

Essays on Domestic Abuse and Violence Against Women

by
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Introduction

This dissertation addresses three events that had an impact on violence against women. Violence could be represented in several ways: physically, psychologically, or sexually. In this work, the effect on the three forms of violence are explored. First, I study the increase in the number of rapes in the 24 subsequent hours after a major YouTube outage occurred on October 16th, 2018. Second, I explore the long-term causal impact of military conscription on sexist attitudes and intimate partner violence. Lastly, I analyze the extent to which the quarantine implemented following the coronavirus pandemic had unintended consequences on intimate partner violence (IPV). A feature of this dissertation is the accounting of motives and mechanisms, to the extent possible, associated with each event.

First, using high-frequency crime data from the U.S., the study documents a significant increase in rapes in the 24-hour period following the major YouTube outage that occurred on October 16th, 2018. We investigate various potential underlying channels that may link the YouTube outage to the subsequent observed increase in rapes: we explore a direct effect on crime, time substitution, an effect on the consumption of drugs and alcohol, and the increase in pornography viewing. The overall evidence only supports the hypothesis that the increase in rapes could have been driven by an increase in pornography viewing.

In my second essay, I provide empirical evidence on the long-term causal impact of military conscription on sexist attitudes and intimate partner violence. To address potential endogeneity, we exploit the conscription draft lottery in Argentina. We combine the draft administrative data with self-reported survey data. We find that conscripted men are more likely to report embracing more sexist attitudes in dimensions such as justification of sexism and violence, sexual machismo, negative attitude towards homosexuality, old-fashioned sexism, and hostile sexism. We also find

that men who served are more likely to self-report engagement in intimate partner violence, as measured by non-physical abuse and physical violence.

In my third essay, we use self-reported survey data from Argentina to study the extent to which the quarantine implemented following the coronavirus pandemic had unintended consequences on intimate partner violence (IPV). Since the disease arrived late to Argentina and the government reacted fast, the full national lockdown was imposed when few people felt threatened by the virus. The quarantine decree also established clear exceptions for subsets of the population and, for reasons plausibly exogenous to the prevalence of IPV, only some individuals were placed in quarantine. Exploiting this variability in exposure, we find a positive link between quarantine and IPV.

Chapter I:

When You Can't Tube...

Impact of a Major YouTube Outage on Rapes*

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Abstract

On Tuesday, October 16th, 2018, YouTube experienced a major and rare global service outage. Using high-frequency crime data from the U.S., we document an important increase in rapes in the 24-hour period following the outage. We investigate various potential underlying channels that may link the YouTube outage to the subsequent observed increase in rapes. The overall evidence only supports the hypothesis that the increase in rapes was driven by an increase in pornography viewing.

Keywords: Sexual crime; sexual offenses, event study, social media, pornography.

JEL classification: K42.

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I. Introduction

Social media is an important part of many people's lives. According to a 2017 survey, an average adult spends approximately 54 minutes a day consuming social media.¹ Among the many social media sites available, YouTube is the most widely used site by adults in the United States. According to a nationally representative survey conducted in January 2019, 73% of American adults use YouTube regularly, and 51% of YouTube users say they visit the site daily.²

The increase in the use of social media raised an important debate in the public sphere on the potential effects of social media exposure on economic and social outcomes. Our paper contributes to this debate by studying the short-run effects of deprivation of consumption of social media. In particular, we study the impact of a major YouTube outage on subsequent rapes. On Tuesday, October 16th, 2018, between 9pm and 11pm Eastern time, YouTube experienced a major and rare global service outage. Using high-frequency data on reported criminal incidents from the U.S. for the period January 1st, 2017 to April 1st, 2019, we document an important increase in rapes in the 24-hour period following the outage. Our results are robust to controlling for day of the week dummies, day of the month dummies, month dummies, and a time trend.

We also investigate potential underlying channels or mechanisms that may link the YouTube outage to the subsequent observed increase in rapes. We find that other crimes and offenses (including drug, alcohol, and traffic) were not affected by the outage. We also report that the observed increase in rapes did not occur in the 2-hour period during the outage, but in the 22-hour period after YouTube service had been restored. Finally, we document that in the 2-hour period during YouTube's disruption there was an important increase in traffic on the online adult video

¹ Millennials spend approximately 114 minutes a day. Retrieved from <http://www.thevab.com/wp-content/uploads/2017/>, on May 7th, 2019.

² Retrieved on April 10th, 2019, from <https://www.pewresearch.org/fact-tank/2019/04/10/>.

site Pornhub (the world's biggest pornography site), which implied millions of additional viewers during Pornhub's peak hours. Overall, these findings suggest that the observed link between YouTube outage and rapes may be operating through the increase in pornography viewing.

There is some important literature on the drivers of sexual aggression, as well as on the characteristics of sexual crime perpetrators and their modus operandi.³ According to this literature, most rapes are committed by relatives or acquaintances of the victim (Russell 1984; Koss et al. 1988; National Research Council 1996; Gavey 2013).⁴ We do not have information on the perpetrator, so we are unable to contribute to this particular point. Though being related to this general literature, our paper does not focus on the causes of rapes, but on the causal effect of deprivation of a social media on rapes. As such, our research only explains a relatively small fraction of rapes.

Our paper is related to the literature on the impact of media on different outcomes, such as education, family choices, labor and migration decisions, environmental choices, health, crime, attitudes, consumption and savings, and financial choices (for a review of this literature -mainly radio and television-, see DellaVigna and La Ferrara 2015). There is also small literature on the effects of social media (Enikolopov, Petrova, and Sonin 2018; Enikolopov, Makarin, and Petrova 2017) and a recent experimental research that focuses on how people react to deprivations of the consumption of social media (Mosquera et al. 2018; Allcott et al. 2019).⁵

³ Groth (1979) identified a least four types of rapists: opportunist rapists (30%) who exhibit no anger toward the women they assault and usually use little or no force; anger rapists (40%) who batter the survivor and use more physical force than is necessary to overpower her; power rapists (25%) who do not intend to physically harm their victim but rather to possess or control her to gain sexual gratification; and sadistic rapists (5%) who become sexually excited by inflicting pain on their victim.

⁴ According to RAINN, 8 out of 10 rapes are committed by someone known to the victim (<https://rainn.org/statistics/perpetrators-sexual-violence>).

⁵ There is also some research in economics that studies the impact of internet availability and sexual crime (Bhuller et al. 2013; Nolte 2019).

Our finding that pornography viewing may lead to an increase in rapes adds to a long-standing debate in the U.S. regarding the effects of pornography. As far back as 1968, President Lyndon B. Johnson sets up the President's Commission on Obscenity and Pornography to study the effects of pornography on crime and on other antisocial conducts. The Commission concluded there was insufficient evidence to link the exposure to pornography to subsequent aggression, particularly in sexual crime. The report triggered an important amount of research (mainly in the fields of criminology, experimental psychology, and sociology) on the effects of pornography on sexual aggression. Ferguson and Hartley (2009) provide a review of this research and, in line with the report, conclude that pornography is not associated with increased sexual assault behavior. However, some authors have challenged these findings, providing evidence that pornography is associated with an increase in violent sexual behavior. Malamuth, Addison, and Koss (2000) and Foubert (2017) provide a review of this literature and conclude that the evidence supports the existence of a positive association between frequent use of pornography and sexually coercive behavior, particularly for men at high risk for sexual aggression. Additionally, the literature that analyzes sexual offenders' modus operandi documents that most sexual offenders use pornography to feed their violent fantasies (see Johnson 2006).

Given that most of the evidence available on the relationship between pornography and rapes come from correlational studies, it is difficult to interpret this evidence causally. The experimental research available focuses on hypothetical behavior in the lab, and the experimental subjects are mainly college students (Malamuth and Ceniti 1986; Linz, Donnerstein, and Penrod 1988; Fisher and Grenier 1994; Hald and Malamuth 2005; Yang, Dong-ouk, and Gahyun Youn 2012).⁶ Thus, the question of whether pornography actually increases or reduces rapes is still open. Our paper

⁶ There is also literature discussing the limitations of this type of studies (Mould 1988; Jensen 1995).

provides evidence that supports the hypothesis that an increase in pornography viewing may lead to an increase in rapes.

Finally, our paper contributes to the literature on criminal decision-making. The rational choice theory postulates that rational agents decide whether to engage in criminal activities by comparing the benefits and costs of committing a crime (Becker 1968). A recent literature shows that emotional cues or visceral factors (i.e., frustration and euphoria) also affect crime decisions, such as the decisions to engage in domestic violence (Card and Dahl 2011), violent crime (Munyo and Rossi 2013), and sexual crime (Lindo, Siminski, and Swensen 2018). In line with this literature, our results suggest that a fraction of sexual crime can be better characterized as a breakdown of control rather than a behavior driven by rational choice.

The organization of the paper is as follows. Section II presents the natural experiment and describes the data. Section III presents the empirical strategy and reports the results. Section IV explores mechanisms. Section V concludes.

II. Natural experiment and data

YouTube experienced a major and rare global service outage on October 16th, 2018, between 9pm and 11pm Eastern time. Users who tried to access the website during this period were greeted with a blank page that showed no videos. On the app, they saw an error message saying ‘There was a problem with the network [503].’ According to Downtdetector, the first massive reports (13,650) were found at 9:01:11 pm, Eastern time.

The outage received extensive coverage in the media.⁷ Figure 1 displays the evolution of daily reported problems in the YouTube site for the period May 2017 to February 2019. We obtain

⁷ See, for example, www.msn.com/en-gb/money/technology/googles-youtube-suffers-a-major-outage/; www.cnbc.com/2018/10/17/googles-youtube-outage-affected-users-in-us-australia-asia-europe.html; www.usatoday.com/story/tech/talkingtech/2018/10/16/youtube-offline-worldwide-social-media-internet/.

YouTube reports data from Downtdetector, which collects status reports from a series of sources (such as Twitter). Through a real-time analysis of this data, Downtdetector automatically detects outages and service interruptions at a very early stage. An outage exists when the number of reports shows a significant jump relative to the baseline. As observed in Figure 1, there is a clear and unusual spike in reported problems on October 16th, 2018.⁸

We use high-frequency (hourly) data on reported criminal incidents in the U.S. for the period January 1st, 2017, to April 1st, 2019. These data were collected by Socrata.⁹ The Socrata dataset aggregates all reported incidents at 295 police departments and sheriffs' offices (from a total of 17,784 police departments and sheriffs' offices in the U.S.), and represents 6.4% of the U.S. population. The data is on reported offenses and the time of the incident is the one reported by the victim. There is no information on arrests.

From the original hourly data, we generate a "daily" dataset that uses the date and time of the reported incident, so that all "days" start at the time of the outage (9pm Eastern time). For example, in our built dataset October 1st corresponds to the 24-hour period that starts at 9pm Eastern time on October 1st and ends up at 8.59pm Eastern time on October 2nd. In this way, we end up with 820 "daily" observations.

We define an incident as a rape if the record has the word "rape" or the corresponding police code in the primary incident type column or in the incident description.¹⁰ In our sample, there is an average of 6.5 rapes per day.¹¹

⁸ Other minor outages were on June 16th, 2017 at 10am for two hours; November 12th, 2018 at 5pm for an hour; November 18th, 2018 at 7pm for half an hour. Note that these outages did not occur at night.

⁹ Socrata provides a data-as-a-service platform bringing together existing government data. Datasets downloaded on April 21st, 2019 from <https://moto.data.socrata.com/browse?limitTo=datasets> (not currently available, see capture at web.archive.org/web/20191013052124/https://moto.data.socrata.com).

¹⁰ A police code is a numerical brevity code for a crime, incident, or instructions for police officers. https://en.wikipedia.org/wiki/Police_code and <https://web.stanford.edu/~renee/bill/n.radio.code.html>.

¹¹ 66% of the police stations do not report any rapes in our sample.

The dataset also includes other criminal and non-criminal offenses. There are traffic offenses, community policing, disorder, theft (includes theft from vehicle, theft of a vehicle, robberies, property crime, and breaking & entering), and assault. All of these together account for approximately 80% of the total number of reports. Among these categories, we group theft and assault as criminal offenses, and traffic offenses, community policing, and disorder as non-criminal offenses. Additionally, we create variables on drug and alcohol-related offenses by aggregating all offenses that include the word “drug” and “liquor” in the incident description, respectively. Table 1 reports the summary statistics of the data.

III. Empirical strategy and results

We are interested in estimating the impact of YouTube outage on rapes during the 24 hours following the outage. Figure 2 anticipates the main finding. The figure plots the distribution of rapes in all “Tuesdays” in our sample, and it shows that only 3.42% of Tuesdays have more reported rapes than October 16th, 2018.¹²

We first compare October 16th, 2018, to the average Tuesday in our sample. We analyze it on three different periods: January 1st, 2017 to April 1st, 2019 (117 Tuesdays), January 1st to December 31st, 2018 (52 Tuesdays), and September 1st to December 31st, 2018 (17 Tuesdays). Formally, we estimate the following equation:

$$Rapes_t = \alpha + \beta YouTube\ outage_t + \varepsilon_t \quad (1)$$

where $Rapes_t$ is the total number of rapes on day t , $YouTube\ outage_t$ is a dummy variable that takes the value one from October 16th, 2018, 9pm to October 17th, 2018, 8.59pm Eastern time, and zero otherwise, β is the parameter of interest, and ε_t is the error term.

¹² These Tuesdays are 08/22/2017, 05/15/2018, 06/12/2018, and 07/10/2018. Note that all of these Tuesdays are either in spring or in summer.

Table 2 reports Ordinary Least Squares (OLS) estimates of equation (1). In column (1), we report estimates of equation (1) for the 117 Tuesday in the sample period. The coefficient on YouTube outage is positive and statistically significant. The value of the coefficient implies a 1.6 standard deviation increase in rapes in the 24-hour period following the outage (the standard deviation of rapes on Tuesdays is 2.75). Columns (2) and (3) report estimates of equation (1) for alternative time periods (January to December 2018 and September to December 2018, respectively). Again, the coefficient on YouTube outage is positive and statistically significant.

Given that in these regressions the number of observations is relatively small, we conduct randomized inference and report p-values obtained from permutation tests (over time) based on Monte Carlo simulations. We use the *ritest* command in Stata (Heß 2017), and we compute two-sided p-values. We perform 500, 1,000, 5,000, and 10,000 replications. In all cases, the coefficients are statistically significant.

Then, we analyze the entire sample (820 daily observations). Table 3 reports OLS estimates of the following equation:

$$Rapes_t = \alpha + \beta YouTube\ outage_t + \varphi X_t + \varepsilon_t \quad (2)$$

Depending on the particular specification, the set of controls (X_t) includes day of the week dummies (7), day of the month dummies (31), month dummies (12), and a quadratic daily time trend. We report White-Huber robust standard errors and Newey-West heteroskedasticity- and autocorrelation-consistent standard errors.¹³

In column (1) of Table 3, we report estimates of equation (2) without controls. The coefficient on *YouTube outage* is positive and statistically significant at the usual levels of confidence. The value of the coefficient implies a 1.4 standard deviation increase in rapes in the

¹³ In all cases, the heuristic applied to obtain the number of lags is taken from the first step of Newey and West's (1994) plug-in procedure that sets the number of lags as $\text{floor}[4(T/100)^{2/9}]$, where T is the number of observations.

24-hour period following the outage. In columns (2) to (5) in Table 3 we show that results are robust to controlling for day of the week dummies, month dummies, day of the month dummies, and a daily time trend.¹⁴ Given that the average number of rapes is 6.54, the estimated coefficient indicates an approximately 55% increase in rapes in the 24-hour period following the outage.

Given the time-series nature of the exercises at hand, however, inference is always a concern. To deal with potential deviations of standard homogeneity assumptions, we implement a variant of Fisher's (1935) randomization test (see also Buchmueller, DiNardo, and Valletta 2011). We compare our estimate to the 819 "placebo" estimates obtained by running 819 additional regressions for each day and re-computing. In each case, we replace *YouTube outage_t* with an indicator that takes the value one for each of the other 819 days in the sample period. That is, we treat the 819 placebo estimates as the sampling distribution for the parameter of interest. The significance level is determined by the rank of the October 16th effect in the distribution of placebo effects. The rank of October 16th, 2018 in the model in column (1) in Table 3 is 760, this means that 7% of placebo estimates are above the estimate for October 16th, 2018. In the models in columns (2) to (5), the rank of the estimated coefficient corresponding to October 16th, 2018 is 749, 749, 749, and 727 out of 820, respectively.

In a similar fashion, and in order to further address the causal interpretation of our finding, we conduct a series of placebo analyses around the treatment window. First, we use the 4 Tuesdays around October 16th as placebos. That is, we generate 4 placebo treatments by rolling the treatment artificially back and forward one week and two weeks, respectively. Second, we use the 12 days around October 16th as additional placebos. That is, we generate 12 placebo treatments by rolling

¹⁴ Results are robust to using a quadratic time trend. In addition, to alleviate potential concerns arising from the use of a linear probability model when the outcome is discrete, as further robustness check we reproduce previous results using a regression model for discrete count outcomes. Conclusions are robust to using count models. All results mentioned and not shown are available upon request.

the treatment artificially back and forward one day, two days, three days, four days, five days, and six days, respectively. In all cases, we estimate the model in column (5) in Table 3. We report these placebo exercises in Figures 3 and 4. Our findings indicate that, out of the 17 estimated coefficients (16 placebos plus the real treatment), the coefficient for October 16th is the largest one.

As an additional analysis, we conduct a kind of triple-difference approach, using total crime as a within-day counterfactual. We estimate a variation of equation (2) in which the dependent variable is the share of rapes on total criminal offenses. As observed in Table 4, the coefficients on YouTube outage are, in all cases, positive and significant, indicating that rapes are increasing on October 16th, 2018, relative to total crime.

