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Gender violence, enforcement, and human capital: Evidence from women's justice centers in Peru☆

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ABSTRACT

In many developing countries, access to justice remains unequal, especially for women. What are the implications of this inequality for gender-based violence and investments in children? This paper provides evidence from Peru's women's justice centers (WJCs), which are specialized institutions that provide police, medical and legal services to reduce gender-based violence. Examining the gradual rollout of WJCs across districts and villages, we find that the opening of a center reduces the incidence of gender-based violence, as measured by self-reported domestic violence, female deaths due to aggression, and hospitalizations due to mental health, by about 10%. This decrease in women's exposure to violence has intergenerational effects: WJCs substantially increase human capital investments in children, raising school attendance and test scores. The evidence suggests that these results are driven by an increase in enforcement against gender violence. After a WJC opens, there is an increase in the reporting and prosecutions for gender-specific crimes.

1. Introduction

Gender-based violence is a widespread social problem that affects 30% of women each year worldwide (WHO, 2013) and has long-term negative consequences for women's human capital and their children (e.g. Borker, 2017; Aizer, 2011). This problem is particularly relevant in developing countries, where women cannot rely on the justice system as a credible enough threat to prevent violence against them. High rates of under-reporting of gender-based violence and low arrest rates for crimes against women in these locations imply unequal access to the law, particularly for women (Eswaran, 2018).[1] Women may not trust formal institutions enough to report violence, given that police regularly ignore gender-based violence complaints (e.g. Jubb et al., 2010; Boesten, 2012).[2] Might this weak enforcement against gender-based violence generate substantial costs for women and children?

In this paper, we provide evidence that, increasing the enforcement against violence against women reduces the incidence of gender-based violence, and consequently improves children's outcomes. We exploit the impact of an innovative form of access to and representation of justice: women's justice centers (WJCs). WJCs are specialized state institutions designed to reduce gender-based violence, bringing together police, legal, and medical services in a single office in order to integrate all steps of the complaint process. WJCs have gained popularity in

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[1] Evidence from India finds that only 3 percent of women have ever had contact with the police, although the rate of gender violence is very high (Banerjee et al., 2012).

[2] In cases of family violence in rural Peruvian communities, for example, women are often assumed to be partially to blame for the conflict (Revilla, 1999). In many cases, police ignore domestic violence complaints entirely, reasoning that "domestic disputes" should be worked out within families and are not a police matter. Moreover, traditional methods of justice based on local customs are also often discriminatory towards women (Franco and González, 2009).

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developing countries in recent years, yet little is known about their effectiveness.[3]

This paper explores two questions about the relationship between WJCs, gender-based violence, and human capital investments in the context of Peru. The problem of violence against women is particularly acute in Peru, which has one of the highest rates of intimate partner physical and sexual violence in Latin America and the Caribbean, at about 30.1% (Bott et al., 2018; WHO, 2012). First, we ask whether improving law enforcement responses to gender-based violence increase the reporting and deterrence of gender-based violence. Second, we examine the inter-generational effects of increasing women's access to justice, focusing on investments in children's human capital. In particular, we provide insight into whether household investments in children increase when violence against women declines.

To estimate these effects, we combined highly detailed and novel datasets during the period 2006–2014. Our panel comprises geocoded individual and household-level survey data, geocoded administrative school-level data, administrative crime data, and female hospitalizations for mental health problems and deaths due to aggression. These categories of data enable us to analyze gender-based violence at a very disaggregated level before and after the opening of WJCs. Moreover, since a large part of our data comes from non-self-reported administrative records (e.g., hospitalizations), we can disentangle the effects of the reporting bias usually present in crime data.

Using a dynamic difference-in-differences design that exploits hyperlocal variation in the opening and timing of WJCs, we compare changes in gender violence and schooling among households and children who lived very close to WJCs to households and children from the same district who lived slightly farther away. In particular, we geomatch households and schools with detailed data on WJC locations and founding years in order to construct the presence of a WJC within 1 kilometer of a household/school. This empirical strategy allows us to compare changes over time in the outcomes of (a) households (including women and their children) and (b) schools already residing in proximity to a WJC center ("treatment households/schools") to those not yet reached by the program ("control households/schools").

