Dynamic Impacts of Lockdown on Domestic Violence: Evidence from Multiple Policy Shifts in Chile*

Sonia Bhalotra† Emilia Brito‡ Damian Clarke§ Pilar Larroulet¶
Francisco Pino‖

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Abstract

We identify dynamic impacts on domestic violence (DV) of the staggered imposition and lifting of lockdown across Chile's 346 municipalities. Lockdown increases DV helpline calls and shelter occupancy without increasing DV police reports. These results are consistent with lockdown raising incidence and creating barriers to reporting. Once lockdown is lifted, shelter occupancy falls and police reports surge, but helpline calls remain elevated in line with state dependence in DV. We identify male job loss as a mechanism driving DV. Our findings accentuate controversy around welfare impacts of lockdown mandates. Adverse impacts of lockdown on DV are mitigated by cash transfers.

Keywords: domestic violence, lockdown, job loss, social safety net, public health, COVID-19.
JEL codes: J12, I38, H53.

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†University of Warwick, CAGE, CEPR, IEA, IFS, IZA, MiSoC. sonia.bhalotra@warwick.ac.uk.
‡Brown University, Department of Economics, ebritore@brown.edu.
§University of Exeter, FEN University of Chile, MIPP and IZA, damian.clarke@uchile.cl.
¶Rutgers University-Newark, Pontificia Universidad Católica de Chile and VioDemos Millennium Institute, pilar.larroulet@rutgers.edu.
‖University of Chile, Department of Economics and IZA, fjpino@fen.uchile.cl.

1 Introduction

Reports of a substantial surge in domestic violence (DV) during the COVID-19 pandemic emerged from several countries, a phenomenon referred to as the shadow pandemic [UN Women, 2020]. The fact of an almost universal increase suggests common mechanisms at play. In this paper we investigate the role of lockdown mandates. Analysis of optimal lockdown policy has focused on the trade off between fatalities and economic growth [Alvarez et al., 2021]. We extend the scope of debate over lockdown mandates by investigating their impact on DV. We advance analysis of mechanisms driving incidence and reporting at a generic level, and provide estimates of the extent to which income support schemes have mitigated impacts on DV, allowing differences in impacts by lockdown status.

Numerous studies have analyzed the phenomenal increase in DV since the pandemic. Our contribution lies in providing the first unbiased estimates of dynamic impacts of both *entry* to and *exit* from lockdown on multiple indicators of DV conditional upon COVID infection and test rates, and in investigating mechanisms and the role of income transfers.[1] We investigate male and female job loss and mobility restrictions as mechanisms by which lockdown can lead to DV. Lockdown is associated with a decline in economic activity, manifest in employment and earnings losses. Individual job loss triggers DV by generating both a negative income shock (that generates stress) and a positive time shock (that increases exposure or opportunities

[1]Without attempting to be comprehensive, we note that the literature has analyzed helpline calls in the US (Leslie and Wilson, 2020; McCrary and Sanga, 2021; Erten et al., 2022), UK (Ivandić et al., 2020), Peru (Agüero, 2021) and Argentina (Perez-Vincent et al., 2020), complaints and reports to the police in India (Ravindran and Shah, 2023) and the US (Piquero et al., 2020), both calls and police reports in the US (Hsu and Henke, 2021; Bullinger et al., 2021; Miller et al., 2020; Miller et al., 2022), Mexico (Silverio-Murillo et al., 2020) and various countries in Latin America (Perez-Vincent and Carreras, 2022), internet search activity (Anderberg et al., 2022; Berniell and Facchini, 2021), female homicides (Asik and Ozen, 2021), and incidence measures from surveys (Arenas-Arroyo et al., 2021; Gibbons et al., 2021). In general, these studies investigate either the COVID shock or imposition of lockdown. The findings differ because they look at different indicators, with most of those using calls to the police showing an increase in domestic violence. We provide results in parallel for three indicators.

for crime) (see Bhalotra et al. [2021b]). Mobility restrictions imposed by lockdown reinforce exposure. Additionally, by forcing isolation from social networks, they may impact the tolerance levels of men and women, and limit the freedom of the victim to seek social or state support.

The study is set in Chile, which has implemented rolling lockdowns across municipalities. We analyze the first-wave of COVID-19 when, between March and September 2020, lockdown was imposed in 128 of Chile's 346 municipalities, generating 128 natural experiments, which we stack in an event study design. Events of lifting lockdown provide a further series of experiments. We investigate dynamic impacts, allowing for treatment effect heterogeneity using the de Chaisemartin and D'Haultfœuille [2020] estimator. The identifying assumption is that the timing of lockdown mandates is conditionally random. We scrutinize this by displaying placebo or anticipation effects. To allow for the possibility that we are underpowered to detect differences in pre-trends, we additionally study the sensitivity of our estimates to violations of the assumption of counterfactual parallel trends, following Rambachan and Roth [2023].

We use high-frequency administrative data on three alternative measures of DV that, together, capture incidence, tolerance and reporting, allowing that lockdown mandates may have acted differently on these margins. We focus attention on cases where men are perpetrators and women are victims but also report results for male victims. We gather administrative data on the exact dates at which municipalities enter and exit lockdown, tri-weekly indicators of COVID-19 infection and testing rates, daily data on mobility from cell phone towers, monthly data on formal sector employment rates, and monthly data on stimulus payments.

The estimated placebo effects are consistent with the timing of lockdown mandates being idiosyncratic. We find that calls to a police-managed DV helpline (#149–*Fono Familia*) increase with imposition of lockdown and remain elevated when lockdown is lifted, consistent with state

dependence in DV. Women's use of public shelters for DV victims, a hard indicator of DV that is likely to capture more severe cases, similarly increases with imposition of lockdown. However, shelter use falls once lockdown is lifted. This is consistent with repeat violence having been made impossible on account of the woman being in a shelter, and with the option to stay with family and friends opening up after lockdown is lifted. The increase in helpline calls and shelter use reflect increased incidence, or reduced tolerance.[2] Yet DV crime reports to the police do not increase during lockdown and, if at all, show a tendency to decrease. This suggests that lockdown raised barriers to reporting, a case of increased distress coincident with more limited redress. In line with reporting barriers being the mechanism, crime reporting recovers when lockdown is lifted. In fact, crime reports rise above baseline levels, consistent with the forward displacement (pent-up demand) and ratchet effects (persistence) in helpline calls.[3] In general, the direct impacts of COVID-19 infection rates is, similarly, to increase helpline calls and reduce DV reports. Thus lockdown mandates *reinforce* impacts of COVID-19 on these outcomes. The estimates for male victims exhibit similar patterns but are muted, and there is no persistence in calls after lockdown is lifted.

Turning to mechanisms, lockdown imposition results in sharp declines in mobility and male employment, with both exhibiting some persistence after lockdown is lifted, potentially explaining the ratchet effect in helpline calls.[4] In contrast to emerging results for other OECD countries [Cajner et al., 2020, Adams-Prassl et al., 2020, Farré et al., 2020], impacts of lockdown

[2]Most studies infer incidence but, typically, this cannot be distinguished from tolerance.

[3]Effect sizes are computed as weighted averages of the dynamic coefficients. We estimate that lockdown entry leads to an escalation of calls by 94%, and lockdown exit only partially reverses this, such that calls remain 55% higher than they were before lockdown. Lockdown entry increases shelter use by 9.2% although this is not statistically significant after an initial period, and lockdown exit more than reverses this, with shelter use falling to about 7.3% below its baseline level. Crime reports decline by an imprecise 2.4% after lockdown is imposed and then shoot up by 9.0%, thus remaining 6.6% above their baseline level.

[4]Lockdown reduces mobility by 30%. After lockdown is removed mobility only partially recovers, remaining 22% below baseline. Lockdown imposition reduces male employment by 1.5%. Lifting lockdown again generates partial recovery, with employment remaining 1.2% below its baseline level.

on (formal sector) job loss are primarily experienced by men. Using a shift-share approach to facilitate identification, we estimate that male job loss explains about 54% of the increase in helpline calls. It cannot explain changes in DV crime reports. Female job loss is associated with fewer calls and fewer DV reports. The fact that the fall in reports exceeds the fall in calls indicates that female job loss deters reporting. However, in our setting, lockdown does not cause female job loss.

Further insight into the mechanisms leading from lockdown to DV is gained by analyzing impacts of federal stimulus payments (IFE) targeting low income households. Using idiosyncratic municipality-month variation in IFE, we show that it reduced shelter use and increased DV crime reports irrespective of lockdown status. For helpline calls, it led to a significant decline in municipalities under lockdown. Overall, the results are consistent with income shocks and exposure being mechanisms driving male perpetration. The mechanisms are of generic interest, outside pandemic situations. Existing studies and media coverage have highlighted impacts of lockdown on exposure, but not considered the role of male vs female job loss, or identified causal impacts of income support.

Recent research establishes a case for lockdown, showing that voluntary social distancing is below the socially optimal level (Eichenbaum et al., 2021; Farboodi et al., 2021; and Toxvaerd, 2020). It is estimated that the optimal policy involves losing 6% of one year's GDP (or, equivalently, a permanent reduction of 0.3 percent) and that the total welfare costs are more than four times bigger due to the cost of deaths (Alvarez et al., 2021). Our estimates suggest that the large and persistent increase in DV caused by lockdown should be added to its welfare costs. Section 2 details the estimation strategy. The DV outcomes are analyzed in Section 3, mechanisms in Section 4 and robustness in Section 5. Policy remediation is analyzed in Section

6 and Section 7 concludes.

2 Empirical Design

2.1 Rolling Lockdown Entry and Exit

Quarantine has a long tradition, dating back to the plagues in Europe and Asia in the Middle Ages (Adda, 2016), but historical analyses have not considered impacts on DV. We analyze the first COVID-19 wave in Chile, March 14th to September 30th 2020, during which 128 of Chile's 346 municipalities entered lockdown, with entry and exit graduated across municipalities over time. By September 30th, 43 municipalities had exited lockdown. Essential workers including hospital and supermarket workers were exempt but otherwise the mandate was strict, with citizens allowed to go out just twice a week for 3 hours at a time, using a permit. Violations of lockdown conditions could be penalized with fines of up to 10 million Chilean pesos (12,000 US$), or short prison terms. Police personnel conducted spot checks, and thousands of individuals were penalized. Overnight curfews were in place through the study period, irrespective of lockdown status. Lockdown was put in place at the discretion of the Ministry of Health based upon their assessment of the risk of contagion, but there was no declared metric. As a result, announcement of the mandate at the municipality level was unexpected. Once announced, lockdown took effect within 1-3 days and lasted between 6-172 days, with an average length of 32.5 days. When lockdown was lifted it was initially lifted for weekdays, and only subsequently for weekends as well.

2.2 Data

We manually gathered data on the exact dates of entry to and exit from lockdown for each municipality, available for other researchers here. Figure 1 provides a snapshot of the dynamics of entry and exit for the country. We created a video describing the dynamic nature of lockdown entry and exit, available here. We downloaded administrative data on COVID-19 infection and testing rates from open repositories updated several times a week by the Chilean Ministry of Science. Data on calls to the DV hotline of the Chilean Police, police reports of DV, and formal sector employment are obtained from administrative sources at the municipality-month level. The share of calls made by women over the sample period was 85% and the share of DV crime reports with a female victim was 80%.[5]

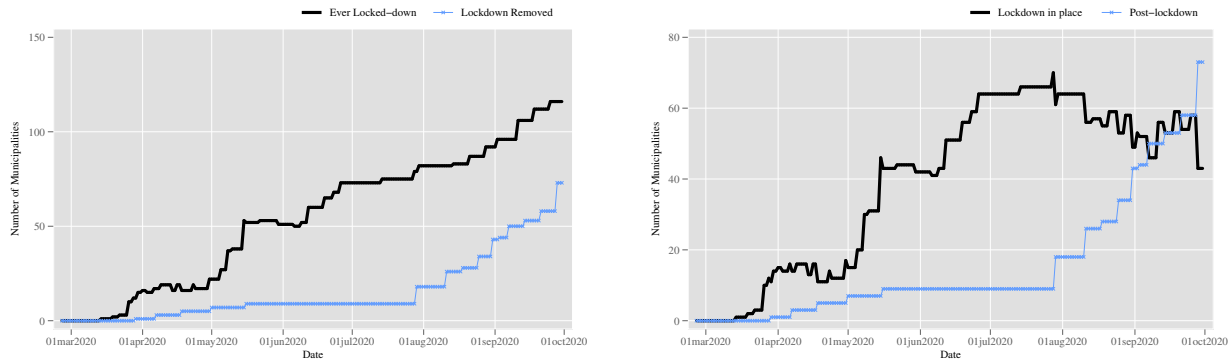
Mobility from cell phone towers is modeled at the municipality-day level to capture the fact that lockdown continued on weekends after it was lifted. Women's use of public shelters is only available at the region level. To increase power, we use daily variation in this outcome. Appendix C provides more information on the data, summary statistics, and raw trends in the outcome variables. Table C1 provides full variable definitions and sources.

2.3 Empirical Specification

We look to identify impacts of lockdown imposition *and* removal on three indicators of DV, as well as on potential mechanisms– mobility (which determines exposure to the perpetrator, isolation from social network and reporting opportunities), and male and female employment loss (which can activate income and time shocks that fuel DV). Identification leverages temporal variation in exposure to lockdown across municipalities. Denote domestic violence $DV_{s,t}$, where

[5]The person making the call may not be the victim so there is some potential misclassification there.

Figure 1: Temporal Dynamics of Lockdown Imposition and Removal



(a) Ever Locked-down

(b) Current Lockdowns

Panel (a) documents the cumulative number of municipalities ever locked-down, and for which lockdown has subsequently been removed. Panel (b) documents the number of municipalities currently in or out of lockdown at a given moment of time between March and September 2020.

s indexes municipality and t time. We use the de Chaisemartin and D'Haultfœuille [2020] estimator (DID_M) to obtain unbiased estimates of dynamic and placebo effects when treatment effects are heterogeneous, and the number of municipalities contributing to each lag and lead estimate is variable. Denote lockdown status $Q_{s,t}$, which takes the value of 1 if lockdown is in place in municipality s at time t, and 0 if not. Then define

$$DID_{+,t}^{Lockdown} = \sum_{s:Q_{s,t}=1,Q_{s,t-1}=0} \frac{N_{s,t}}{N_{(1,0),t}}(DV_{s,t}-DV_{s,t-1}) \quad - \sum_{s:Q_{s,t}=Q_{s,t-1}=0} \frac{N_{s,t}}{N_{(0,0),t}}(DV_{s,t}-DV_{s,t-1}).$$

For a particular t this provides the change in mean outcomes between $t-1$ and t among municipalities which adopted lockdown (switchers), relative to those that did not (non-switchers). The estimates are weighted by municipality population, $N_{s,t}$, but the broad results are not sensitive to weighting. We estimate the average dynamic impact of lockdown by taking the weighted average over all time-periods in which any switch occurs, the weight being the share

of individuals switching in the equation at each t:

$$DID_M^{Lockdown} = \sum_{t=2}^{T} \frac{N_{(1,0),t}}{N} DID_{+,t}^{Lockdown}. \quad (1)$$

To estimate each lag, we replace t in the equations above with $t + k \forall k \in \{1, \ldots, K\}$, while maintaining $t - 1$ as the relevant comparison period, and conduct a similar procedure for pre-treatment leads. We conduct inference on each parameter using a block (cluster)-bootstrap based on municipalities. We consistently include 18 leads and 4 lags, as after 4 months, the number of municipalities in lockdown declines (Table A2). In order to capture only impacts of lockdown imposition, and not any additional impacts once municipalities exit lockdown, the analysis sample includes municipalities up until the point that they graduate out of lockdown.