Overall, results in Tables 2 and 3 indicate that rapes spiked on October 16th, 2018, whereas results in Table 4 suggest that this spike in the number of rapes does not appear to be representing general crime spikes.

IV. Further results

We now investigate various potential underlying mechanisms that may link the YouTube outage to the subsequent observed increase in rapes. We explore: (i) a direct effect of the outage on other crimes and offenses, (ii) an effect on drug and alcohol-related offenses, (iii) an effect on more people going out after the outage for alternative entertainment, (iv) an effect on time substitution, and (v) pornography viewing.

We first analyze the effect of the outage on criminal offenses (theft, assault, property crime, theft from vehicle, breaking & entering, and theft of vehicle) and non-criminal offenses (traffic offenses, disorder, community policing, and vehicle stop). As mentioned before, frustration, for instance, could be an emotional cue expressing from the unexpected outage, and this could have

led to an increase in crime. As shown in columns (1) and (2) in Table 5, however, there is no significant association between the outage and criminal and non-criminal offenses.

We then investigate the effect of the outage on drug and alcohol-related offenses. This is potentially important since approximately half of sexual assaults involve alcohol consumption by the perpetrator, victim, or both (Abbey et al. 2001). Columns (3) and (4) in Table 5 show the outage is not significantly related to an increase in drug-related offenses nor to an increase in alcohol-related offenses.

Finally, we also explore if the lack of online entertainment led people to go out. In principle, more people going out after the outage may be correlated with the increase in rapes. We indirectly check this potential path by looking at the effect of the outage on traffic offenses. As shown in column (5) in Table 5, the estimated coefficient is statistically not significant.

Overall, results reported in Table 5 suggest there is no positive effect of the outage on other crimes and offenses.

A plausible hypothesis is that watching YouTube and committing rape are substitutes. This may arise, for example, if some individuals that were not able to access YouTube react by committing rape.¹⁵ We name this hypothesis as the time-substitution channel. An observational implication of the time-substitution channel is that we should observe an increase in rapes during the outage (that is, in the 2-hour period starting at 9pm Eastern time on Tuesday October 16th, 2018). To explore this potential channel, we constructed an hourly dataset for the period January 1st, 2017 to March 31st, 2019 (19,680 hours). An anticipation of time-substitution results is reported in Figure 5, which shows there is no increase in rapes during the outage, and all the observed

¹⁵ The literature on sexual offender's modus operandi discusses about several offender's typologies. For example, an offender who is trolling for victims may choose to acquire an opportunistic victim at a location with increased victim availability and vulnerability. Thus, the opportunistic offenders may rape the first person they see (Johnson 2006; Turvey 2013).

increase occurs after the service was restored. To formally test the time-substitution channel, we generate two new variables. *During outage* is a dummy variable that is equal to 1 in the 2-hour period from 9pm to 10.59pm Eastern time on Tuesday October 16th, 2018. *After outage* is a dummy variable that is equal to 1 in the 22-hour period starting at 11pm Eastern time on Tuesday October 16th, 2018. As reported in Table 6, all the observed effect comes from rapes in the 22-hour period after the outage. Indeed, rapes fell during the outage. These findings do not support the time-substitution channel.

Finally, we explore the pornography-viewing channel. During YouTube's disruption, there was an important increase in traffic on the online adult video site Pornhub, the world's largest pornography site. The top panel of Figure 6 displays hourly data on YouTube reported problems. The bottom panel of Figure 6 displays hourly data on Pornhub's traffic from noon Eastern time October 16th, 2018, until 2am Eastern time October 17th, 2018. The Pornhub site detected a spike in traffic during YouTube's outage: traffic increased 12 percent above average at around 9pm Eastern time, when the outage was reported, climbing to 21 percent increase over average traffic one hour later. According to information provided by Pornhub, this increase in traffic implies millions of additional viewers during Pornhub's peak hours. Traffic dropped rapidly once YouTube's service was restored, dropping to slightly below average numbers around midnight Eastern time.¹⁶

If YouTube viewers switched to Pornhub during the outage, there must be some substitutability between these two sites. Their traffic peak hours are late at night in comparison to

¹⁶ Using hourly data on YouTube reported problems and Pornhub traffic for the 15-hour window around the outage (from noon October 16th until 2am October 17th, Eastern time), we run a regression of Pornhub's traffic on YouTube reported problems. As expected, the estimated coefficient is positive and highly significant (the estimated coefficient is 0.11, with a standard error of 0.01), indicating that the outage is highly correlated with pornography viewing.

other entertainment platforms.¹⁷ YouTube's higher traffic is from 7pm to 10pm, and Pornhub's higher traffic is from 9pm to 1am.¹⁸ Thus, they are both video platforms that are mostly consumed at night.

What were YouTube users viewing before the outage? What happened to Pornhub searches during the outage? According to information provided by Pornhub, ASMR (Autonomous Sensory Meridian Response) was the word with the highest search growth during YouTube outage:¹⁹ ASMR searches in Pornhub increased by 201% compared to the October 16th, 2018, hourly average. In Pornhub, searching for ASMR leads to hardcore material that combines the sound effects of ASMR with explicit sexual content. Even though we do not have information on YouTube searches around the outage, there is evidence that ASMR searches are very popular on YouTube,²⁰ and therefore it is likely that YouTube users that were watching ASMR at that site switched and searched for ASMR at Pornhub.

According to specialized literature (see Schmidt 1975; Both et al. 2004), sexual arousal (and the increase in sexual activity) after pornography viewing last for up to 24 hours, so our findings are compatible with pornography viewing being the channel behind the observed increase in rapes in the 22-hour period after the outage. Additionally, the results are in line with the observed fact that one third of sex offenders consume pornography before committing a sexual assault (Marshall 1988).

¹⁷ Social media platforms have different peak hours than the ones we are analyzing. Facebook's higher engagement is every day around 8–10 am and 5pm, and it drops before 5am and after 6pm. Instagram's higher engagement on Monday through Friday, is from 9 am to 4 pm and it is lower every day before 6 am and after 9pm. Twitter has similar patterns, however engagement drops off later in the day, after 10pm (see <https://sproutsocial.com/insights/best-times-to-post-on-social-media/>).

¹⁸ See <https://edgy.app/best-time-to-post-on-youtube-live-youtube-subscriber-count> and <https://www.pornhub.com/insights/pornhubs-fappyhour>.

¹⁹ ASMR is an experience or feeling triggered by specific auditory or visual stimuli, such as quiet and whispery noises, usually accompanied by feelings of relaxation and well-being.

²⁰ According to BBC news, there are over 13 million videos of people trying to trigger ASMR feeling on YouTube (<https://www.bbc.com/news/av/newsbeat-45957504/asmr-i-can-make-your-brain-tingle>).

Overall, we conclude that evidence only supports the mechanism of pornography viewing. In the appendix, we develop a simple model to rationalize our interpretation that pornography viewing increases rapes.

V. Final remarks

YouTube experienced a major global interruption on Tuesday October 16th, 2018. Using high-frequency crime data from the U.S., we document an increase in the number of rapes in the 24-hour period following the outage. We explore various potential mechanisms, and we find evidence that the increase in rapes might be driven by an increase in pornography viewing.

The association between the increase in pornography viewing and the increase in rapes can be rationalized by combining previous research in psychology and behavioral economics. Research in psychology indicates that an important fraction of male students in the U.S. (25 to 30 percent) admit to some likelihood of raping or forcing sex acts on a woman if they could get away with it (Malamuth 1984; Edwards, Bradshaw, and Hinsz 2014). The behavioral economics literature indicates that under the influence of visceral factors (such as being sexually aroused), individuals decide without fully taking into account the consequences of their acts. In a nutshell, our results suggest that pornography consumption and rapes could be strategic complements: the consumption of pornography increases sexual arousal, which in turn increases the utility from raping and decreases the perceived cost of being caught, thus increasing the probability of rape. Thus, our interpretation proposes that a small fraction of rapes could be better characterized as a failure of control rather than a behavior driven by rational choice.

VI. References

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VII. Figures and Tables

Table 1. Summary statistics of crime data

	Mean	Standard deviation	Minimum	Maximum
Rapes	6.54	3.21	0	21
Criminal offenses	1,976.49	211.78	1,089	2,523
Non-criminal offenses	4,281.58	477.96	2,232	6,088
Alcohol offenses	53.04	21.09	17	140
Drug offenses	187.49	25.72	66	260
Traffic offenses	1,690.13	217.64	653	2,274
Observations	820			

Notes: Table 1 reports summary statistics of “daily” data. Data was constructed using the date and time of the incident and normalized so that all “days” start at the time of the outage (9pm Eastern time). Criminal offenses include theft (a category that includes theft from vehicle, theft of vehicle, property crime, robberies, and breaking & entering) and assaults. Non-criminal offenses include traffic offenses, community policing, and disorder.

Table 2. Impact of YouTube outage on rapes: sample of “Tuesdays”

	Dependent variable: Rapes		
	(1)	(2)	(3)
YouTube outage	4.41*** (0.25)	3.63*** (0.36)	3.75*** (0.50)
<i>Permutation analysis</i>			
500 replications	[0.018]	[0.014]	[0.006]
1,000 replications	[0.025]	[0.011]	[0.003]
5,000 replications	[0.023]	[0.013]	[0.003]
10,000 replications	[0.025]	[0.011]	[0.003]
Observations	117	52	17

Notes: Table 2 reports coefficients obtained from Ordinary Least Squares regressions of Rapes on YouTube outage. In all cases, the samples are restricted to “Tuesdays” (the 24-hour period starting on Tuesdays at 9pm Eastern time). Column (1) uses the period January 1st, 2017 to April 1st, 2019. Column (2) uses the period January 1st, 2018 to December 31st, 2018. Column (3) uses the period September 1st, 2018 to December 31st, 2018. White-Huber robust standard errors are in parentheses. P-values (two-tailed) obtained from randomized inference using the *ritest* command in Stata are in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 3. Impact of YouTube outage on rapes

	Dependent variable: Rapes				
	(1)	(2)	(3)	(4)	(5)
YouTube outage	4.47 (0.11)*** {0.16}***	4.41 (0.25)*** {0.25}***	4.16 (0.40)*** {0.61}***	4.36 (0.81)*** {0.95}***	3.62 (0.77)*** {0.84}***
R-squared	0.002	0.094	0.105	0.187	0.260
Day of the week	No	Yes	Yes	Yes	Yes
Month	No	No	Yes	Yes	Yes
Day of month	No	No	No	Yes	Yes
Daily time trend	No	No	No	No	Yes
Observations	820	820	820	820	820

Notes: Table 3 uses daily data for the period January 1st, 2017 to March 31st, 2019. White-Huber robust standard errors are in parentheses. Newey-West heteroskedasticity- and autocorrelation-consistent standard errors are in braces. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 4. Impact of YouTube outage on the participation of rapes on total crime

	Dependent variable: (Rapes/Criminal offenses)*1000				
	(1)	(2)	(3)	(4)	(5)
YouTube outage	1.89 (0.05)*** {0.07}***	1.96 (0.12)*** {0.12}***	1.97 (0.19)*** {0.27}***	2.05 (0.41)*** {0.46}***	1.73 (0.40)*** {0.43}***
R-squared	0.002	0.011	0.095	0.167	0.228
Day of the week	No	Yes	Yes	Yes	Yes
Month	No	No	Yes	Yes	Yes
Day of month	No	No	No	Yes	Yes
Daily time trend	No	No	No	No	Yes
Observations	820	820	820	820	820

Notes: Table 4 uses daily data for the period January 1st, 2017 to March 31st, 2019. White-Huber robust standard errors are in parentheses. Newey-West heteroskedasticity- and autocorrelation-consistent standard errors are in braces. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 5. Mechanisms: other crimes and offenses

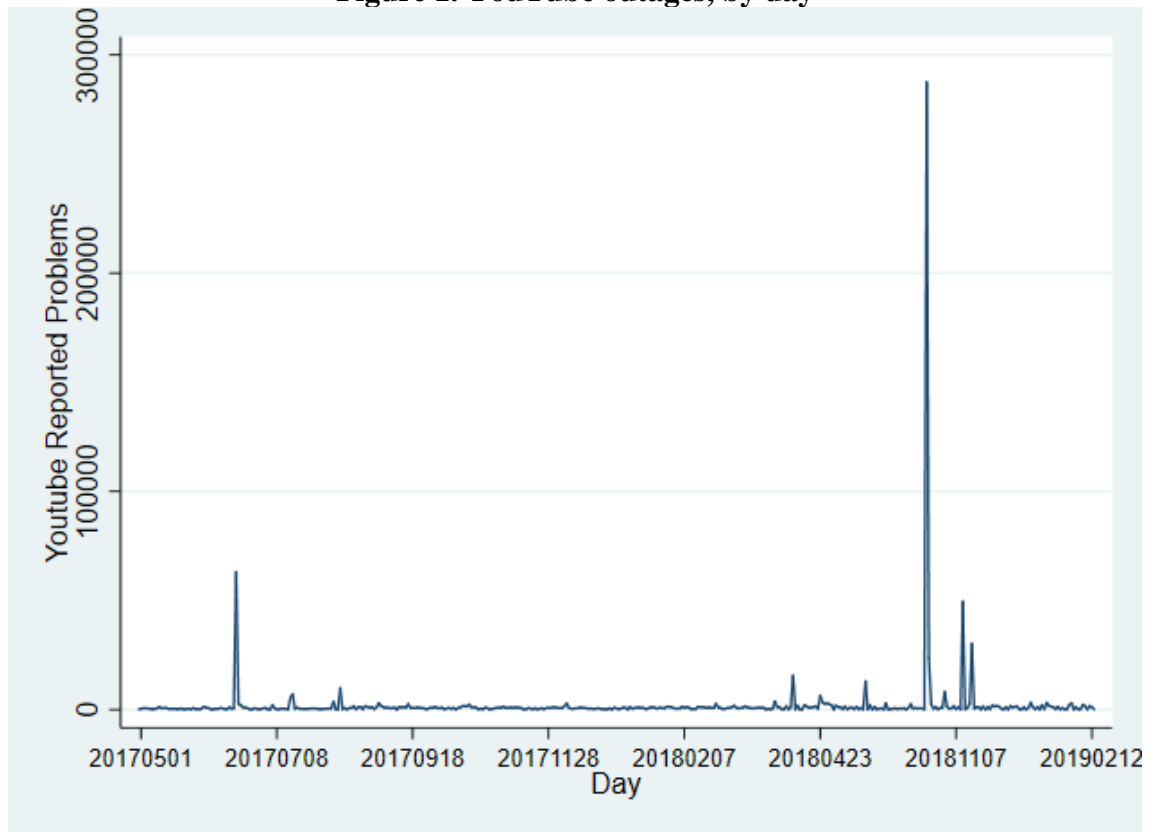
	(1) Criminal offenses	(2) Non- criminal offenses	(3) Drug offenses	(4) Alcohol offenses	(5) Traffic offenses
YouTube outage	-45.21 (35.11) {35.60}	-83.70 (75.87) {73.06}	-3.16 (3.15) {4.06}	3.21 (5.48) {2.42}	-43.35 (27.97) {30.29}
Day of the week	Yes	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes	Yes
Day of month	Yes	Yes	Yes	Yes	Yes
Daily time trend	Yes	Yes	Yes	Yes	Yes
Observations	820	820	820	820	820

Notes: Criminal offenses include theft (a category that includes theft from vehicle, theft of vehicle, property crime, robberies, and breaking & entering) and assaults. Non-criminal offenses include traffic offenses, community policing, and disorder. White-Huber robust standard errors are in parentheses. Newey-West heteroskedasticity- and autocorrelation-consistent standard errors are in braces. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 6. Mechanisms: time substitution

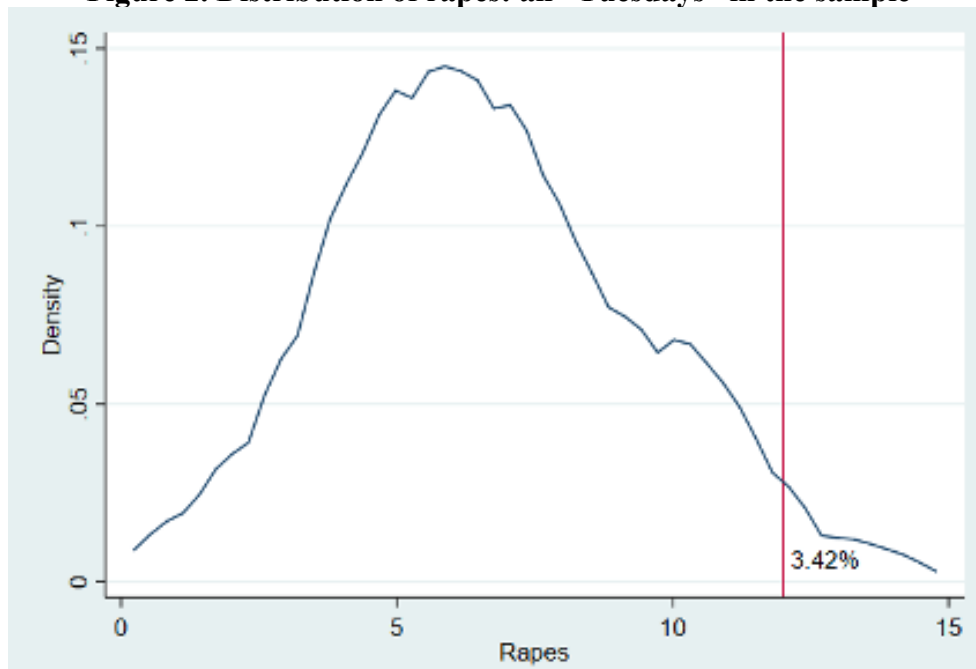
	Dependent variable: Rapes					
	(1)	(2)	(3)	(4)	(5)	(6)
During outage	-0.27 (0.00)*** {0.00}***	-0.27 (0.01)*** {0.01}***	-0.27 (0.02)*** {0.02}***	-0.28 (0.02)*** {0.02}***	-0.29 (0.03)*** {0.03}***	-0.32 (0.03)*** {0.03}***
After outage	0.23 (0.18) {0.10}**	0.23 (0.15) {0.10}**	0.23 (0.15) {0.10}**	0.21 (0.16) {0.10}**	0.22 (0.16) {0.10}**	0.19 (0.16) {0.10}*
Hour of the day	No	Yes	Yes	Yes	Yes	Yes
Day of the week	No	No	Yes	Yes	Yes	Yes
Month	No	No	No	Yes	Yes	Yes
Day of the month	No	No	No	No	Yes	Yes
Time trend	No	No	No	No	No	Yes
Observations	19,680	19,680	19,680	19,680	19,680	19,680