We start with a simple conceptual framework to highlight the role of WJCs on reported gender violence, occurrence, and outcomes for women and children. In the model, WJCs are effective in decreasing violence in two ways. First, men would decrease violence if reporting is more likely to lead to prosecution. Second, this increase in the probability of successful reporting leads to women being more willing to report violence, which further discourages men from using violence in the first place. Both mechanisms imply a decrease in the use of violence by men. The model also provides insight into the effect of gender violence on children. In particular, the reduction in gender violence from WJCs may increase children's educational outcomes through a direct and an indirect channel. The former one relates to the direct effects on children's well-being from not being exposed to gender violence. The latter relates to an increase in the bargaining power of women, which under the regular assumption that women care more for their children leads to more resources allocated to them. Using the model, we are also able to discuss the option of reporting violence to the authorities, which is often overlooked in previous studies.[4]

We then provide a causal analysis showing that improving access to justice for women reduces domestic violence, female deaths due to aggression and improves women's mental health. In particular, using survey data we find that after a WJC opens, women who live within a 1-kilometer radius are significantly less likely to experience physical and emotional violence at the hands of their spouses. In addition, using administrative data the presence of a WJC center is associated with a 7% reduction in female deaths due to aggression and a 10% decline in mental health hospitalizations.

Consistent with the conceptual framework, we find evidence that after a WJC opens in a district, women increase the reporting of gender-based violence cases, and the probability that a perpetrator is prosecuted increases. Specifically, we find that gender-based violence complaints increase by 40%.[5] In addition, we also show that WJCs actually increase the costs for perpetrators through their ability to better collect evidence against them. We find a significant increase in the probability of being prosecuted for sexual assault, femicide, and rape after the opening of these centers.

Our second main finding is that WJCs can have positive intergenerational effects on children by reducing gender violence. We find that after the opening of a WJC, children living in households located near the center are significantly more likely to be enrolled, attend school, and have better national test scores. These results are robust to using different datasets that measure educational outcomes. Moreover, we find that the main results for children are driven by those from historically violent households (measured by whether their grandmother was subject to domestic violence), suggesting that WJCs' intervention in abusive households may change the behavior of offenders and victims by improving the situation of the women in the household and consequently their investments in their children.

Based on the model, we distinguish between a direct and an indirect way by which a decrease in violence may affect children's educational outcomes. First, according to the direct mechanism, WJC can increase children's education by improving their psychological well-being. While we do not have data to test this mechanism, a large literature shows that as violence against women declines, children can be less emotionally and psychologically affected, which in turn could improve their educational outcomes.[6] Second, WJC may increase investments in children by improving the bargaining power of women in the household.[7] Consistent with this mechanism, we find some evidence

[3] This type of intervention has been implemented in Brazil, El Salvador, Argentina, Ghana, India, Pakistan, Mexico, Ecuador, Uganda, and South Africa.

[4] Previous literature has mainly focused on the role of women's income as the determinant of power dynamics inside the household through the threat of separation or divorce. For example, Stevenson and Wolfers (2006) show how divorce laws affect domestic violence by changing the outside option of women. Others, such as Aizer (2010) and Chiappori et al. (2002), study how the gender wage gap in the labor market affects the distribution of resources and prevalence of violence inside the household. We propose a second threat point, which comes from allowing women to pursue a third route distinct from staying or leaving their husbands: reporting a case of violence to the authorities.

[5] This result is consistent with administrative data for 2017, which shows that 75% of women who went to a WJC completed the entire complaint process against their aggressor, compared to 10% of those who went to a traditional police station. Press release issued by the Peruvian Ministry for Women and Vulnerable Populations on January 8, 2018. https://www.mimp.gob.pe/salaprensa/nota-prensa.php?codigo=2662.

[6] In particular, previous research in developed countries document, as a correlation, that children exposed to domestic violence tend to have more health, emotional, and behavioral problems, as well as poorer academic performance (Edleson, 1999; Wolfe et al., 2003; Pollak, 2004; Fantuzzo et al., 1997; Koenen et al., 2003; Holt et al., 2008; Baldry, 2003; Carlson, 2000; Currie, 2006; Black et al., 2010).

[7] Several economic theories of household bargaining power suggest that policies designed to increase women's outside options when they are in abusive relationships may also affect intra-household allocation of resources through changes in their relative bargaining positions (Farmer and Tiefenthaler, 1996; McElroy and Horney, 1981; Manser and Brown, 1980). Similarly, the threat point for women may increase when they have access to justice and support services are more helpful. Previous empirical studies have shown that an increase in women's income appears to benefit children (Bobonis, 2009; Attanasio and Lechene, 2002; Thomas, 1990; Lundberg et al., 1997). Most of this literature finds that households in which women's income share is higher spend a larger fraction of their income on children's clothing and food. Although in the case under study we do not find a change in women's income or labor-force participation, when justice for women increases (thus triggering a decline in gender-based violence), women are more likely to invest in children.

that women living near a WJC are more likely to make joint decisions with their husband. We also examine other potential mechanisms but do not find evidence in their support. In particular, we show that results cannot be explained by a direct reduction in violence against children or improvements in general safety after a WJC opens.