When the event is exit from lockdown, pre-event coefficients compare municipalities that are all in lockdown, treated municipalities being those that in the future exit lockdown and control municipalities being those that remain in lockdown. The coefficient at time zero compares municipalities which exited lockdown between the current and previous period to those that remained under lockdown. By definition, the DID_M estimator purges region and time fixed effects (FE). As schools closed nationwide on March 16, impacts of school closure are absorbed by time FE. Other predictors of lockdown timing such as population density and gender of mayor are absorbed by municipality FE. We control for COVID-19 infection and testing, also showing results without these controls.

The identifying assumption is that the exact timing of lockdown mandates is quasi-random conditional upon region and time FE and the COVID indicators. The placebo (pre-event) coefficients provide a partial test of parallel trends, revealing whether switchers and non-switchers

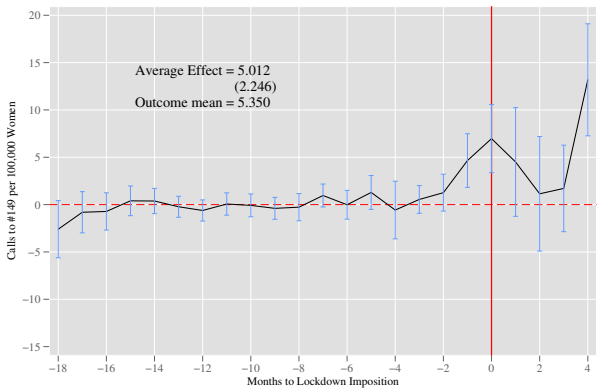
had similar outcome trends prior to lockdown.[6] Even if they do, this may be because we are underpowered to reject parallel trends. We therefore estimate partially identified bounds [Rambachan and Roth, 2023] on the dynamic estimates under the assumption that any possibly non-parallel pre-lockdown trends are projected forward. Rather than only consider strictly linear projections, for each lag we present bounds additionally allowing further divergence from pre-trends between each period. As the bounds are applied to the standard DiD estimator, we obtain the diagnostics for bias in the standard estimator [Goodman-Bacon, 2021, de Chaisemartin and D'Haultfœuille, 2020]. These show between zero and 4.5% negative weights (Table A1), suggesting small, if any, bias in the standard estimator. This is consistent with our sample containing a large share of never-treated municipalities.

The DID_M estimator resolves potential bias arising in two-way FE model estimates when treatment varies over time and there is heterogeneity in treatment effects across units and time. It is not sensitive to compositional changes that arise as different municipalities enter (or exit) treatment.[7] Nevertheless, different lags and leads will be driven by different exposure groups, for instance, longer lags will be driven by the small fraction of municipalities that had longer-duration lockdowns. We will investigate heterogeneity in impacts by lockdown duration. This is relevant to the substantive question of treatment intensity, and to checking whether our estimates are valid across municipalities with different exposures rather than driven by a particular set.

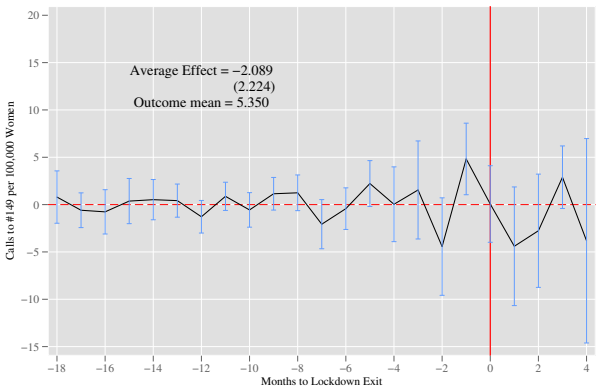
[6] de Chaisemartin and D'Haultfœuille [2020] pre-trend coefficients differ from pre-trend coefficients in standard event studies [Autor, 2003], which are potentially invalid when treatment effects are heterogeneous [Sun and Abraham, 2021].

[7] In the standard panel event study, given that a single baseline period is omitted, if lags and leads are not balanced, this can lead to changes in point estimates that reflect compositional rather than causal changes. This problem does not arise with the DID_M estimator because it chooses adoption period lag-specific baseline groups.

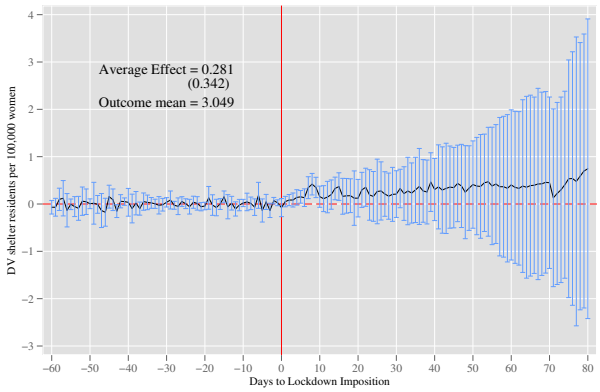
Figure 2: Impacts of Lockdown on Incidence of Domestic Violence



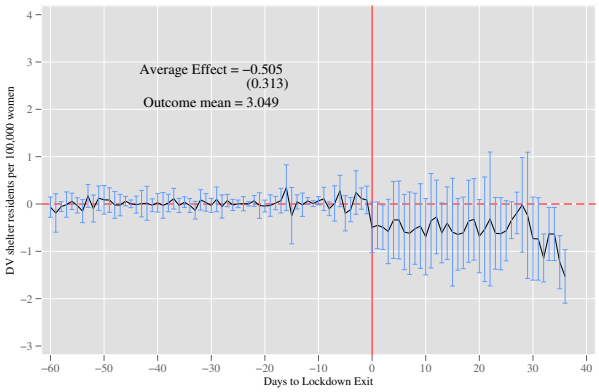
(a) Helpline Calls (Entry)



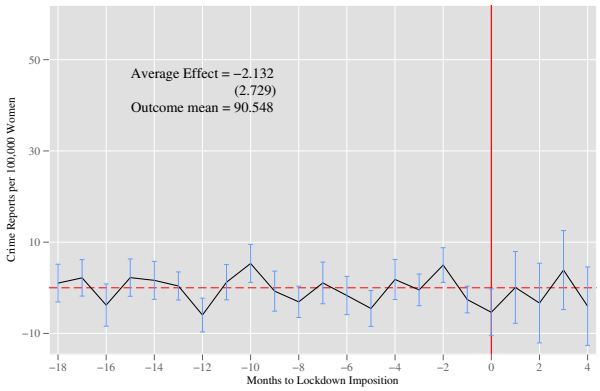
(b) Helpline Calls (Exit)



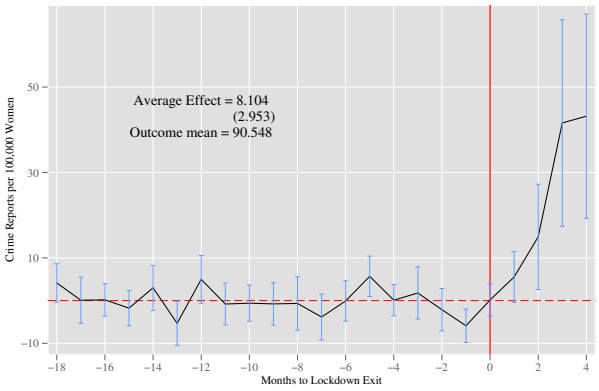
(c) Shelter Usage (Entry)



(d) Shelter Usage (Exit)



(e) Crime Reporting (Entry)



(f) Crime Reporting (Exit)

Notes: Estimates obtained using the DID_M estimator [de Chaisemartin and D'Haultfœuille, 2020]. Outcome variables are defined in Section 2.2 and Appendix C. Calls and police reports are at the municipality-month level, shelter use at region-day level. For lockdown entry, the sample contains all municipalities at risk of imposition. For lockdown exit the sample consists of all municipalities that have ever entered lockdown and hence are at risk of exit. All estimates are conditional on controls for rates of COVID infection, testing and population. Specifications without controls are discussed in Section 5. Confidence intervals are estimated using a block-bootstrap by municipality or, for shelter use, region. Average dynamic effects are computed as weighted averages of the year-specific dynamic effects, the weights being the number of municipalities switching in each year. Axes are scaled across rows to allow comparison of effect sizes.

3 Results

Estimates for DV indicators are in Figure 2, and for mobility and male and female employment rates in Figure 3. The results in this section are for female victims, results for male victims are in Appendix Figure A1.

3.1 Calls to the DV Hotline, 149

Calls capture fairly serious concerns. The number is formally linked to the police and calls are geographically tracked. When the call relates to an emergency, a patrol car is sent to the victim's address, and this results in a formal crime report. However, not all calls result in criminal complaints and, conversely, criminal complaints can be initiated without first calling this hotline. Figure 2 shows an increase in calls following lockdown imposition, that increases sharply four months in. It shows parallel pre-trends for lead months -18 until -2. There is an increase in the month preceding lockdown consistent with our data showing that coronavirus infections rise, on average, 5 weeks before lockdown, leading to voluntary distancing and stress. A weighted average of the dynamic effects over the four months following lockdown gives a 94% increase.

The right panels provide estimates for causal impacts of exiting lockdown. There is no significant decline in helpline calls when lockdown is lifted. The averaged dynamic effect indicates that DV remains 55% above its baseline mean, consistent with wider evidence of path dependence in DV. Helpline calls are tagged as being related to physical, psychological or economic violence. Appendix Table A3 shows that lockdown drives similar proportional increases in physical and psychological violence. The effect is larger for economic violence, but the baseline

average is two orders of magnitude smaller.

3.2 Occupancy of Public Shelters for Abused Women

These estimates are relatively noisy, being at the region-day level. The middle row of Figure 2 shows placebo coefficients that closely track the zero line until the event of lockdown imposition or removal. After imposition, there is a tendency for shelter use to increase (9.2%) through the 80-day window, albeit the coefficients are increasingly imprecise. Once lockdown is lifted, there is a 16.6% decline in shelter use (and we cannot reject that this is equal to the effect of lockdown entry).

Shelter occupancy will under-estimate the demand for shelter during the pandemic if women perceive shelter use as elevating the risk of infection. Although Chilean shelters had infection limiting protocols, we see evidence consistent with this in Table A4, which shows that a higher COVID-19 infection rate (included as a control in the analysis) is associated with lower shelter use (imprecisely estimated), even though COVID is associated with more helpline calls. We retain shelter use as it is a "harder" indicator of DV than calls, possibly capturing more extreme cases. It is still useful to study helpline calls as these are not directly sensitive to infection concerns, and will capture cases of varying intensity.

Given the surge in DV following outbreak of the pandemic, we investigated whether shelter occupancy might under-estimate shelter demand for a different reason, which is that shelters were filled to capacity. This appears not to be the case. First, before the pandemic struck, there were spare spaces in shelters, the average occupancy rate was 62% (standard deviation 42%). Second, we estimated impacts of lockdown on shelter capacity and found an (imprecise) increase of 6.9% in shelter spaces (Figure A2).

3.3 Police Reports of DV

The last row of Figure 2 shows no increase in DV crimes filed with the police following lockdown. In fact the average dynamic effect is an imprecise decline of 2.4%. As this is despite increases in helpline calls and shelter use, it indicates that lockdown caused impediments to reporting DV. These may include feeling economically vulnerable and hence more likely to tolerate DV; isolation from social networks; mobility restrictions limiting access to police stations; police capacity being compromised by social distancing and officer absences; and forced proximity to the perpetrator intensifying fear of backlash. We cannot discriminate between each of these, but we investigate mobility restrictions and the role of job loss.

Lifting lockdown leads to a reversal, indeed, crime reports rise 8.9%, exceeding baseline levels by 6.6%, consistent with new demand (reflected in helpline calls persisting at levels above baseline) added to pent-up demand.

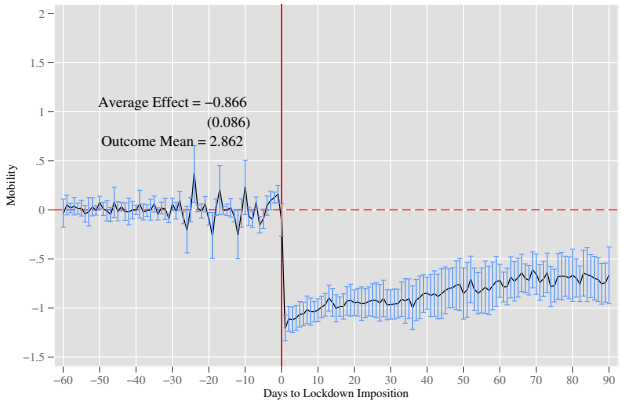
4 Mechanisms: Mobility and Employment

We estimate dynamic impacts of lockdown entry and exit on mobility and employment. Then, using a shift-share instrument, we estimate the contribution of male and female job loss to DV.