Notes: Table 6 uses hourly data dataset for the period January 1st, 2017 to March 31st, 2019. *During outage* is a dummy variable that takes value 1 in the 2-hour period 9pm to 10.59pm Eastern time on Tuesday October 16th, 2018. *After outage* is a dummy variable that equals 1 in the 22-hour period starting at 11pm Eastern time on Tuesday October 16th, 2018. The variable *Rapes* has an hourly average equal to 0.27. White-Huber robust standard errors are in parentheses. Newey-West heteroskedasticity- and autocorrelation-consistent standard errors are in braces. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

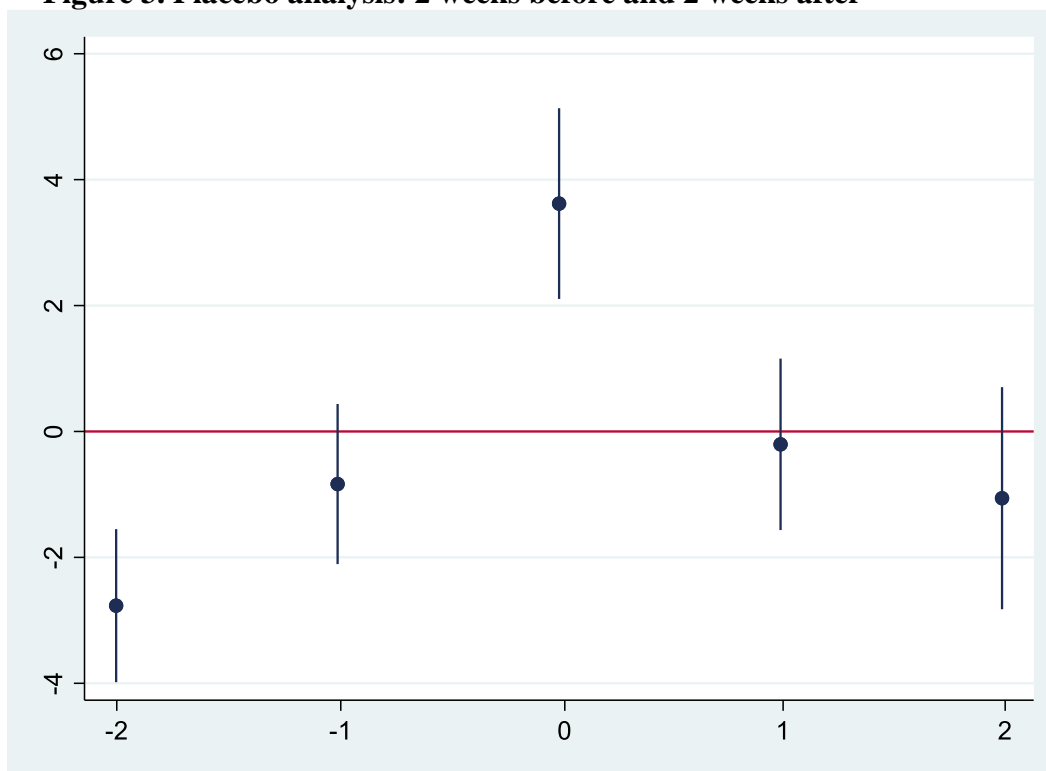
Figure 1. YouTube outages, by day

Source: Own elaboration, based upon data obtained from Downdetector. Downloaded on April 21st, 2019.

Figure 2. Distribution of rapes: all “Tuesdays” in the sample

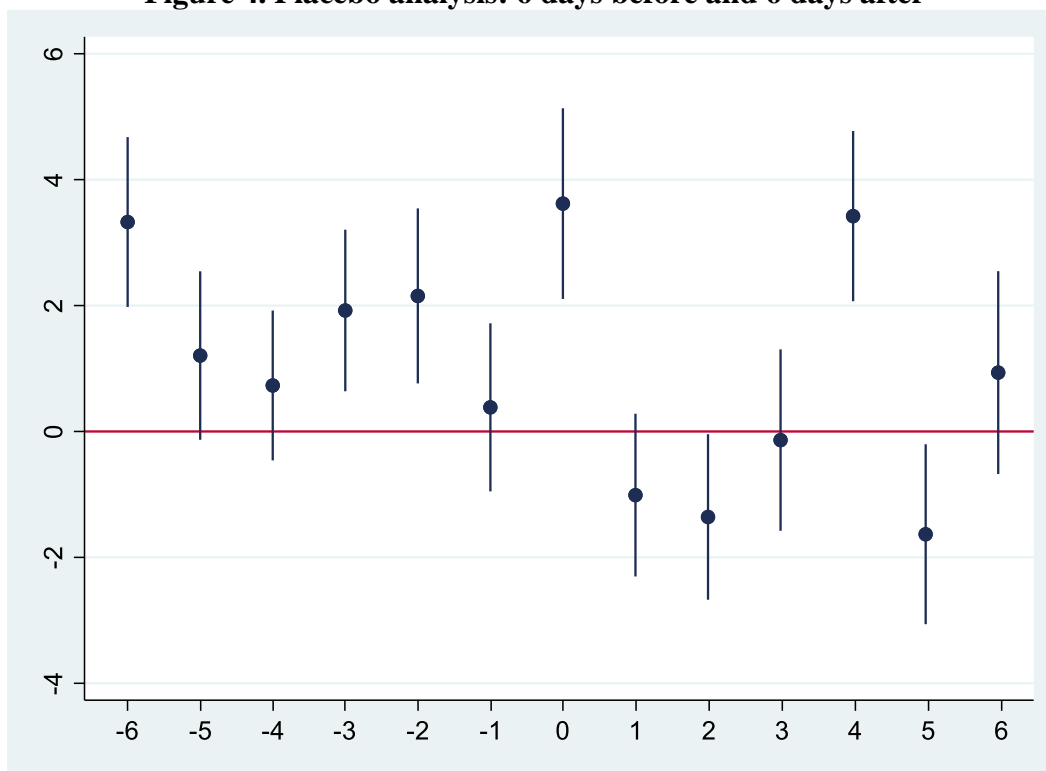


Source: Own elaboration. Notes: 3.42% is the percentage of Tuesdays with more rapes than October 16th, 2018.

Figure 3. Placebo analysis: 2 weeks before and 2 weeks after

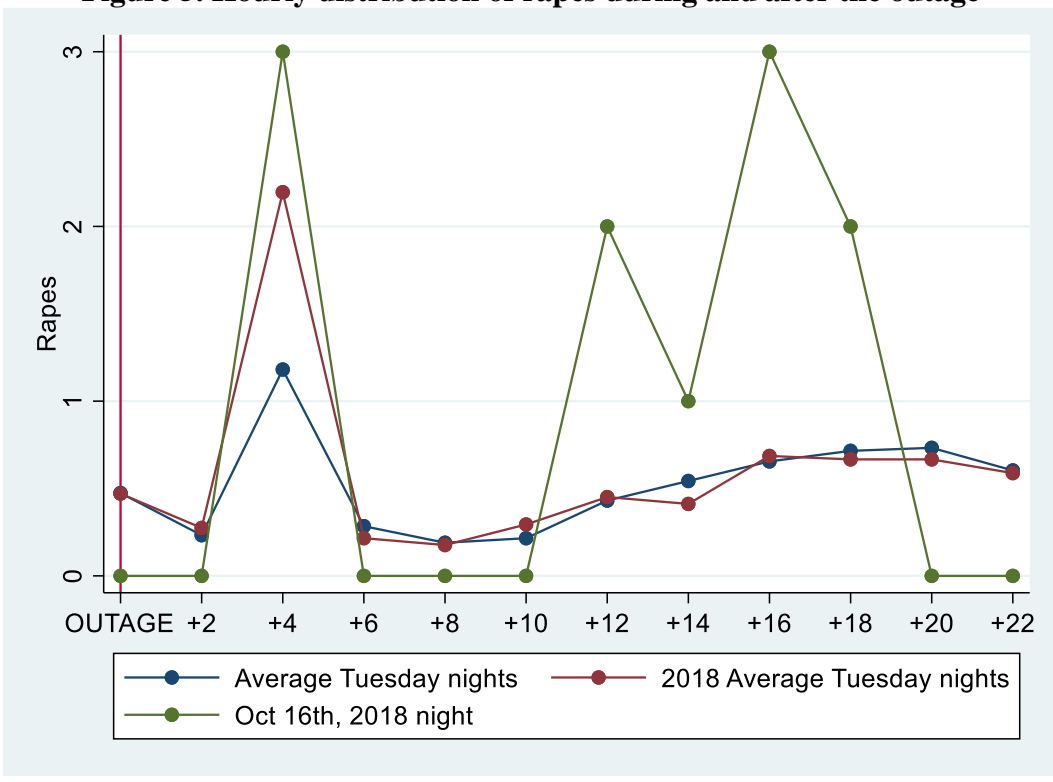
Source: Own elaboration. Notes: Figure 3 displays estimates of equation (2) using the 4 Tuesdays around October 16th as placebo treatments. In all cases we estimate the model in column (5) in Table 3. In the horizontal axis, 0 corresponds to October 16th, 2018, whereas -1, -2, 1, and 2, are placebo treatments obtained by rolling the treatment artificially back and forward 1 week and 2 weeks, respectively.

Figure 4. Placebo analysis: 6 days before and 6 days after



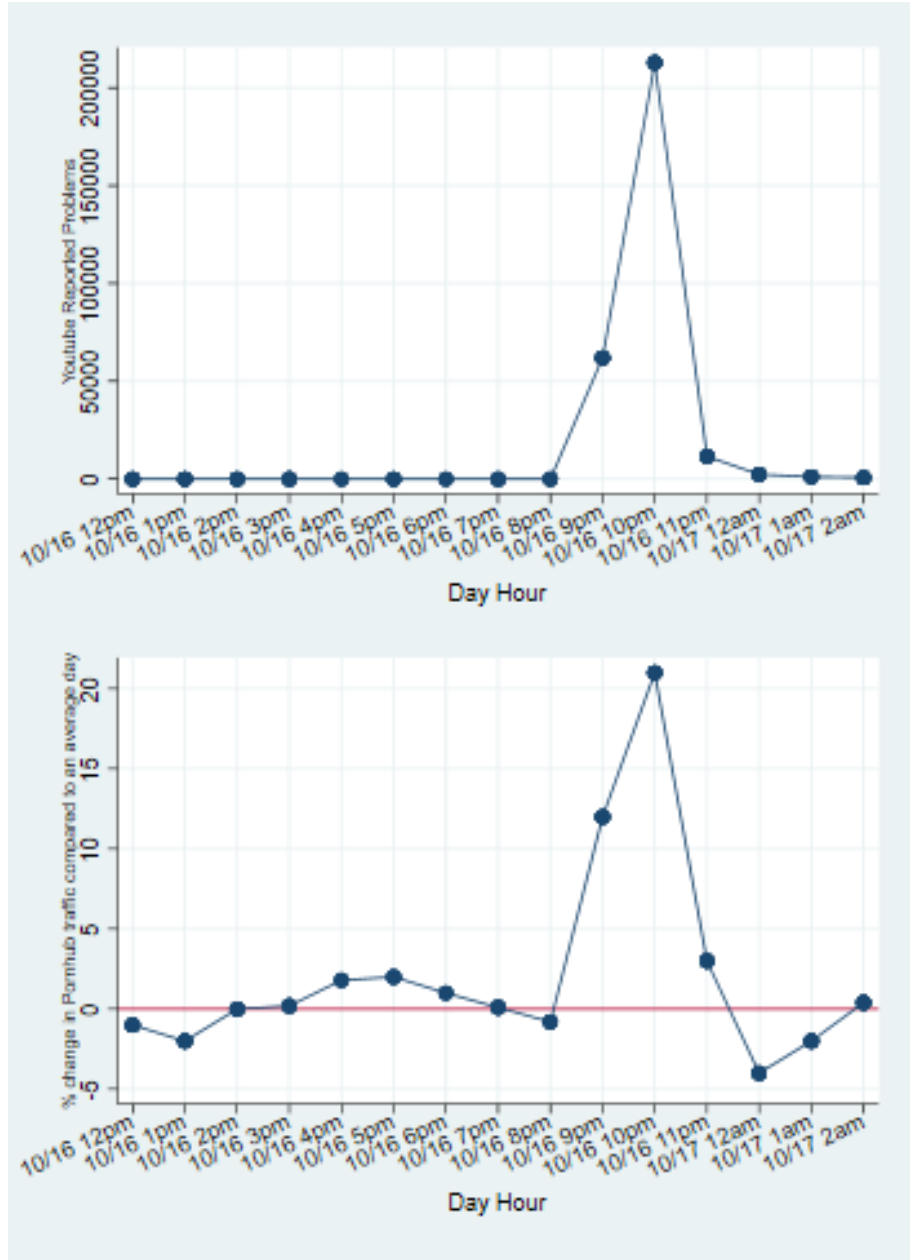
Source: Own elaboration. Notes: Notes: Figure 3 displays estimates of equation (2) using the 12 days around October 16th as placebo treatments. In all cases we estimate the model in column (5) in Table 3. In the horizontal axis, 0 corresponds to October 16th, 2018, whereas -1 to -6 and 1 to 6 are placebo treatments obtained by rolling the treatment artificially back and forward 1 to 6 days, respectively.

Figure 5. Hourly distribution of rapes during and after the outage



Source: Own elaboration.

Figure 6. YouTube outage and the increase in Pornhub's traffic



Source: Own elaboration, based upon data obtained from Downdetector and Pornhub (www.pornhub.com/insights/youtube-outage). Downloaded on December 22nd, 2018.

VIII. Appendix

Theoretical framework: pornography viewing and rapes

We focus on the behavior of potential male sexual offenders.²¹ Our model has 2 stages. In the first stage, the agent decides how much pornography to consume subject to his time constraint. In the second stage, the agent decides whether or not to rape taking into account the costs and benefits associated with raping. Important to our setting is that in the first stage the agent is unable to predict his future behavior perfectly if he were sexually aroused in the second stage. The behavioral economics literature names this as the hot-cold empathy gap, a cognitive bias in which individuals underestimate the influences of visceral factors (such as sexual arousal) on their own future behavior (Loewenstein 2000).

Formally, sexual arousal of individual i (v_i) depends on the consumption of pornography by individual i , X_{iP} . We assume that sexual arousal increases with the consumption of pornography, $v_i'(X_{iP}) > 0$. An agent is in “hot” or “cold” mode depending on whether sexual arousal is above or below a personal threshold, \bar{v}_i . An agent is in hot mode if $v_i(X_{iP}) \geq \bar{v}_i$, and he is in cold mode if $v_i(X_{iP}) < \bar{v}_i$.

We assume that in the first stage, being in cold mode, the agent naively predicts that in the second stage his sexual arousal will always be below his personal threshold (i.e, that in the second stage, he will always be in cold mode). Under this assumption, in the first stage, the agent solves the following maximization problem, where X_{iY} is YouTube consumption, X_{iO} is the consumption of all other leisure activities, and L_i is leisure endowment (X_{iP} , X_{iY} , X_{iO} , and L_i are measured in hours):

²¹ Since males are by far the predominant perpetrators of rapes as well as the biggest consumers of pornography (see, for example, Russell 1984), we are calling the offender a “he.”

$$\max U(X_{iP}; X_{iY}; X_{iO}), \text{ s.t. } X_{iP} + X_{iY} + X_{iO} \leq L_i, \text{ for } i = 1, \dots, N.$$

The agent solves this problem and chooses the optimal bundle of leisure consumption, including the optimal consumption of pornography (X_{iP}^*).²²

In the second stage, the agent decides whether or not to rape conditional on the amount of pornography viewing chosen in the previous stage. According to the rational crime model (Becker 1968), the agent decides whether or not to rape by comparing the costs and benefits of raping. In our model, we follow the behavioral economics literature and we assume that being in hot mode affects both perceived costs and benefits of raping: it decreases the perceived cost of being caught (see Nagin 2008; Van Winden and Ash (2012) and increases the utility from raping (Loewenstein 2000).²³

For simplicity, we normalize the utility of not raping at zero. Thus, the agent rapes if the utility from raping is greater than zero. Formally, the agent rapes if

$$U(\text{Rape}) = \alpha + \beta \mathbf{1}(v_i(X_{iP}^*) \geq \bar{v}_i) - (c - \delta \mathbf{1}(v_i(X_{iP}^*) \geq \bar{v}_i)) > 0,$$

where $\mathbf{1}(v_i(X_{iP}^*) \geq \bar{v}_i)$ is an indicator that takes the value one if the agent is in hot mode, α, β , and δ are parameters greater than zero, c is the agent's expected cost of being caught (includes the probability of being caught and the length of the sentence), and $(c - \delta \mathbf{1}(v_i(X_{iP}^*) \geq \bar{v}_i))$ is the agent's perceived cost of being caught. We assume $\alpha < c$ and $\alpha + \beta + \delta > c$.