The main threat to our identification strategy is the potential for time-varying unobservables that are correlated with both the timing of the opening of a WJC and changes in the prevalence of domestic violence and education outcomes. To ensure that our results are not driven by selection or time-varying unobservables, we perform several falsification exercises and robustness checks. In particular, we show that WJC placement was not anticipated by changes in gender-based violence and schooling. All these results are confirmed by an event study. We find no evidence of pre-trends on the main outcomes, and we do not find any effects on several district time-varying outcomes, such as municipal income and expenditures on education by the government.[8] We also use the procedures developed by Altonji et al. (2005) and Oster (2019) to show that the results are unlikely to be driven by omitted variable bias. In addition, all results are robust to including district specific trends and to limiting the samples to urban clusters, and to the use of different datasets that measure the same outcomes.

We also present the results for a number of placebo outcomes. In particular, we look at the data for non-gender-specific complaints, such as property crimes, which we consider as a placebo outcome because WJCs were not intended to address those type of complaints. We find no difference.[9] In addition, we find no effects on children's education for historically non-violent households. Finally, we find that effects dissipate for household and schools located further away from the WJCs. These results help rule out other confounding factors, such as an improvement in police presence or investments in education in these areas.

This paper is closely related to the literature studying the determinants of domestic violence. The results are related to the literature linking economic conditions, women's bargaining position in the household, and gender-based violence (e.g. Haushofer and Thomas, 2018; Aizer, 2010). While it is often assumed that improving the economic situation of women and ensuring they get an equal share of resources within their households will alleviate gender-based violence, research has shown that this is not always the case (Bobonis et al., 2013; Eswaran and Malhotra, 2011; Bloch et al., 2004).[10] A potential explanation for this result could be that the enforcement of justice around crimes committed against women may be perceived to be low, allowing perpetrators to use violence without repercussions. We show that WJCs offer an alternate scenario, in which offenders are punished for their crimes.

While most of the literature studying domestic violence has focused on the economic conditions, there is less research on the role of alternative interventions aim at improving law enforcement or shifting social norms. In relation to social norms, one exception is Shah and Muz (2020) who show how an intensive educational health program for girls and boys can reduce domestic violence in Tanzania by potentially shifting social norms. However, other type of lighter interventions such as, education-entertainment videos that discouraged gender violence were not successful at changing attitudes toward domestic violence (Green et al., 2016). While social norms can be difficult to change and may be persistent over time (Alesina et al., 2016), we complement this literature by looking at changes in the probability or severity of punishment for gender-based violence which may have an immediate impact on violence against women.[11]

In this respect, our paper is related to recent evidence looking at the role of laws to criminalize domestic violence (Chin and Cunningham, 2019; Ferraz and Schiavon, 2019; Iyengar, 2009; Aizer and Dal Bo, 2009), and the role of female officers' presence at police stations (Amaral et al., 2018; Miller and Segal, 2018; Perova and Reynolds, 2017). Both of these strands of the literature provide evidence indicating that these policies, independently, can increase the reporting of gender-based violence and reduce the incidence of several forms of violence, including feminicides. This paper complements this findings by analyzing an integral approach that increases both women's access to and representation in law and enforcement at all stages of the complaint process. Having a more integrated approach that combines all services in one office can be particularly important in developing countries, given that most victims do not follow up on their case after visiting the police and that only a small fraction of gender-based violence complaints pass to the next step due to lack of evidence. For example, evidence from India (Amaral et al., 2018) shows that while increasing the share of women police officers increased reporting, it did not affect the number of arrests and female homicides. This distinction is crucial because the previous literature has mostly focused on interventions that do not provide legal support, such as increasing the share of females in the police force (Amaral et al., 2018; Miller and Segal, 2018; Perova and Reynolds, 2017). By having an integrated approach, we show that WJCs increase the chances of punishment.

