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COVID-19 and The Rise of Intimate Partner Violence

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Abstract

Stay-at-home policies have been implemented worldwide to reduce the spread of the SARS-CoV-2 virus. However, there is a growing concern that such policies could increase violence against women. We find evidence in support of this critical concern. We focus on Peru, a country that imposed a strict nationwide lockdown starting in mid-March and where nearly 60% of women already experienced violence *before* COVID-19. Using administrative data on phone calls to the helpline for domestic violence (*Línea 100*), we find that the incidence rate of the calls increased by 48 percent between April and July 2020, with effects increasing over time. The rise in calls is found across all states and it is not driven by baseline characteristics, including previous prevalence of violence against women. These findings create the need to identify policies to mitigate the negative impact of stay-at-home orders on women's safety.

Keywords: Intimate partner violence, domestic violence, lockdowns, Peru, COVID-19.

♣ Contact information: Department of Economics and El Instituto, 365 Fairfield Way, Unit 1063, Storrs, CT 06269-106, USA. Email: jorge.aguero@uconn.edu. Phone: (+1) 860.486.0263. Fax: (+1) 860.486.4463. I would like to thank Patricia Ritter, Wilson Hernández Breña and seminar participants at Peru's *Ministerio de la Mujer y Poblaciones Vulnerables* and at the Group for the Analysis of Development (Grade) for valuable comments and suggestions to an earlier draft.

1. Introduction

Eliminating violence against women is not only a major public health issue (Krug et al., 2002; Bott et al., 2012) but also a key objective of the Sustainable Development Goals (United Nations, 2015). However, progress in this area could be stopped and even reversed by the onset of the SARS-CoV-2 virus. This research note provides one of the first systematic and nationwide analysis documenting the unintended consequences of nonpharmaceutical policies to decrease the spread of the virus on intimate partner violence.

Stay-at-home policies have been widely used to reduce the impact of the virus. It is estimated that at least three billion people around the world sheltered in place (Hall and Tucker, 2020) and 142 countries imposed some form of stay-at-home requirements (Hale et al, 2020). These policies have raised multiple concerns, especially for their impacts on developing countries and on gender equality.[i] Scholars and International Organizations have argued that stay-at-home policies would increase violence against women (e.g., van Gelder et al, 2020; Peterman et al, 2020; Bradbury-Jones and Isham, 2020; UNFPA, 2020). This argument is often based on recent scholarship suggesting that intimate partner violence increased during past epidemics (Roesch et al, 2020; Durevall, and Lindskog, 2015) but also with economic downturns (e.g., see Buller et al, 2018; Cools and Kotsadam, 2017 for low- and middle-income countries and Van der Berg and Tertilt, 2012 for advanced economies). This could be further exacerbated in developing countries where most homes lack sufficient space, which forces people to be in much closer proximity (Brown, Ravallion, and Van De Walle, 2020).

However, most of the current reporting about the increase on intimate partner violence (IPV) during the ongoing pandemic is anecdotal. In most cases, it relies on increases in reporting compared to same month the year before.[ii] Such analysis could be misleading. For instance, in

Peru, the country where we focus on this research note, calls to the national helpline for violence against women (*Línea 100*) increased by 33 percent in April 2020 with respect to April 2019. But so, did the volume of calls in January (26 percent), a time before Peru implemented its lockdown policies to combat COVID-19. Others have compared calls before and after March *within* 2020 alone. Yet, previous work in Peru has documented seasonal increases in IPV in years before the onset of the virus (Agüero, 2019). Thus, each of these approaches alone fails to provide unbiased estimates of the possible rise in intimate partner violence as an unintended consequence of the nonpharmaceutical policies to combat COVID-19

We use monthly data on the number of calls to the *Linea 100* by state from January 2007 to July 2020.[iii] To overcome prior limitations, I use two methodologies. First, I compare calls to the help line made before and after March but also *across* multiple years. This double comparison allows us to eliminate seasonal patterns as well as secular trends in calls to the helpline that could biased the estimates. This is done by including month, year and state fixed effects. The use of multiple years (2007-2020) also allows for state- and month-specific trends. Using this methodology, we find that calls to *Línea 100* increased its incidence rate by 1.48 times since Peru started its lockdown in Mid-March. The effects increase over time, with higher calls in July (2.12 times), June (1.72), May (1.58) than in April (1.02).

The assumption needed for this methodology is validated by our second approach: an event study restricting the sample to 2019 and 2020. This analysis confirms our main findings and show that they are not driven by distant years. Most importantly, it validates the main approach by showing a lack of a pre-trend between January 2019 and February 2020 and supports the parallel trend assumption.

An important finding comes from the robustness checks and heterogenous analysis. The rise in calls to the helpline is observed across the board. It is not driven by any specific state, including Lima, who has the largest call volume in any given year. Removing one state at a time from the analysis does not change the conclusions. Furthermore, we explore whether the increase varies by background characteristics at the state level measured in 2007, the first full year of operation of the *Línea 100.* It does not. We consider education levels, access to health insurance, urban population, number of rooms in dwellings, access to public services and durable goods. The rise in calls is the same in states with high and low values (relative to the median) of these background characteristics. We also consider prior prevalence of physical and sexual intimate partner violence and continue to find the same effect in states with low and higher prevalence.

Focusing on calls to a helpline has many advantages with respect to other data sources during the ongoing pandemic. First, it is very well documented that police records are not reliable measures of violence against women (e.g., Palermo et al, 2014 and United States Bureau of Justice Statistics, 2017). Second, lockdown measures have severely limited the use of face-to-face interviews such as the widely use Demographic and Health Surveys. These surveys represent the main data source for intimate partner violence in many developing countries. However, in many countries, these surveys are currently not been implemented. In countries where they are conducted, the data will not be available until next year. Third, for the handful of countries with a well-established network of shelters for women, including Peru (e.g., Kavanaugh et. al., 2018), workers in these centers have not been declared as essential, so these centers were not operating during the first months after the lockdown. Thus, calls to *Línea 100* helpline provide the best available data to measure violence against women during the pandemic.

Finally, the national coverage from the *Línea 100* offers an important advantaged relative to recent papers, increasing the external validity of our study.[iv]

2. Peru's stay-at-home policies and intimate partner violence before COVID-19

On March 15, 2020, Peru's government announced that a severe *nationwide* lockdown would be implemented, effectively, the next day.[v] The first positive test of COVID-19 was detected on March 6 and the first death was confirmed on March 19 after the lockdown had started. Peru's was one of the earliest coronavirus lockdowns in the region. The lockdown suppressed constitutional rights on free mobility around the country. Individuals must stay at home and a curfew was implemented. The lockdown was originally scheduled for two weeks, but it has been renewed many times. In May, the government started first of four re-opening phases. For example, for the first time, it allowed only certain restaurants to offer on-site pick-up and home delivery services. But stay-at-home orders continued nationwide.

In June and July, the government started phases two and three of the re-opening, allowing every time more sectors of the economy to open. It also started *focalized* lockdowns, mandating sheltering-in-place in some states but not others based on the prevalence of COVID-19 cases (DS 116-2020-PCM). However, the curfew continued nationwide (from 10PM to 4AM). At the end of July, the government renewed the focalized lockdown orders until August 31st (DS-135-2020-PCM). Throughout this time schools never opened. The school calendar runs from March to December and the timing of the pandemic prevented the start of the school year, forcing millions of students to remain at home.