In the cold mode, $v_i(X_{iP}^*) < \bar{v}_i$, and $U(\text{Rape}) = \alpha - c$. Since $\alpha < c$, in the cold mode the agent decides not to rape. In the hot mode the agent rapes since $v_i(X_{iP}^*) \geq \bar{v}_i$, and $U(\text{Rape}) = \alpha + \beta - c + \delta > 0$.

²² We assume local non-satiation so that the time constraint will hold with equality.

²³ In general, visceral factors determine the trade-off between different goods and activities; thirst, for example, increases one's preference for water, and sexual arousal increases one's preference for having sex (Loewenstein 2000).

In terms of our model, YouTube outage implies an additional restriction to the optimization problem: $X_{iY} = 0$. This implies that, in equilibrium, some agents end up consuming more pornography, thus increasing the probability of being in hot mode.

In sum, YouTube outage decreases the opportunity cost of pornography viewing relative to alternative activities, thus potentially increasing the equilibrium level of pornography viewing. The increase in pornography viewing leads to some agents crossing their sexual arousal threshold. Those agents that cross the threshold end up raping.

References to the appendix

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Chapter II:

Military Conscription, Sexist Attitudes, and Intimate Partner Violence*

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Abstract

We provide empirical evidence on the long-term causal impact of military conscription on sexist attitudes and intimate partner violence. To address potential endogeneity, we exploit the conscription draft lottery in Argentina. We combine the draft administrative data with self-reported survey data. We find that conscripted men are more likely to report embracing more sexist attitudes in dimensions such as justification of sexism and violence, sexual machismo, negative attitude towards homosexuality, old-fashioned sexism, and hostile sexism. We also find that men who served are more likely to self-report engagement in intimate partner violence, as measured by non-physical abuse and physical violence.

Keywords: Military service; sexism; physical violence; non-physical abuse.

JEL classification: K42.

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I. Introduction

We provide empirical evidence on the long-term effect of military conscription on men's self-reported sexist attitudes and behaviors. To identify causality, we exploit the conscription lottery in Argentina that, for almost all of the twentieth century, randomly assigned eligibility of all young males to military conscription based on the last three numbers of their national ID. Our empirical strategy combines the conscription lottery administrative data with self-reported survey data on sexist attitudes and intimate partner violence. We first report that men who served are more likely to embrace sexist attitudes in dimensions such as justification of sexism and violence, sexual machismo, negative attitude towards homosexuality, old fashioned sexism, and hostile sexism. We then take a step further to see if these sexist attitudes are associated with sexist behaviors, and we find that conscripted men are more likely to engage in intimate-partner violence, as measured by non-physical abuse and physical violence.

Military conscription is one of the most widespread policies around the world, affecting men typically in early adulthood.²⁴ Given the vast numbers of people who go through military indoctrination during their formative years, our findings are important to understand the potential effects of military conscription (and military indoctrination) on the formation of sexist attitudes and related behaviors for a sizeable proportion of world's population. Our results suggest a systemic source of misogynistic attitudes and violent behavior – and one that is state-supported, making it particularly insidious.

Our paper lies in the intersection between two previous studies that exploit the Argentine conscription lottery. Galiani, Rossi, and Schargrotsky (2011) find that conscripted men are more likely to develop a criminal record during adulthood, especially for crimes against property and

²⁴ Nowadays, 35 percent of nations have military conscription. Although the age of service varies among different countries, most commonly men are conscripted between the ages of 18 and 20.

white-collar crimes. They do not provide any evidence on crimes related to intimate partner violence. Compared to theirs, our results may be capturing some personal features and behaviors that in more extreme forms and for a much smaller proportion of individuals may also be expressed as involvement in criminal activities. Ertola Navajas et al. (2020) find that being conscripted increases the likelihood of adopting a military mindset. In particular, they report men who were conscripted are less tolerant, more disciplined, more politically conservative, more authoritarian, and more belligerent.

Our research also relates to the literature that studies the impact of military conscription in other countries and on a wide set of outcomes, including criminal behavior (Siminski, Ville, and Paull 2016; Albaek et al. 2017; Lyk-Jensen 2018) and participation in the labor market (Paloyo 2010; Grenet, Hart, and Roberts 2011; Bauer et al. 2012; Card and Cardoso 2012). There is also an important amount of research (starting with Angrist 1990) that exploits the Vietnam-era draft lottery to identify the causal impact of combat exposure on many outcomes. Combat exposure may be, however, a very different intervention compared to peacetime conscription.

Various authors compare the pro-military values of individuals who are in (or planning to follow) a military career against individuals who do not. Goertzel and Hengst (1971) compare Army Reserve Officers' Training Corps (ROTC) cadets with undergraduate students and find that Army cadets score higher on personality scales measuring authoritarianism, misanthropy, intolerance, aggressive nationalism, political-economic conservatism, and belief in imperialism. More recently, Jackson et al. (2012) report that people lower in agreeableness and openness to experience are more likely to enter the military. Closer to our paper, Dahl, Kotsadam, and Rooth (2018) document that men with less gender-equal attitudes select into military service. An obvious drawback of these studies is that people self-select into the military service. Our approach avoids

selection problems by exploiting a well-documented random assignment. To the best of our knowledge, our paper represents the first effort to identify the causal effect of military conscription on sexist attitudes and intimate partner violence.

There is a related psychology literature that focuses on the positive link between sexist attitudes and intimate partner violence. Sakalli (2001) shows that men who score high on hostile sexism view wife-beating as being acceptable and blame women for eliciting domestic violence. Glick et al. (2002) report a positive correlation between sexism (either hostile or benevolent) and attitudes that legitimize abuse.

Finally, our study also relates to the literature that looks at the long-term impacts of events that occur during the impressionable years (for example, Malmendier, Tate, and Yan 2011; Giuliano and Spilimbergo 2014; Cantoni et al. 2017). In line with this literature, our paper shows that major events experienced during early adulthood have life-long effects.

II. Military culture and sexism

Our paper focuses on the role that military conscription may have in the socialization process that influences and shape masculine role definitions, attitudes, and related behaviors. Even though the military organization may have changed in recent years in their image regarding sexism, the analysis of the effect of military training must focus on the traditional processes and images of masculinity upon which the system is based (Arkin and Dobrofsky 1978).

Despite some idiosyncratic differences across countries, the purely masculine surroundings of the military and the values associated with the virility ideal play a determining role in molding soldiers' self-image (Elkin 1946). Mechanisms of social control are constantly operating to

reinforce the appropriate masculine self-image by negating menaces (like showing emotions) or threats (like homosexuality) to that image.²⁵

Several studies provide evidence on the differences in sexist attitudes between the military and the general population. For example, in a recent study, Dahl, Kotsadam, and Rooth (2018) use data from Norway to compare attitudes related to traditional gender roles between military recruits and the general population. They report that men in the military have less gender-egalitarian attitudes compared to the general population and conclude that males with less gender-equal attitudes select into military service.

The differences in sexist attitudes between the military and the general population may explain the observed differences in prevalence rates of intimate partner violence between these two groups. Prevalence rates of intimate partner violence among active-duty servicemen and veterans range from 13.5% to 58% (Marshall, Panuzio, and Taft 2005). These relatively high rates are sometimes rationalized as explained by possible over-representation of specific forms of psychopathology. However, studies using military samples not selected on the basis of psychopathology find intimate partner violence perpetration rates that are one to three times higher than rates found in studies of the general population (Straus and Gelles 1990; Marshall, Panuzio, and Taft 2005).

III. Military conscription in Argentina

Military conscription in Argentina was mandatory between 1901 and 1994. The length of service was at least one year in the Army and the Air Force, and up to two years in the Navy.

²⁵ According to Williams and Weinberg (1971), the official reasons given by the army and the navy for fearing homosexuals are that “(T)he Army considers homosexuals to be unfit for military service because their presence impairs the morale and discipline of the Army, and that homosexuality is a manifestation of a severe personality defect which appreciably limits the ability of such individuals to function effectively in society... Homosexuals and other sexual deviates are military liabilities that cannot be tolerated in a military organization.”

These services began with a three-month instruction period where recruits learned military norms and were exposed to military training. Following the initial training, conscripts were allocated to a military unit to perform a specific duty, which not necessarily involved military tasks.²⁶

From 1901 to 1976, males served at the age of 21; later, this was modified to age 18. The cohort born in 1955 was the last to serve at age 21, and the cohort born in 1958 was the first to serve at age 18. The cohort born in 1975 was the last that served, as conscription was abolished in December 1994.²⁷ Our analysis focuses on all cohorts that served at age 18, that is, on cohorts born between 1958 and 1975. Males in these cohorts were eligible to serve in the period 1976 to 1994, and thus our empirical analysis identifies the long-term effects of being exposed to military conscription.

The eligibility of young males for military service was determined through a public lottery and based on the last 3 digits of their national IDs, a unique number assigned at birth to every citizen. Each year a lottery assigned a number between 1 and 1,000 to each combination of the last 3 ID digits. The lottery system was run in a public session using a lottery drum filled with a thousand balls numbered 1 to 1,000. The first ball released from the lottery drum corresponded to ID number 000, the second released ball to ID number 001, and so on. The random assignment was administered by the National Lottery and supervised by the National General Notary in a public session. Results were broadcasted over the radio and published in major newspapers.

Following the lottery, all men were called to have mental and physical examinations. Later on, the government announced a cut-off number. Individuals whose ID number had been assigned a lottery number higher than the cut-off number (and who had also passed the mental and physical

²⁶ For more details on military conscription in Argentina, see Rodriguez Molas (1983), Galiani, Rossi, and Schargrofsky (2011), and Ertola Navajas et al. (2020).

²⁷ A small proportion of recruits from the cohorts born in 1962 and 1963 participated in the Malvinas War. All results are robust to excluding the cohorts 1962 and 1963.

examinations) were mandatorily called to military conscription. In the population, 47.7% of men born between 1958 and 1975 were draft eligible, and approximately 60% of draft-eligible men were actually conscripted. Those individuals whose ID number was below the cut-off could serve as volunteers, though the number of volunteers was not high (in our cohorts, approximately 3.5%).

IV. Data and the survey

We measure men's sexist attitudes and sexist behaviors using a confidential web-based survey conducted in April and May 2020.²⁸ We hired a polling and market research firm that sent an e-mail invitation to participate in the survey to an e-mail list of approximately 29,500 men. To participate in the survey, men had to be born between 1958 and 1975, in Argentina. We received 1,219 completed and valid surveys.

The call to answer the survey did not mention military conscription or sexism.²⁹ To encourage participation on the survey, participants were included in a raffle for smartphones. Participants entered the raffle with their last three ID digits. Asking for the last three ID digits to participate in raffles is a common practice in Argentina, so there is no reason to expect participants to associate the request of the last three ID digits with military conscription.

Survey questions

Our survey measures five metrics of attitudes (negative attitude towards homosexuality, justification of sexism and violence, sexual machismo, old fashioned sexism, and hostile sexism), and two metrics of intimate partner violence (non-physical abuse and physical violence).³⁰ All metrics are constructed from a set of statements obtained from specialized literature: (i) the module

²⁸ The English version of the survey is presented in the appendix.

²⁹ The English version of the recruitment e-mail is presented in the appendix.

³⁰ Justification of sexism and violence (Díaz-Aguado and Carvajal 2011), sexual machismo (Díaz Rodríguez, Rosas Rodríguez and González Ramírez 2010), negative attitude towards homosexuality (Zuckerman 1998), old-fashioned sexism (Swim et al. 1995), hostile sexism (Glick and Fiske 1997), non-physical abuse (Garner and Hudson 1992), and physical violence (Straus et al. 1996).

on attitudes toward homosexuality e consists of questions regarding the rights of homosexuals to marry or adopt children and whether homosexuals are “normal” or psychiatrically disturbed; (ii) the module on justification of sexism and violence comes from a survey that aims to capture different factors: domestic violence as a private matter, justification of violence as a reaction (victim’s fault), and defense of traditional sexist attitudes; (iii) sexual machismo is the belief that men are superior, which leads to sexual behaviors that could risk the sexual health and physical well-being of the individual and his partner. The module on sexual machismo aims to evaluate male sexist attitudes and beliefs from a sexual perspective; (iv) the module on hostile sexism aims to capture dominative paternalism and competitive gender differentiation, that in a more egalitarian social context, are reflected as resistance against women who want “too much” power (for example, feminists), and in the belief that women cannot succeed in men’s roles unless given preferential treatment; (v) the module on old-fashioned measures old-fashioned prejudices, endorsing traditional gender roles, unequal treatment, and stereotypes about lower female competence.

All metrics are constructed from a set of statements obtained from specialized literature. We follow the literature and group the answers to get a single value for each metric. For question on attitudes, the respondents indicate how much they agree or disagree with each statement, on a scale ranging from “Totally disagree” to “Totally agree.” For questions on intimate partner violence, the respondents indicate the frequency on a scale ranging from “Never” to Always.” In all cases, we follow the Likert scale used by the original authors.

From the survey, we also obtained self-reported information on year of birth, conscription status, and pre-treatment characteristics (province of origin, parents’ education, parents’ nationality, and father’s conscription status).

Using the self-reported last three ID digits, year of birth, the lottery draft results, and the cut-off numbers by cohort, we construct the dummy variable *Draft eligible*, which takes the value of one for men whose last three ID digits obtained a lottery draft number above the cut-off, and zero otherwise. We also construct the treatment variable *Conscription*, which takes the value of one for men who report being conscripted, and zero otherwise.

Table 1 reports summary statistics of the data. We allowed participants to skip questions on physical violence since they could feel upset or uncomfortable for revealing an illegal behavior. Five participants skipped that question.

We check the representativeness of the sample in all pre-treatment variables for which there is population information available. Table 2 compares our sample and the population in pre-treatment parents' nationality and pre-treatment province of origin. Population and sample proportions in parents' nationality are statistically indistinguishable from zero. For 20 out of 22 pre-treatment provinces of origin, the differences between population and sample proportions are statistically indistinguishable from zero. Finally, Figure 1 compares our sample and the population in pre-treatment parents' education. As observed in the figure, the population with low education is under-represented in our sample.

Interpretation of survey responses

The survey was anonymous and conducted online, so there is no reason to expect social stigma attached to particular responses or any changes in answers due to cues about what constitutes appropriate behavior.

The response rate to our survey is 4.13%. A natural concern in this context is potential selection into the sample. If selection into the sample were nonrandom, our estimated treatment effects might be biased. For nonrandom selection into our sample to threaten the internal validity

of our estimates, the selection would need to be differential by draft-eligibility status. We test for differential selection into the survey by draft-eligibility status in five ways.

First, we examine whether the sample proportion of draft-eligible in our sample is similar to the population proportion. Table 3 reports population and sample proportions of draft eligibility, by cohort. For the 17 out of 18 cohorts, the difference between population and sample proportions of draft-eligible is statistically indistinguishable from zero.

Second, we check whether the sample distribution of the last three ID digits in our sample is similar to the population (uniform) distribution. In Figure 2, we display the sample distribution of the last three ID digits, grouping the last three ID digits in bins of 100 consecutive numbers (10 bins of 100 numbers each). The sample distribution looks like a uniform distribution, and we cannot reject the hypothesis that the sample distribution of the last three ID digits is statistically not different from a uniform distribution.

Third, we check whether the sample distribution of the lottery numbers in our sample is similar to the population (uniform) distribution. Again, we first display the sample distribution of the lottery number, grouped in bins of 100 consecutive numbers. As shown in Figure 3, the sample distribution of the lottery numbers looks like a uniform distribution. In addition, we cannot reject the hypothesis that the sample distribution is statistically not different from a uniform distribution.

Fourth, even though eligibility to serve in the conscription was randomly determined, we examine whether individuals' pre-treatment characteristics are balanced across the draft-eligible and the draft-exempted groups within our sample. Table 4 reports differences in parents' education, parents' nationality, and whether his father served in the conscription, by draft-eligibility status. For 10 out of 11 pre-treatment characteristics available, there are no statistically significant differences between the draft-eligible and the draft-exempted groups. In addition, Table

5 reports differences, by draft-eligibility status, in the pre-treatment province of origin. For all the 22 pre-treatment provinces of origin, there are no statistically significant differences between the draft-eligible and the draft-exempted groups.

Fifth, we look at within-survey attrition. The proportion of those that started the survey but did not complete it is low (9.10%). In addition, attrition is orthogonal to draft-eligibility assignment: the proportion of attrition is 9.40% in the draft-eligible group, 8.84% in the draft-exempted group, and the difference between these two proportions is statistically not significant.

Since (i) population and sample proportion of draft-eligible are statistically indistinguishable, (ii) the sample distribution of the last three ID digits is statistically not different from the population (uniform) distribution, (iii) the sample distribution of lottery numbers is statistically not different from the population (uniform) distribution, (iv) pre-treatment characteristics are balanced within our sample, and (v) attrition is low and orthogonal to draft-eligibility status, we conclude results reported below are not subject to significant sources of selection bias.³¹

V. Econometric methods and results

We examine the causal effect of conscription on sexism in a regression framework. Formally, we want to estimate the following equation:

$$Y_{ic} = \beta + \alpha \text{Conscription}_{ic} + \gamma X_{ic} + \delta_c + \varepsilon_{ic} \quad (1)$$

where Y_{ic} is a given outcome for individual i from birth cohort c , Conscription_{ic} is a dummy variable that takes the value of one for those individuals who served, X_{ic} is a vector of individuals' pre-

³¹ Our survey data was collected at the time of a national lockdown implemented in Argentina with the objective to prevent the spread of the coronavirus. The lockdown took place in an environment where few people really felt threatened by the disease. As a consequence of the lockdown, some men were placed in quarantine while others were not (in our sample, 65% of men report being in quarantine). Important for identification purposes, quarantine status is orthogonal to draft-eligibility assignment.

treatment characteristics, δ_c is a cohort fixed effect, and ε_{ic} is an error term. The coefficient of interest is α , which we expect to be positive for all outcomes. In all estimates, we cluster standard errors at the ID-cohort level.