These results are in line with (Ferraz and Schiavon, 2019) who show how a legal reform that fostered the creation of special courts and increased the penalties for domestic violence cases was successful at reducing female homicides. In contrast to policies that are limited to improving representation, the Brazilian case also offers victim support, such as special courts, much like our setting. However, the Brazilian context differs from ours in that it focuses on legal actions and protective measures against perpetrators. In contrast, the WJCs in Peru include first-response services by providing immediate assistance, counseling, and medical care to victims in crisis situations, while also providing legal assistance in the aftermath of an event. Finally, we extend our knowledge of the scope of the effects of these policies beyond what was previously known. We show that policies aimed at reducing gender-based violence not only improve its reporting and reduce its incidence, but also have positive intergenerational effects of the children of the beneficiaries.

The remainder of this paper is organized as follows. Section 2 presents a brief background on the WJC intervention and a simple theoretical framework. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 presents the main results and investigates the channels through which WJCs affect domestic violence and children's schooling. Section 6 provides supporting evidence consistent with the identification assumptions. Section 7 concludes.

2. Background and conceptual framework

In this section, we first provide background information on WJCs and expansion in Peru. We then present a simple theoretical framework to understand how different features of WJCs may affect gender violence and children's outcomes.

[8] Our results are also robust to using the estimator proposed by de Chaisemartin and D'Haultfœuille (2020) that deals with heterogeneous treatment effects.

[9] Property crimes include theft, robbery, fraud, extortion, and usurpation. Nor do we find any effects on economic, finance, public, or drug crimes.

[10] On the one hand, employment opportunities, conditional cash transfers, or access to welfare services may empower women by increasing their resources within the household and outside options, increasing bargaining status in their relationship and thus decreasing their exposure to violence (Farmer and Tiefenthaler, 1996; Stevenson and Wolfers, 2006; Aizer, 2010; Hidrobo and Fernald, 2013). On the other hand, increasing the resources available to women may strengthen men's incentives to threaten or use violence to control these newly obtained resources or to regain decision-making power within the household. As a result, women may become more vulnerable to mistreatment (Bobonis et al., 2013; Eswaran and Malhotra, 2011; Bloch et al., 2004).

[11] Interestingly, changes in law enforcement can lead to shift in social norms in the long-run if a substantial share of individuals change their behavior (Acemoglu and Jackson, 2017).

2.1. Women's justice centers program

The 1994 Inter-American Convention on the Prevention, Punishment, and Eradication of Violence against Women, known later as the Convention of Belem do Pará, significantly expanded Latin America's definition of domestic and sexual violence by describing violence against women and establishing their fundamental right to a life without it. As a consequence, many countries in the region modified or enacted new legislation incorporating these issues into their political agenda. In particular, Peru altered the jurisdiction of its police and justice system to encompass domestic and sexual violence complaints and resolution. This new legal framework, paired with the government's awareness of the country's high levels of domestic violence, led in 1999 to the creation of women's justice centers (WJCs) by the Peruvian Ministry for Women and Vulnerable Populations (MIMP) as part of the National Program against Sexual and Family Violence.[12]

Women's justice centers (WJCs) are free-of-charge public centers that aim to strengthen the justice system's capacity to detect, process, and assist victims of domestic and sexual violence through an interdisciplinary approach that includes access to legal, social, and psychological resources. Basically, incoming victims receive a service designed to integrate all the steps of the complaint process (e.g., police station, attorney's office, and medical doctor) in a single office.[13] The goal is to reduce, to the extent possible, the time and hassle required to file a complaint and follow the legal procedures of the corresponding court of justice. In addition, to make the process easier after a traumatic experience, the personnel at WJCs are especially trained with human rights and gender equality values to reduce potential mistreatment of the denouncing victims (MIDIS, 2016).

In particular, the centers operate as follows. First, an incoming victim meets with an admissions officer who identifies the reason for their visit. If the victim is experiencing domestic or sexual violence-related issues, they are admitted. If the issue is unrelated, they are referred to the appropriate authorities. The admissions officer avoids in-depth discussions with the victim to prevent re-victimization; instead, they only inquire about the purpose of the visit and create an administrative record for admitted victims. Second, trained psychologists conduct initial interviews, perform psychological assessments, offer emotional support, and make referrals to specialized health centers. In cases of high risk, a psychological report is issued to assist victims in legal proceedings. The primary aim of the psychological assessment is to collect evidence for the legal process and to secure protective measures for victims of violence. If necessary, medical assistance is also provided at this stage. Victims receive medical support for as long as required based on the severity of their condition. Third, a legal team at the WJCs provides legal assistance to the victims. An attorney informs the victim about available legal options, offers guidance, actively participates in the legal process, and supports it until a verdict is reached. Case sponsorship entails representing and providing legal advice to the victim throughout the police, prosecutorial, and judicial phases. Finally, the WJCs offer social assistance to the victims. They assess the victims' risk levels as mild, moderate, or severe and propose actions to protect victims and prevent further instances of violence. Social workers offer counseling to victims and their families to help them cope with the violence and develop a safety plan.