Peru is also a country with high levels of intimate partner violence. The 2019 Peruvian DHS, the latest available, shows that 58 percent of women aged 15 to 49 had experienced violence by their current or last partner. This figure has shown an important decline over the past ten years. In 2009, the rate was 77 percent. Understanding whether policies that seek to control a major pandemic lead to unintended negative consequences for women's safety is an important policy question. This is even more salient for developing countries, such as Peru, where the slow but consistent reductions in violence over the past ten years could be quickly reversed by the responses to COVID-19. The data and models to estimate the change in violence against women during the pandemic is described in next section.

3. Data and methods

The main variable for our analysis is the number of calls to the helpline *Línea 100* adjusted by population size. This helpline was created in late 2006. Dialing 100 from any phone (landline or mobile) is free and connects the caller to a trained operator who records the call and if needed, refers the caller to the women shelters (*Centros de Emergencia Mujer*) located near the caller's location. However, from mid-March to the end of June, these centers were closed severely limiting the services provided to callers. For our analysis we use data at the month and state level on the volumes of calls per 100,000 people. Information on calls is publicly available on the website of the Ministry of Women and Vulnerable Populations (*Ministerio de la Mujer y Poblaciones Vulnerables*), the government entity in charge of delineating the policy to reduce violence against women. This dataset is updated monthly and the most recent release, including calls during the month of July 2020, was made available at the end of August. At this frequency and aggregation level (by state), the data are available since January of 2007.[vi] This leads to 4,075 state-month-year observations (25 states, 13 full years plus 7 months in 2020). Microlevel data for the calls during 2020 will not be available until the second quarter of 2021 at best. Thus, given the urgency of identifying a rise in calls during the pandemic, we opted to use the aggregated data. A possible drawback is that the aggregation would reduce the variance in the sample and would prevent us from finding statistically significant effects. As shown in the next section, this is not the case suggesting that delaying the analysis until the publication of the microdata would have prevented us from documenting the increase while it was happening.[vii]

To test for the increase in calls to the helpline during the pandemic, we use a Poisson count model as follow:

$$Calls_{imt} = exp(\beta Post_m * Y2020_t + \alpha_m + \alpha_t + \alpha_i + \alpha_{it} + \alpha_{mt}) \quad (1)$$

where the variable $Calls_{imt}$ represents the expected number of calls to the *Línea 100* in state i, in month m and year t. The variable $Post_m$ is binary and takes the value of one for months from March onwards and zero for January and February in any given year. $Y2020_t$ is a binary indicator for the year 2020. Thus, $Post_m$*$Y2020_t$ is the interaction of interest. Our identification strategy compares calls to *Línea 100* before and after March across multiple years. Therefore, Equation (1) includes month fixed effects (α_m) as well as year fixed effects (α_t). Parameter β captures this double comparison across months and years. Values bigger than one (for the exponentiated coefficients) indicate an increase in the rate of incidence of the calls during the nonpharmaceutical policies to combat COVID-19. As explained by Cameron and Trivedi (2013), an advantage of a Poisson specification is that fixed effects can be included without creating an incidental parameters problem. This is particularly important as some states, earlier in the sample, have relatively low counts by month. A second advantage is the consistency of the maximum likelihood estimates of the parameters, which does not require that the arrival process for calls to the *Línea 100* is actually Poisson (Cameron and Trivedi, 1986).

This specification helps us avoid the bias due to seasonal effects in calls to the helpline documented by Agüero (2019) by including month fixed effects (α_m). Also, as shown in Appendix Figure A2 and Table A1, the number of calls has grown over time across all states. In 2007, the first full year of operation of the *Línea 100*, an average of 25 calls per 100,000 people per month were registered nationwide. In 2019, that number jumped to 565 and in the first seven months of 2020, the national average is 1,020. To incorporate this secular increase, the model includes year fixed effects (α_t). The model also accounts for time-invariant as well as time-variant both observed and unobserved factors at the state level (e.g., cultural differences, gender norms, socioeconomic status, altitude and climate) as we added state fixed effects (α_i) and state-specific trends (α_{it}). Furthermore, we include month-specific trends to account for secular trends at the month level.

We compute robust standard errors clustered at the state-year level (350 clusters) and our findings are robust if using alternative clusters and constructions of the standard errors. For instance, we consider clustering by state-month without altering our findings. Using the Newey-West HAC correction yields smaller standard errors, which would imply larger t-statistics, hence, we opted for a more conservative approach. All these additional results are available upon request.

Figure 1 provides a graphical preview of our main findings. We plot the number of calls (averaged across all states) by month in 2020 (red) and 2019 (blue), together with the 95% confidence intervals of the mean (shaded areas). Applied to this sample, the identification strategy in Equation (1) compares calls made in 2020 before and after March (indicated by the vertical dashed line) and contrasts it with the difference in calls also before and after March 2019. It is easy to see that volume of calls after March 2020 is larger than those in the first two months of the year. This gap is even larger than the analog difference within 2019. Thus, the observe volume of calls to *Línea 100* since the lockdown policies started exceeds what would have been predicted based on the pattern of early 2020 and in the first seven months of 2019. It is this counterfactual that allows us to estimate the impact on calls to the helpline during the pandemic.

[Figure 1 around here]

The Figure provides two additional elements. First, it serves as a visual validation of the parallel trend assumption for our empirical strategy. The pre-March trend is undistinguishable in 2019 compared to 2020 and the confidence intervals further support this claim. Second, it also shows that, unlike Equation (1), there is evidence suggesting that the effect varies by month post-March 2020. Equation (2) addresses this issue and provides a more flexible specification:

$$Calls_{imt} = \exp\left(\sum_{j=\left\{\substack{Mar,Apr,\\ May,Jun,Jul}\right\}} \left(\theta_j 1(Month_m = j) * Y2020_t\right) + \alpha_m + \alpha_t + \alpha_i + \alpha_{it} + \alpha_{mt} \right) \quad (2)$$

In particular, we replace the variable $Post_m$ with month-specific binary variables from March to July who now interact with the $Y2020_t$ indicator. In this specification we allow the effect to vary by month relaxing the assumption of equality across all post-March 2020 parameters as in Equation (1). In the new equation, we continue to control for all the previous fixed effects and trends.

To further strengthen the validity of our identification strategy we conduct an event study. We focus on 2019 and 2020 and estimate one parameter for each month from January

2019 to July 2020, leaving February 2020 as the omitted category. This model continues to include fixed effects by state as well as state-specific trends. Month and year fixed effects are not included due to redundancy. Equation (3) presents this model formally:

$$Calls_{imt} = \exp\left(\sum_{\tau=\{Jan19,\ldots,Dec19,Jan20,\ldots Jul20\}-\{Feb20\}} \delta_\tau 1(Month_t = \tau) \quad + \alpha_i + \alpha_{it}\right) \quad (3)$$

The event time coefficients for δ_τ with τ={Mar20,…,July20} capture the dynamic effects starting in March 2020. All previous parameters, δ_τ for τ={Jan19,…,Dec19,Jan20}, capture pre-trends, i.e., trends on calls to *Línea 100* before the pandemic. As such, the pre-trends will allow us to test for the parallel trend assumption. In all cases, the parameters measure differences with respect to February 2020, the omitted category. The results of applying these methods are discussed in the next section.

