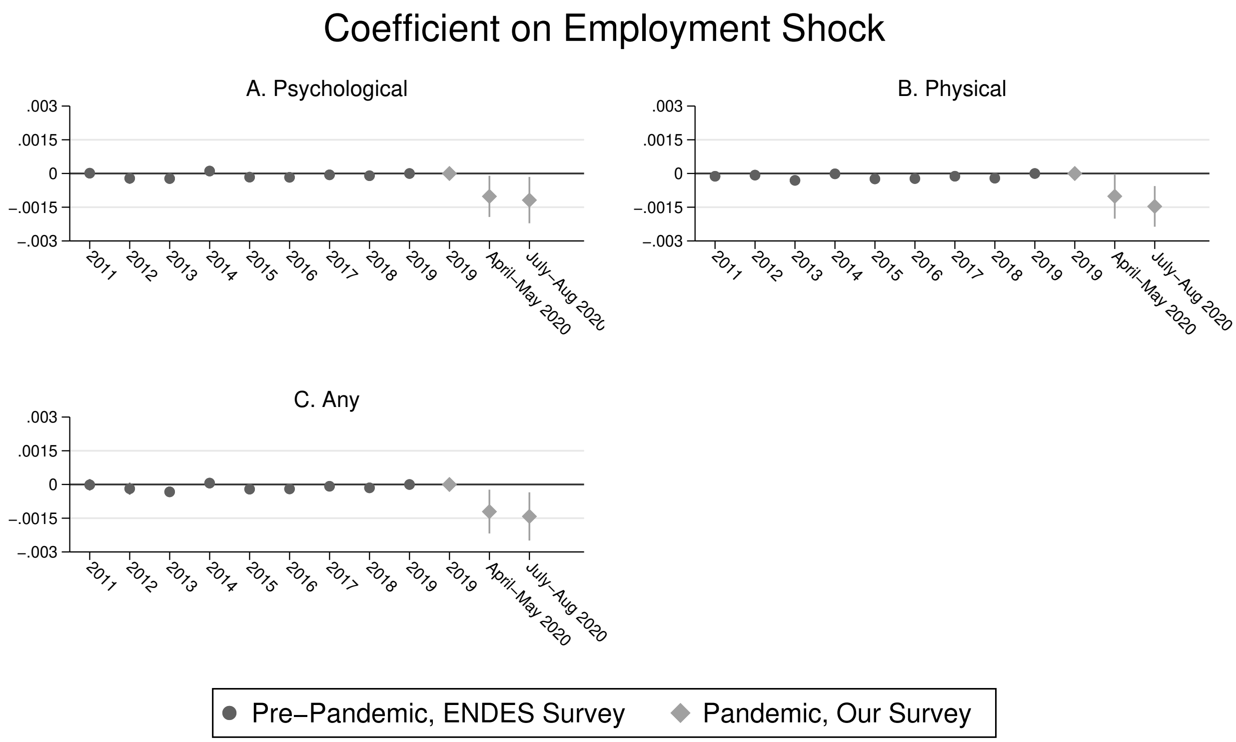
To estimate the frequency of occurrence, we asked respondents with which frequency IPV occurred. The options were Never, One Time, Sometimes or Many Times. We use these answers to estimate counts. We coded Never as 0, One Time as 1, and Sometimes or Many Times as 2. We then grouped these six questions into two categories: physical and psychological IPV. Questions 1-2 are used to construct our psychological IPV measure, while questions 3-6 are used for the physical IPV measure. We add all the sub questions related to physical IPV to form an estimated count of physical IPV, and do the same for psychological IPV.