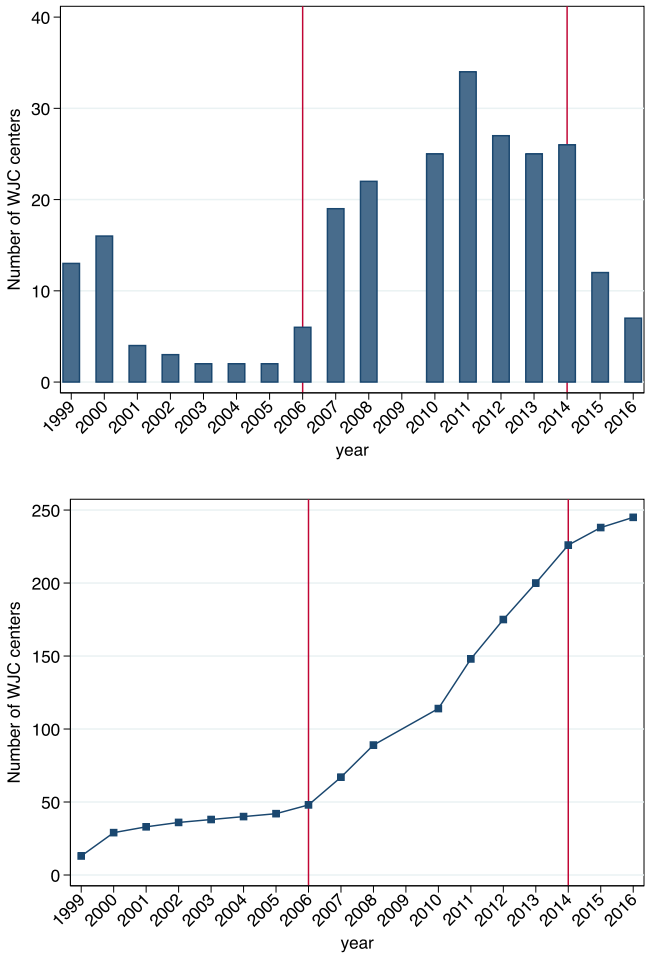

Fig. 1. Distribution and growth of the opening of the *Women's Justice Centers* (WJCs) by Year — Peru (1999–2016).
Notes: Author's estimates based on WJC centers data from the Peruvian Ministry for Women and Vulnerable Populations (MIMP).

The first women's justice center opened in the district of Lima in 1999. During the period 1999–2014, the number of centers has grown from 13 to 226, covering 100% of Peru's 24 regions and 96% of its provinces (188 of 196). Fig. 1 shows the distribution and growth of the opening of the WJCs over time. Whereas WJCs opened gradually throughout the first years of implementation, the program expanded exponentially after 2006. Up to that year, the average opening rate was about six WJCs per year; from 2006 to 2014, this rate climbed to 22 WJCs per year. Such escalation was provoked by a 2006 decentralization decree that granted local governments the right to open their own WJCs at the district level. Since the end of our study period, the only noteworthy change to the rollout of WJCs is that, starting in 2016, MIMP and the Peruvian Ministry of the Interior began coordinating certain operations related to gender-based violence. In particular, they started to open WJCs within police stations from 2017.

From a geographical coverage point of view, as of 2014, most of the WJCs were concentrated in Metropolitan Lima and Lima Provinces (31 WJCs). Outside that was the Callao region, with 4 WJCs; the rest of the coastal region outside Callao and Lima, with 46; the sierra region, with 117; and the jungle region, with 28 (Fig. 2).

According to MIMP's statistics, the number of domestic violence cases registered in the WJCs has increased substantially: from 29,759 in 2002 to more than 60,000 in 2016 (see Figure A-1). One of the most telling pieces of data on their effectiveness is a 2006–2008 survey of 51 WJCs administered by MIMP, which revealed that for the majority

[12] Note that the Peruvian Ministry for Women and Vulnerable Populations, now known as the *Ministerio de la Mujer y Poblaciones Vulnerables* (MIMP), was called the Ministry for Women and Social Development (*Ministerio de la Mujer y Desarrollo Social, or MIMDES*) when the WJC program was rolled out in 1999. http://www.mimp.gob.pe/contigo/contenidos/pncontigo-articulos.php?codigo=14.