4. Results

Table 1, Panel A, reports the estimation of Equation (1) with the exponentiated coefficients, assuming that the effect is the same for all months since March 2020. We start with a simpler model ignoring state- and month-specific trends (column 1). In this case, the incident rate increases by 1.61 times since March 2020. This is statistically significant at the 1% level. Expanding the model by including state-specific trends (column 2) and all trends (column 3), continues to show an excess of calls since March 2020 of around 1.48 times.

[Table 1 around here]

We expand this model to be consistent with the observed pattern in Figure 1. Panel B of Table 1 reports estimates of Equation (2) where we allow for month-specific effects. In all specifications, we find an acceleration in the rise of calls to *Línea 100*, with larger effects for each new month.

To validate our main specification, we test for parallel trends before March in 2020 and 2019. Figure 2 plots the (exponentiated) parameters from the even study described in Equation (3). Relative the February 2020, the omitted category, the pre-trend coefficients help validate our identification assumption. There is no clear trend in volume of calls between January 2019 and January 2020. Indeed, we cannot reject the null hypothesis that the calls in January 2020 are the same as those a year before (p-value=0.734). Similarly, February 2019 does not differ in the number of calls compared to February 2020. Furthermore, this analysis also documents that the rise in calls to *Línea 100* starts in April and accelerates in the following months. This further confirms our previous conclusion and validates our main methodology.

[Figure 2 around here]

We conducted several robustness checks. First, our findings are robust to measuring calls to the helpline without adjusting for population (Appendix Table A2). Second, they are also robust to the use a linear regression (adjusted for population) instead of a Poisson model (Appendix Table A3). Third, we test whether the findings are sensitive to the years selected to construct the counterfactual. In Appendix Figure 3, we show that reducing the sample period one year at a time does not alter our conclusion. In particular, we removed a year from the beginning until we are left with the minimum number of years to estimate state- and month-specific trends. In these tests we maintained our preferred specification, which includes state, month and year fixed effects in addition to the aforementioned specific trends. The robustness of the estimates is found when considering the restricted model (Equation 1) or the more flexible specification (Equation 2), shown in Panels A and B of Appendix Figure 3, respectively.

We also explore whether the results are driven by a specific state. In Appendix Figure 4 we remove one state at a time from the analysis. Again, whether we considered the flexible model or the more restricted one, our results are not driven by any specific state. This is an important result. As described in section 2, Peru moved from an initial national lockdown to a focalized one by June. Yet, the pattern of an ever-increasing volume of calls over time does not depend on the states included in the sample.

To investigate this issue further we conduct an heterogenous analysis applied to Equation (2). Following Palermo et al (2004), we focus on variables measuring education, urbanization, health insurance coverage, durable goods and access to public services. We gather state-level data from the Population Census of 2007 and the Demographic and Health Survey (DHS) of the same year. We focus on 2007, the first full year of the *Línea 100*, as a way to capture background characteristics of the states. From the Census, we obtained data on the share of urban population living in the state as well as the average number of rooms per dwelling. These two variables try to capture population density as urban homes tend to be closer to each other and families will be forced to be closer together when there are fewer rooms. The DHS is a nationally representative household survey sampling women of reproductive age. It allows us to capture women-specific variables. In particular, we obtain the share of women with a high school degree or more and with health insurance. The DHS contains questions on intimate partner violence, and we focus on physical and sexual violence. We also create two indices capturing information about access to public services (i.e., electricity at home, piped water and sanitation) and ownership of durable goods (i.e., radio, television, phone, computer, bicycle, car, motorcycle, boat and refrigerator). These indices were created following Kling et al (2007). Finally, we use a broader welfare index encompassing all these variables. For each variable (or index) we split the sample by the median value and estimate Equation (2) separately for each subsample. The results of this exercise are displayed in Figure 3.

[Figure 3 around here]

We find that the pattern of increase calls to *Línea 100* since March 2020 is observed across all background characteristics. The surge is observed in states with high and low levels of education, density, health insurance coverage, access to public services and ownership of durable goods. It is also present in areas with high and low pre-determined prevalence of intimate partner violence.

Finally, we compile an additional dataset on calls to *Linea 100* by age of the victim using public data from Ministry of Women and Vulnerable Populations. Unfortunately, these data are available only at national level and not by state (but by month and year) so we cannot conduct the exact same analysis as before. In particular, we cannot longer account for all the controls of Equation (2). For instance, we cannot add state fixed effects or state-specific trends. We also are not able to include month-specific trends. With those caveats, we use this restricted dataset and show that for all age groups, calls to the helpline rise since March with larger effects in July relative to prior months (see Appendix Table A4).

All this evidence suggests that the rise in calls to the domestic violence helpline are broad and not limited to any of the large set of features considered in this study.

5. Discussion and conclusion

We provide the one of the first systematic analysis of the unintended effects of the efforts to control the pandemic caused by the SARS-CoV-2 virus in developing countries. We document an increase in calls to the helpline for violence against women in Peru after stay-at-home policies started in mid-March. Our estimates show a 48 percent increase since the pandemic, with effects increasing over time. This rise takes place in a country where already almost 60 percent of women experienced violence before the virus arrived. The results are not driven by any particular demographic group or background characteristics, even when considering pre-determined prevalence of domestic violence.

Given our findings, there is an urgent need to identify policies that could help mitigate the unintended effects of stay at home orders to combat COVID-19.[viii] For example, the work by Buller et al (2018) suggests that cash transfers could reduce intimate partner violence. This resonates with the findings by Leslie and Wilson (2020). They observed that the initial increase in calls to 911 about domestic violence in the United States was followed by a decline that coincides with checks sent out as part of the CARES Act in the middle of April. Some developing countries, including Peru, had provided cash transfers during the pandemic too. Understanding whether those transfers brought some relief from financial strain and reduced intimate partner violence is a pending research question.