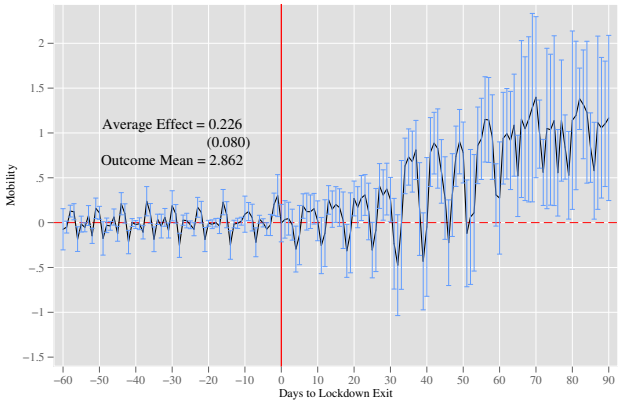
4.1 Mobility

The pre-trends indicate no evidence that municipalities that went into lockdown exhibited larger voluntary declines in mobility before lockdown than municipalities that did not go into lockdown in the sample period. This is interesting in its own right, and it supports the identifying assumption. Lockdown leads to a sharp drop in mobility which persists but attenuates over

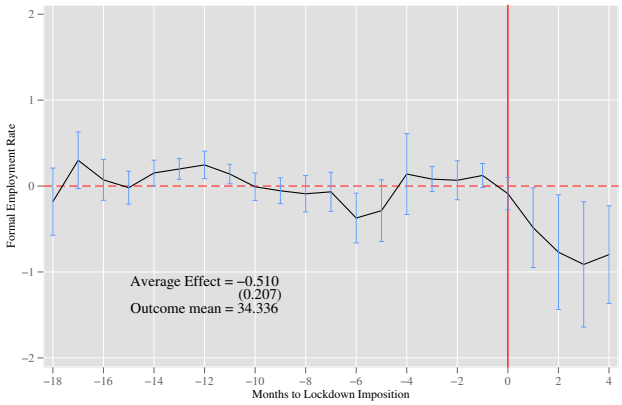
Figure 3: Impacts of Lockdown on Mechanism Variables



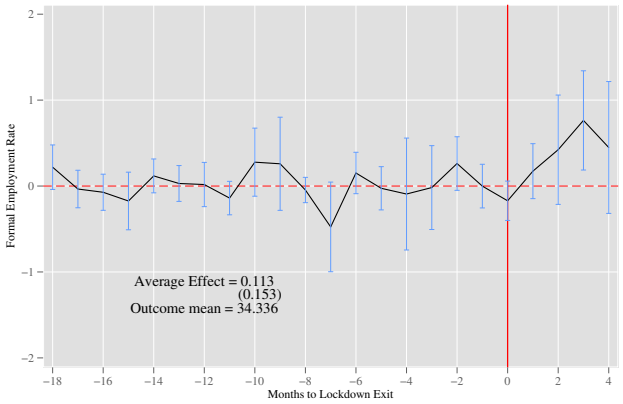
(a) Cell Phone Mobility Patterns (Entry)



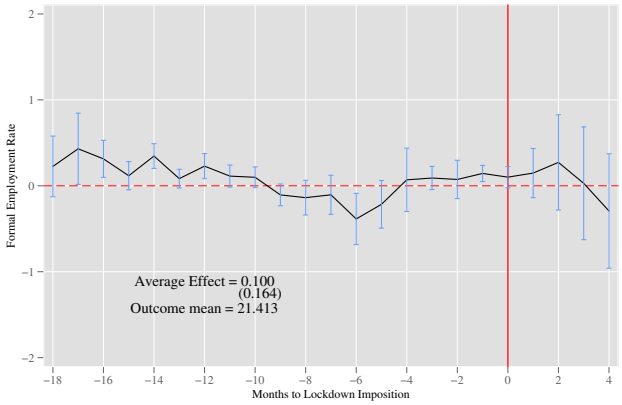
(b) Cell Phone Mobility Patterns (Exit)



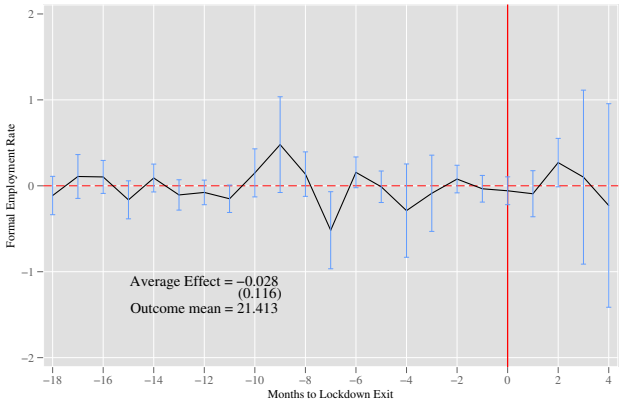
(c) Male Formal Sector Employment Rate (Entry)



(d) Male Formal Sector Employment Rate (Exit)



(e) Female Formal Sector Employment Rate (Entry)



(f) Female Formal Sector Employment Rate (Exit)

Notes: Refer to notes to Figure 2. Mobility is within-municipality daily movement got from cell phone data (panels (a) and (b)), and employment is formal sector at the municipal level generated from administrative records (panels (c) to (f)).

the three months after, averaging 30%. This measures not the absolute mobility decline but the difference between areas that were and were not under lockdown. Volatility in the series reflects that these are daily data. Lifting lockdown results in an uptick in mobility, though it remains lower than at baseline, consistent with weekend restrictions remaining in place (evident in the spikes in the plot) and with infection rates only starting to fall, on average, 5 weeks after lockdown withdrawal. This ratchet effect in mobility lines up with that in helpline calls, consistent with mobility being a mechanism by which lockdown impacted DV.

4.2 Formal Sector Employment Decline

Lockdown imposition leads to a decrease in male employment in the formal sector of 0.51 percentage points, approximately 1.5%. Lifting lockdown does not reverse this. We see a statistically insignificant partial recovery that leaves employment about 0.4 pp below baseline. This ratchet effect mirrors the pattern for helpline calls, suggesting male employment as a mechanism, which we formalize in the next section. There is no evidence that lockdown entry or exit affect female employment.

We see a sharp increase in worker contract suspensions under the state furlough scheme (Figure A3). The displayed estimates average over men and women but are dominated by movements for men. Although furlough protects income relative to job loss, the total impact of lockdown on earnings uncertainty will include its impacts on job loss and contract suspension.

4.3 Identifying the Contribution of Employment Decline to the Increase in DV

We use a shift-share strategy to identify impacts of employment loss on DV. This is a contribution in itself, adding to a small causal literature discussed in Bhalotra et al. [2021b]. We

combine this with the identified impacts of lockdown on employment to estimate employment-mediated impacts of lockdown on DV. Specifically, to complete the mechanism chain, we require an estimate of β from:

$$DV_{st} = \alpha + \beta Employment_{st} + \mu_s + \phi_t + \varepsilon_{st}. \tag{2}$$

We identify this using a Bartik instrument for $Employment_{st}$ [Card, 2009, Goldsmith-Pinkham et al., 2020]. The procedure and tests of the identifying assumptions are set out in Appendix B. Table B1 shows the first stage estimates. The Cragg-Donaldson F-statistic is consistently larger than 248. We additionally report tests that assuage any potential concerns over weak-IV or under-identification.

The identifying assumption is that baseline sector shares are uncorrelated with the structural error term once we condition upon municipal and month FE. We elaborate and test the assumptions in Appendix B. First we show that the Bartik instrument does not predict DV before the COVID shock, but that it does after. Second, we show that baseline industry shares are uncorrelated with changes in domestic violence in the pre-COVID period. Third, we reference results in Section 5 showing that the estimates do not move a lot upon controlling for time-varying COVID rates, which increases our confidence in the exclusion restriction.

See Panel A, Table 1.[8] We estimate that a 1 percentage point decrease in male employment raises helpline calls by 5.3 relative to a mean of 5.3 per 100,000 women. Breaking this down, we see this emerge from physical and psychological violence, with no impact on economic violence. Male and female job loss have opposing impacts. Male job loss leads to a significant increase

[8]We do not provide results for shelters because shelter data are at region and not municipality level, and IV consistency requires asymptotics which are likely to hold with 346 municipalities, but not 16 regions.

in helpline calls, and an insignificant decline (but no increase) in crime reports. Male job loss typically constitutes a large income shock to the household, and hence more stress. This is likely reinforced by the psychological stress of the breadwinner losing his job. If, at baseline, fewer women are at work, then male job loss also has larger impacts on exposure than female job loss. Female job loss lowers both helpline calls and reports, consistent with jobless women being more willing to tolerate DV. The fact that DV reports fall more than DV calls suggests that female job loss lowers the willingness to report. Research on DV has been challenged by the fact that it is often difficult to disentangle incidence from reporting. Our finding that lockdown and, independently, male vs female job loss move incidence and reporting in opposite directions highlights the relevance of this distinction.

Table 1: Decomposing the Contribution of Gender-Specific Employment Shocks to Changes in Domestic Violence

	Calls				Crime
	All Calls	Economic	Physical	Psychological	Reporting
	(1)	(2)	(3)	(4)	(5)
Panel A: Shift Share IV					
Formal Employment (Women)	1.740**	-0.00595	1.044**	0.702*	2.850**
	(0.690)	(0.0194)	(0.403)	(0.360)	(1.209)
Formal Employment (Men)	-5.284**	-0.0114	-2.894***	-2.379**	-1.394
	(2.074)	(0.0220)	(1.075)	(1.037)	(1.339)
Observations	5,520	5,520	5,520	5,520	5,520
Mean Dep. Var.	5.311	0.020	1.970	3.321	91.169
First Stage F-Statistic (C.-D.)	427.1	427.1	427.1	427.1	427.1
Panel B: Lockdown and Domestic Violence					
Lockdown	5.012**	0.164**	2.305*	2.542*	-2.132
	(2.365)	(0.069)	(1.347)	(1.359)	(3.426)
Panel C: Scaled Estimates					
Mediator: Women's Job Loss	3.5%	-0.4%	4.5%	2.8%	-13.4%
Mediator: Men's Job Loss	53.7%	3.5%	64.0%	47.7%	-33.3%

Notes: Panel A reports impacts of job loss on DV using the Bartik instrument. Panel B reproduces impacts of lockdown on DV from Figure 2, further summarized in Table A3. Panel C shows the proportion of the total impact of lockdown on DV (panel B) that can be explained by the impact of gender specific job loss on DV (panel A), interacted with the actual rates of job loss resulting from lockdown imposition (Figure 3c and 3e).

Impacts of male job loss on DV mirror the pattern of impacts of lockdown on DV, with calls increasing alongside no increase in crime reports. A decomposition exercise indicates that male employment loss accounts for close to 54% of the increase in helpline calls. Female employment loss accounts for only 4% of the increase in calls, and it makes a negative contribution of 13% to explaining DV reports.

We generalized the model to allow for an interaction effect between male and female job loss, see Table B2. There is a small but meaningful interaction for calls, and no interaction for reports. The increase in calls following male job loss is larger when women are also losing their jobs. This is in line with the mechanisms triggered by male job loss being income-related stress and exposure, as both are exacerbated if female job loss coincides with male job loss. The decrease in calls following female job loss is attenuated when men are also losing their jobs. This is consistent with the increased tolerance that we hypothesize being moderated by the man being unemployed.

5 Robustness Checks

We now discuss sensitivity checks on the estimates in Figures 2-3. Figures A5-A6 present the DID_M models of de Chaisemartin and D'Haultfœuille [2020] with the COVID-19 infection and test rates as in Figure 2 and, for comparison, we overlay DID_M models without the pandemic controls, the standard event study, and Rambachan and Roth [2023] bounds on the event study under a range of priors (see notes to the Figures) relating to potential violations of parallel trends.

In every case, the placebo coefficients are indistinguishable from zero, confirming that, under alternative assumptions, the events of lockdown entry and exit are quasi-experimental.

Removing the COVID indicators from the controls leads to larger dynamic effects, for example, for calls, the coefficients are 10% larger. This is consistent with COVID indicators and lockdown moving helpline calls (and crime reports) in the same direction.[9]

Direct impacts of COVID-19 infection rates on DV are in Table A4. A 1 SD increase in COVID infection rates (4.32 in the post-COVID period) increases helpline calls by 58% and decreases reporting by 5% conditional on lockdown. The units of change are not comparable as lockdown is a binary event, but the direct impacts of lockdown are, in general, not smaller than the impacts of infection rates. The standard event study produces estimates similar to DID_M. The summary single index coefficients are in Table A4. Table A1 and Figure A4 present diagnostics to assess bias in the conventional estimator [Goodman-Bacon, 2021]. These reveal consistency across estimates that rely on comparison of treated municipalities with those treated earlier vs later vs never (Figure A4), and negative weights sum to < 0.10. This explains why the conventional estimator agrees with DID_M. The Rambachan and Roth [2023] bounds on the dynamic coefficients confirm the main findings, though, by construction, they widen with time from adoption.

We explained in Section 2.3 that the estimates are unbiased even when the municipality panel is unbalanced across leads and lags. We provide estimates by lockdown duration in Appendix Figure A7. The baseline estimates are weighted combinations for all units that have lockdowns of at least the stated length. Thus, lag 1 is estimated on all municipalities which have at least 1 month in lockdown (durations of 1, 2, 3, etc. months); lag 2 on municipalities with at least 2 months in lockdown (2, 3, etc. months). In Figure A7 the group is fixed across lags, so each estimate is on a balanced panel. The results are fairly similar across samples, though

[9]In this regard, shelter occupancy is different. Shelter use decreases as COVID spreads. But conditional upon infection rates, lockdown increases shelter use.

more so for calls than for crime reports where the actual numbers are smaller and precision harder to achieve in subsamples.

6 Policy Remediation

The national government introduced an emergency family income program (*Ingreso Familiar de Emergencia*, IFE) in May 2020, providing cash transfers to low income households most affected by the pandemic, with eligibility based on income, assets and need. Benefits comprised payments of US$80-130 per person for families without any formal income, while households with some formal income received a partial payment. By end September 2020, four payments had been disbursed and the state had spent over $2,223 million, 0.79% of 2019 GDP.[10] The trend in roll-out in Figure A8a shows that, by September 2020, stimulus payments had reached nearly 8 million individuals, 42% of the population. Figure A8b shows the distribution of payments, confirming widespread exposure to IFE.

We investigate whether IFE mitigated impacts of lockdown mandates on DV. If it did, this would constitute evidence that economic stress is a mechanism by which lockdown impacts DV, at least among low income households who were the main beneficiaries.[11] We define IFE_{ct} as the proportion of the municipal population that received IFE payments. This proportion is initially zero, and jumps discontinuously with the first payment. As we are interested in an interaction term, we use the single coefficient model, having shown diagnostics that suggest any

[10] For comparison,the first stimulus payment under the CARES act in the US amounted to 1.36% of 2019 US GDP, a total of 162 million payments that cost $292 billion.