Table A1: Employment Changes By Sector

Economic Sector	Employment Count 2019	Employment Count Q2 2020	Percentage Change (%)
Art and Entertainment	176374	34253	-81
Hotel and Food Service	1284874	255782	-80
Household Employment	432118	110382	-74
Construction	1072992	301739	-72
Mining	200181	72742	-64
Water supply; sewerage, waste management	75938	28699	-62
Technical, Professional and Scientific Activities	387731	154740	-60
Manufacturing	1532773	624387	-59
Transportation and Storage	1308055	547316	-58
Fishing	97231	40867	-58
Administrative and support service activities	523687	224175	-57
Retail	3300452	1460108	-56
Other Service Activities	483815	244670	-49
Information and Communication	144148	83607	-42
Human health and social work activities	435638	272295	-37
Public administration and defence	708701	473513	-33
Real Estate	26502	18661	-30
Education	883133	664248	-25
Insurance and Financial Activities	133927	122273	-9
Agriculture	4091243	4633594	13
Electricity, gas, steam and air conditioning supply	15656	19034	22

Notes: The table shows national employment estimates in 2019 and the second quarter of 2020 using the ENAHO surveys. The last column is the percentage change between Q2 2020 and 2019. These results use the sampling weights provided in the ENAHO.

Figure A1: Pre-trends, Employment Change and IPV



Notes: Graph of pre-trend coefficients of equation 3, using ENDES data from 2011-2019. 2019 is the reference year. These results are based on an OLS linear probability model. The dark grey points are our estimates of δ_j from equation 3. The estimates in light grey are the results of using our survey and estimating 2, and we report γ_1, γ_2 from equation 2. Standard errors are clustered by the household's district.

Table A2: Employment Shocks on Savings Use

	(1) Used Savings Apr. May (2020)
g_i^{sector}	-0.000922**
	(0.000451)
Outcome Mean (2020)	0.852
N	1076

Notes. OLS regression with an indicator of any savings use during April or May 2020 on the left hand side. On the right hand side is our economic sector shock.
Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.2 Geographic variation in exposure

In addition to our main specification, we construct a shift-share variable that captures the economic shock to the household's geographic district during the COVID-19 pandemic constructed from the ENAHO employment data and the 2017 Peruvian Population Census microdata. In particular, we calculate employment shares by district and industry from the 2017 Census to construct the shift-share variable defined as $\pi_{dn} = L_{dn}^{2017}/L_d^{2017}$, where L_{dn}^{2017} denotes total employment in industry n in district d in the 2017 Census, and L_d^{2017} denotes total employment in district d.

We then combine these shares with information on employment shocks during the COVID-19 pandemic, in a similar fashion to the economic sectors specification. We first calculate the changes in employment for each economic sector n as before. We then weight these employment changes with the district-sector shares from the census, yielding the expression:

$$g_i^{\text{shift-share}} = \sum_n \pi_{dn} \frac{L_{n1} - L_{n0}}{L_{n0}} \times 100 \tag{6}$$

$g_i^{\text{shift-share}}$ is therefore an industry-weighted average of employment changes in i's district. In these specifications, we cluster standard errors by district.

Table A5 shows summary statistics of local employment shocks at the district level. The

Table A3: IPV by Employment Shocks, with Shift-Share Controls

	(1) Psychological	(2) Physical/Sexual	(3) Any
April-May (2020)$\times g_i^{\text{sector}}$	-0.00102	-0.00101*	-0.00121*
	(0.000663)	(0.000508)	(0.000619)
July-August (2020)$\times g_i^{\text{sector}}$	-0.00118*	-0.00146***	-0.00142***
	(0.000603)	(0.000345)	(0.000460)
April-May (2020)$\times g_i^{\text{sector}}$	-0.000807	-0.000700	-0.00108
	(0.000614)	(0.000514)	(0.000632)
July-August (2020)$\times g_i^{\text{sector}}$	-0.000958	-0.00126***	-0.00128***
	(0.000561)	(0.000346)	(0.000448)
April-May (2020)$\times g_i^{\text{shift-share}}$	-0.00104	-0.00155*	-0.000627
	(0.00128)	(0.000897)	(0.00135)
July-August (2020)$\times g_i^{\text{shift-share}}$	-0.00112	-0.000999	-0.000710
	(0.00116)	(0.000960)	(0.00134)
Outcome Mean (2019)	0.273	0.162	0.305
Observations	3231	3231	3231