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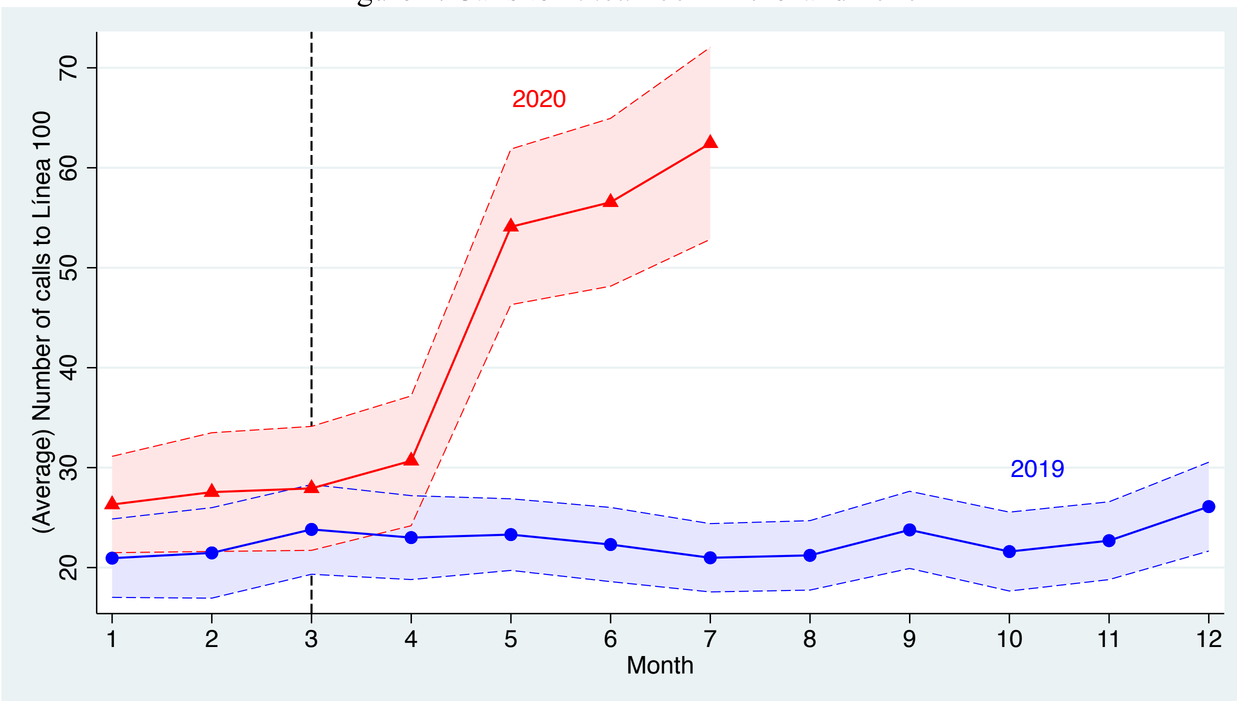
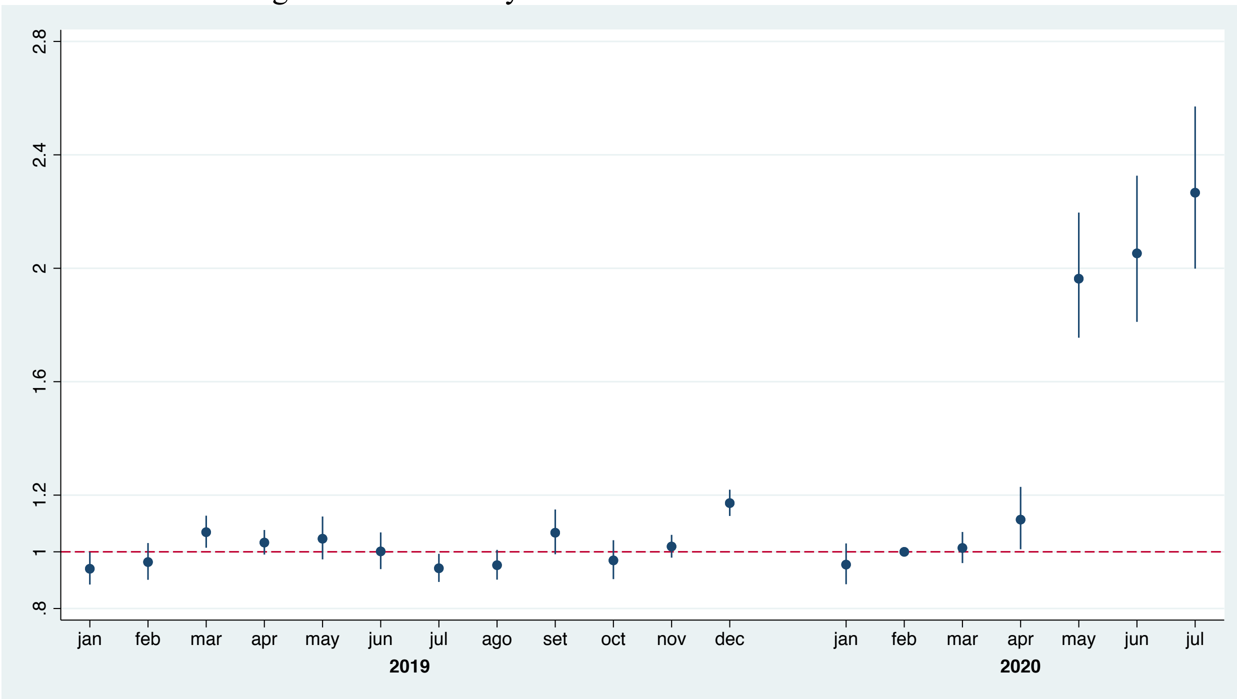
Figure 1. Calls to *Línea 100* in 2019 and 2020



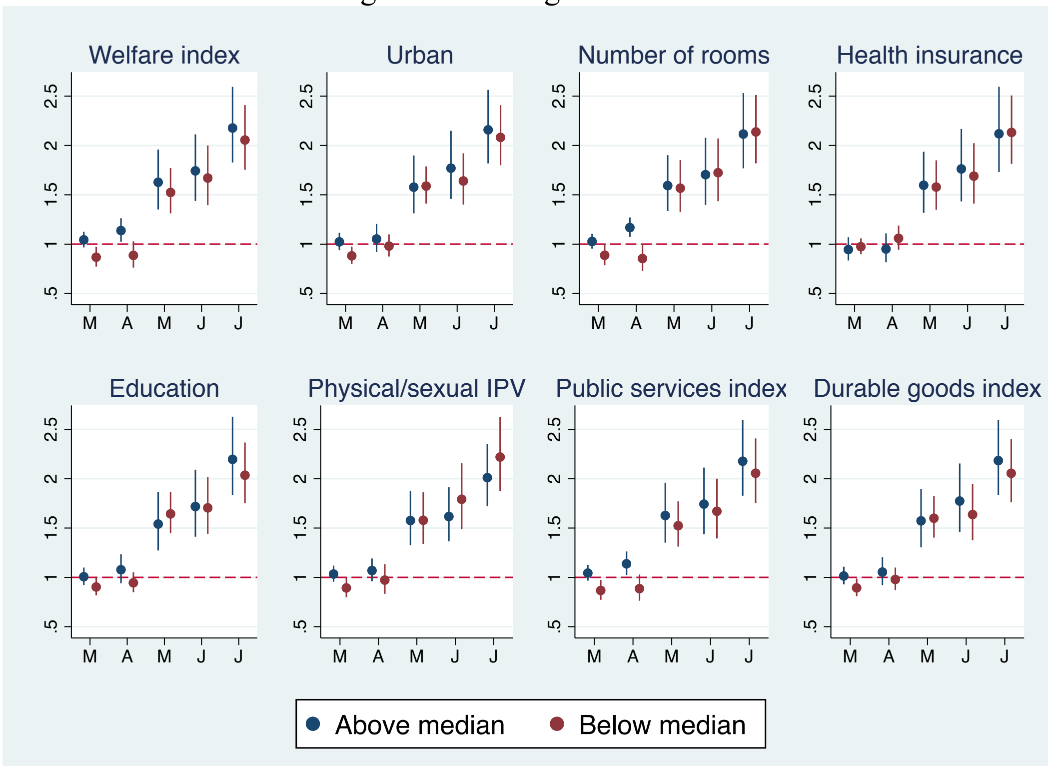
Note: Red and blue lines report the number of calls to the helpline *Línea 100* (averaged across all states) per 100,000 people for 2020 and 2019, respectively. Shaded areas represent the 95% confidence intervals. The (black) dashed vertical line represents the month when the national lockdown started.