[11] Chile also introduced contract suspension or furlough, for which only formal sector workers paying into an unemployment insurance scheme were eligible. During March-June 2020, 800,000 workers were on furlough, a lot fewer than IFE. We investigated mitigation with furlough protection but the estimates were imprecise.

potential bias is small. We estimate:

$$DV_{ct} = \alpha + \beta Lockdown_{ct} + \gamma IFE_{ct} + \delta(Lockdown \times IFE_{ct}) + X_{ct}\Gamma + \mu_c + \phi_t + \varepsilon_{ct}.$$

where γ measures IFE policy impact in municipality-months not under lockdown. The impact of IFE in areas under lockdown is γ plus δ. Municipality FE absorb the impacts of all fixed and slowly evolving factors like poverty and the baseline proportion eligible for IFE, and time FE capture impacts of the pandemic across municipalities. We again consistently control for COVID-19 infection and test rates, and examine sensitivity to excluding them.

A potential concern is that, conditional upon all controls, exposure to IFE is not quasi-random. In particular, if municipalities with higher shares of IFE beneficiaries already had DV declining more rapidly than municipalities with lower shares, we might spuriously attribute changes in DV to IFE. To assess this, we regress lagged DV on IFE shares, using a series of lags for pre-COVID months (or, for shelters, days). Figure A9 reports contemporaneous (top row) and placebo coefficients (the latter, like pre-event coefficients in an event study). The placebos are, in general, small and insignificant, so we can reject the concern. In Appendix Figure A10, we show that IFE policy intensity is orthogonal to job loss after lockdown, consistent with IFE targeting informal sector workers and job loss being measured for the formal sector.

Results are in Table 2. The total impact of IFE payments, across municipalities that were and were not in lockdown is shown in row 4. IFE is associated with fewer helpline calls and lower shelter occupancy (indicating lower DV), and with higher crime reporting (which, conditional on lower DV, is a good thing). Only the estimate for shelter use is statistically significant. The main effect of IFE (row 2) is a significant decline in shelter use and a significant increase in

Table 2: Did Cash Transfers Lower Domestic Violence?

	Calls to 149		Crime Reporting		Shelter Residence	
	(1)	(2)	(3)	(4)	(5)	(6)
Lockdown	8.216***	10.70***	-5.275***	-4.640*	0.149	0.236
	(1.255)	(1.719)	(1.806)	(2.371)	(0.153)	(0.159)
IFE	0.104	1.217	16.00***	16.29***	-3.057***	-2.997**
	(4.503)	(4.410)	(5.734)	(5.696)	(1.007)	(1.024)
Lockdown× IFE		-11.64**		-2.973		-0.356
		(5.430)		(6.739)		(0.385)
IFE+						
Lockdown x IFE		-10.419		13.315		-3.353***
		(8.587)		(8.796)		(1.006)
Observations	5520	5520	5520	5520	4528	4528
R-squared	0.784	0.785	0.622	0.622	0.860	0.860

Notes: IFE refers to the proportion of the municipal (for shelters, regional) population receiving transfers. All regressions include region and time fixed effects and rates of COVID infection and testing. We cluster standard errors by area. The fourth row is the linear combination shown. *** p<0.01, ** p<0.05, * p<0.1.

DV reports across municipalities, irrespective of their lockdown status. For helpline calls, IFE is effective in bringing about a significant decline conditional upon lockdown. The effects are sizeable. For instance, lockdown results in an increase in helpline calls of 10.7 calls per 10,000 women in municipalities that did not receive IFE payments, and this ratio falls to 5.6 for the notional municipality with the median share of individuals receiving IFE payments (0.437), see column 2, rows 1 and 3.

The estimates are slightly larger if we do not condition on COVID variables (Table A5). Analyzing the US equivalent, Erten et al. [2022] also find it reduces DV. Their effect sizes are not comparable because they use a dummy for the national timing of the introduction of stimulus payments, while we use variation in program intensity by sub-region.

7 Conclusions

This paper uses Chilean administrative data to investigate impacts of lockdown on multiple indicators of DV that capture variation in intensity and in incentives to record or report it. We leverage the fact that lockdown mandates were staggered across hundreds of municipalities, creating variation in exposure within the same country, region and even city. Using state-of-the-art observational techniques, we estimate dynamic effects of both entry to and exit from lockdown, investigate mobility and job loss of women and men as mechanisms, and the mitigation afforded by cash transfers.

We find that lockdown exacerbates DV, reinforcing direct impacts of COVID-19, and that DV remains elevated after lockdown is lifted. Male job loss plays a significant role in triggering higher incidence. Female job loss tends to reduce reported DV, but we see no significant increase in female job loss as a result of lockdown. Consistent with job loss constituting an income shock, income support mitigates DV. The analysis reveals income and exposure shocks as mechanisms triggered by job loss that are of broader relevance to understanding DV, even outside pandemic conditions.

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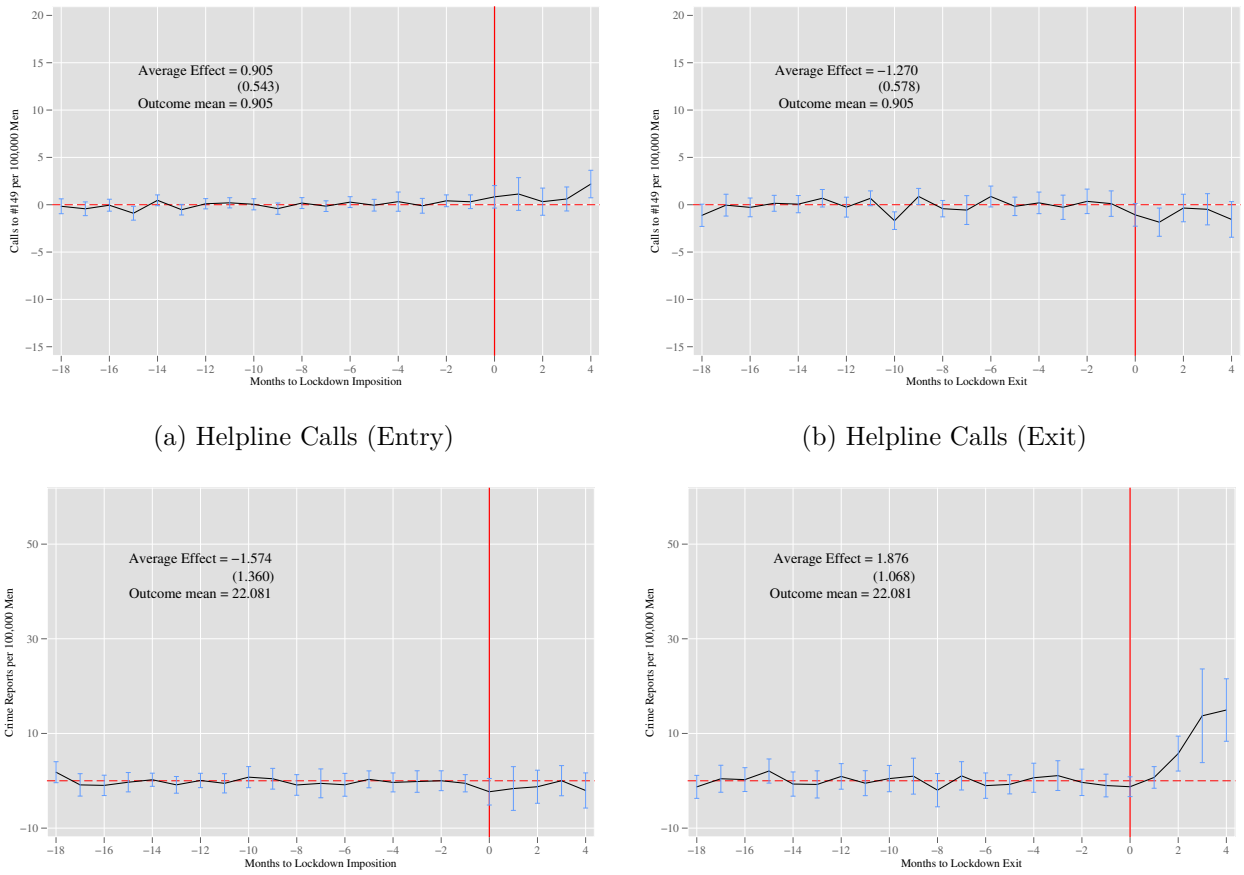
Supplementary Materials
Dynamic Impacts of Lockdown on Domestic Violence: Evidence from Multiple Policy Shifts in Chile

Sonia Bhalotra, Emilia Brito, Damian Clarke, Pilar Larroulet, Francisco Pino

A Appendix Figures and Tables

Figure A1: Impacts of Lockdown on Incidence of Intra-family Violence (Men)



(a) Helpline Calls (Entry)

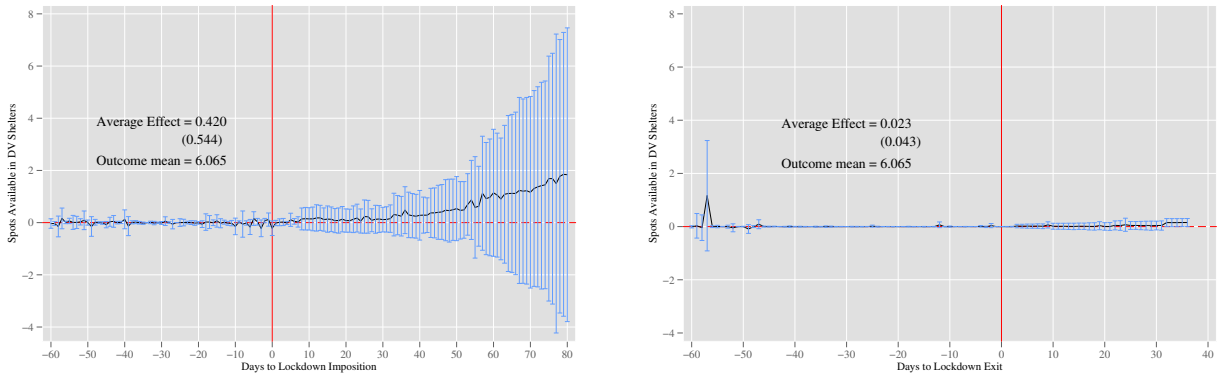
(b) Helpline Calls (Exit)

(c) Crime Reporting (Entry)

(d) Crime Reporting (Exit)

Notes: Refer to notes to Figure 2. Identical specifications are estimated however examining rates of calls to the Chilean police family support hotline (panels (a) and (b)), and formal criminal complaints (panels (c) and (d)). Calls are limited to those made by men and crimes are limited to male victims. All other estimation details follow those indicated in notes to Figure 2.

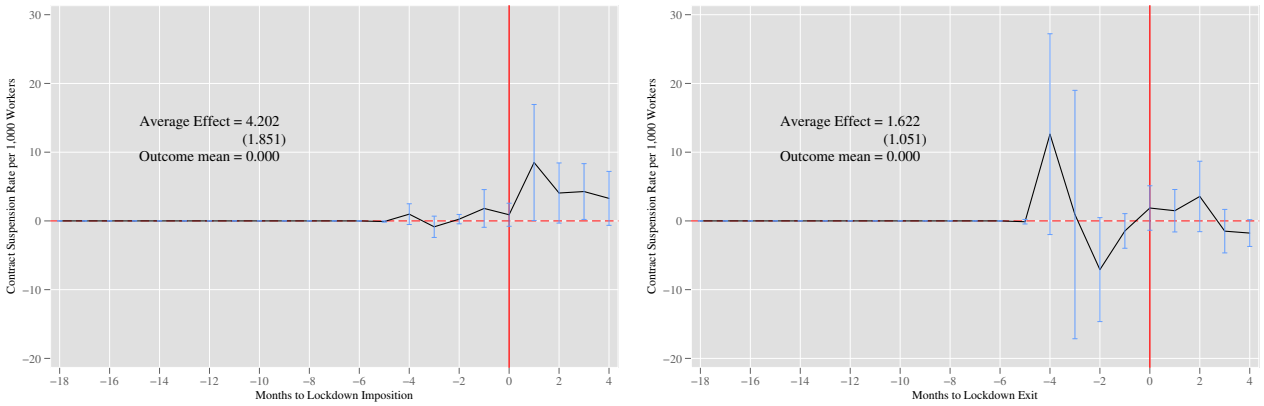
Figure A2: Lockdown Imposition and Removal and Supported Spots at DV Shelters



(a) Lockdown Entry (b) Lockdown Exit

Notes: Refer to notes to Figure 2. Identical specifications are estimated however examining available spots at DV shelters. All other estimation details follow those indicated in notes to Figure 2.

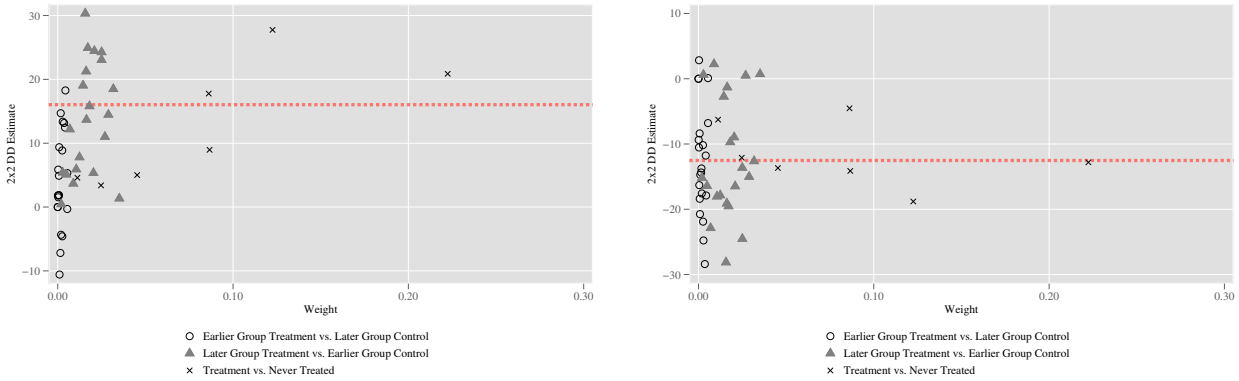
Figure A3: Lockdown Imposition and Removal and Job Suspensions



(a) Lockdown Entry (b) Lockdown Exit

Notes: Refer to notes to Figure 2. Identical specifications are estimated however examining contract suspensions. All other estimation details follow those indicated in notes to Figure 2.