Notes. Results of an OLS regression, with an indicator for any amount of violence on the left hand-side. The reference periods are all of 2019, April-May (2020) and July-August (2020). Each column refers to a different measure of intimate partner violence from our survey. "Psychological" refers to psychological violence. "Physical/Sexual" refers to acts of physical or sexual violence. "Any" refers to any type of violence, either "Physical/Sexual" or "Psychological".

g_i^{sector} measures decreases in pandemic related job loss. It is the percentage employment change in the main breadwinner's economic sector between the second quarter of 2020 and 2019, using the ENAHO labor force survey. For each economic sector we estimate the national employment counts using the provided survey weights, which we use to calculate the percentage change in employment between 2020 and 2019.

$g_i^{\text{shift-share}}$ is our shift share variable constructed with employment changes by occupation between the second quarter of 2020 and the average of 2019. These changes are combined with district level occupational shares as described in Appendix Section A.2

April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.

Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: IPV by Employment Shocks, Poisson Model

	(1) Psychological	(2) Physical/Sexual	(3) Any Violence
April-May (2020)	0.465***	0.434***	0.453***
	(0.0751)	(0.118)	(0.0870)
July-August (2020)	0.0360	-0.339	-0.103
	(0.0525)	(0.249)	(0.107)
April-May (2020)$\times g_i^{\text{sector}}$	-0.00210	-0.00117	-0.00172
	(0.00159)	(0.00225)	(0.00169)
July-August (2020)$\times g_i^{\text{sector}}$	-0.00492***	-0.0142***	-0.00864***
	(0.00187)	(0.00413)	(0.00211)
Outcome Mean (2019)	1.985	2.223	3.042
Observations	981	606	1083

Notes. Results of a Poisson regression, where we control for different lengths of exposure between our 3 reference periods (all of 2019, April-May (2020) and July-August (2020)). The results use the sum of IPV related events for each time period, not the binary indicator. Each column refers to a different measure of intimate partner violence from our survey. "Psychological" refers to psychological violence. "Physical/Sexual" refers to acts of physical or sexual violence. "Any" refers to any type of violence, which is the sum of "Physical/Sexual" and "Psychological".

g_i^{sector} refers to the percentage employment change in the main breadwinner's economic sector between the second quarter of 2020 and 2019, using the ENAHO labor force survey. For each economic sector we estimate the national employment counts using the provided survey weights, which we use to calculate the percentage change in employment for each sector.

April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.

Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

median individual in the sample resides in a district that experienced a 50.95% drop in employment.

Table A6 reveals a pattern of IPV over the period similar to the main strategy based on household economic sector, in that negative local labor market shocks are associated with significant increases in reported IPV. Consistent with our previous estimates, the effects are stronger at later stages of the pandemic. For the median shift-share shock in the sample (-50.98 percentage points), the rate of any violence increases by 7.6 pp ($-50.98 \times -0.00151 = 0.076$) in July-August of 2020. The effects for each type of violence are less precisely estimated than in our main specification, which one would expect given that the shift-share shock measures local-labor market shocks rather than households' sector-specific shocks.

Table A5: Summary Statistics of $g_i^{\text{shift-share}}$

	$g_i^{\text{shift-share}}$
p1	-57.38879
p10	-56.49663
p25	-55.37397
p50	-50.98249
p75	-39.17353
p99	1.894225
mean	-44.47882
N	3231
Districts	305

Notes. Summary statistics for the shift-share shocks, showing different percentiles and the mean. These statistics are generated using our final sample, hence they are weighted according to the distribution of households.