The outcomes are sexist attitudes, non-physical abuse, and physical violence. In order to draw general conclusions in the context of multiple metrics on sexist attitudes, we construct an index that aggregates the five metrics. The index of sexist attitudes is the equally-weighted average of the z-scores of its five components (for more details, see Kling, Liebman, and Katz 2007). A higher z-score is associated with being more sexist. We also report effects on each separate metric.

Conscription may be endogenous in equation (1) due to reverse causality, self-selection, and unobserved heterogeneity. To address potential endogeneity biases, we estimate equation (1) by Two-Stage Least Squares (2SLS), where we use *Draft eligible* as an instrument for *Conscription*. The 2SLS estimator recovers the average treatment effect for draft-lottery compliers, that is, for those who served in the military because they were assigned a high lottery number but would not have served otherwise. Thus, 2SLS estimates do not generalize to the population of volunteers or to the population of young men who, under no circumstances, would have passed the pre-induction medical examination.

Table 6 reports first-stage estimates, with and without controls.³² The point estimates of the coefficients on *Draft eligible* indicate that the probability of being conscripted is 37.9 percentage points higher for men in the draft-eligible group than for those in the draft-ineligible group. First-stage effects are precisely estimated and significantly different from zero.³³

³² In the Appendix we present the first-stage estimates for cohorts 1958 to 1965 and for cohort 1966 to 1975, separately. We also report the results for Tables 7 and 8 for each group of cohorts.

³³ Our first-stage estimates are similar to the ones reported in Ertola Navajas et al. (2020) and smaller to the ones reported in Galiani, Rossi, and Schargrotsky (2011). While Ertola Navajas et al. (2020) use the same cohorts as we do (1958 to 1975), Galiani, Rossi, and Schargrotsky (2011) analysis is restricted to cohorts 1958 to 1962. If we restrict to cohorts 1958 to 1962, our first-stage estimates are similar to the ones in Galiani, Rossi, and Schargrotsky (2011).

As a benchmark, in panel A of Table 7, we report OLS estimates of equation (1). OLS estimates indicate that men who served have more sexist attitudes than those that did not serve.³⁴ This result holds for the index of sexist attitudes and all of its separate metrics.

Panel B in Table 7 reports our main (2SLS) estimates. The estimated coefficient in column (1) is positive and statistically significant at the 5% level, indicating that being conscripted significantly increases sexist attitudes. The value of the coefficient implies that the index of sexist attitudes is 0.44 standard deviations higher for conscripted men.

To determine whether the effects of military conscription on sexist attitudes are wide-ranging or concentrated on a few outcomes, we estimate the effects on each separate metric. The effect appears quite general. For all five metrics, the point estimates have the expected signs, and 4 of them are statistically significant. The size differences among sexist attributes are important. Regarding 2SLS estimates, sexual machismo is 0.33 standard deviations higher for conscripted men, old-fashioned sexism is 0.40 standard deviations higher, hostile sexism is 0.32 standard deviations higher, and the probability of justifying violent behaviors goes up by 0.50 standard deviations for conscripted men. The percentage increase with respect to the mean of the non-conscripted men is 35%, 52%, 16%, and 70%, respectively.

We then take a step further and we ask whether military conscription increases intimate partner violence. As shown in Table 8, the answer is yes. Men who were conscripted are more prone to self-report engagement in non-physical abuse and physical violence. The size differences are important. Non-physical abuse and physical violence are 0.48 standard deviations higher for conscripted men.

Interpreting the treatment effect

³⁴ In all cases, we obtain similar results in regression models without controls. All results mentioned and not shown are available from the authors upon request.

Military conscription may affect intermediate outcomes (e.g., criminal records, as in Galiani, Rossi, and Schargrodsky 2011) that themselves may play a causal role in shaping attitudes and behavior. An effect of randomly assigned conscription on misogynistic attitudes and behavior working through changed criminal records (or other channels) would have quite a different interpretation from the effect working through military exposure alone. Therefore, identifying a pure “military exposure effect” is challenging.

Even though our natural experiment does not identify the mechanisms through which military conscription affects misogynistic attitudes and violent behavior, the magnitudes of our estimated effects compared to the ones in Galiani, Rossi, and Schargordsky (2011) suggest that the change in misogynistic attitudes and violent behavior found here cannot be explained by the increase in criminal records alone.

An additional issue on the interpretation of our findings is related to the fact that we are asking survey respondents to self-report on their own abuse of domestic partners. As usual with self-reported data, the assumption is that reporting is orthogonal to the assignment. In our case, this assumption may be challenge, for example, if conscripted men are less likely to lie about their sexist attitudes and behaviors.³⁵

VI. Final remarks

We provide novel evidence on the role military conscription has on subsequent men’s sexist attitudes and intimate partner violence. Our empirical strategy combines administrative data on the conscription lottery in Argentina with self-administered survey data. We find strong evidence that military experience in the conscription causes men to adopt more misogynistic attitudes and

³⁵ Some authors argue that since violence and power are elements of military normality, domestic violence could be legitimated by its socially constructed gender relations, and this might lead to higher reporting. On the other hand, the military culture makes a strong separation between public and private practices, and domestic abuse is a form of violence which could be ‘continually privatised’ (see Gray 2016).

to report engaging in more acts of domestic abuse. The magnitudes of the estimated effects are both statistically significant and quite large.

Our findings have important policy implications. Many countries (mostly European, such as Italy, Romania, France, and Germany) are currently evaluating the reintroduction of some kind of military conscription as a policy tool to address multiple purposes, such as producing men that can potentially serve in military conflicts, keeping young men off the streets (so to reduce involvement in criminal activity), improving young men subsequent inclusion into society, etc. Our results are useful for a better understanding of the overall effects of this policy tool.

The military culture, despite some occasional national differences, is similar in most countries around the world (Soeters 1997). This suggests that our results from Argentina are likely to be valid in other countries and contexts as well, regardless of the specific type of instruction received by recruits.

VII. References

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VIII. Tables and Figures

Table 1. Summary statistics

	Obs.	Mean	SD	Min.	Max.
Conscription	1,219	0.28	0.45	0.00	1.00
Draft eligible	1,219	0.46	0.50	0.00	1.00
Negative attitude towards homosexuality	1,219	0.27	0.26	0.00	1.00
Hostile sexism	1,219	0.48	0.23	0.00	1.00
Old-fashioned sexism	1,219	0.15	0.18	0.00	1.00
Justification of violence	1,219	0.13	0.16	0.00	1.00
Sexual machismo	1,219	0.17	0.17	0.00	1.00
Index of sexist attitudes	1,219	0.07	0.87	-1.20	4.64
Physical violence	1,214	0.01	0.04	0.00	0.56
Non-physical abuse	1,219	0.09	0.12	0.00	0.83
Father's country of birth	1,219	0.88	0.32	0.00	1.00
Mother's country of birth	1,219	0.91	0.29	0.00	1.00
Father served in conscription	1,219	0.62	0.49	0.00	1.00
Father: no instruction or incomplete primary	1,219	0.12	0.33	0.00	1.00
Father: complete primary school	1,219	0.31	0.46	0.00	1.00
Father: complete high school	1,219	0.26	0.44	0.00	1.00
Father: complete university or more	1,219	0.28	0.45	0.00	1.00
Mother: no instruction or incomplete primary	1,219	0.12	0.32	0.00	1.00
Mother: complete primary school	1,219	0.35	0.48	0.00	1.00
Mother: complete high school	1,219	0.29	0.45	0.00	1.00
Mother: complete university or more	1,219	0.24	0.42	0.00	1.00

Table 2. Representativeness: parents' nationality and province of origin

	Population proportion	Sample proportion	Difference
<i>Parents' nationality</i>			
Father's country of birth	0.896	0.884	0.013
Mother's country of birth	0.917	0.906	0.010
<i>Province of origin</i>			
Buenos Aires	0.500	0.554	-0.054***
Catamarca	0.008	0.005	0.003
Chaco	0.025	0.019	0.006
Chubut	0.010	0.009	0.001
Cordoba	0.075	0.071	0.004
Corrientes	0.024	0.018	0.006
Entre Rios	0.033	0.026	0.007
Formosa	0.011	0.007	0.004
Jujuy	0.015	0.011	0.004
La Pampa	0.008	0.008	-0.000
La Rioja	0.006	0.005	0.001
Mendoza	0.043	0.041	0.002
Misiones	0.021	0.016	0.005
Neuquen	0.009	0.009	-0.000
Rio Negro	0.014	0.019	-0.005
Salta	0.024	0.023	0.001
San Juan	0.017	0.012	0.005
San Luis	0.008	0.006	0.002
Santa Cruz	0.005	0.005	0.000
Santa Fe	0.089	0.084	0.005
Santiago del Estero	0.022	0.015	0.007**
Tucuman	0.035	0.039	-0.004

Notes: Data obtained from Argentine Census 2010. Santa Cruz includes the former National Territory of Tierra del Fuego. Buenos Aires includes both the city and the province. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 3. Draft-eligibility status, by cohort

Cohort	Sample size	Population proportion of draft eligible	Sample proportion of draft eligible	Difference
1958	42	0.827	0.833	0.006
1959	74	0.682	0.716	0.034
1960	64	0.661	0.578	-0.083
1961	80	0.652	0.563	-0.090
1962	66	0.682	0.742	0.060
1963	57	0.652	0.596	-0.056
1964	66	0.602	0.621	0.019
1965	65	0.620	0.523	-0.097
1966	64	0.391	0.438	0.046
1967	60	0.326	0.283	-0.043
1968	65	0.410	0.385	-0.025
1969	62	0.442	0.532	0.090
1970	70	0.505	0.486	-0.019
1971	76	0.257	0.355	0.098*
1972	77	0.179	0.169	-0.010
1973	71	0.236	0.324	0.088
1974	80	0.219	0.188	-0.032
1975	80	0.243	0.200	-0.043
Total	1219	0.477	0.459	-0.018

Notes: The population of draft eligible by cohort was obtained from the Argentine Army. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 4. Pre-treatment characteristics, by draft-eligibility assignment

	Draft eligible mean	Non draft eligible mean	Difference
Father's country of birth	0.878 (0.327)	0.888 (0.316)	-0.010 [0.018]
Mother's country of birth	0.909 (0.288)	0.905 (0.294)	0.004 [0.017]
Father served in conscription	0.610 (0.488)	0.621 (0.485)	-0.011 [0.028]
Father: no instruction or incomplete primary	0.131 (0.337)	0.118 (0.323)	0.012 [0.019]
Father: complete primary school	0.326 (0.469)	0.302 (0.459)	0.024 [0.027]
Father: complete high school	0.249 (0.433)	0.271 (0.445)	-0.023 [0.025]
Father: complete university or more	0.272 (0.445)	0.285 (0.452)	-0.013 [0.026]
Mother: no instruction or incomplete primary	0.118 (0.323)	0.115 (0.319)	0.003 [0.018]
Mother: complete primary school	0.363 (0.481)	0.335 (0.472)	0.028 [0.027]
Mother: complete high school	0.301 (0.459)	0.277 (0.448)	0.023 [0.026]
Mother: complete university or more	0.208 (0.406)	0.261 (0.439)	-0.053** [0.024]

Notes: Standard deviations are shown in parentheses. Standard errors are shown in brackets.

*Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 5. Province of origin, by draft-eligibility assignment

	Draft eligible mean	Non draft eligible mean	Difference
Buenos Aires	0.562 (0.497)	0.547 (0.498)	0.015 [0.029]
Catamarca	0.005 (0.073)	0.005 (0.067)	0.001 [0.004]
Chaco	0.014 (0.119)	0.023 (0.149)	-0.008 [0.008]
Chubut	0.013 (0.111)	0.006 (0.078)	0.006 [0.005]
Cordoba	0.061 (0.239)	0.079 (0.270)	-0.018 [0.015]
Corrientes	0.021 (0.145)	0.015 (0.122)	0.006 [0.008]
Entre Rios	0.027 (0.162)	0.026 (0.159)	0.001 [0.009]
Formosa	0.007 (0.084)	0.008 (0.087)	-0.000 [0.005]
Jujuy	0.014 (0.119)	0.008 (0.087)	0.007 [0.006]
La Pampa	0.009 (0.094)	0.008 (0.087)	0.001 [0.005]
La Rioja	0.005 (0.073)	0.005 (0.067)	0.001 [0.004]
Mendoza	0.047 (0.211)	0.036 (0.187)	0.010 [0.011]
Misiones	0.013 (0.111)	0.018 (0.134)	-0.006 [0.007]
Neuquen	0.013 (0.111)	0.006 (0.078)	0.006 [0.005]
Rio Negro	0.013 (0.111)	0.024 (0.154)	-0.012 [0.008]
Salta	0.025 (0.156)	0.021 (0.144)	0.004 [0.009]
San Juan	0.011 (0.103)	0.014 (0.116)	-0.003 [0.006]
San Luis	0.005 (0.073)	0.006 (0.078)	-0.001 [0.004]
Santa Cruz	0.005 (0.073)	0.005 (0.067)	0.001 [0.004]
Santa Fe	0.077 (0.267)	0.089 (0.286)	-0.012 [0.016]
Santiago del Estero	0.016 (0.126)	0.014 (0.116)	0.002 [0.007]
Tucuman	0.038 (0.190)	0.039 (0.195)	-0.002 [0.011]

Notes: Santa Cruz includes the former National Territory of Tierra del Fuego. Buenos Aires includes both the city and the province. Standard deviations are shown in parentheses. Standard errors are shown in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 6. First-stage estimates

	Conscription	
	(1)	(2)
Draft eligible	0.379*** (0.024)	0.377*** (0.024)
F-Test	232.13 {0.00}	231.42 {0.00}
Controls	No	Yes
Observations	1,219	1,219

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. All models are estimated by OLS and include cohort dummies. The set of controls includes province of origin dummies and all variables listed in Table 4. F-test is the F-test of excluded instruments (p-values are shown in braces). *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 7. Impact of conscription on sexist attitudes

Panel A	Index of sexist attitudes (1)	Negative attitude towards homosexua- lity (2)	Sexual machismo (3)	Justification of sexism & violence (4)	Old- fashioned sexism (5)	Hostile sexism (6)
Conscription	0.344*** (0.072)	0.048** (0.019)	0.050*** (0.014)	0.055*** (0.013)	0.071*** (0.015)	0.060*** (0.017)
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS
Observations	1,219	1,219	1,219	1,219	1,219	1,219
Panel B	Index of sexist attitudes (7)	Negative attitude towards homosexua- lity (8)	Sexual machismo (9)	Justification of sexism & violence (10)	Old- fashioned sexism (11)	Hostile sexism (12)
Conscription	0.403*** (0.140)	0.047 (0.043)	0.056** (0.028)	0.080*** (0.025)	0.073** (0.029)	0.074* (0.040)
Mean of dependent variable	0.07	0.27	0.17	0.13	0.15	0.48
Estimation method	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Observations	1,219	1,219	1,219	1,219	1,219	1,219

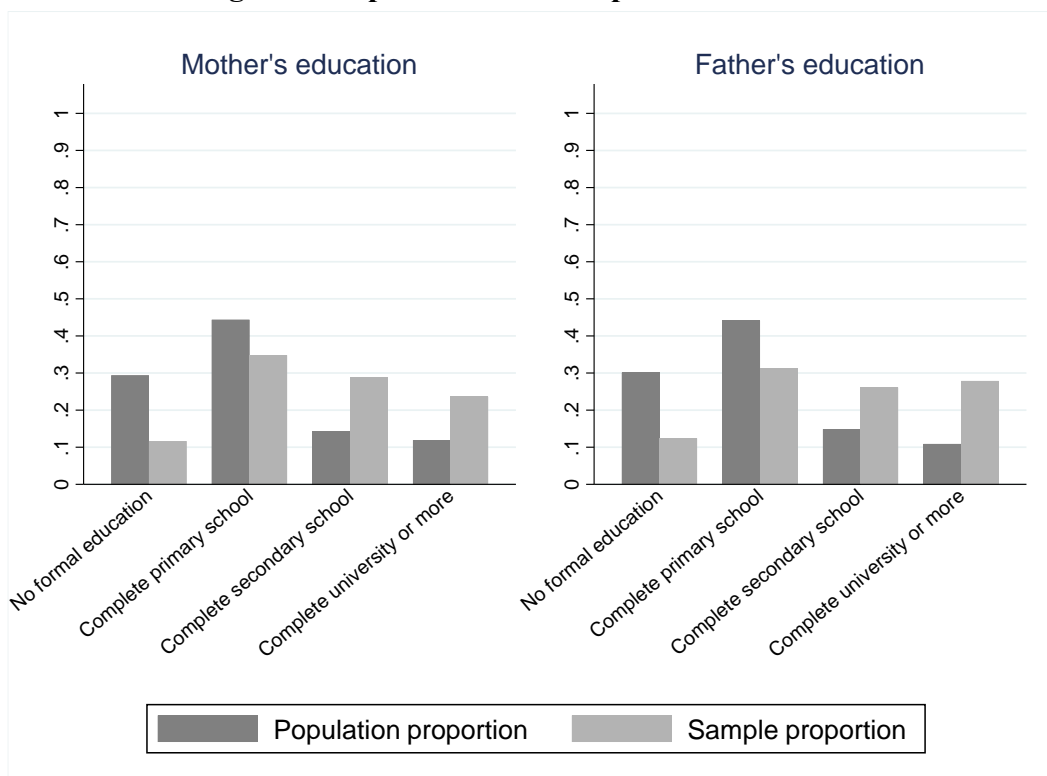
Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. All models include cohort dummies, province of origin dummies, and the set of pre-treatment characteristics listed in Table 4. In 2SLS models, Conscription is instrumented using Draft eligible. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 8. Impact of conscription on intimate partner violence

	Non-physical abuse (1)	Physical violence (2)	Non-physical abuse (3)	Physical violence (4)
Conscription	0.033*** (0.010)	0.010** (0.004)	0.056*** (0.019)	0.020** (0.008)
Mean of dependent variable	0.09	0.01	0.09	0.01
Estimation method	OLS	OLS	2SLS	2SLS
Observations	1,219	1,214	1,219	1,214

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. All models include cohort dummies, province of origin dummies, and the set of pre-treatment characteristics listed in Table 4. In 2SLS models, Conscription is instrumented using Draft eligible. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Figure 1. Representativeness: parents' education



Notes: Data obtained from Argentine Census 2010 (education 60+ year-old men and women in 2010).