[13] There is substantial qualitative evidence that traditional police fail to adequately investigate reports of sexual assault in Peru. In many cases, for example, police neglect to request lab testing of rape kits and other forensic evidence.

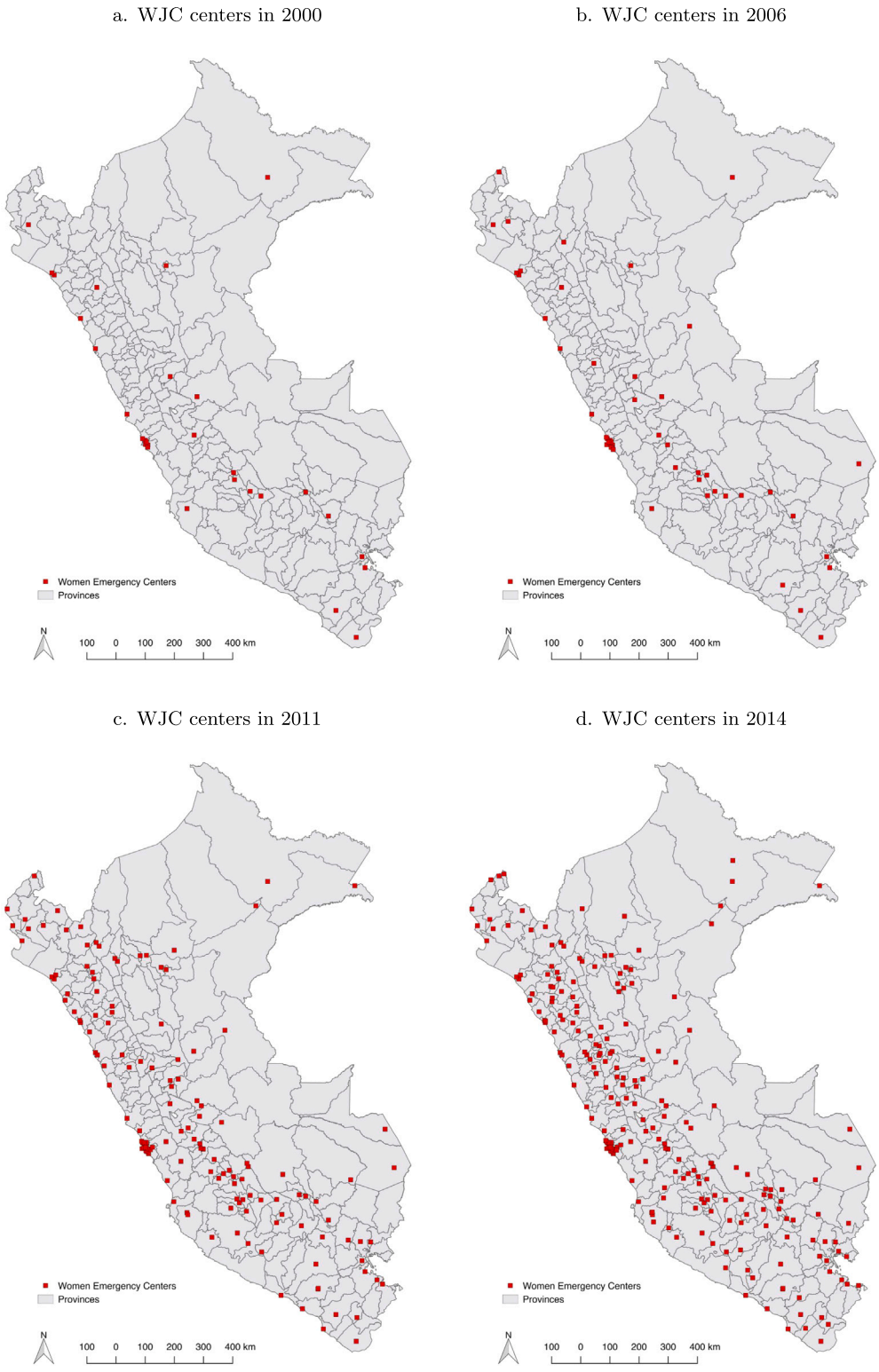

Fig. 2. Rollout of WJCs across time and space (1999–2014).
Notes: Author's estimates based on WJC centers data from the Peruvian Ministry for Women and Vulnerable Populations (MIMP).