Figure 2. Event study: Calls to *Línea 100* in 2019 and 2020



Note: Each circle shows the exponentiated coefficients of a Poisson regression for the number of calls per 100,000 people. The omitted category is the month of February 2020. Controls include state fixed-effects and state-specific trends. Confidence intervals at the 95% are shown by the vertical lines and are calculated using robust standard errors clustered at the state-year level.

Figure 3. Heterogenous effects



Note: Each circle shows the exponentiated coefficients for the months of March, April, May, June and July, respectively, interacted with Year2020 from a Poisson regression for the number of calls to *Línea 100* per 100,000 people. Within each figure, a separate regression is estimated for the sample above (blue) and below (red) the median of the baseline characteristic. Controls include those in column 3 of Table 1: year, state and month fixed effects in addition to state-specific and month-specific trends. Confidence intervals at the 95% are calculated using robust standard errors clustered at the state-year level.

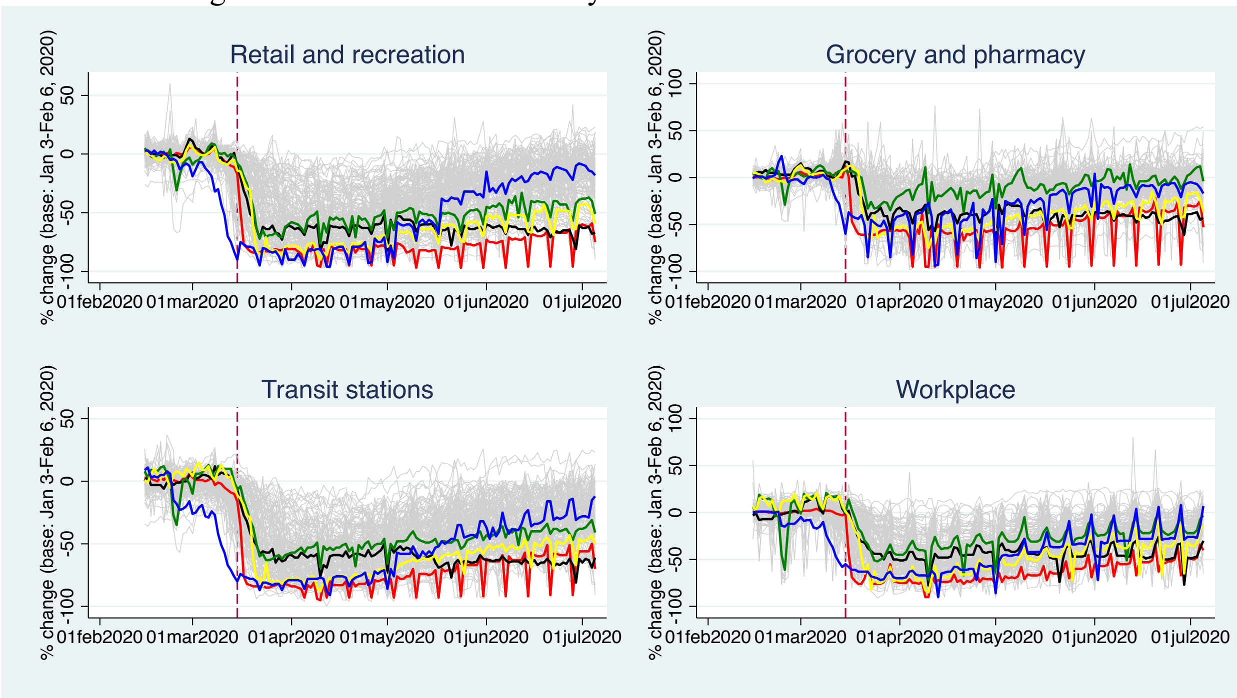
Table 1. Estimates of the calls to helpline during the pandemic (Poisson)

	Dependent variable		
	Number of calls to *Línea 100* per 100,00 people		
	(1)	(2)	(3)
	Panel A. Post period		
Post*Year2020	1.606***	1.606***	1.478***
	(0.072)	(0.066)	(0.073)
Pseudo R^2	0.638	0.647	0.651
Log-likelihood	-9220	-8991	-8876
	Panel B. Effects by month		
March*Year2020	0.960	0.960	0.965
	(0.026)	(0.026)	(0.034)
April*Year2020	1.099**	1.099**	1.023
	(0.047)	(0.045)	(0.049)
May*Year2020	1.839***	1.839***	1.582***
	(0.104)	(0.095)	(0.104)
June*Year2020	1.937***	1.937***	1.712***
	(0.121)	(0.115)	(0.125)
July*Year2020	2.181***	2.181***	2.124***
	(0.139)	(0.125)	(0.143)
Pseudo R^2	0.645	0.654	0.655
Log-likelihood	-9028	-8798	-8772
N	4075	4075	4075
Month fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
State fixed effects	Y	Y	Y
State-specific trends	N	Y	Y
Month-specific trends	N	N	Y

Note: Exponentiated coefficients. Robust standard errors, clustered at the state-year level in parentheses. See text for variables definitions and sources. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