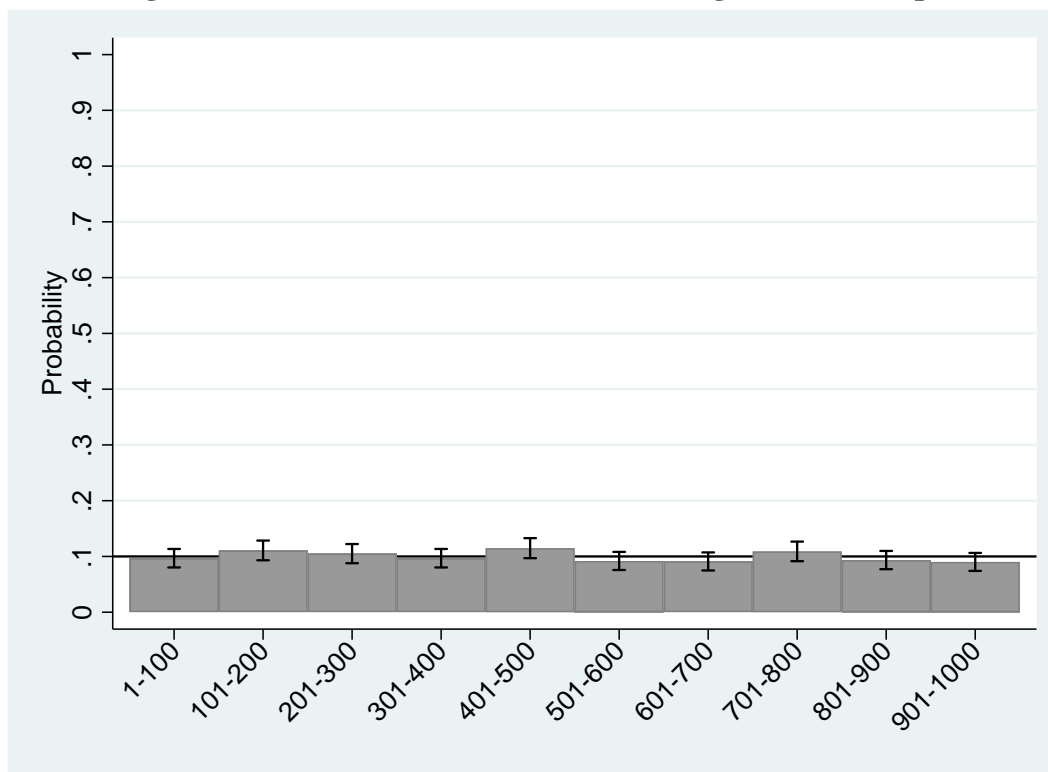
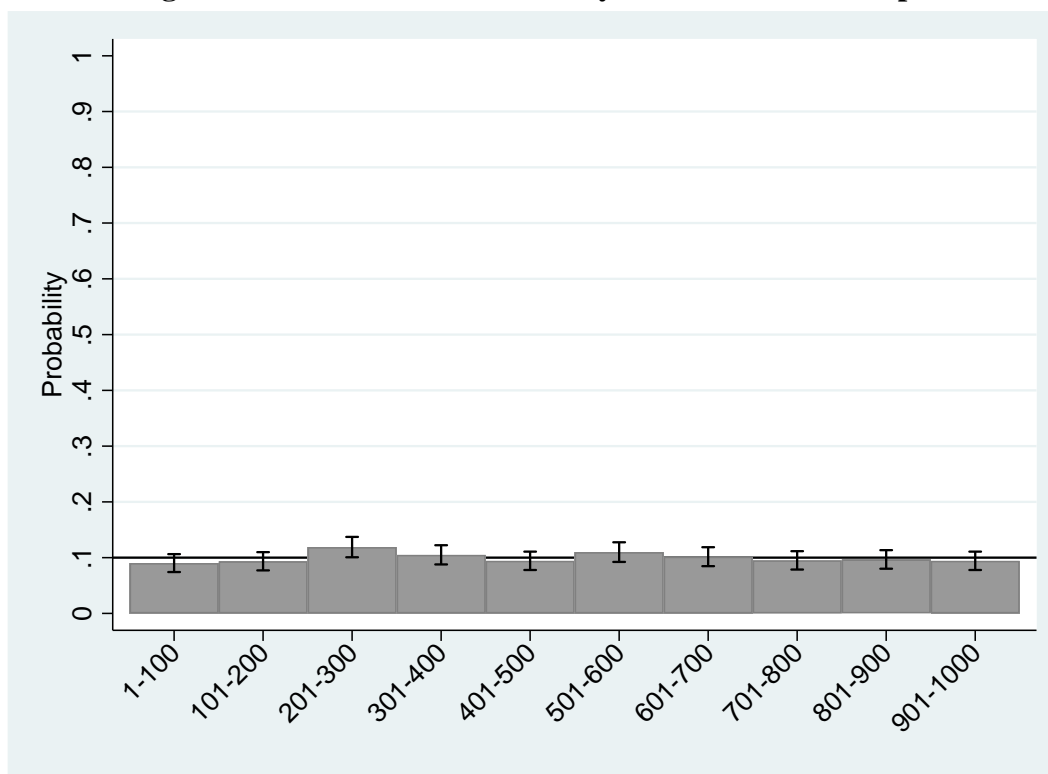
Figure 2. Distribution of the last three ID digits in our sample

Figure 3. Distribution of the lottery numbers in our sample

IX. Appendix

Invitation to answer the survey

We invite you to participate in an investigation about the relationship between men and women in society. This is a strictly academic project directed by a team of researchers from Universidad de San Andrés and the University of Wisconsin-Madison. Answering this survey should take you about 15 minutes. Your answers are completely anonymous. After completing the questionnaire, you will be given a code with which you will be participating in the raffle for a Samsung Galaxy A20 on May 31st, 2020. At the end of the survey, we will give you the details to participate in the raffle.

Survey

Attitudes

a) Homosexuality aversion

1. Homosexuals (male or female) should have the right to legally marry.
2. Homosexual couples (male or female) should have the right to adopt children.
3. Nearly all homosexuals are psychiatrically disturbed.
4. Except for differences in sexual preference, homosexuals are as normal as heterosexuals.

b) Hostile sexism

5. Many women are actually seeking special favors, such as hiring policies that favor them over men, under the guise of asking for "equality."
6. Most women interpret innocent remarks or acts as being sexist.
7. Feminists are making entirely reasonable demands to men.

c) Old-fashioned sexism

8. Women are generally not as smart as men.
9. I would be equally comfortable having a woman as a boss as a man.
10. It is more important to encourage boys than to encourage girls to participate in athletics.
11. Women are just as capable of thinking logically as men.
12. When both parents are employed and their child gets sick at school, the school should call the mother rather than the father.

d) Sexual machismo

13. That only the man has sex before marriage.
14. That a married man or stable partner has sex with prostitutes.
15. A woman must accept the infidelities of her partner.
16. The man needs to have several sexual partners at the same time.
17. Regardless of the situation or mood, the woman must have sexual relationships when her partner wants to have them.
18. The man must make his male son start his sex life (have his first sexual relationship).

e) Justification of sexism and violence

19. Violence that occurs within the home is a family matter and should not be disclosed to anyone outside the home.
20. When a woman is attacked by her husband, she probably has done something to provoke him.
21. A man is justified in assaulting his wife or girlfriend when she decides to leave him.
22. If a woman is abused by her partner and does not leave him, it is because she does not dislike the situation as much.
23. For the sake of her children, a woman that has to endure violence from her husband or partner, she should not report it.
24. In order to have a good relationship, it is desirable that the woman avoids disagreeing with her male partner.

25. A good father should let the rest of his family know who is in charge.

Physical violence

26. I threw something at my partner that could have hurt her.
27. I pushed my partner violently.
28. I beat up my partner.

Non-physical abuse

29. I insulted my partner.
30. I destroyed something that belonged to my partner.
31. I make fun of my partner's poor ability to do things.
32. I expect my partner to obey me.
33. I get very upset and angry if my partner says I've had too much to drink.
34. I demand that my partner perform sexual acts that she does not like.
35. I carefully control the money I give to my partner.
36. I don't want my partner to have any male friends.
37. I tell my partner that she is ugly or fat.
38. I don't want my partner to work or go to school.
39. I don't want my partner to socialize with her friends.

Appendix tables

Table A6. First stage

	(1)	(2)	(3)	(4)	(5)	(6)
	Conscription	Conscription	Conscription	Conscription	Conscription	Conscription
	All	All	1958-1965	1958-1965	1966-1975	1966-1975
Draft eligible	0.379*** (0.024)	0.377*** (0.024)	0.573*** (0.034)	0.567*** (0.035)	0.225*** (0.032)	0.222*** (0.032)
Observations	1,219	1,219	514	514	705	705

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A7A1. Impact of conscription on sexist attitudes for cohorts 1958-1965

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Index of sexist attitudes	Negative attitude towards homosexuality	Sexual machismo	Justification of sexism & violence	Old- fashioned sexism	Hostile sexism
Conscription	0.376*** (0.092)	0.055** (0.025)	0.062*** (0.018)	0.060*** (0.016)	0.076*** (0.020)	0.057** (0.023)
Observations	514	514	514	514	514	514

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A7B1. Impact of conscription on sexist attitudes for cohorts 1958-1965

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Index of sexist attitudes	Negative attitude towards homosexuality	Sexual machismo	Justification of sexism & violence	Old- fashioned sexism	Hostile sexism
Conscription	0.380*** (0.147)	0.042 (0.045)	0.056* (0.029)	0.069*** (0.026)	0.085*** (0.031)	0.055 (0.039)
Observations	514	514	514	514	514	514

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A8A1. Impact of conscription on sexist behavior for cohorts 1958-1965

VARIABLES	(1)	(2)	(3)	(4)
	Non-physical abuse	Physical violence	Non-physical abuse	Physical violence
Conscription	0.037*** (0.012)	0.010*** (0.004)	0.052*** (0.019)	0.014** (0.006)
Observations	514	513	514	513

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A7A2. Impact of conscription on sexist attitudes for cohorts 1966-1975

VARIABLES	(1) Index of sexist attitudes	(2) Negative attitude towards homosexuality	(3) Sexual machismo	(4) Justification of sexism & violence	(5) Old- fashioned sexism	(6) Hostile sexism
Conscription	0.258** (0.102)	0.039 (0.030)	0.023 (0.020)	0.035* (0.019)	0.058*** (0.022)	0.072*** (0.026)
Observations	705	705	705	705	705	705

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A7B2. Impact of conscription on sexist attitudes for cohorts 1966-1975

VARIABLES	(1) Index of sexist attitudes	(2) Negative attitude towards homosexuality	(3) Sexual machismo	(4) Justification of sexism & violence	(5) Old- fashioned sexism	(6) Hostile sexism
Conscription	0.345 (0.287)	0.025 (0.094)	0.038 (0.057)	0.074 (0.054)	0.026 (0.059)	0.141 (0.091)
Observations	705	705	705	705	705	705

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A8A2. Impact of conscription on sexist behavior for cohorts 1966-1975

VARIABLES	(1) Non-physical abuse	(2) Physical violence	(3) Non-physical abuse	(4) Physical violence
Conscription	0.018 (0.016)	0.007 (0.007)	0.038 (0.041)	0.024 (0.018)
Observations	705	701	705	701

Notes: Standard errors clustered at the ID-cohort level are shown in parentheses. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Chapter III:

Confinement and Intimate Partner Violence*

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Abstract

We use self-reported survey data from Argentina to study the extent to which the quarantine implemented following the coronavirus pandemic had unintended consequences on intimate partner violence (IPV). Since the disease arrived late to Argentina and the government reacted fast, the full national lockdown was imposed when few people felt threatened by the virus. The quarantine decree also established clear exceptions for subsets of the population and, for reasons plausibly exogenous to the prevalence of IPV, only some individuals were placed in quarantine. Exploiting this variability in exposure, we find a positive link between quarantine and IPV.

Keywords: Physical violence, non-physical abuse, lockdown, quarantine.

JEL classification: J12; J16; H12.

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I. Introduction

Roughly one in three women around the world experience some form of violence throughout their lives (WHO 2013). In some regions of Asia, Latin America, and Sub-Saharan Africa, this ratio goes up to almost one in two, and in Central Sub-Saharan Africa as high as two in three (Devries *et al.* 2013: 1528). Physical and psychological trauma from these experiences leads to different kinds of injuries and mental health problems, often stimulate substance abuse, and in many cases end up in death (both homicide and suicide). Health and social consequences, and their associated economic costs, are so large that they make violence against women a major public policy problem. Since the great majority of this violence is perpetrated by the victim's intimate partner in the form of physical, sexual and/or emotional abuse (Devries *et al.* 2013), understanding the causes of intimate partner violence (IPV) have become central in various academic and policy debates (Heise 2011). Naturally, *fundamental* determinants of IPV, like deeply-rooted social norms on violence, persistent gender inequality, or chronic poverty, have received considerable attention in the literature (e.g., Jewkes 2002, Gibbs *et al.* 2020). But understanding its *proximate* determinants or situational triggers, such as alcohol consumption (Angelucci 2008, Devries *et al.* 2014), labor market fluctuations (Aizer 2010) or even male frustration after a football game (Card and Dahl 2011), sometimes carry equally –or even more– important information for public policy, as policymakers usually have better tools to control them than they do large structural factors. Also, by systematically tinkering with immediate determinants, cultural priors could slowly, but eventually, be altered as suggested by some tipping models.³⁶

³⁶ Tipping models, popularized by Gladwell (2000), are those where many members in a group adopt a new practice. They have been used extensively in criminology and sociology. In economics, their precursor has been Schelling (1969, 1978).

The recent COVID-19 pandemic has arguably stimulated some of these situational triggers for IPV, most notably the time partners spend with each other. Increasing day-to-day exposure to potential perpetrators can, in principle, increase violence. Peterman *et al.* (2020: 11), for example, summarizes evidence on this connection from other crisis' settings where families are forced to be together for extended periods of time (e.g., in refugee camps) or from situations when potential perpetrators are temporarily away (e.g., with seasonal male migration programs). Quarantines implemented worldwide following the spread of coronavirus resulted in many people spending more time with their families and partners, raising concerns about the potential unintended consequences that this policy could have on the level of intra-family violence. Confinements also contributed to reducing family income, another potential trigger of IPV.

There is, in fact, abundant anecdotal evidence suggesting IPV may have increased with the implementation of lockdowns. Journalistic pieces, for instance, report that calls to hotlines around the world increased between 40% and 300%.³⁷ Yet, the academic literature on the subject is limited and ambiguous.³⁸ Our paper contributes to this public policy debate by providing evidence on the causal impact of quarantines on IPV, especially in a context where the potential stress caused by the actual incidence of the disease was relatively small.

Assessing the impact of coronavirus quarantines on IPV has, of course, a series of challenges. Although most governments soon recognized restricting movement of people and social distancing was important to limit the spread of the disease, their reactions varied enormously in the types of restriction they imposed, from localized suggestions to full national lockdowns.

³⁷ A recent entry in Wikipedia summarizes some of these: USA (6 to 18%), UK (25%), Russia (25% to 100%), Australia (40%), Spain (47%), Netherlands (50%), Belgium (70%), India (94%), Colombia (225%), China (230%). See https://en.wikipedia.org/wiki/Impact_of_the_COVID-19_pandemic_on_domestic_violence, retrieved on May 1, 2020. And in the few places where this pattern did not materialize have only risen concerns that the problem is there, but hidden: <https://www.newsweek.com/domestic-abuse-europe-italy-coronavirus-1496173>.

³⁸ For the US, for example, Leslie and Wilson (2020) report a 10% increase in domestic violence calls, yet Campedelli *et al.* (2020) report no significant effects of COVID-19 containment policies on intimate partner violence.

Compulsory quarantines were imposed in some countries, and restrictions to movement also varied substantially in their timing with respect to the advance of the virus. Many developed countries in the Northern Hemisphere acted only when the spread of the disease was already imposing a serious health threat to the population, making it hard to figure out whether any change in an outcome of interest (in our case, IPV) is a consequence of the confinement *per se*, or partly a consequence of the stress triggered by the fear of the disease.

In this respect, Argentina offers a stark contrast. Given that the disease started in the boreal winter, it arrived relatively late to the country and, since by then there was already a global consensus that the virus represented a serious threat, the Argentine government reacted immediately with strong measures. The first recorded case appeared on March 3 –more than two months after the beginning of the outbreak in China– and the first death, on March 7. Four days later, on March 11, the WHO declared the coronavirus outbreak a pandemic, more or less when first ‘autochthonous’ cases began to surface in the country. Despite these low figures, with the world in alert, the Argentinean government decided to take extreme measures to control the disease. Following the cancelation of a series of mass events and closure of schools on March 16, a full compulsory national lockdown was declared on March 20, when a total of only 3 deaths had been confirmed within the country.