Figure A4: Decomposing the Two-way Fixed Effect Estimate: Municipal-level Lockdown Entry

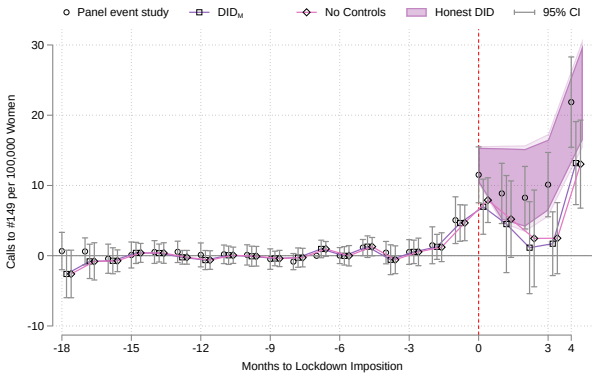


(a) Helpline Calls

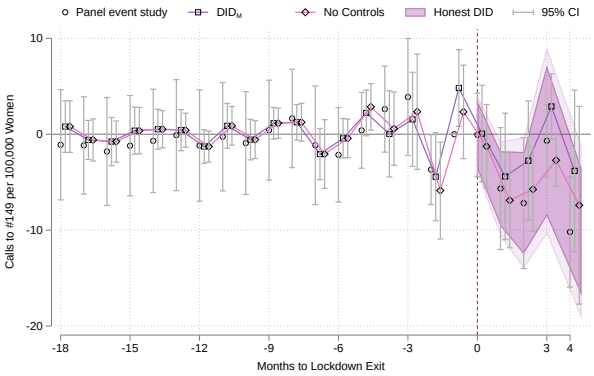
(b) Crime Reporting

Notes: Decomposition of the two-way fixed effect estimator follows Goodman-Bacon [2021], plotting each group×time estimate as well as its weight in two-way fixed effect models. Results are presented for municipal by month records, in which each of the three Goodman-Bacon [2021] groups exist (treated vs never treated, early versus late adoption, late versus early adoption). The group-specific estimates are presented as points on the plot, while the aggregate two-way FE estimate is presented as the dashed horizontal line.

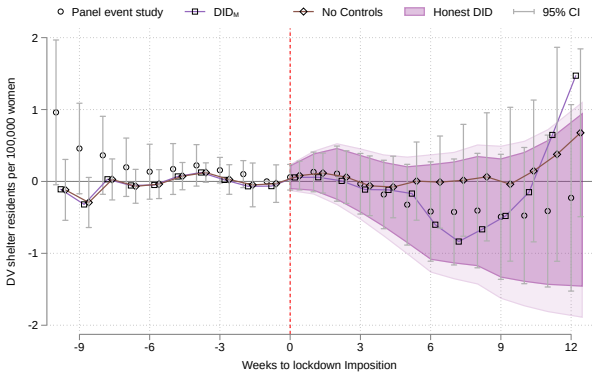
Figure A5: Estimated Impacts of Lockdown on Incidence of Intra-family Violence



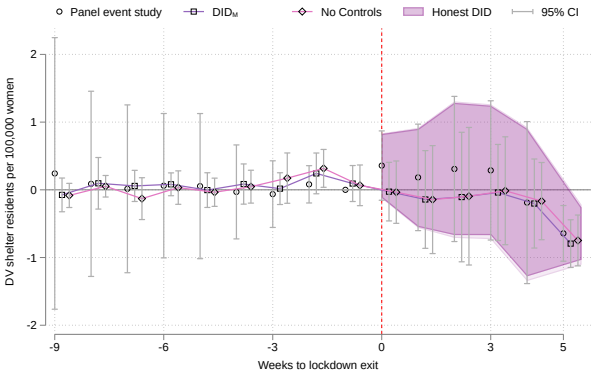
(a) Helpline Calls (Entry)



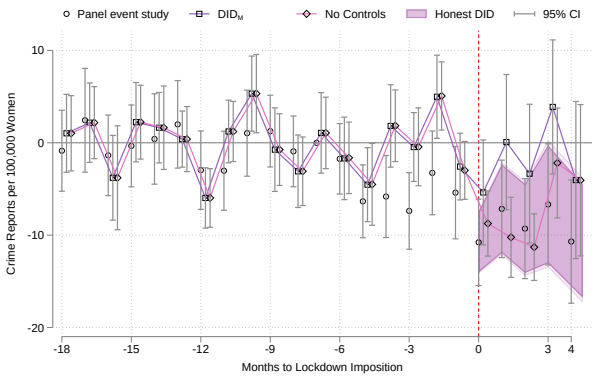
(b) Helpline Calls (Exit)



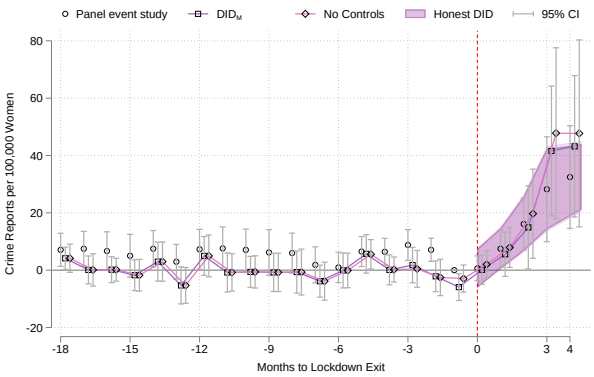
(c) Shelter Usage (Entry)



(d) Shelter Usage (Exit)



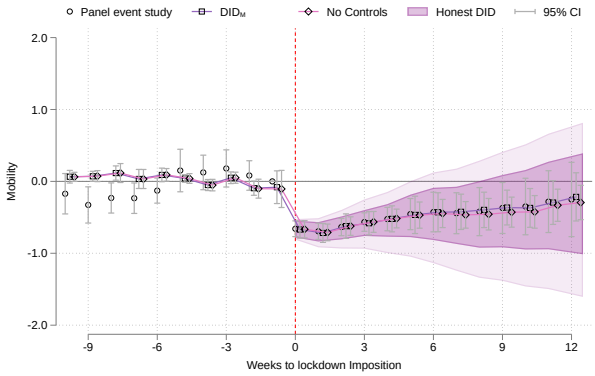
(e) Crime Reporting (Entry)



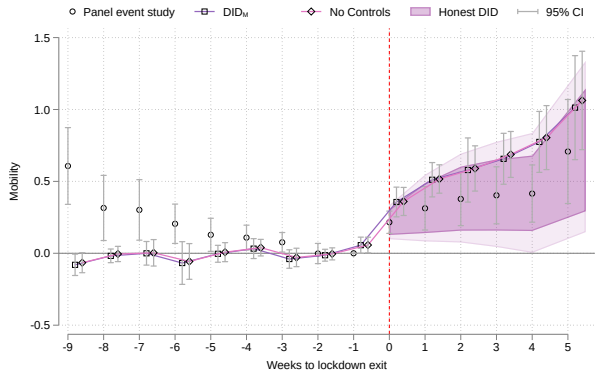
(f) Crime Reporting (Exit)

Notes: Figures display point estimates (where relevant) and confidence intervals of the impacts of lock-down on measures of DV incidence and reporting under a range of identifying assumptions and specifications. In each case, standard panel event studies are displayed as hollow circles and 95% CIs, along with de Chaisemartin and D'Haultfœuille [2020]'s DID$_M$ estimator with full dynamic lags and leads. For event studies, month -7 is used as the baseline omitted period, as this is pre-pandemic, even for municipalities which adopt lockdown in the final period of study. Specifications with epidemiological controls are indicated by hollow squares, and unconditional estimates are displayed as diamonds. Finally, Rambachan and Roth [2023]'s partially identified 'Honest DiD' bounds are presented as shaded purple areas, however now rather than assuming parallel counterfactual trends between lockdown and non-lockdown municipalities, project any prevailing trends forward, where these trends are additionally allowed to vary by as much as M between each period. Darker shaded areas correspond to $M = \{0.03, 0.005, 0.02\}$ for helpline calls, shelter residence, and crime reporting respectively, and lighter shaded areas correspond to $M = \{0.1, 0.03, 0.05\}$. In the case of shelter usage only, weekly, rather than daily estimates are plotted, given computational demands on bounds when many lags are included.

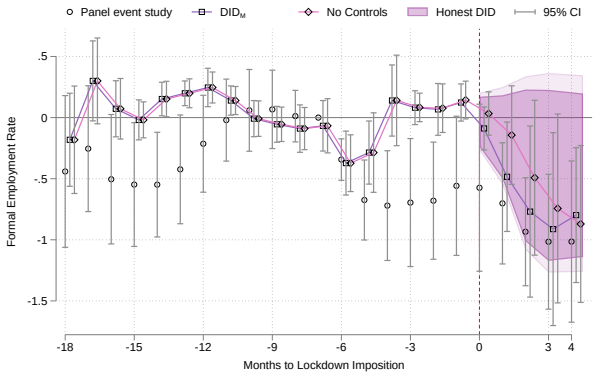
Figure A6: Estimated Impacts of Lockdown on Mechanism Variables



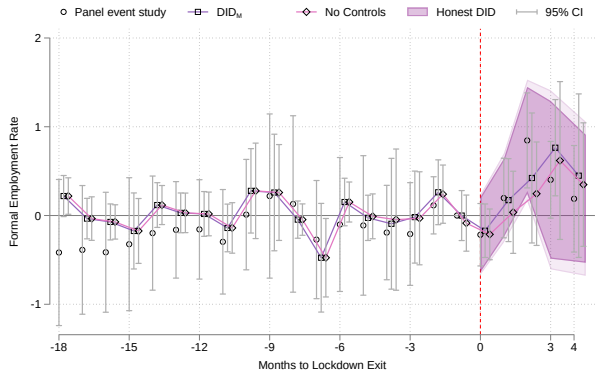
(a) Cell Phone Mobility Patterns (Entry)



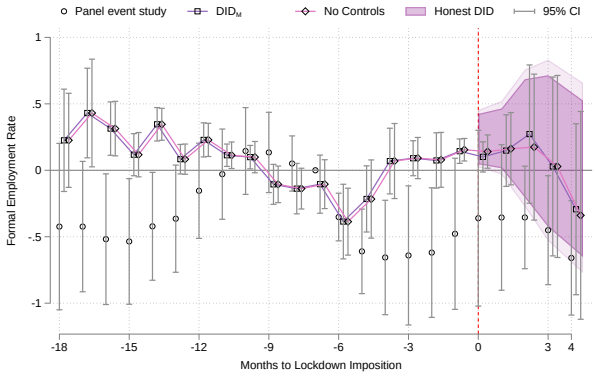
(b) Cell Phone Mobility Patterns (Exit)



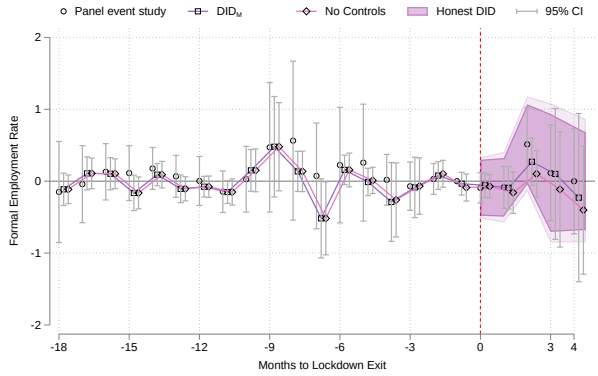
(c) Male Formal Employment Rate (Entry)



(d) Male Formal Employment Rate (Exit)



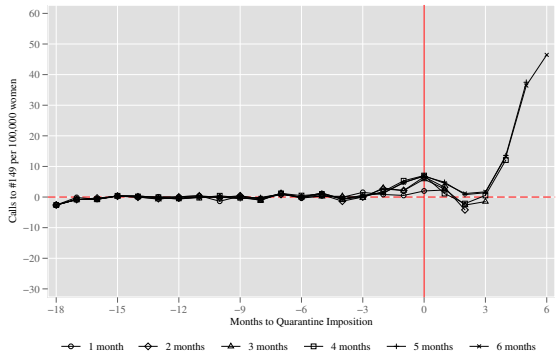
(e) Female Formal Employment Rate (Entry)



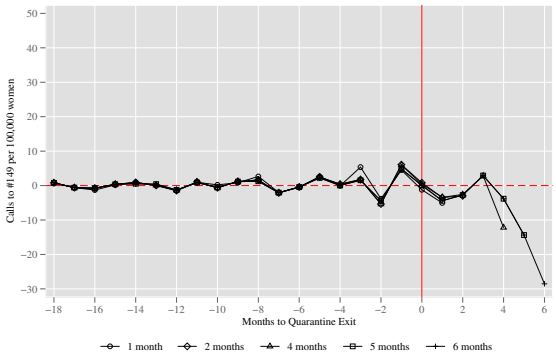
(f) Female Formal Employment Rate (Exit)

Notes: Refer to Notes to Figure A5. All details are identical, however here plots for each mechanism variable are displayed. In the case of Rambachan and Roth [2023]'s partially identified 'Honest DiD' bounds, darker shaded areas correspond to $M = \{0.05, 0.02, 0.02\}$ for mobility, male and female employment respectively, and lighter shaded areas correspond to $M = \{0.15, 0.10, 0.10\}$. In the case of shelter mobility only, weekly, rather than daily estimates are plotted, given computational demands on bounds when many lags are included.