Table A6: IPV by Shift-Share Shocks

	(1) Psychological	(2) Physical/Sexual	(3) Any
April-May (2020)	-0.223***	-0.184***	-0.226***
	(0.0449)	(0.0420)	(0.0493)
July-August (2020)	-0.265***	-0.190***	-0.273***
	(0.0434)	(0.0390)	(0.0436)
April-May (2020)$\times g_i^{\text{shift-share}}$	-0.00155*	-0.00199**	-0.00130
	(0.000938)	(0.000862)	(0.00101)
July-August (2020)$\times g_i^{\text{shift-share}}$	-0.00172*	-0.00179**	-0.00151*
	(0.000906)	(0.000787)	(0.000908)
Constant	0.273***	0.162***	0.305***
	(0.00800)	(0.00668)	(0.00814)
Outcome Mean (2019)	0.273	0.162	0.305
Observations	3231	3231	3231

Notes. Results of an OLS regression, with an indicator for any amount of violence on the left hand-side. The reference periods are all of 2019, April-May (2020) and July-August (2020). Each column refers to a different measure of intimate partner violence from our survey. "Psychological" refers to psychological violence. "Physical/Sexual" refers to acts of physical or sexual violence. "Any" refers to any type of violence, either "Physical/Sexual" or "Psychological".

$g_i^{\text{shift-share}}$ is our shift share variable constructed with employment changes by occupation between the second quarter of 2020 and the average of 2019. These changes are combined with district level occupational shares as described in Appendix Section A.2

April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.

Standard errors are clustered by the household's district.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.3 Analysis of Recall Bias

Since we rely on a retrospective panel for estimation, a natural concern is the extent to which there is recall bias in our the responses. Respondents may be systematically over or under-reporting IPV in a way that biases our results.

In this section, we leverage the sub-sample that was actually surveyed in 2019 to compare recalled 2019 values in our 2020 phone survey with baseline values surveyed in 2019. We can therefore analyze the extent of recall bias in our results within this sub-sample. We are limited in what we can do, however, since this sub-sample is very small. There are only 283 respondents for whom we have both baseline and recalled 2019 values.

For physical IPV, we repeated the exact same 4 sub-questions in both surveys. For psychological IPV, we only have the question "With what frequency has your partner said or done things to humiliate you in front of others" repeated in both surveys.

We first begin with a simple cross tabulation of the recalled versus baseline 2019 values in Tables A7 and A8. There is some evidence of overall under-reporting in Table A7 and there is no systematic difference in overall reporting rate in A8.

We then run a simple OLS with the recalled errors, defined as recalled value - baseline value, on the left hand side and our employment shocks on the right hand side, in Table A9. The coefficient on g_i^{sector} is insignificant, which is suggestive evidence that the recall error is not correlated with our main regressor of interest.

Table A7: Physical IPV: Cross Tabulation of Recalled Values and 2019 Baseline Values

	Recalled Value Physical = 0	Recalled Value Physical > 0	Total
Baseline Value Physical = 0	216	15	231
Baseline Value Physical > 0	31	21	52
Total	247	36	283

Table A8: Psychological IPV: Cross Tabulation of Recalled Values and 2019 Baseline Values

	Recalled Value Psychological = 0	Recalled Value Psychological > 0	Total
Baseline Value Psychological = 0	199	28	227
Baseline Value Psychological > 0	28	23	51
Total	227	51	278

Table A9: Correlation of Recall Error and Employment Shocks

	(1) Recall Error Physical	(2) Recall Error Psychological
g_i^{sector}	-0.00200	0.00159
	(0.00125)	(0.000988)
Error Mean	-0.233	-0.00722
N	283	277

Notes. OLS regression with the calculated 2019 recall error. On the right hand side is our economic sector shock.

Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Using Weights to Match Nationally Representative Survey

As seen in Table 1, our phone survey is dissimilar to nationally representative surveys. To investigate how sensitive our results are to these differences, we employ synthetic controls (Abadie et al., 2010; Robbins et al., 2017) to match our survey the nationally representative ones. We can match either on the demographic data or on the IPV data separately. We cannot match on both the IPV and demographic data, because these come from two different surveys. Therefore there are no individuals with both the demographic data and the IPV data from nationally representative surveys. Tables B1 and B2 show the results of the matching exercise. As expected, the synthetic control methods generate weights to make the two sample means similar.

We then employ these weights to rerun our main specification. Our results are not sensitive to weighing our phone survey to be more similar to the nationally representative ones. The main coefficients are still negative and statistically significant. If anything, when we match to the 2019 demographics in Table B3, the magnitudes are more negative. We note the sample size is smaller in Table B3 relative to our main estimates. This is because the synthetic control procedure drops some units from our sample, because these units are too dissimilar. 118 households were dropped from the sample out of the main sample of 1077.

We take these results as evidence that the observable differences between our sample and the nationally representative surveys are not meaningfully affecting our results. Households that were more insulated from the negative economic COVID-19 shock had lower rates of IPV.

Table B1: Descriptive Statistics: Weighted to Match 2019 Demographics

Variable	Nationally Representative Survey	Phone Survey (2020)	Difference
A: Demographics			
Age (women)	35.804	35.810	-0.007
Age (male partner)	40.098	40.099	-0.002
Household size	4.401	4.407	-0.007
% of women w/complete secondary	0.661	0.661	-0.001
Number of children	1.860	1.861	-0.001
Household income, Soles (2019)	1522.047	1526.141	-4.094
Household income, Soles (2020Q2)	791.505	804.441	-12.936
Household income, Soles (2020Q3)	1069.353	1007.573	61.780
B: IPV			
Psychological IPV	0.102	0.282	-0.180***
Physical and/or sexual IPV	0.105	0.169	-0.063***
Any IPV	0.152	0.311	-0.159***

Notes. Descriptive statistics comparing our 2020 phone survey to the urban sub-samples from two nationally representative surveys conducted in 2019 and 2020 (ENAHO and ENDES). In practice, our 2020 phone survey skews urban, hence we only compare our survey to urban sub-samples. % of women w/ complete secondary refers to fraction of women that completed secondary education. Age refers to year of age. Household size refers number of people living in the household. Household income refers to the total income earned by all members in the household, measured in Peruvian Soles. Unless otherwise noted, all questions for the Nationally Representative Survey refer to 2019 values. Panel A compares our 2020 survey to ENAHO.

Panel B compares the ENDES survey to our measures of IPV. These refer to the fraction of women that have had an IPV event during 2019, as reported by the ENDES and our survey. Both surveys use 2019 as the reference period.

This table uses synthetic weights used to match 2019 demographic variables in the descriptive table.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B2: Descriptive Statistics: Weighted to Match 2019 IPV Variables

Variable	Nationally Representative Survey	Phone Survey (2020)	Difference
A: Demographics			
Age (women)	35.804	35.470	0.333
Age (male partner)	40.098	37.897	2.200***
Household size	4.401	5.031	-0.630***
% of women w/complete secondary	0.661	0.756	-0.095***
Number of children	1.860	1.710	0.150***
Household income, Soles (2019)	1522.047	1498.238	23.809
Household income, Soles (2020Q2)	791.505	793.184	-1.679
Household income, Soles (2020Q3)	1069.353	977.481	91.872
B: IPV			
Psychological IPV	0.102	0.102	0.000
Physical and/or sexual IPV	0.105	0.105	0.000
Any IPV	0.152	0.147	0.005

Notes. Descriptive statistics comparing our 2020 phone survey to the urban sub-samples from two nationally representative surveys conducted in 2019 and 2020 (ENAHO and ENDES). In practice, our 2020 phone survey skews urban, hence we only compare our survey to urban sub-samples. % of women w/ complete secondary refers to fraction of women that completed secondary education. Age refers to year of age. Household size refers number of people living in the household. Household income refers to the total income earned by all members in the household, measured in Peruvian Soles. Unless otherwise noted, all questions for the Nationally Representative Survey refer to 2019 values. Panel A compares our 2020 survey to ENAHO.