Argentina’s response to the crisis stands in stark contrast with most other places in the world. Asian countries had mostly localized lockdowns, not national ones. The U.S. and Canada also reacted late and with localized policies, many of them rather moderate. Most European countries did eventually engage in national compulsory lockdowns, but only when they were already considerably compromised in terms of cases and deaths. In Argentina, the lockdown was particularly strict and affected the whole country when the effect of the actual disease was minimal.

It was as severe as those of hard-hit European and Asian countries, and the government was reluctant to loosen it in any way for weeks. Circulation was extremely restricted: all non-essential activities were canceled, and people were not allowed to leave their homes except to buy groceries or deal with emergencies, and when they did, they had to do it alone (e.g., it was not permitted to enter a supermarket with a partner or a child). It was forbidden to take children to parks or for people to run outside. Permits had to be requested to move around and people circulating without them were penalized: between March and early June, more than 93,000 were detained for breaching the quarantine without a permit and nearly 5,000 cars were confiscated. At the same time, it was clear that this policy was mostly preventive, as there were very few cases around. Two months into the lockdown, the *total* death toll was 401 (10 deaths per million population). More than 600 had died in Italy and nearly 200 in Spain when similar national lockdowns were declared, and two months into the lockdowns they had, respectively, circa 30,000 and 27,000 deaths (about 540 and 610 death per million population), nearly 218,000 and 272,000 recorded cases, and arguably many more unrecorded.

This makes Argentina's case particularly interesting because the strict full national lockdown took place in an environment where few people had yet been directly threatened by the disease. In this context, another noteworthy feature of the Argentine quarantine decree is that it established clear exceptions for an important subset of the population (workers in, e.g., health care, food sales, delivery, etc.). Therefore, for reasons plausibly exogenous to the prevalence of IPV, some families were placed in full quarantine, while others were not. We exploit this variability in individual exposure to quarantine requirement to explore the effect of the lockdown on physical, sexual, and emotional IPV using a confidential web-based survey aimed at women that were quarantined. This is an important aspect of our empirical aspect, since the information we use comes from women

exposed to the quarantine and the treatment is determined by the quarantine status of women's partners. Our main finding is that women whose partners are also in quarantine are more likely to report an increase in all three of these dimensions of IPV. In our analysis of potential mechanisms, we find evidence that the quarantine increased the time spent with the partner (increasing the chances of violent encounters) and decreased family and partner's income (increasing economic-related stress or tension regarding the relative contributions to the family income). We do not find evidence for other of the usual culprits such as alcohol or drug consumption.

Our research relates to 2 important strands of literature. First, it connects to the recent but rapidly increasing literature on the socio-economic impact of the coronavirus pandemic and its associated lockdowns, in particular, the one on the differential impact on gender (e.g., Adams-Prassl *et al.* 2020, Alon *et al.* 2020, Wenham *et al.* 2020). Most of this literature focuses on the impact on the labor market and household work, especially as school and childcare closures have increased unpaid household work, and these additional reassignments have a differential effect on women and could limit their work and economic opportunities. Ours expands this literature to provide strong evidence that lockdowns can lead to increased IPV, and this should probably be factored in when assessing whether to continue or not with that policy, or what additional measures to consider as part of the lockdown policies.

Second, it links with the long-standing research on the sources of violence against women, especially by intimate partners, that spans various disciplines. In economics, this line of research is related to how violence against women is affected by female economic dependence, wage gaps and job opportunities (e.g., Farmer and Tiefenthaler 1997, Aizer 2010, Basu and Famoye 2010, Bobonis *et al.* 2013, Munyo and Rossi 2015, Bhalotra *et al.* 2020, Bowlus and Seitz 2006), alcohol abuse (Angelucci 2008), health (Papageorge *et al.* 2019), or structural poverty (Aizer 2011). There

is, in fact, part of this literature linking pandemics to intra-family violence. Peterman *et al.* (2020) review the literature and document that quarantines and social isolation are important channels to explain the observed increase in violence against women and girls during pandemics as, e.g., in the quarantines enforced during the 2014-16 Ebola outbreak in West Africa (UNDP 2015). Our study is able to show an effect even in the absence of a direct disease-related stress on all three forms of IPV, including emotional, which is often less studied than physical and sexual violence (Devries *et al.* 2013).

II. Survey

We measure IPV using a confidential web-based survey we conducted in May 2020.³⁹ At that moment, people had experienced 7 to 8 weeks of strict lockdown. We sent an email invitation to participate in the survey to an email list of approximately 29,000 women. To participate in the survey, women had to be at least 18 years old, cohabiting with a male partner for at least one year, and they had to be under quarantine according to their job activity. To increase survey response rates, participants were included in a raffle for a smartphone. The survey was active for two weeks and we received 1,502 completed, valid surveys. We asked for IPV in the one-year period before quarantine and since the beginning of the quarantine (a period of two months). Questions explored 3 dimensions of IPV: along with the frequently studied physical and sexual dimensions, we also looked at emotional violence. We included 4 questions on physical, 2 on sexual, and 6 on emotional domestic violence. Respondents indicated frequencies on a 5-point scale ranging from “Never” to Always.” From the raw data, in which each question was scaled from 0 to 4, we constructed the 3 variables on violence (before and during quarantine) by adding the scores on

³⁹ The English version of the survey is presented in the online appendix. The survey was adapted from the “Survey on the Perception and Incidence of Violence against Women”, City of Buenos Aires, published in February 2020, which can be found here <https://www.estadisticaciudad.gob.ar/eyc/?p=107456> (Retrieved on April 9, 2020).

each dimension, as usually done in the literature. The range of scores is 0 to 16 (physical), 0 to 8 (sexual), and 0 to 24 (emotional). To have comparable scores, we divided each score by the maximum possible score in each dimension, and multiply it by 100. In this way, we got a metric for each dimension ranging from 0 to 100 (i.e., an individual would have a score equal to 100 if she answered “Always” in all the questions of that dimension). We also constructed a metric for IPV as the average of the 3 individual metrics. In this way, we ended up with 4 primary outcomes (IPV, emotional violence, sexual violence, and physical violence).

In our sample, 58.7% of women report some level of emotional violence, 10.1% sexual violence, and 13.2% physical violence in the sense that they did not answer “never” to all of the questions of that dimension (before the quarantine). Even though there is not a nationwide victimization survey, there is a 2018 survey in the City of Buenos Aires, we can compare our survey data for the City of Buenos Aires to those. Reported violence in our survey is in line with reported violence in the 2018 survey in the City of Buenos Aires, conditional on education levels.

Aside from the primary outcomes, we also collected information on 5 secondary outcomes: increase in alcohol and drug consumption (a dummy variable that takes the value of 1 if her partner increased the consumption of alcohol and/or drugs), change in hours spent with her partner, drop in family income (a dummy variable that takes the value of 1 if the family income decreased), drop in partner’s income (a dummy variable that takes the value of 1 if partner’s income decreased), and change in the number of household members (a variable that takes the value of 1 if the number of household members increased, 0 if it didn’t change, and -1 if the number of household members decreased). Table 1 presents summary statistics of primary and secondary outcomes.

The treatment variable is Quarantine, a dummy variable that takes the value of one for those women who report that, according to the decree, their partners had to comply with quarantine.

Notice that Quarantine captures intention to treat, since we ask whether they have to comply, not if they did comply.

From the survey, we also obtained self-reported information on a set of pre-treatment characteristics, including marital status, number of children, number of rooms in the house of residence, number of people cohabitating, own age, partner's age, own maximum level of education, partner's maximum level of education, and province of residence.

Interpreting survey responses

The survey was anonymous and conducted online, so there is no reason to expect social stigma attached to particular responses or any changes in answers due to cues about what constitutes appropriate behavior. The response rate was 5.18%. A natural concern in this context is potential selection into the sample: if selection into the sample was non-random, our estimated treatment effects may be biased. For non-random selection into our sample to threaten the internal validity of our estimates, selection would need to be differential by partner's quarantine assignment status. We test for differential selection into the survey in 3 ways. First, we examine whether the sample proportion of men who have to comply with quarantine in our sample is similar to the population proportion. In our sample, 79% of women's partners had to comply with quarantine. Even though an exact figure for the population is not available, according to casual evidence, more than 75% of the population had to comply with quarantine.⁴⁰ Second, we look at within-survey attrition. The proportion of women that started the survey but did not complete is low (9.95%). Also, attrition is orthogonal to partner's quarantine assignment status: the proportion of attrition is 9.29% in the quarantine group, 12.39% in the non-quarantine group, and the

⁴⁰ Infobae and La Nación news portals, published some notes on the approximate calculation. See <https://www.lanacion.com.ar/economia/exceptuados-cuarentena-cuanta-gente-esta-trabajando-nid2345764> and <https://www.infobae.com/politica/2020/04/26/coronavirus-en-argentina-alberto-fernandez-anuncio-la-prorroga-de-la-cuarentena-hasta-el-10-de-mayo-pero-flexibilizan-las-salidas-de-los-hogares/>.

difference between these two proportions is not statistically significant. Third, we examine whether individuals' pre-quarantine variables are balanced across the quarantine and non-quarantine groups. We have information on 3 sets of pre-quarantine variables: outcomes, socio-economic characteristics, and province of residence. Tables 2, 3, and 4 report differences, by partner's quarantine assignment status, in pre-quarantine levels of IPV, socio-economic characteristics, and province of residence. Overall, there are no statistically significant differences between the quarantine and non-quarantine groups for 37 out of the 46 individuals' pre-quarantine characteristics available. Most important, for the 4 primary outcomes, the differences between the quarantine and non-quarantine groups are very small and statistically not significant. Since population and sample proportion of men under quarantine status are similar, within-survey attrition is low and orthogonal to quarantine status, and most pre-quarantine characteristics are balanced across the quarantine and non-quarantine groups, we conclude that results reported below are not likely to be subject to significant sources of bias due to differential selection into the survey.

III. Empirical strategy and results

Even though a full compulsory national lockdown was declared in Argentina on March 20, the quarantine decree established clear exceptions for an important subset of the population, who were allowed to continue with their laboral activities. Our identification strategy exploits this variability in partner's exposure to quarantine requirement to explore the effect of partner's quarantine on IPV (all the women in the sample are under quarantine).

Formally, we estimate the following equation:

$$Y_{1i} = \alpha_1 + \beta_1 \text{Quarantine}_i + \mu_1 Y_{0i} + \gamma_1 X_i + \varepsilon_{1i} \quad (1)$$

where Y_{1i} is IPV for individual i during quarantine, Quarantine_i is a dummy variable that takes the value of one for those women whose partners, according to the decree, have to comply with

quarantine, Y_{0i} is a vector of pre-quarantine outcomes, X_i is a vector of individuals' pre-quarantine characteristics, and ε_i is an error term. The coefficient of interest is β_1 .

Our identification assumption is that IPV in the non-quarantine group is a good counterfactual of IPV in the quarantine group in the absence of quarantine, conditional on the set of pre-quarantine variables available. A potential concern would arise if men employed in quarantine exempted activities have a different propensity to engage in IPV relative to the general population. However, the simple observation of exempted activities suggests that this is not the case. Exempted activities are very heterogeneous: they include, for example, health care, food sales, delivery, and security forces (the full list of exempted activities is presented in the online appendix). Also, as shown in the previous section, and as expected if quarantine status were as if randomly assigned, pre-quarantine IPV is not correlated with quarantine status, and the values of the pre-quarantine primary outcomes are remarkably similar between the quarantine and non-quarantine groups.

Table 5 reports Ordinary Least Squares estimates of equation (1). In order to draw general conclusions in the context of multiple metrics, we first report results for IPV (the average of the 3 metrics). As shown in column (1), the coefficient of Quarantine is positive and statistically significant, indicating that women whose partners are in quarantine are more likely to report episodes of violence.⁴¹

In the remaining columns of Table 5 we report effects on each separate metric. The effect of quarantine on IPV is generalized. For all 3 metrics, the point estimates have the expected signs and all coefficients are statistically significant. The size differences are important: focusing on

⁴¹ The survey was active for two weeks. Results are robust to including day-of-answer fixed effects. All results mentioned and not reported are available from the authors upon request.

mean effects, we see from Table 5 that emotional violence is 12% higher, sexual violence is 35% higher, and physical violence is 23% higher for women whose partners are also in quarantine.

Comparing our estimates to those available in the literature, the magnitude of these effects are larger than the increase in calls to domestic violence hotlines in 15 large US cities (10.2%) (Leslie and Wilson, 2020), and similar to the increase in calls to the hotline in Argentina (Perez-Vincent and Carreras, 2020) of about 26%. Perez-Vincent and Carreras (2020) also report an increase of 50% in calls related to emotional domestic violence and no effect on the increase in physical domestic violence. Nevertheless, the outcome variables are not easily comparable since our results are on scores and not on the number of cases.⁴²

Mediation analysis

Having established a causal link between quarantine and IPV, we now focus on the underlying mechanisms.

We conduct mediation analysis in various steps. First, we identify potential mediators, that is, variables that may lie on the causal pathway between quarantine and IPV. Second, we check if these potential mediators are correlated with Quarantine. Third, we keep as mediators those potential mediators that are statistically significant to explain IPV, conditional on Quarantine. Finally, we decompose the total effect of Quarantine on IPV on the Average Direct Effect (ADE) and the Average Causal Mediation Effect (ACME). The direct effects and the indirect effects sum up to the total causal effect, and therefore this decomposition assesses the relative importance of the mechanism. Aside from the assumptions needed for quarantine to have a causal interpretation in explaining IPV, the identification of causal mechanisms requires the additional assumption of

⁴² Table A1 in the appendix reports interaction effects between Quarantine and pre-treatment variables. We explore differential effects by previous IPV, university education, and number of children under 18 years old. Table A2 explores the interaction with number of children under 12 years old.

sequential ignorability (see Imai et al. 2011). This assumption requires that conditional on the set of pre-treatment variables available, the mediator status is ignorable, i.e., there are no unobservable pre-treatment or post-treatment covariates that affect both the mediator and IPV.

The literature recognizes a series of pathways through which a pandemic could lead to increased intimate partner violence, well summarized in a recent article by Peterman *et al.* (2020). Many factors are, in fact, related to the actual impact of the disease on society and unlikely to be relevant for this study, because the virus had not really spread too much in Argentina when the lockdown was introduced.⁴³ Our survey took place roughly in the seventh and eighth weeks into the lockdown, when the *total* death toll from COVID-19 went from 300 to 400 people and total reported cases from 6,000 to 9,000 –indeed underreported, because very few tests were run, but still the number the public perceive as a signal of health threat.⁴⁴ The effect we find is more likely to result from the actual lockdown rather than the pandemic *per se*. The effect could not even come from the stress for the future of the lockdown or the future of the pandemic, since after 8 weeks of strict lockdown, the population thought that the risk of a massive spread was low and that normal activity could return soon.

Peterman et al. (2020) document three channels in which the lockdown could affect intimate partner violence. The first pathway is coming from the *inability to escape* an abusive partner during lockdowns, which could lead to more opportunities for the partner to engage in violent behavior. Second, quarantines lead to *social isolation*, which can contribute through at least two channels.

⁴³ These pathways are: disaster and conflict-related unrest, exposure to exploitative relationships due to changing demographics, reduced health service availability, virus specific sources of violence, exposure to violence and exploitation, and violence perpetrated against health workers. As it is clear from the extensive discussion in Peterman *et al.* (2020), all these are dependent on the virus being far more aggressive and/or the pandemic having reached a larger penetration in society.

⁴⁴ Spain, a country with a similar population to Argentina (46.9 and 44.5 million, respectively), had a *daily* death toll of 100 when the lockdown was implemented (Argentina reached a *total* death toll of 100 a month into the lockdown), nine days later that number surpassed 500, a level that was not lowered for more than a month, with peaks of nearly a thousand casualties *per day*.

On the one hand, social isolation has been associated with anxiety and various mental health disorders, all potential triggers of violence or of behaviors that might be related to this violence, such as increased alcohol consumption. On the other hand, isolation limits the occasional monitoring other people can do of potentially violent behavior. The third pathway through which lockdowns typically affect intimate partner violence, perhaps the most salient in the literature, is by increasing *economic* insecurity and creating poverty-related stress via fall in income, sudden unemployment, or increased economic uncertainty. Our empirical specification allows us to investigate some of these potential underlying channels. We look into the first pathway using our estimation of time spent with the partner. It is less clear how to explore the problem of social isolation, still we have two elements that are arguably related: alcohol and drug consumption (as the context of isolation could have triggered abuse), and members living in the household (as the lockdown might have decreased the number of people in the house, reducing the chance of monitoring). We study the economic pathway with the reported information on family income.

We start the mediation analysis by estimating the following equation:

$$M_i = \alpha_2 + \beta_2 \text{Quarantine}_i + \mu_2 \text{Y-before}_i + \gamma_2 X_i + \varepsilon_{2i} \quad (2)$$

where the dependent variables (or potential mediating variables, M_i) are the set of secondary outcomes described in section 2: the change in the average number of hours per day couple spend together, the decrease in alcohol and drug consumption, the change in the number of household members, and the decrease in income (family and partner's).

Table 6 reports results on the impact of quarantine on these secondary outcomes. As observed in column (1), the number of hours that couples spent together increased by about 3.8 hours when the partner is in quarantine. Columns (2) and (3) show there is no effect on alcohol

and drug consumption and in the number of household members. Columns (4) and (5) show that quarantine is associated with a decrease in both family income and partner's income.