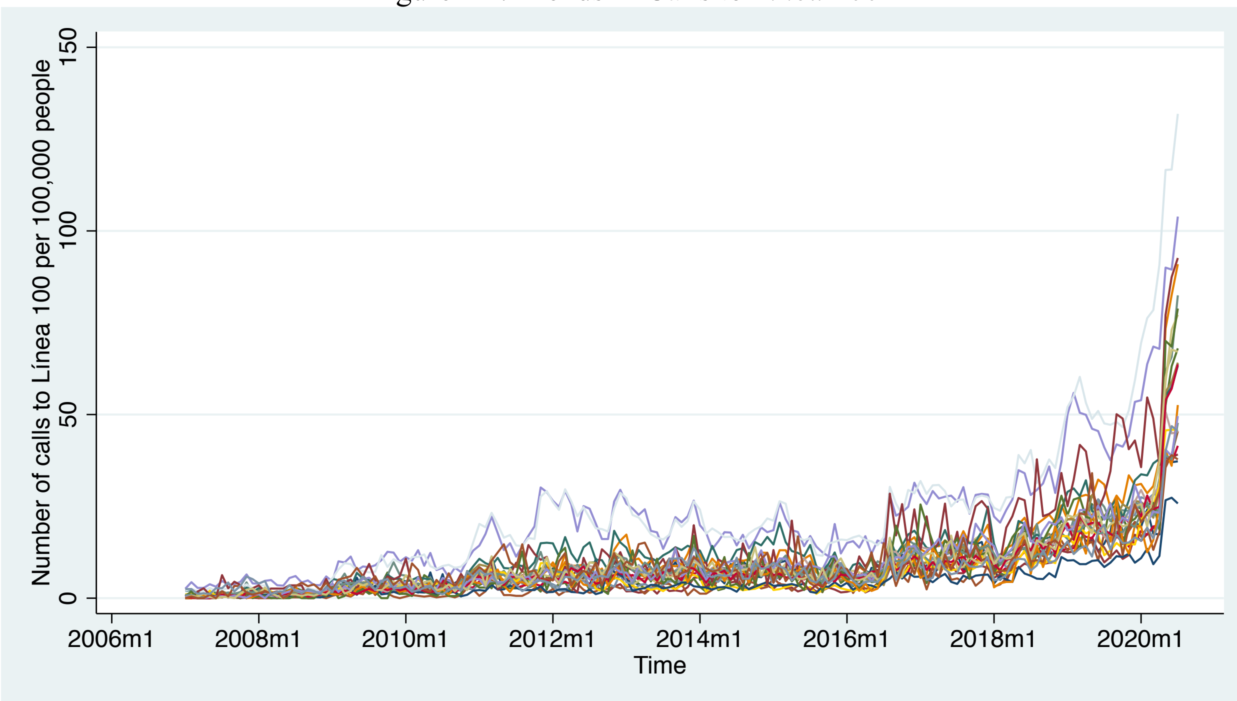
Online Appendix: Not for publication

Figure A1. Measures of mobility in Peru and selected other countries



Note: Author's calculation based on Google mobility data available on https://www.google.com/covid19/mobility/. Peru is marked by a red solid line. All other countries are marked in grey, except for Brazil (green), Chile (black), Colombia (yellow) and Italy (blue). The dashed vertical line refers to March 15, the day before Peru implemented its lockdown.

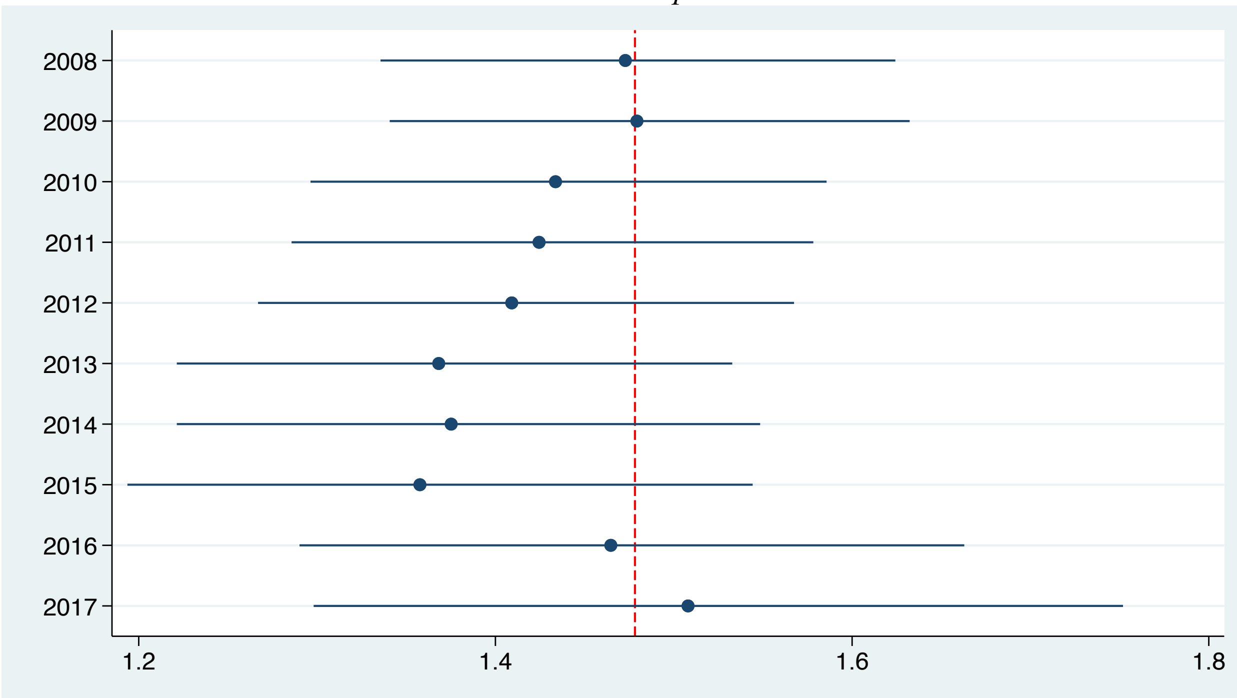
Figure A2. Trends in Calls to *Línea 100*



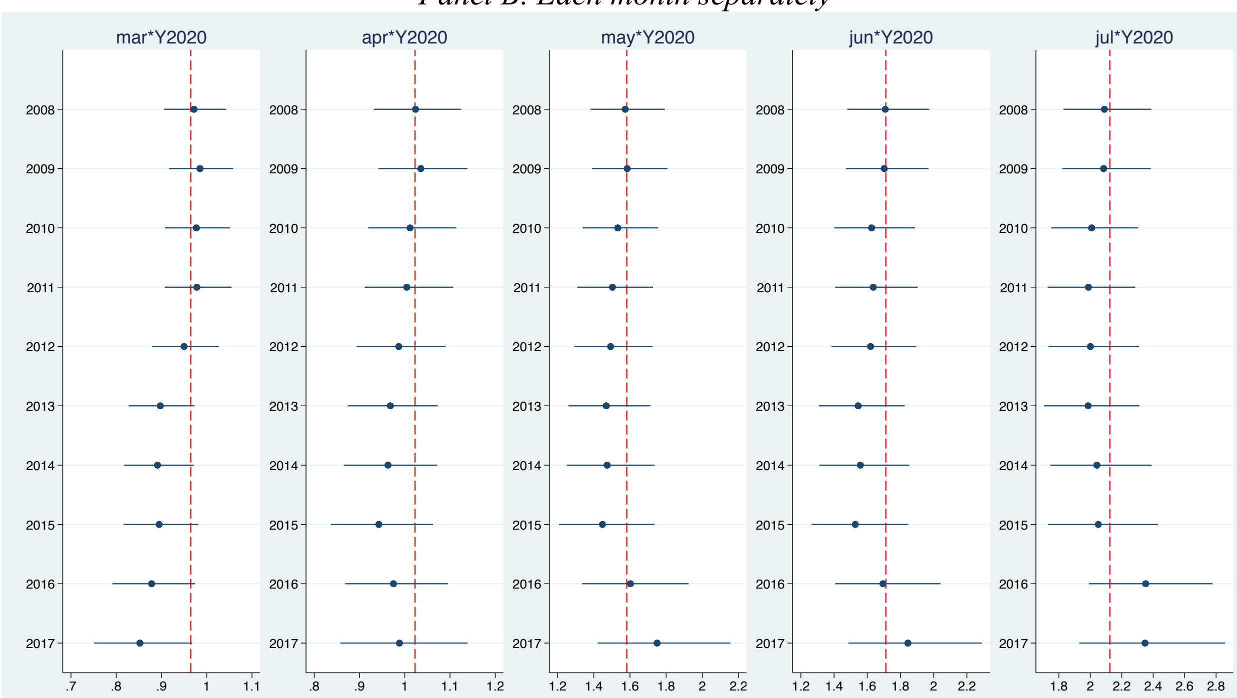
Note: Author’s calculation based on the data described in text. Each line represents a state.