(75%) of women who visited a WJC, domestic violence stopped during or after the program intervention (MIMDES, 2009).[14]

2.2. A model of gender violence and household bargaining

Building on the previous literature, we present a simple household bargaining model to better understand the channels by which WJCs affect women and children's outcomes. In particular, we focus on a model with asymmetric information in which violence can be used by the husband as a bargaining tool to increase resources allocated to him. In what follows, we assume that both men and women are egotistical toward each other. This is a common simplifying assumption, and results do not change if spouses care for each other, as long as their well-being is ranked higher in their preferences than their partners.

Inspired by the aim and design of the program, we focus on two effects of the introduction of a WJC. First, WJCs may lower the cost of reporting by women. This can be seen, for example, by the wider use of female officers. Second, since WJCs have under the same roof all the services necessary to fill in a gender-based violence complaint, they may also increase the likelihood of prosecution.[15]

A household is composed of three members: a woman W; a man M, and a child CH. Men's utility, $U_M(C_M)$, depends only on his consumption. Women, on the other hand, have an additional component representing their children's educational outcomes: $U_W(C_W, S(C_{CH}, V))$, where $V \in \{V_L, V_H\}$ represent low and high levels of violence. The arguments of S relate to two theoretical channels by which WJCs may affect children. First, a reduction in the exposure to violence increases their outcomes ($S(C_{CH}, V_L) > S(C_{CH}, V_H)$). Second, changes in household dynamics that lead to an increase in the amount of resources available to them will have a similar effect. Absent the utility cost associated with reporting and prosecution discussed later in this section, preferences are described in Assumption 1:

Assumption 1 (*Setup*)**.** Preferences are given by:

- Men's preferences can be represented by $U_M(C_M)$, with

$$\frac{\partial U_M}{\partial C_M} > 0, \quad \frac{\partial^2 U_M}{\partial C_M \partial C_M} < 0 \tag{1}$$

- Women's preferences can be represented by $U_W(C_W, S(C_{CH}, V))$, with

$$\frac{\partial U_W}{\partial C_W} > 0, \quad \frac{\partial^2 U_W}{\partial C_W \partial C_W} < 0, \quad \frac{\partial U_W}{\partial S} > 0, \quad \frac{\partial^2 U_W}{\partial S \partial S} < 0 \tag{2}$$

- Children's educational outcome function S satisfy

$$\frac{\partial S}{\partial C_{CH}} > 0, \quad \frac{\partial^2 S}{\partial C_{CH} \partial C_{CH}} < 0, \quad S(C_{CH}, V_L) > S(C_{CH}, V_H) \tag{3}$$

Inside a household, husband and wife bargain to allocate a total amount of resources I between each member. The final allocation of resources depends on their outside option, d_M and d_W, as well as on whether the man chooses to use violence to increase his bargaining power. Following (Aizer, 2010), the final allocation follows Kalai's (1983) asymmetric Nash bargaining solution:

Assumption 2 (*Bargaining*)**.** The bargaining process between men and women leads to outcomes that are a solution to the problem:

$$\max \{C_M, C_W, C_{CH}\}(U_M - d_M)^{\beta(V)}(U_W - d_W)^{(1-\beta(V))} \tag{4}$$

$$\text{s.t.} \begin{cases} U_M \geq d_M \\ U_W \geq d_W \\ C_M + C_W + C_{CH} \leq I \end{cases}$$

Kalai's bargaining solution is attractive in this setting because it allows for asymmetric bargaining while remaining efficient.[16] In (4), β should be interpreted as a measure of men's bargaining power relative to women. In particular, for this model we assume that violence can be used by men to improve his standing inside the household: $\beta(V_H) > \beta(V_L)$. Let $C_M(V)$, $C_W(V)$, and $C_{CH}(V)$ be each member's solution to (4) as a function of violence. It is straightforward to show that $C_M(V_H) > C_M(V_L)$, $C_W(V_H) < C_W(V_L)$, and $C_{CH}(V_H) < C_{CH}(V_L)$. This implies that $U_M(V_H) > U_M(V_L)$ and $U_W(V_H) < U_W(V_L)$: more violence increases men's consumption and utility while decreasing resources for women and children, as well as their utility.