Results in Table 6 suggest that the effect of quarantine on intimate partner violence may be explained by the effect the quarantine has on increasing the time couples spent together and on decreasing income. The next step is to estimate the following equation:

$$Y_{1i} = \alpha_3 + \beta_3 \text{Quarantine}_i + \delta M_i + \mu_3 Y_{0i} + \gamma_3 X_i + \varepsilon_{3i} \quad (3)$$

where M_i is the vector of candidate mediators that “survive” the previous test (change in hours spent together, drop in family income, and drop in partner's income). OLS estimates of equation (3) are shown in Table 7. From the set of candidates to be mediating variables, only the change in hours spent together is statistically significant.

The mediation analysis suggests that the change in hours spent together mediates the effect of quarantine on IPV. As explained above, the underlying assumption is that there are no unobservable pre-treatment or post-treatment covariates that affect both the change in hours spent together and IPV.

To complete the mediating analysis, we now proceed to estimate the ACME (the indirect effect through the change in the number of hours spent together) and the ADE (the average direct effect, which represents all the other mechanisms not contemplated in our analysis). To estimate the ACME we generate two sets of predictions for the mediator, one under the treatment and the other under the control. The ACME is then computed as the average difference between the outcome predictions using the two different values of the mediator.

Results from mediation analysis, using the package *mediation* in R, are reported in Table 8. The total effect for Quarantine is 0.749 (the effect we find when there was no mediator in our model, see column (1) in Table 5). The total effect is statistically significant. The direct effect for

Quarantine is 0.542 which, while still significant, is smaller than the total effect. The indirect effect of Quarantine that passes through the change in the number of hours spent together is 0.207, significant at 1% level, and accounts for about 28% of the total effect.

IV. Final remarks

Since the start of the coronavirus crisis, many governments across the world have asked individuals to self-isolate to limit the spread of the virus, preserve lives, and minimize the burden on healthcare services. Children were sent home from school, workers were asked to work from home, and some forced not to work at all, and millions of people were placed under quarantines. In this context, it is crucial for policy makers to understand the multiple impacts these policies have on societies. Even though it is mostly accepted that quarantines have benefits in terms of saving lives from the virus, there is significant concern about collateral impacts. Much of the debate has concentrated on the economic vs. health costs of continuing with the lockdowns (e.g., Eichenbaum *et al.* 2020, Lin and Meissner 2020, Pindyck 2020). Our paper provides important input to policymakers about one of important *social* cost. We exploit the plausibly exogenous variability in the individual exposure to quarantine declared in Argentina to explore its effect on intimate partner violence for women that were placed in quarantine in a context where the actual threat of the disease was minimal. We find that when these women's partner is also placed under quarantine there is indeed a higher prevalence of IPV in all its forms (emotional, sexual, and physical). We also find that quarantined couples end up spending more time together, which might ignite tension or simply give more opportunities for potential abusers to engage in violence. This suggests additional targeted policies may be needed to avoid exacerbating the extent of domestic violence occurrence.

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VI. Tables and Figures

Table 1. Summary statistics of primary and secondary outcomes

	Mean	Standard deviation	Min.	Max.
<i>Primary outcomes</i>				
IPV	3.69	6.98	0.00	70.14
Emotional	8.46	13.39	0.00	91.67
Sexual	1.92	8.29	0.00	87.50
Physical	0.70	3.26	0.00	43.75
<i>Secondary outcomes</i>				
Change in hours w/partner	5.66	4.48	-7.00	15.00
Increase in alcohol & drug consumption	0.03	0.18	0.00	1.00
Change in household members	-0.02	0.31	-1.00	1.00
Decrease in family income	0.56	0.50	0.00	1.00
Decrease in partner's income	0.44	0.50	0.00	1.00

Table 2. Pre-quarantine outcomes, by quarantine status

	Quarantine mean	Non quarantine mean	Difference
IPV	4.300 (7.634)	4.254 (6.823)	0.046 [0.476]
Emotional	9.285 (13.492)	9.285 (12.998)	0.000 [0.853]
Sexual	2.288 (8.727)	2.371 (8.394)	-0.083 [0.551]
Physical	1.328 (4.832)	1.105 (3.465)	0.222 [0.292]

Notes: Standard deviations are shown in parentheses. Standard errors are shown in brackets.
 *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 3. Pre-quarantine socio-economic characteristics, by quarantine status

	Quarantine mean	Non quarantine mean	Difference
Age	44.255 (12.348)	41.817 (10.523)	2.439*** [0.764]
Partner's age	46.962 (13.180)	43.916 (10.915)	3.046*** [0.812]
Time in the relationship	19.257 (12.947)	17.251 (11.056)	2.006** [0.801]
Time cohabitating	17.669 (13.184)	15.227 (10.688)	2.442*** [0.809]
Number of rooms	3.273 (1.177)	3.212 (1.098)	0.061 [0.074]
Number of household members	3.599 (1.455)	3.788 (1.628)	-0.188** [0.095]
Number of children 0-5	0.287 (0.601)	0.328 (0.581)	-0.041 [0.038]
Number of children 6-12	0.368 (0.672)	0.531 (0.841)	-0.163*** [0.045]
Number of children 13-17	0.330 (0.697)	0.418 (0.704)	-0.088** [0.044]
Married	0.607 (0.489)	0.595 (0.492)	0.012 [0.031]
Not married	0.393 (0.489)	0.402 (0.491)	-0.009 [0.031]
Woman: no instruction or incomplete primary	0.024 (0.154)	0.035 (0.185)	-0.011 [0.010]
Woman: complete primary school	0.119 (0.324)	0.141 (0.349)	-0.022 [0.021]
Woman: complete high school	0.336 (0.472)	0.350 (0.478)	-0.015 [0.030]
Woman: complete university or more	0.521 (0.500)	0.473 (0.500)	0.048 [0.032]
Partner: no instruction or incomplete primary	0.046 (0.210)	0.061 (0.240)	-0.015 [0.014]
Partner: complete primary school	0.232 (0.422)	0.289 (0.454)	-0.058** [0.027]
Partner: complete high school	0.343 (0.475)	0.334 (0.473)	0.009 [0.030]
Partner: complete university or more	0.379 (0.485)	0.315 (0.465)	0.064** [0.031]

Notes: Standard deviations are shown in parentheses. Standard errors are shown in brackets.

*Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 4. Pre-quarantine province of residence, by quarantine status

	Quarantine mean	Non quarantine mean	Difference
Buenos Aires province	0.512 (0.500)	0.508 (0.501)	0.004 [0.032]
Buenos Aires city	0.160 (0.366)	0.141 (0.349)	0.018 [0.023]
Catamarca	0.004 (0.065)	0.006 (0.080)	-0.002 [0.004]
Chaco	0.018 (0.132)	0.006 (0.080)	0.011 [0.008]
Chubut	0.012 (0.108)	0.003 (0.057)	0.009 [0.006]
Cordoba	0.046 (0.210)	0.058 (0.234)	-0.012 [0.014]
Corrientes	0.009 (0.096)	0.006 (0.080)	0.003 [0.006]
Entre Rios	0.020 (0.141)	0.026 (0.159)	-0.006 [0.009]
Formosa	0.006 (0.076)	0.003 (0.057)	0.003 [0.005]
Jujuy	0.006 (0.076)	0.006 (0.080)	-0.001 [0.005]
La Pampa	0.007 (0.082)	0.010 (0.098)	-0.003 [0.005]
La Rioja	0.006 (0.076)	0.006 (0.080)	-0.001 [0.005]
Mendoza	0.021 (0.143)	0.032 (0.177)	-0.011 [0.010]
Misiones	0.017 (0.129)	0.026 (0.159)	-0.009 [0.009]
Neuquen	0.013 (0.115)	0.003 (0.057)	0.010 [0.007]
Rio Negro	0.018 (0.132)	0.029 (0.168)	-0.011 [0.009]
Salta	0.022 (0.146)	0.029 (0.168)	-0.007 [0.010]
San Juan	0.007 (0.082)	0.006 (0.080)	0.000 [0.005]
San Luis	0.009 (0.096)	0.013 (0.113)	-0.004 [0.006]
Santa Cruz	0.008 (0.087)	0.003 (0.057)	0.004 [0.005]
Santa Fe	0.057 (0.232)	0.055 (0.228)	0.002 [0.015]
Santiago del Estero	0.007 (0.082)	0.003 (0.057)	0.004 [0.005]
Tierra del Fuego	0.006 (0.076)	0.003 (0.057)	0.003 [0.005]
Tucuman	0.011 (0.104)	0.016 (0.126)	-0.005 [0.007]

Notes: Standard deviations are shown in parentheses. Standard errors are shown in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 5. Main results: impact of quarantine on intimate partner violence

	IPV (1)	Emotional (2)	Sexual (3)	Physical (4)
Quarantine	0.753*** (0.211)	1.148*** (0.395)	0.809** (0.315)	0.302** (0.147)
Mean of dependent variable	3.692	8.455	1.922	0.699
Observations	1,502	1,502	1,502	1,502

Notes: Robust standard errors are shown in parentheses. IPV is the principal component of emotional, sexual, and physical violence. All models are estimated using Ordinary Least Squares and control for the set of pre-quarantine variables listed in Tables 2, 3, and 4. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 6. Potential mechanisms

	Change in hours w/partner	Increase in alcohol & drug consumption	Change in household members	Drop in Family income	Drop in partner's income
	(1)	(2)	(3)	(4)	(5)
Quarantine	3.789*** (0.257)	0.004 (0.011)	-0.007 (0.018)	0.240*** (0.031)	0.266*** (0.029)
Observations	1,502	1,502	1,502	1,502	1,502

Notes: Robust standard errors are shown in parentheses. All models are estimated using Ordinary Least Squares and control for the set of pre-quarantine variables listed in Tables 2, 3, and 4. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 7. Mediating analysis

	Dependent variable: IPV			
	(1)	(2)	(3)	(4)
Quarantine	0.542** (0.260)	0.754** (0.338)	0.754** (0.341)	0.555* (0.296)
Change in hours w/partner	0.056*** (0.017)			0.058** (0.021)
Drop in family income		-0.006 (0.219)		-0.099 (0.200)
Drop in partner's income			-0.003 (0.228)	0.013 (0.192)
Observations	1,502	1,502	1,502	1,502

Notes: Robust standard errors are shown in parentheses. IPV is the principal component of emotional, sexual and physical violence. All models are estimated using Ordinary Least Squares and control for the set of pre-quarantine variables listed in Tables 2, 3, and 4. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 8. Estimates of mediating effects

Effect	Mean	95% confidence interval		p-value
Average Causal Mediating Effect (ACME)	0.207	0.092	0.327	0.002
Direct Effect	0.542	0.048	1.029	0.028
Total Effect	0.749	0.207	1.293	0.012
% of Total Effect mediated	0.276	0.155	0.652	0.010

Notes: Estimates computed using the command *mediation* in R (1,000 simulations).

VII. Appendix

Survey

How old are you?

What is your gender?

What is your marital status?

Are you in a relationship with a man?

How long have you lived with your partner?

What is the highest educational level you have reached?

How old is your partner?

What is the highest educational level your partner has reached?

How many rooms does your home have? (**excluding** kitchen, bathroom, hallways, laundry room, garage).

Where do you live?

How many people live in your household?

According to your work activity, did you have to comply with the social, preventive and compulsory isolation and the prohibition to move during quarantine in accordance with Decree of Necessity and Urgency 297/2020?

Have you had a drop in your income during quarantine?

If yes, how much less income you had during quarantine.

According to your partner's work activity, did **YOUR PARTNER** have to comply with the social, preventive and compulsory isolation and the prohibition to move during quarantine in accordance with Decree of Necessity and Urgency 297/2020?

HAS YOUR PARTNER suffered a drop in his income during quarantine?

If yes, how much less income **your partner** had during quarantine.

What was the number of household members just before quarantine started greater, less than or equal to today?

Indicate the number of children you have in each age range (Between 0 and 5 years old, between 6 and 12 years old, between 12 and 17 years old).

How many hours a day do you spend with your partner on weekdays (while you are awake)? (Before quarantine, during quarantine).

You will be asked a series of questions and we ask you to answer them with complete confidence and sincerity. Your answers are completely **anonymous**. Answer if these situations have happened to you and / or your children with your **current partner**.

Has your partner insulted you?

Has your partner belittled or humiliated you? (making you feel bad about yourself, calling you "stupid" or useless, telling you are ugly or fat, for example).

Does your partner become jealous or upset if you communicate to another man, friends or family?

Has your partner followed, watched or checked your personal items (for example, the cell phone)?

Does your partner consume alcohol and/or drugs to excess?

Does he look at you in a way that scares you or act in a way that causes you fear?

Has your partner threatened to hurt you or someone you care about?

Has your partner tried to strangle you?

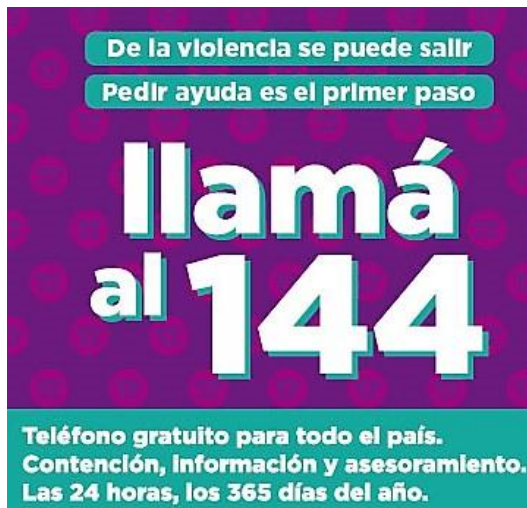
Has your partner slapped you, pushed, cornered, or pulled your hair?

Has your partner hit you with your fist, kicked you, dragged you, or beaten you?

Has your partner threatened to use or used a gun, knife, fire, or other weapon against you?

Out of fear of your partner, did you have sex without wanting it?

In a relationship with your partner, were you forcibly forced to have sex when you didn't want to?



(Telephone line 144 provides telephone attention for victims of gender violence)

I have read the information on the hotline that provides care, containment and telephone advice in situations of gender-based violence.

Full list of exempted activities

1. Health Personnel, Security Forces, Armed Forces, migratory activity, national meteorological service, firefighters and air traffic control.
2. Senior authorities of the national, provincial, municipal and the Autonomous City of Buenos Aires governments; Workers of the national, provincial, municipal and the Autonomous City of Buenos Aires public sector, appointed to guarantee essential activities required by the respective authorities.
3. Justice-service personnel on duty, as established by the competent authorities.
4. Foreign diplomatic and consular personnel authorized by the Argentine Government, in the framework of the Vienna Convention on Diplomatic Relations and the Vienna Convention of 1963 on Consular Relations, and the personnel of international organizations accredited to the Argentine Government, of the Red Cross and White Helmets.
5. Persons who must assist others with disabilities; family members who need assistance; elderly persons; children and adolescents.
6. People who must attend to a situation of force majeure.
7. People affected by the performance of funeral services, burials, and cremations. Within this framework, activities that involve the gathering of people are not authorized.
8. Persons in charge of school and community kitchens.
9. Staff working in audiovisual, radio, and graphic communication services.
10. Staff involved in public construction work.
11. Wholesale and retail supermarkets and local shops. Pharmacies. Hardware stores. Vets. Provision of gas.
12. Food industries, their productive chain, and inputs; personal hygiene and cleaning; medical equipment supplies, medicines, vaccines, and other health inputs.
13. Activities related to agricultural and fisheries production, distribution, and commercialization.
14. Telecommunications, home, and mobile internet and digital services activities.
15. Activities linked to foreign trade that cannot be postponed.
16. Collection, transport and treatment of solid urban, hazardous and pathogenic waste.
17. Maintenance of basic services (water, electricity, gas, communications, etc.) and emergency care.
18. Public passenger transport, transport of goods, oil, fuel and Liquid Petroleum Gas.
19. Home delivery of food, medicines, hygiene and cleaning products, and other supplies of need.
20. Laundry services.
21. Postal and parcel delivery services.
22. Essential surveillance, cleaning, and guard services.
23. Minimum guards to ensure the operation and maintenance of oil and gas reservoirs, oil and gas treatment and/or refining, transport and distribution of electrical energy, fuels, oil and gas, fuel dispensing stations, and electric power generators.
24. *Casa de Moneda* (Mint, Engraving, and Printing), ATM services, cash transport, and all those activities that Argentina's Central Bank provides essential to guarantee the functioning of the payment system.

Appendix Tables

Table A1. Interaction effects

	Dependent variable: IPV			
	(1)	(2)	(3)	(4)
Quarantine	0.527** (0.215)	1.125*** (0.342)	0.514* (0.306)	0.795** (0.389)
Quarantine x IPV-before	0.046 (0.059)			0.040 (0.060)
Quarantine x University education		-0.825* (0.429)		-0.709* (0.416)
Quarantine x Number of children (under 18 years-old)			0.174 (0.186)	0.088 (0.193)
Observations	1,502	1,502	1,502	1,502

Notes: Robust standard errors are shown in parentheses. All models are estimated using Ordinary Least Squares and control for IPV-before, pre-quarantine province of residence, and pre-quarantine socio-economic characteristics. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A2. Interaction effects

	Dependent variable: IPV	
	(3)	(4)
Quarantine	0.312 (0.270)	0.503 (0.338)
Quarantine x IPV-before		0.040 (0.060)
Quarantine x University education		-0.592 (0.416)
Quarantine x Number of children (under 12 years-old)	0.511 (0.230)**	0.439* (0.233)
Observations	1,502	1,502

Notes: Robust standard errors are shown in parentheses. All models are estimated using Ordinary Least Squares and control for IPV-before, pre-quarantine province of residence, and pre-quarantine socio-economic characteristics. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.