Figure A3. Robustness: Changing the starting year

Panel A. Post period



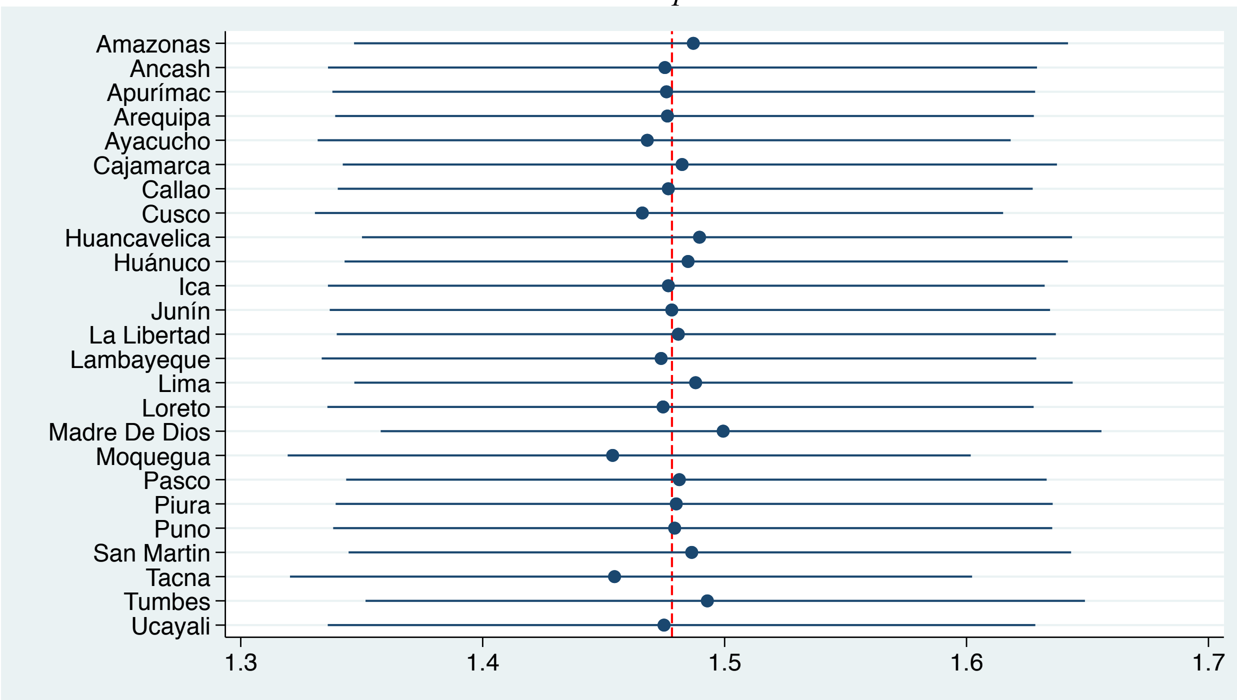
Panel B. Each month separately



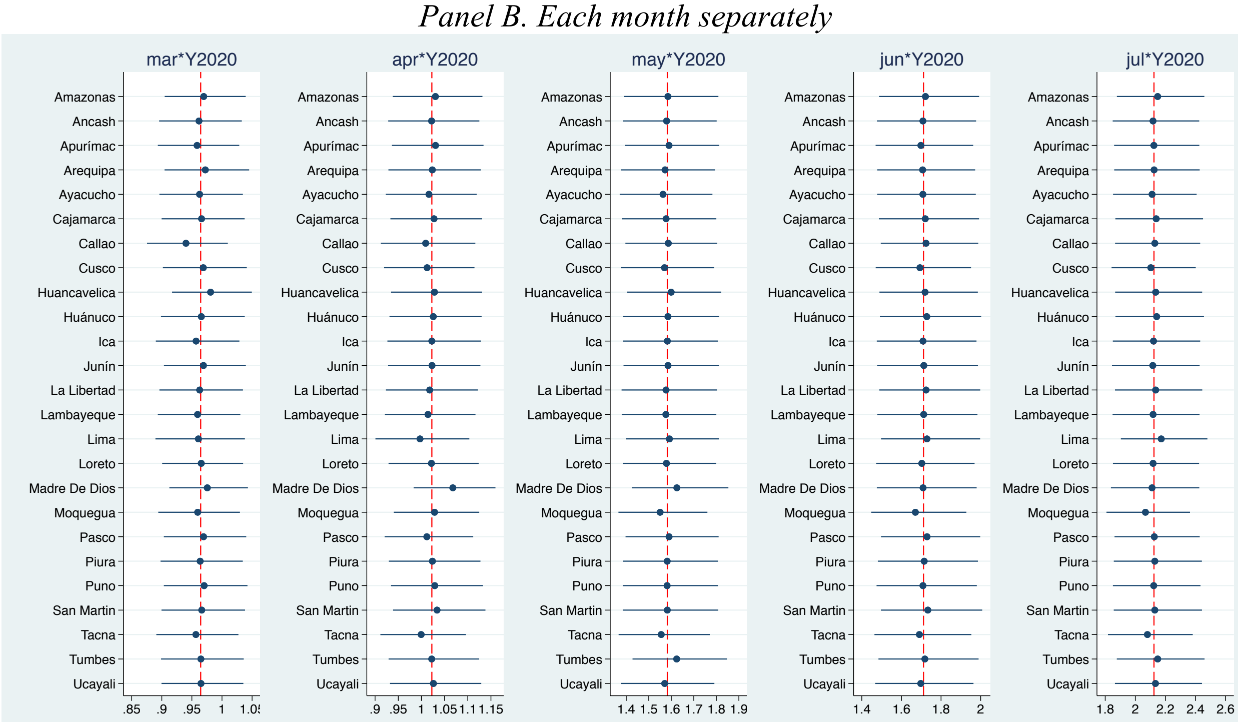
Note: Figures show the estimated coefficient (exponentiated) where the listed years is the new starting period (up to 2020). The vertical dashed line (red) represents the coefficients for the full sample (2007-2020). Each Poisson regression for the number of calls to Línea 100 per 100,000 people includes the same controls as of column 3 in Table 1. Confidence intervals at the 95% are calculated using robust standard errors clustered at the state-year level.

Figure A4. Robustness: Removing one state at the time

Panel A. Post period



Panel B. Each month separately



Note: Figures show the estimated coefficient (exponentiated) when the listed state is removed from the analysis. The vertical dashed line (red) represents the coefficient for the full sample. Each Poisson regression for the number of calls to *Línea 100* per 100,000 people includes the same controls as of column 3 in Table 1. Confidence intervals at the 95% are calculated using robust standard errors clustered at the state-year level.