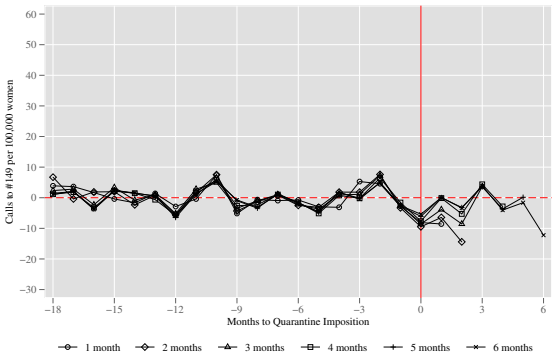
Figure A7: Heterogeneity in Lockdown Impacts by Lockdown Duration



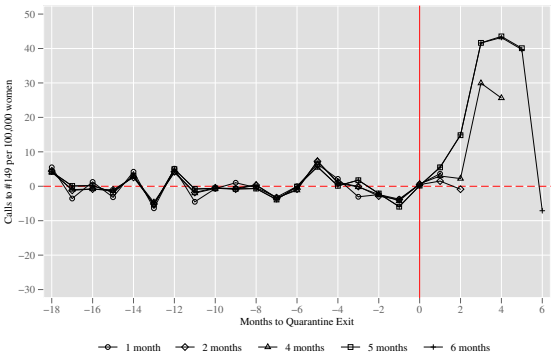
(a) Helpline Calls (Entry)



(b) Helpline Calls (Exit)



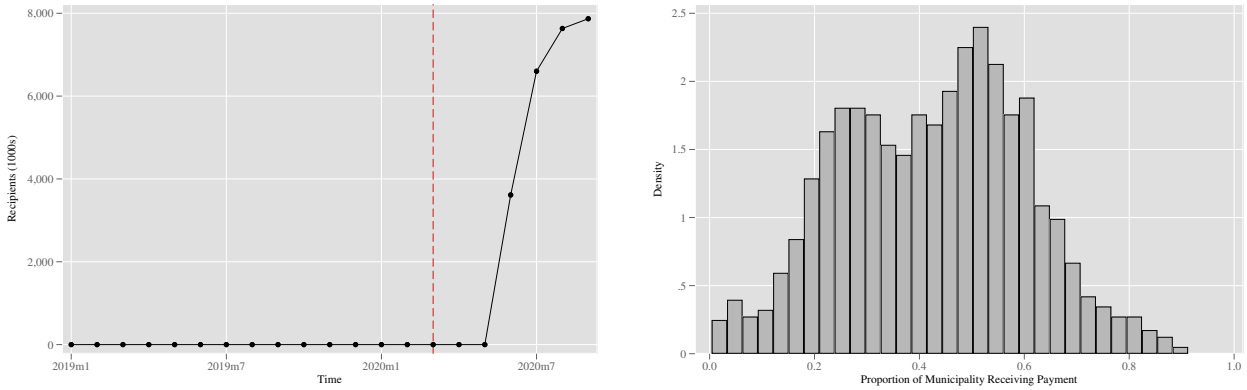
(c) Crime Reporting (Entry)



(d) Crime Reporting (Exit)

Notes: Refer to notes to Figure 2. Identical specifications are estimated however examining rates of calls to the Chilean police family support hotline (panels (a) and (b)), and formal criminal complaints (panels (c) and (d)) on different samples defined by maximum lockdown length: from 1 to 6 months (including municipalities with 0 months of lockdown). All other estimation details follow those indicated in notes to Figure 2.

Figure A8: IFE Stimulus Payments

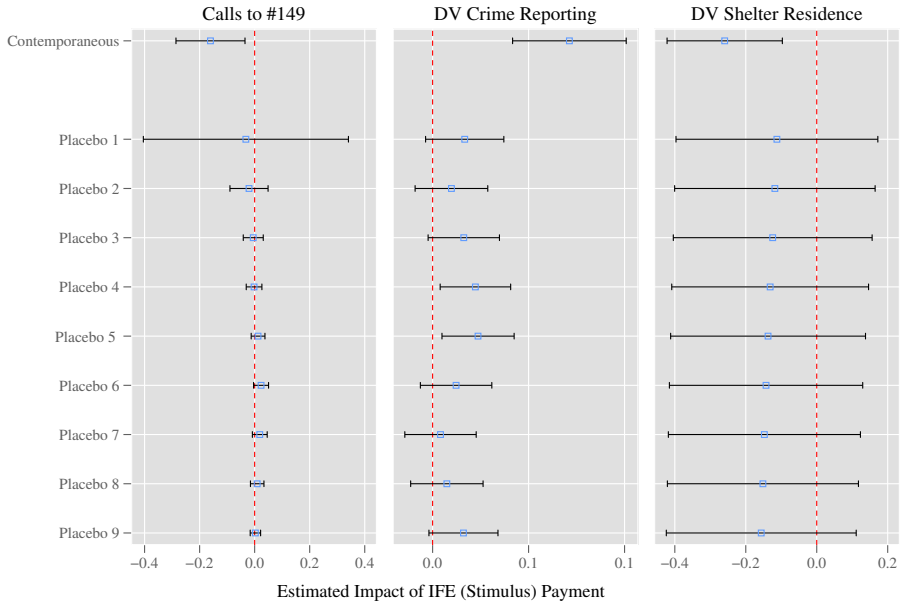


(a) Payments over Time

(b) Proportion of Recipients per Municipality

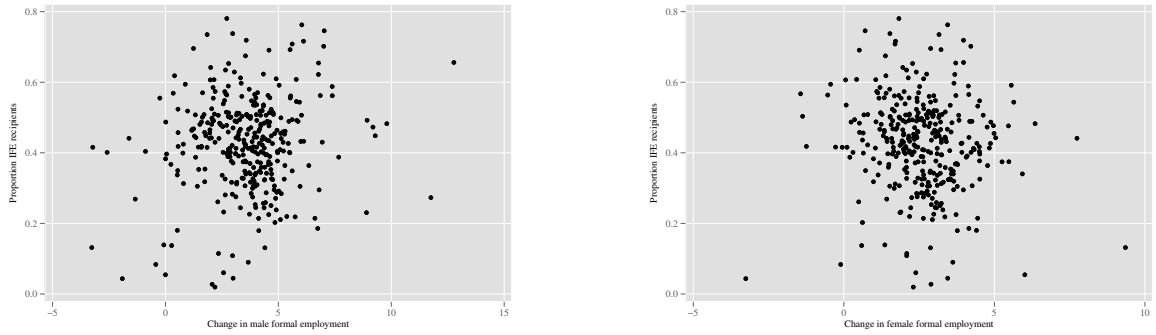
Notes: Panel (a) plots the temporal expansion of the number of IFE recipients nation-wide, based on administrative records provided by the Ministry of Social Development of Chile (Undersecretary for Social Evaluation). Panel (b) plots the average proportion of each municipality's residents receiving an IFE payment, calculated in the period following the introduction of the payment.

Figure A9: Placebo Tests – Stimulus Payment Impacts on DV Measures



Notes: Placebo tests consist of estimating two-way FE models with rates of IFE payments as the independent variable of interest on rates of domestic violence. In the case of 'Contemporaneous' models, this consists simply of a regression of rates of DV in time t on IFE (family emergency income payments) at time t, whereas placebos regress lagged rates of DV at time $t-k$ on IFE at time t. In the case of Calls and Crime, alternative placebos use lags $k \in \{10, 11, \ldots, 18\}$, such that in all cases DV outcomes are considered in months of 2019 or prior. In the case of shelters where daily data is used (starting from January 2020), lags $k \in \{60, 61, \ldots, 68\}$ are considered.

Figure A10: Stimulus Payment Coverage and Employment Losses



(a) Male Employment Change

(b) Female Employment Change

Notes: Plots for employment change and IFE recipients as share of population are displayed. Each dot is a municipality and values are calculated as averages for June-Sept 2020, which is the period when IFE was delivered. Employment change is defined as the average employment rate before COVID (Jan 2019-Feb 2020) minus the average employment rate after COVID (March-Sept 2020).

Table A1: Negative Weights in Two-way Fixed Effect Models

	Number g,t	Number Negative g,t	Total Sum of Negative Weights
Lockdown Entry			
Helpline Calls	334	15	-0.0084
Police Reporting	334	7	-0.0019
Shelters	1844	225	-0.0906
Lockdown Exit			
Helpline Calls	130	0	—
Police Reporting	130	0	—
Shelters	132	0	—

Notes: The number of group×time specific estimates are displayed, as well as the number of units receiving negative weights following de Chaisemartin and D'Haultfœuille [2020]. Each g,t group refers to treatment effects in municipalities which adopted at a specific time, in a specific post treatment year t. A full decomposition of the two-way fixed effect estimator for lockdown entry in models based on municipal data is provided in Figure A4.

Table A2: Proportion of Municipalities and Population Driving Estimates

	Lockdown Imposition		Lockdown Exit	
Time Period	Municipalities	Population	Municipalities	Population
0	128	16,568,433	43	5,808,802
1	91	14,076,074	43	5,808,802
2	59	10,831,426	18	1,978,723
3	41	7,571,212	9	694,585
4	27	5,094,053	9	694,585
5	7	1,707,023	–	–

Notes: DID_M dynamic (post-lockdown) effects are based on municipalities which have entered lockdowns, and been in lockdown for at least x months (or exited lockdowns, and been out of lockdowns for x months in the case of lockdown exit), where x refers to the time period indicated on the horizontal axis of the DID_M result graph. As lockdown length varies by municipality, estimates of varying length are driven by different sub-groups of municipalities. The sub-groups having had at least 0-6 months of lockdown are indicated in the left-hand panel, and the subgroups having had at least 0-6 months out of lockdown are indicated on the right-hand panel.

Table A3: DID_M Estimates of Lockdown Imposition and Removal on Helpline Calls by Type of Violence

	All Calls		Physical Violence		Psychological Violence		Economic Violence	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lockdown Imposed	5.841***	5.012**	3.977***	2.305*	1.768	2.542**	0.096**	0.164***
	(1.888)	(2.273)	(1.143)	(1.200)	(1.110)	(1.222)	(0.039)	(0.058)
Lockdown Removed	-4.390**	-2.089	-2.383***	-1.214	-2.009	-0.907	0.002	0.032
	(1.791)	(1.686)	(0.806)	(1.255)	(1.441)	(1.240)	(0.042)	(0.054)
Baseline Mean	5.350	5.350	2.002	2.002	3.326	3.326	0.021	0.021
COVID-19 Controls		Y		Y		Y		Y

Notes: Each column displays aggregate DID_M estimates of impacts of lockdown imposition and removal on DV helpline calls by type of violence as classified by police telephone operators. Columns (1)-(2) replicate results using all calls (as in Figure 2), while columns 3-8 break this down by each violence type. Aggregate DID_M estimates are presented weighting by the number of affected individuals at each lag. Standard errors are estimated using a blocked bootstrap clustered by municipality. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table A4: Two-Way FE Models: DV and Mechanism Measures

	DV Measures			Mechanisms		
	Calls	Crime	Shelters	Mobility	Male Emp	Female Emp
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: No COVID Controls						
Lockdown Imposed	12.80***	-11.32***	0.0594	-0.567***	-0.397***	-0.00405
	(1.631)	(1.755)	(0.149)	(0.0925)	(0.113)	(0.0782)
Lockdown Removed	2.833	0.821	0.212	-0.0183	-0.162	-0.237
	(1.818)	(2.657)	(0.457)	(0.0853)	(0.201)	(0.171)
Observations	7,245	7,245	4,576	74,992	7,245	7,245
Panel B: COVID Controls						
Lockdown Imposed	8.627***	-6.072***	0.154	-0.599***	-0.318***	0.0639
	(1.268)	(1.737)	(0.166)	(0.0949)	(0.120)	(0.0922)
Lockdown Removed	1.722	-0.221	0.280	-0.0281	0.0738	-0.117
	(1.401)	(2.610)	(0.468)	(0.0824)	(0.209)	(0.177)
Diagnosed COVID Cases per 1,000 People	0.714***	-1.163***	-0.508	0.260**	-0.0243	0.00300
	(0.144)	(0.151)	(0.335)	(0.101)	(0.0160)	(0.0139)
PCR Tests per 1,000 People	-0.123***	0.0197	-0.0856	-0.0308	0.0262***	0.00643
	(0.0431)	(0.0614)	(0.145)	(0.0454)	(0.00548)	(0.00446)
Population (1,000s)	0.695***	-0.233			-0.0516***	-0.0401***
	(0.205)	(0.177)			(0.0164)	(0.0115)
Observations	7,245	7,245	4,528	74,992	7,245	7,245
Mean of Dep. Var. (baseline)	5.350	92.577	3.049	2.862	34.336	21.413
Mean of Indep. Var. (post-COVID)						
Diagnosed cases	2.614	2.614	0.109	0.086	2.614	2.614
PCR tests/1000	21.105	21.105	0.921	0.685	21.105	21.105

Notes: Each column displays a two-way fixed effect regression of the impact of lockdown on domestic violence measures (columns 1-3) or mechanism variables (columns 4-6). In columns 1-3, all measures are cast per 100,000 population. Column 4 refers to average trips within municipality per day, columns 5-6 are cast per 100,000 working age population. Lockdown Imposed is a binary variable taking the value of 1 only when municipalities are under lock-down, and 0 pre or post-lockdown. Lockdown removed switches to 1 post-lockdown in areas where lockdown has been imposed and then removed. Population is not included in columns 3-4 given that these are day by municipality measures for 2020 only. Fixed effects for area and time are consistently included, and standard areas are clustered by geographic unit. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table A5: Stimulus Payments, Lockdown and Domestic Violence (no COVID controls)

	Calls to 149		Crime Reporting		Shelter Residence	
	(1)	(2)	(3)	(4)	(5)	(6)
Lockdown	11.38***	14.76***	-10.12***	-10.40***	0.0682	0.155
	(1.462)	(2.341)	(1.851)	(2.410)	(0.134)	(0.153)
IFE	-9.359	-7.172	15.12***	14.94***	-3.081***	-3.021***
	(6.896)	(6.300)	(5.618)	(5.592)	(0.963)	(0.993)
Lockdown× IFE		-17.08**		1.410		-0.348
		(7.122)		(6.920)		(0.408)
IFE+						
Lockdown x IFE		-24.252*		16.351*		-3.369***
		(12.437)		(8.738)		(0.929)
Observations	5520	5520	5520	5520	4576	4576
R-squared	0.770	0.773	0.617	0.617	0.858	0.858

Notes: Each column regresses measures of DV incidence or reporting on time-varying receipt of IFE stimulus payments, which began in May, 2020, and further rolled out to broader populations thereafter. IFE refers to the proportion of the municipal (columns 1-4) or regional (columns 5-6) population receiving transfers. Lockdown×IFE interacts each municipality's lockdown status with the proportion of IFE receipt. All regressions include geographic and time fixed effects and cluster standard errors by area. "Joint Estimate" refers to the linear combination of the parameters on IFE and Lockdown×IFE along with the standard error of this combination, reported in columns where interactions are considered. *** p<0.01, ** p<0.05, * p<0.1.

B The Shift Share Design

In this Appendix, we set out the identifying assumptions of the shift share design used in Section 4.3 of the paper, and provide supporting evidence. Equation 2 of the paper describes how domestic violence depends on employment in a municipal by month panel. In Section 4.3 we explain that we address endogeneity of employment using the Bartik instrument. The estimated equations for male and female employment are:

$$DV_{st} = \alpha + \beta_1 \text{Male Employment}_{st} + \beta_2 \text{Female Employment}_{st} + \mu_s + \phi_t + X'_{st}\Gamma + \varepsilon_{st}, \quad (3)$$

The first stage equations of the IV model are:

$$\text{Female Employment}_{st} = \pi_0^F + \pi_1^F B_{st}^{female} + \pi_2^F B_{st}^{male} + \mu_s^F + \phi_t^F + X'_{st}\xi^F + \eta_{st}^F \quad (4)$$

$$\text{Male Employment}_{st} = \pi_0^M + \pi_1^M B_{st}^{female} + \pi_2^M B_{st}^{male} + \mu_s^M + \phi_t^M + X'_{st}\xi^M + \eta_{st}^M, \quad (5)$$

where B_{st}^G represents the Bartik instrument for gender $G \in \{female, male\}$, and is defined as in the body of the paper:

$$B_{st}^G = \sum_{k=1}^{K} z_{s0k}^G \times g_{kt}^G,$$

with k indexing employment sectors, G indexing sex (female or male), z are the baseline industry shares at the municipality level, and g is the aggregate COVID shock.