Panel B compares the ENDES survey to our measures of IPV. These refer to the fraction of women that have had an IPV event during 2019, as reported by the ENDES and our survey. Both surveys use 2019 as the reference period.

This table uses synthetic weights used to match 2019 IPV variables in the descriptive table from the ENDES survey.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B3: IPV by Employment Shocks With Weights Matching 2019 Demographics

	(1) Psychological	(2) Physical/Sexual	(3) Any
April-May (2020)	-0.228***	-0.172***	-0.262***
	(0.0276)	(0.0221)	(0.0255)
July-August (2020)	-0.286***	-0.202***	-0.317***
	(0.0239)	(0.0210)	(0.0209)
April-May (2020)$\times g_i^{\text{sector}}$	-0.00157***	-0.00155***	-0.00198***
	(0.000501)	(0.000416)	(0.000450)
July-August (2020)$\times g_i^{\text{sector}}$	-0.00197***	-0.00184***	-0.00229***
	(0.000435)	(0.000367)	(0.000363)
Outcome Mean (2019)	0.283	0.168	0.311
Observations	2835	2835	2835

Notes. Results of an OLS regression, with an indicator for any amount of violence on the left hand-side. The reference periods are all of 2019, April-May (2020) and July-August (2020). Each column refers to a different measure of intimate partner violence from our survey. "Psychological" refers to psychological violence. "Physical/Sexual" refers to acts of physical or sexual violence. "Any" refers to any type of violence, either "Physical/Sexual" or "Psychological".

This table uses synthetic weights used to match 2019 demographic variables in the descriptive table.

g_i^{sector} measures decreases in pandemic related job loss. It is the percentage employment change in the main breadwinner's economic sector between the second quarter of 2020 and 2019, using the ENAHO labor force survey. For each economic sector we estimate the national employment counts using the provided survey weights, which we use to calculate the percentage change in employment between 2020 and 2019.

April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.

Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B4: IPV by Employment Shocks With Weights Matching 2019 IPV Variables

	(1) Psychological	(2) Physical/Sexual	(3) Any
April-May (2020)	-0.0622**	-0.0943***	-0.0990***
	(0.0239)	(0.0171)	(0.0226)
July-August (2020)	-0.0877***	-0.121***	-0.134***
	(0.0148)	(0.0105)	(0.0112)
April-May (2020)$\times g_i^{\text{sector}}$	-0.000566	-0.000652*	-0.000686
	(0.000482)	(0.000336)	(0.000457)
July-August (2020)$\times g_i^{\text{sector}}$	-0.000711*	-0.000960***	-0.000943***
	(0.000345)	(0.000221)	(0.000265)
Outcome Mean (2019)	0.102	0.105	0.147
Observations	3189	3189	3189

Notes. Results of an OLS regression, with an indicator for any amount of violence on the left hand-side. The reference periods are all of 2019, April-May (2020) and July-August (2020). Each column refers to a different measure of intimate partner violence from our survey. "Psychological" refers to psychological violence. "Physical/Sexual" refers to acts of physical or sexual violence. "Any" refers to any type of violence, either "Physical/Sexual" or "Psychological".

This table uses the synthetic weights used to match 2019 IPV variables in the descriptive table from the ENDES survey.

g_i^{sector} measures decreases in pandemic related job loss. It is the percentage employment change in the main breadwinner's economic sector between the second quarter of 2020 and 2019, using the ENAHO labor force survey. For each economic sector we estimate the national employment counts using the provided survey weights, which we use to calculate the percentage change in employment between 2020 and 2019.

April-May (2020) is a time dummy for April and May of 2020. July-August (2020) is a time dummy for July and August of 2020.

Standard errors are clustered by the main breadwinner's economic sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$