Table A1. Total number of calls to *Línea 100* (per 100,00 people) nationwide

	Month											
Year	1	2	3	4	5	6	7	8	9	10	11	12
2007	16.5	21.6	14.1	15.8	19.3	17.1	33.4	27.3	28.5	32.5	38.3	32.8
2008	40.0	40.4	31.4	27.4	28.6	40.0	42.2	36.0	31.8	27.2	30.6	45.9
2009	60.7	77.9	88.3	87.8	90.9	103.5	101.9	84.7	87.9	101.1	99.0	90.2
2010	95.8	87.0	100.4	91.7	95.6	69.5	93.1	83.7	76.0	83.2	125.7	145.5
2011	167.7	155.5	204.8	165.4	142.6	151.2	142.2	160.5	154.0	160.9	210.9	214.8
2012	195.3	177.6	234.0	184.3	175.3	201.2	182.7	174.9	154.2	169.9	250.2	257.5
2013	218.8	181.4	191.6	186.0	179.2	168.1	177.3	189.5	207.9	196.9	211.2	281.3
2014	217.9	157.6	177.3	184.9	189.0	184.2	193.5	194.2	183.7	209.2	226.8	219.3
2015	201.0	233.9	225.8	200.6	189.5	183.1	189.4	133.9	131.9	172.1	207.4	185.4
2016	168.6	144.9	161.4	157.8	166.6	163.1	204.6	338.1	257.7	275.8	315.3	336.6
2017	311.9	276.5	290.5	296.1	330.6	305.4	313.8	321.5	286.0	343.0	352.0	362.5
2018	234.1	232.5	265.1	302.6	417.9	444.4	340.0	405.1	382.4	407.0	420.0	509.8
2019	523.5	536.7	595.4	575.0	582.5	557.7	524.6	530.6	594.3	540.0	567.3	652.5
2020	657.7	688.8	698.2	767.0	1352.3	1413.9	1561.3					

Note: Author’s calculations based on MIMP data.

Table A2. Estimates of the calls to helpline during the pandemic (Poisson)

	Dependent variable		
	Number of calls to *Línea 100*$^{(*)}$		
	(1)	(2)	(3)
	Panel A. Post period		
Post*Year2020	1.556***	1.556***	1.433***
	(0.081)	(0.084)	(0.105)
Pseudo R^2	0.973	0.974	0.976
Log-likelihood	-29292	-28234	-26318
	Panel B. Effects by month		
March*Year2020	0.983	0.983	0.985
	(0.017)	(0.023)	(0.030)
April*Year2020	1.199***	1.199***	1.161***
	(0.034)	(0.039)	(0.043)
May*Year200	1.778***	1.778***	1.565***
	(0.130)	(0.136)	(0.172)
June*Year200	1.816***	1.816***	1.604***
	(0.160)	(0.155)	(0.173)
July*Year2020	2.058***	2.058***	1.878***
	(0.164)	(0.173)	(0.223)
Pseudo R^2	0.976	0.977	0.977
Log-likelihood	-26688	-25628	-25196
N	4075	4075	4075
Month fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
State fixed effects	Y	Y	Y
State-specific trends	N	Y	Y
Month-specific trends	N	N	Y

(*) Not adjusted by population. Note: Exponentiated coefficients. Robust standard errors, clustered at the state-year level in parentheses. See text for variables definitions and sources. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3. Estimates of the calls to helpline during the pandemic (Linear regression)

	Dependent variable		
	Number of calls to *Línea 100* per 100,000 people		
	(1)	(2)	(3)
	Panel A. Post period		
Post*Year2020	18.89***	18.89***	17.66***
	(1.60)	(1.59)	(1.62)
Adjusted R^2	0.784	0.847	0.860
	Panel B. Effects by month		
March*Year2020	0.40	0.40	-0.20
	(0.68)	(0.67)	(0.74)
April*Year2020	3.48***	3.48***	2.57**
	(1.23)	(1.22)	(1.27)
May*Year200	26.49***	26.49***	24.39***
	(1.91)	(1.89)	(1.96)
June*Year200	29.01***	29.01***	27.22***
	(2.47)	(2.46)	(2.52)
July*Year2020	35.06***	35.06***	34.31***
Adjusted R^2	0.831	0.894	0.896
N	4075	4075	4075
Month fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
State fixed effects	Y	Y	Y
State-specific trends	N	Y	Y
Month-specific trends	N	N	Y

Note: Robust standard errors, clustered at the state-year level in parentheses. See text for variables definitions and sources. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A4. Calls to *Línea 100* during the pandemic (Poisson)

	Dep. var: Number of calls to *Línea 100*(*) by characteristics of the victim						
			Age group:				
	All	Women	0-5	6-11	12-17	18-59	60+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
March*Y2020	0.99	1.00	0.97	0.81**	0.78**	1.07	1.28***
	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)	(0.11)	(0.10)
April*Y2020	1.20**	1.22**	0.92	0.81***	0.75***	1.43***	1.61***
	(0.09)	(0.10)	(0.05)	(0.06)	(0.06)	(0.13)	(0.14)
May*Y2020	1.69***	1.75***	1.25***	1.15*	1.07	2.05***	2.28***
	(0.13)	(0.14)	(0.07)	(0.09)	(0.08)	(0.18)	(0.18)
June*Y2020	1.74***	1.82***	1.07	1.01	1.06	2.19***	2.49***
	(0.13)	(0.14)	(0.07)	(0.07)	(0.09)	(0.19)	(0.25)
July*Y2020	1.96***	2.07***	1.20***	1.18**	1.38***	2.41***	2.68***
	(0.13)	(0.14)	(0.06)	(0.08)	(0.10)	(0.20)	(0.18)
Pseudo R^2	0.958	0.955	0.862	0.887	0.908	0.957	0.958
Log-likelihood	-4419	-3717	-756	-899	-973	-3144	-504
N	79	79	79	79	79	79	79

(*) Not adjusted by population. Note: Exponentiated coefficients. Robust standard errors, clustered at the month-year level in parentheses. All regressions include month and year fixed effects. See text for variable definitions and sources. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[i] For a discussion on how COVID-19 could affect gender equality beyond intimate partner violence see Alon et al (2020). See also Chakraborty et al (2018) and Węziak-Białowolska et al (2020) for studies about the link between violence against women and the workplace. Negative effects on gender equality could have intergenerational effects. See for example Imai et al (2014) and Kavanaugh et al (2018).

[ii] See examples from The Guardian and The New York Times, both accessed on May 30, 2020.

[iii] Peru is administratively divided into 25 *regiones* (equivalent states in the United States), *provincias* (equivalente to counties) and *distritos* (municipalities). To ease its understanding, we use the term states instead of *regiones*.

[iv] For example, Leslie and Wilson (2020) use data from only 15 cities in the United States and focus on calls to 911. Mahmud and Riley (2020) focused on a sample of households in rural western Uganda using a phone survey.

[v] An example of the severity of the lockdown can be seen in the indicators reported by Google's Community Mobility Report (available at https://www.google.com/covid19/mobility/). Appendix Figure A1 shows a sharp and intensive reduction in mobility in Peru compared to other countries in Latin American.

[vi] Website: https://www.mimp.gob.pe/contigo/contenidos/pncontigo-articulos.php?codigo=31.

[vii] Microdata are only available for three years: 2017, 2018 and 2019. Using microdata for 2020 would allow us to identify calls by the gender of the victim, which not possible in the state-level report. While created as part of the plan to reduce violence against women, the *Línea 100* receives calls from or about men as victims too. Yet, these calls represent less than 15 percent of all recorded calls. The same can be said by age of the victim, where such classification is not available in the state-level data used in our main analysis. To explore the possible impact by gender and age, in Appendix Table A4, we conduct a time series analysis using coarser data (at the national level and without state-level variation) and for fewer years where these data are available (2014-2020). We continue to

find that the increase is larger in July 2020 compared to previous months. Consistent with our main conclusion, the increases are observed across all ages.

[viii] See Peterman et al (2020) and de Paz et al (2020) for a discussion.