Although COVID is a single aggregate shock, it will tend to have different impacts on job loss in different municipalities on account of their different baseline industry shares. For example, municipalities in which a greater proportion of individuals at baseline were working in construction, the arts and tourism, where COVID had a larger impact will have suffered greater job loss, see Figure C3. This will vary by gender given systematic differences in baseline industry-employment shares by gender.

The shift share IV achieves identification through the conditional exogeneity of the *shares* (baseline exposure) as laid out in Goldsmith-Pinkham et al. [2020], or the *shocks* (COVID), as laid out in Borusyak et al. [2022]. In our setting, we argue that identification is drawn from the shares: at baseline certain municipalities were more exposed to the shock due to baseline sectoral structure, but the COVID shock need not be exogenous. In practice, this implies that consistency does not require the number of industries to be large, but rather the number of municipalities to be large. As we lay out when discussing Assumption 2 below, a fundamental point is not that these baseline shares are independent of municipal-level unobservables, but rather that they are independent of first differences in municipal-level unobservables.
A concern is that even conditional upon municipal and time fixed effects, employment changes may be correlated with unobservable time-varying factors. In particular, municipalities with (endogenously) higher COVID infection (or testing) rates may have had increased domestic violence through stress, or migration, conditional on job loss rates.

Identifying Assumptions We first set out the identifying assumptions, and then discuss the evidence to support them.

Assumption 1. *Relevance – Each shift share instrument must be correlated with employment, and the system must not be under-identified.*

Goldsmith-Pinkham et al. [2020] note that the lower level sufficient condition for this to hold is that there must be an industry and time period when the industry share has predictive power for employment, conditional on controls, and that the growth rates cannot weight covariances between industry share and employment such that they exactly cancel out. We will document the first stage relationships below, and provide details related to under-identification and weak instrument tests.

Assumption 2. *Strict exogeneity – Conditional on included controls and fixed effects, baseline industry shares must be uncorrelated with the structural error term in the following sense:*

$$E[\varepsilon_{st} z^G_{lk0} | D_{st}] = 0 \text{ for all } k \text{ and } G \text{ where } g^G_k \neq 0.$$

Here the notation D_{st} refers to observable controls and municipal and year FEs.

A key point here is that Assumption 2 does not imply that these shares are uncorrelated with municipal specific factors such as the skill distribution, rates of domestic violence at baseline, or other equilibrium conditions. This is because the assumption refers to exogeneity conditional on fixed effects and time-varying controls (refer to discussion in Goldsmith-Pinkham et al. [2020, p. 2598]). Municipal fixed effects will capture all time-invariant factors, and as such, we require

baseline shift shares to be uncorrelated with *changes* in the error term. Identification in this setting requires that the differential effect of higher exposure to a more COVID-impacted industry only impacts domestic violence through employment, and not through other (confounding) time-invariant channels.

The main potential concern in our setting relates to COVID-transmission. It is plausible that a higher concentrations of baseline industries such as construction or hospitality that were more vulnerable to COVID transmission could, through higher COVID infection rates, lead to more stress and, as a result more DV, violating the exclusion restriction. To address this concern, we consistently control for COVID transmission and testing as X_{st} in equation 2. This increases our faith in the conditional exogeneity assumption. It seems unlikely that there are other relevant time-varying factors correlated with baseline sector shares, especially in the relatively short time period of our analysis. Most relevant factors are likely sufficiently slow-moving that they will be captured by municipal fixed effects. We nevertheless proceed to conduct empirical tests designed to examine the plausibility of the identifying assumptions, following Goldsmith-Pinkham et al. [2020].

Testing the Plausibility of The Identifying Assumptions We can test for relevance and under-identification formally, by examining first stage regressions of employment on the shift share instrument. These are presented in Table B1. Here we observe clearly that the instruments for male and female employment are relevant and strong. As expected, baseline employment shares for women are most predictive for employment for women, and baseline employment shares for men are most relevant for explaining employment of men. As we have two instruments (the Bartik for men and for women), we are able to reject a null of under-identification based on the Sanderson and Windmeijer [2016] procedure. We are also able to reject the null of weak IV, based on the Cragg-Donald F-statistic reported in Table 1.

Table B1: Shift Share First Stage

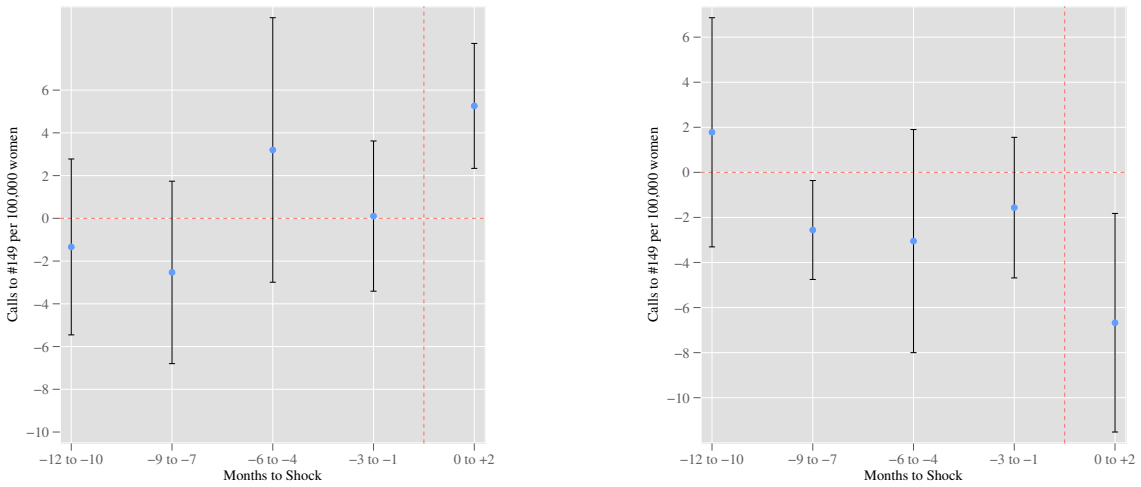
	Formal Employment	
	Women	Men
	(1)	(2)
Bartik Women	0.111***	0.000250
	(0.00872)	(0.00918)
Bartik Men	-0.0192	0.0763***
	(0.0167)	(0.0198)
Observations	5,520	5,520
Joint F-Test of First-stage	98.054	10.871
Weak IV (p-value)	<0.01	<0.01
Under-identification test F-statistic	163.044	78.601
Under-identified (p-value)	<0.01	<0.01

Notes: First stage estimates are reported on each endogenous variable. *** p<0.01, ** p<0.05, * p<0.1.

While we cannot formally test the strict exogeneity assumption given that it is an assumption

about unobservable terms, we can follow the literature in seeking to provide partial support for the strict exogeneity assumption by considering parallel pre-trends. We follow [Goldsmith-Pinkham et al., 2020, §VII.E] in implementing these tests. The logic of a parallel pre-trend test is that if the shift share instrument leverages identification based on a shock which is channelled through conditionally exogenous industry shares, then the IV should have no impact on the outcome of interest before the shock occurs. Figure B1 presents results from a series of regressions where we estimate the Bartik specification, but for bunched time periods before and after the shock. In each case, the outcome is the number of helpline calls per 100,000 women. For female and male employment, the figures make clear that there are no pre-trends, but that a significant impact of job loss on helpline calls emerges after the COVID shock, the timing of which is defined to be zero in event time.

Figure B1: Test of Parallel Pre-Trends in Shift Share Approach



(a) Female Job Loss (Bartik lags)

(b) Male Job Loss (Bartik lags)

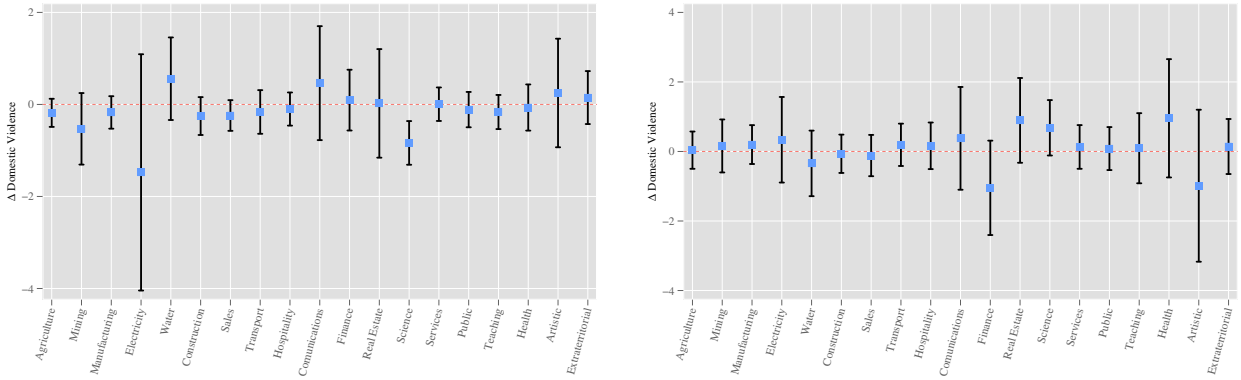
As an additional partial test of the assumption that baseline (pre-COVID) industry shares are exogenous in the 2SLS model using the Bartik instrument, we test whether these baseline shares are correlated with changes in domestic violence in the pre-COVID period. The Bartik IV rests on the premise that the baseline shares determine the intensity of exposure to an aggregate COVID shock which causes local DV. If this is right, we expect a correlation between industry employment shares and changes in DV after the shock but not in the pre-shock period. Figure B2 shows that the data satisfy this condition. Across 38 different industry shares, only one significant effect is observed.

Are There Interactive Effects Between Male and Female Job Loss? The main analysis, reflected in Equation 3, allows that male and female employment influences rates of DV, but assumes that these effects are independent of the level of each other. We now investigate a more general specification that allows multiplicative effects. Specifically, we estimate:

$$DV_{st} = \alpha + \beta_1 \text{Male Emp}_{st} + \beta_2 \text{Female Emp}_{st} + \beta_3 (\text{Male Emp} \times \text{Female Emp})_{st} + \mu_s + \phi_t + X'_{st}\Gamma + \varepsilon_{st},$$

We estimate this model using the shift share approach as before, with each of the 3 employment

Figure B2: Correlations Between Baseline (Pre-Covid) Industry Shares and Pre-Covid Δ Domestic Violence



(a) Women's Industry Shares

(b) Men's Industry Shares

Notes: Each plot presents point estimates and 95% confidence intervals from a regression of first differences (calculated by differencing month by month) in domestic violence (DV helpline calls) on municipal level employment shares for each municipality in 2019 (pre COVID-shock). A single regression is estimated in each sub-plot. Standard errors are clustered by municipality.

terms instrumented by their respective shift share IV. The results are in Table B2. When the outcome is crime reporting, there is no interaction between male and female employment. When the outcome is DV calls, there is a meaningful but small interaction. The increase in calls following male job loss is larger when women are also losing their jobs. This is consistent with our interpretation that the mechanisms triggered by male job loss are income-related stress and exposure. Looking at how the interaction modifies the decrease in calls following female job loss, we see that it is attenuated when men are also losing their jobs. This is consistent with our interpretation that the mechanism triggered by female job loss is that she becomes more tolerant of being victimized (her participation constraint is relaxed).

Table B2: Testing for Additional Interactive Employment Effects

	Calls				Crime
	All Calls	Economic	Physical	Psychological	Reporting
Formal Employment (Women)	3.017***	0.00155	1.680***	1.336***	2.948**
	(0.801)	(0.0195)	(0.472)	(0.395)	(1.187)
Formal Employment (Men)	-2.378***	0.00568	-1.447***	-0.936***	-1.171
	(0.707)	(0.0234)	(0.449)	(0.359)	(1.442)
Interaction (Women × Men)	-0.0249***	-0.000147**	-0.0124***	-0.0124***	-0.00191
	(0.00516)	(0.0000597)	(0.00251)	(0.00275)	(0.00476)
Observations	5,520	5,520	5,520	5,520	5,520
Mean Dep. Var.	5.311	0.020	1.970	3.321	91.169
First Stage F-Statistic (C.-D.)	303.3	303.3	303.3	303.3	303.3

Notes: See Table 1 in the main text. These are results for panel A generalized to allow that impacts of a change in the male employment rate depend upon the female employment rate, and vice versa.

C Data Appendix

Table C1: Data Definitions and Sources

Variable	Description	Frequency	Unit of analysis	Period covered	Source
Helpline calls to the Police DV hotline	Number of calls received by the domestic violence police line #149 made by women per 100,000 women. The calls are classified as owing to complaints related to physical violence, psychological violence, and economic violence.	Monthly	Municipality	Jan 2019 - Sept 2020	Carabineros de Chile
Crimes reported for DV	Domestic violence crimes known to the police with a woman victim. They include formal complaints made to the police, as well as in flagrante offences	Monthly	Municipality	Jan 2015 - Sept 2020	Ministry of Interior, Undersecretary of Crime Prevention
Residents in DV Shelters	Number of residents in government-run domestic violence shelters measured as the number of women who slept in a shelter the night before per 100,000 women	Daily	Region	Jan 1, 2020 - Nov 6, 2020	Ministry of Women and Gender Equality
Spots in DV Shelters	Number of official spots available in government-run domestic violence shelters per 100,000 women	Daily	Region	Jan 1, 2020 - Nov 6, 2020	Ministry of Women and Gender Equality
Lockdown entry	Dummy indicating a municipality is under lockdown	Daily	Municipality	Mar 14, 2020 - Sept 30, 2020	Ministry of Health, hand compiled by project RA.
Lockdown exit	Dummy indicating a municipality has exited lockdown	Daily	Municipality	Mar 14, 2020 - Sept 30, 2020	Ministry of Health, hand compiled by project RA.
COVID-19 testing rate	Number of COVID-19 PCRs over 1,000 inhabitants	Daily	Municipality	Mar 8, 2020 - Sept 30, 2020	Ministry of Science
COVID-19 infection rate	Number of confirmed COVID-19 cases over 1,000 inhabitants (7-day average)	Daily	Municipality	Mar 8, 2020 - Sept 30, 2020	Ministry of Science
Mobility	Number of trips within municipality measured by changes in cell phone connection towers	Daily	Municipality	Feb 26, 2020 - Sept 30, 2020	Cell phone metadata [Bravo and Ferres, 2020].
Employment	Number of employed individuals in the formal private sector over working age population	Monthly	Municipality	Jan 2019 - Sept 2020	Unemployment Insurance data managed by the Pensions Superintendence
Mean salary	Mean salary among employed individuals in the formal private sector	Monthly	Municipality	Jan 2019 - Sept 2020	Unemployment Insurance data managed by the Pensions Superintendence
Contract suspension rate	Number of jobs furloughed under the employment protection law over 100,000 pre-COVID jobs	Monthly	Municipality	April 2020 - Sept 2020	Pensions Superintendence
Jobs sectoral shares	Share of private sector workers under contract working in different economic sectors	Monthly	Municipality	Jan 2012 - Sept 2020	12% random sample of affiliated workers to the Unemployment Insurance, data managed by the Pensions Superintendence
Stimulus payment	Number of people receiving the IFE benefit over population		Municipality	May 2020 - Sept 2020	Ministry of Social Development and Family

We use three measures of domestic violence which capture incidence and reporting: calls received by the police domestic violence hotline, criminal cases of domestic violence, and use of shelters for victims of domestic violence. The data cover the whole country, with information either at the municipal or regional level.

Figure C1a plots the national trend for calls received by the police to the #149 domestic violence hotline. The figure shows a sharp increase of around 300% following the first outbreak of COVID-19, with the first lockdown measures soon after. Prior to March 2020 around 1,000-1,500 calls per month were received, with this quantity increasing to anywhere between around 2,000-5,000 calls per month after March 2020.

Figure C1b shows the national trend for formal criminal cases of domestic violence filed with the police. In this case, we observe daily data from January 1 2018 to 31 May 2020. We see that by mid-March 2020, the rate of DV criminal complaints differs from that of previous years, in particular, being lower than the rate observed in 2018 and 2019.

In the case of shelters, we observe data for each one of the publicly run women's shelters in the country, recording their number of residents, and available spots on each day between January 1, 2020 and November 6, 2020.[12] Not all municipalities have a shelter. In emergency situations, individuals are placed in a shelter in their region (which contains their municipality). The daily averages for rate of residence in DV shelters and total capacity for the period under study are summarized in Table C2, and national trends are plotted in Figures C1c and C1d, respectively.

We additionally have information from late February 2020 (before cases of COVID-19 were detected in Chile) recording movement within and between municipalities based on cellphone records. Baseline mobility within municipalities is approximately 3 trips per day (see Table C2), with trips measured as the average number of changes between cell phone connection towers from cell phone metadata.[13] Figure C2a displays mobility rates, showing a significant decrease in mobility since the start of the pandemic, that is more pronounced in municipalities subject to lockdown.

We have labor market data containing information on individual monthly work histories, along with sector of work, and whether workers suffered job loss. Figure C3 shows sectoral trends in formal employment for men and women, with the red line marking the first outbreak of the pandemic. We observe particularly sharp declines in construction, hospitality and artistic activities after March 2020. The construction sector employs a large share of the labor force, and mostly men. We supplement this information with data from a rolling labor market survey. Recognizing that the survey is not designed to be representative at the municipality level, we do not rely upon these estimates but provide them for descriptive purposes as they cover the entire population of formal and informal sector workers. Figure C1 shows increases in unemployment and declines in labor force participation rates for both men and women following the COVID-19 outbreak in March 2020. They reveal that, starting from a lower baseline level, unemployment rates of men increased more than rates for women. Starting from a lower base too, labor force participation rates of women fell more than rates for men.

Finally, we obtained information on the total beneficiaries of the stimulus payments distributed since May 2020. This information comes from the *Registro Social de Hogares*.[14] These payments

[12]For this variable we extended the period beyond September 30 in order to use all the administrative information available. This allows us to pick up more lockdown exits in our data and to have more variation for a variable that is defined at the regional level.

[13]See [Pappalardo et al., 2020] who also provide a description of the data.

[14]This household register, (RSH for its initials in Spanish), is an information system used by the Ministry

were targeted at those families most severely affected by the socioeconomic crisis brought on by the pandemic. Figure A8 shows the scope of stimulus payments up to September 2020. During this time, four payments were distributed.

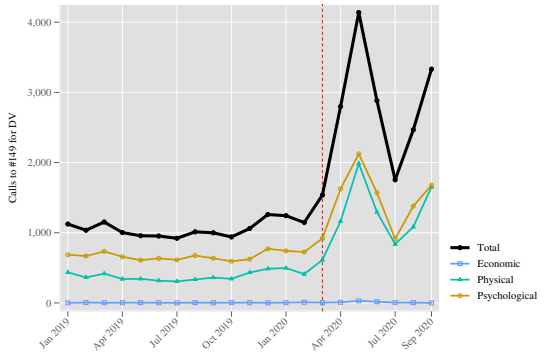
Table C2: Summary Statistics of Principal Measures

	Obs	Mean	Std. Dev.	Min.	Max.
Panel A: Helpline Calls					
All Calls to #149 per 100,000 Women	7245	8.15	14.99	0.0	325.7
Calls to #149 for Economic Violence per 100,000	7245	0.03	0.65	0.0	45.7
Calls to #149 for Physical Violence per 100,000	7245	3.33	9.24	0.0	325.7
Calls to #149 for Psychological Violence per 100,000	7245	4.78	9.05	0.0	200.0
Panel B: Criminal Complaints					
Complaints to Police for DV per 100,000 Women	7245	90.35	75.10	0.0	2985.1
Complaints to Police for DV per 100,000 Men	7245	22.77	24.70	0.0	392.2
Panel C: Women's Shelters					
Residents in DV Shelters per 100,000 Women	4576	3.21	2.43	0.0	13.7
Total Capacity of DV Shelters per 100,000 Women	4576	6.37	3.51	0.0	14.8
Panel D: Lockdown and Mobility Measures					
Quarantine Imposition	75428	0.11	0.32	0.0	1.0
Quarantine Exit	75428	0.04	0.21	0.0	1.0
Mobility within the Municipality per day	74992	2.33	1.82	0.0	16.1
Panel E: Covid Measures					
Diagnosed COVID cases per 1,000 people	7245	0.87	2.79	0.0	75.3
PCR tests per 1,000 people	7245	7.03	16.38	0.0	339.0
Panel F: Economic Measures					
Estimated Monthly Employment Rate (Women)	7245	20.53	9.55	0.0	147.1
Estimated Monthly Employment Rate (Men)	7245	33.21	11.99	0.0	122.5

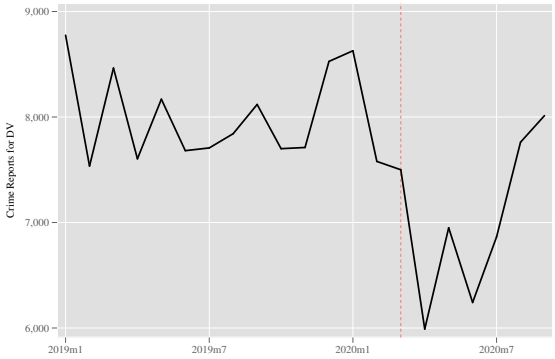
Notes: Summary statistics cover measures used in the paper at the same level used in analysis. For panels A, B, E and F this is by municipality and month. For panel C this is by region (which includes multiple municipalities) and day. For panel D, this is municipality by day.

of Social Development to assign a wide set of subsidies and social programs. It combines verified self-reported information with administrative records.

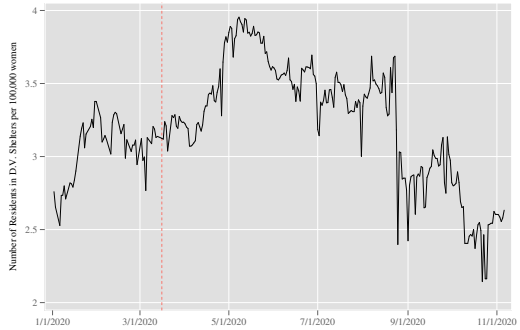
Figure C1: Descriptive Trends – Main Outcomes



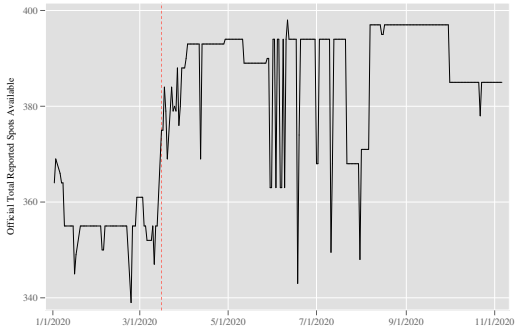
(a) Calls to #149



(b) Criminal Complaints for VIF



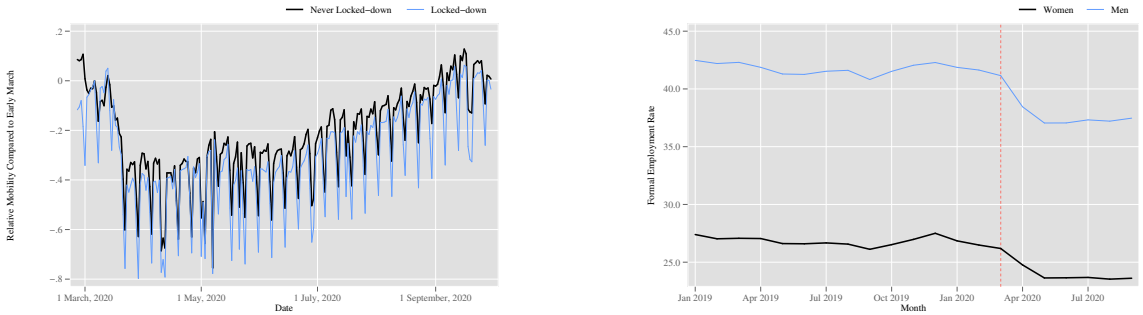
(c) Residents in Domestic Violence Shelters



(d) Spots in Domestic Violence Shelters

Notes: Panel (a) shows total aggregate number of calls in the country for each month between Jan 2019 - Sept 2020. The thick black line shows the total number of all calls related to DV received by the police. These are then sub-classified as related to psychological, physical and economic violence. Vertical red line indicates March 2020, the first month in which a lockdown was applied. Panel (b) shows rates of crime reporting for DV. These are plotted based on the number of reports received nation-wide each day. Grey lines depict rates for the first 5 months of years 2018 and 2019, while the red line depicts similar rates for 2020. In all analyses in the paper, month by municipality aggregates (over a longer period of time) are used. Panel (c) shows the average rate of residence in government run DV shelters, calculated as the total proportion of residents who slept in a shelter the previous night. This figure covers state-run women's shelters only. Panel (d) documents the total number of available spots in these state-run DV shelters according to administrative records.

Figure C2: Descriptive Trends – Mechanism Variables

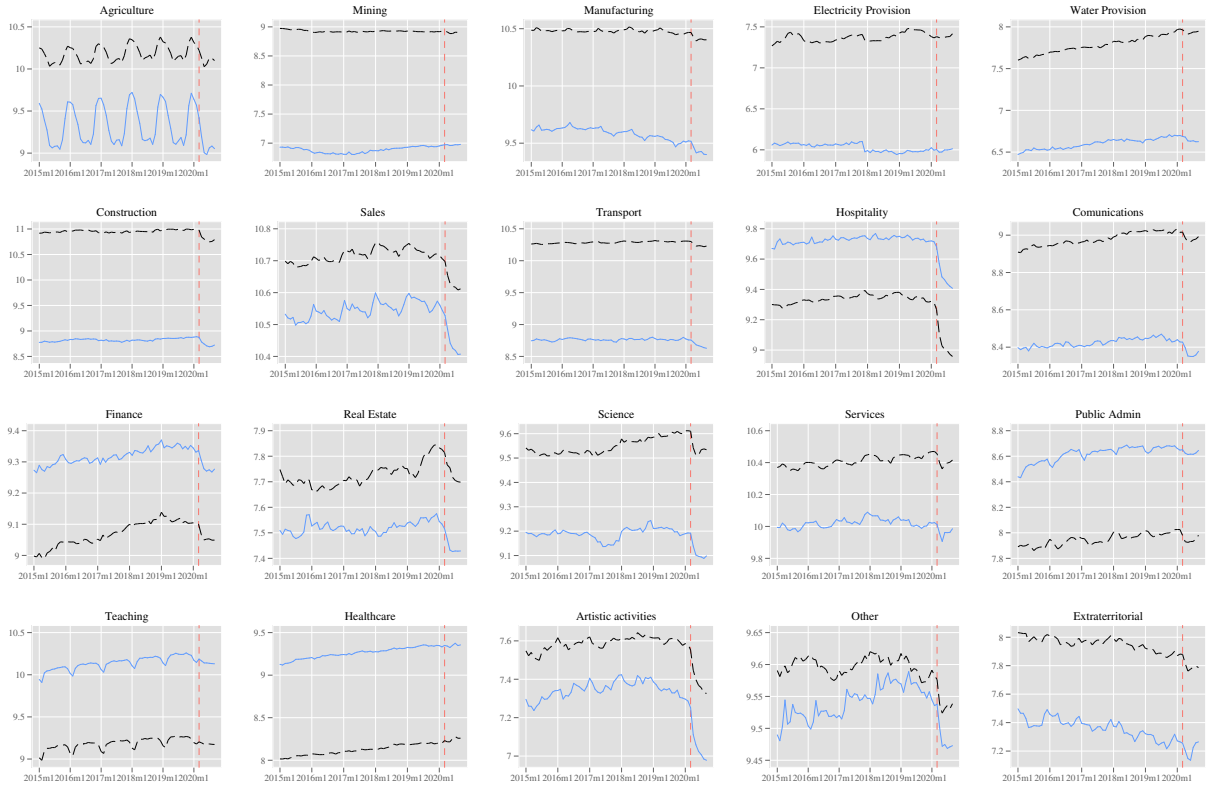


(a) Changes in Mobility by Lockdown Status

(b) Formal Employment Rates

Notes: Panel (a) presents daily changes in mobility (relative to early March) for municipalities that were locked-down compared to those that were never locked-down. Panel (b) presents monthly formal employment rates for men and women. The data for (b) comes from the 12% sample of unemployment insurance records covering the entire formal labour market.

Figure C3: Sectoral Trends in Employment by Gender (Total Employment)



Notes: Data from a 12% sample of the universe of formal private workers is displayed as reported in the country's unemployment insurance database. The log number of workers in each employment class is displayed. Dashed black lines represent men's emplyment, and solid blue lines represent women's employment. Plots showing absolute values are available in Bhalotra et al. [2021a].