The final elements of the model are the timing and the dynamics of violence, reporting, and prosecution. The game starts with the husband deciding whether to use violence ($V = V_H$) or not ($V = V_L$) at the start of the game. If he chooses V_L or the woman does not report V_H, the game ends and the outcome $\{C_M(V_L), C_W(V_L), C_{CH}(V_L)\}$ is implemented. Otherwise, if the man chooses V_H and the woman reports her husband to the authorities, she has to pay a utility cost δc. We assume that the cost of reporting is the product of the institutional context δ, and an idiosyncratic cost c. Policy interventions such as the introduction of WJCs can decrease the institutional cost by making women feel more comfortable. The idiosyncratic cost, on the other hand, captures other factors such as family pressure and values, how much women will be judged if others find out about their case, etc. c defines women's type and it is unknown by men, although in equilibrium they hold correct beliefs about its distribution, with full support in $[0, \infty)$ and CDF $F_c(\cdot)$.

Reports, however, lead to an uncertain outcome. With probability $(1 - p)$, the authorities do not intervene and outcome $\{C_M(V_H), C_W(V_H), C_{CH}(V_H)\}$ is implemented. If instead the report is successful in generating a prosecution, men must pay a utility cost k (e.g. going to jail, shame, or community work) and the couple separate resulting in utilities (U_M^P, U_W^P). These outside-option utilities are a novel characteristic of this model. Previous studies focus on the threat point that leads to women wanting to separate by mutual consent or divorcing their husbands if bargaining does not result in utility of at least d_W (see the second participation constraints in (4)). In our setting, after V_H is chosen women have a second threat point, derived from the utility they can obtain if they manage to prosecute their husbands, U_W^P.[17] Men are characterized by their cost of getting prosecuted, k, with

[14] Ministerio de la Mujer y Desarrollo Social. 2009. Investigacion operativa: "Eficacia de la intervencion de los Centros Emergencia Mujer". Available at http://www.mimp.gob.pe/files/programas_nacionales/pncvfs/estadistica/eficacia_intervencion_cem.pdf.

[15] There are many non-competing theories explaining why men use violence against their partners (see Angelucci and Heath (2020), for a description). Among them, the most commonly used in the literature is the intrinsic theory, which states that violence provides an intrinsic benefit to men (e.g. ego motive). One limitation of this theory is that it implies that violence is a substitute of consumption for men. Under this assumption, an intervention that increases the cost of violence, such as WJCs, should have a positive effect on consumption of men. Since this theory is not supported by our data, we focus on an alternative extractive theory: domestic violence is used by men in order to increase their bargaining power and extract more resources from their spouses. Observationally, this theory differs from the intrinsic one in that interventions that make the use of violence more difficult are expected to decrease men's consumption and decision power inside the household. In line with the conclusions of Angelucci and Heath (2020), we believe that many channels can be simultaneously playing a role in explaining violence, and different interventions may be affecting different channels.

[16] Any efficient bargaining allocation would be a solution of this problem, since it yields the whole contract curve as we let β vary. It is straightforward that strictly monotonic and convex preferences (see Assumption 1) are enough to guarantee uniqueness.

[17] Although not the focus of this paper, alternative policies aim at decreasing violence could directly focus U_W^P. Some examples would be changes in the penal code stipulating higher compensation; housing, job training, or monetary aid to victims of domestic violence, etc.

full support in $[0, \infty)$ and CDF $F_k(\cdot)$.[18] Assumption 3 summarizes the process of violence reporting. Assumption 4 states that under certain prosecution, women prefer to report domestic violence.

Assumption 3 (*Violence and Reporting*)**.** The dynamics of violence and reporting are:

- If $V = V_L$, payoffs are given by:

$$(U_M, U_W) = (U_M(V_L), U_W(V_L)) \tag{5}$$

- If $V = V_H$ and women do not report, payoffs are given by:

$$(U_M, U_W) = (U_M(V_H), U_W(V_H)) \tag{6}$$

- If $V = V_H$ and women report, payoffs are given by:

$$(U_M, U_W) = \begin{cases} (U_M(V_H), U_W(V_H) - \delta c) & \text{if reporting is unsuccessful (w.p. } p) \\ (U_M^P - k, U_W^P - \delta c) & \text{if reporting is successful (w.p. } 1-p) \end{cases} \tag{7}$$

- Men hold correct beliefs about their wives' type, with c.d.f.: $c \sim F_c[0,1]$
- Women hold correct beliefs about their husbands' type, with c.d.f.: $k \sim F_k[0,1]$.[19]

Assumption 4 (*Incentive For Violence and Reporting*)**.** If prosecution is certain, women with no idiosyncratic cost ($c = 0$) prefer reporting to resignation:

$$U_W^P > U_W(V_H) \tag{8}$$

2.3. Solution

We will use the solution concept of Perfect Bayesian Equilibrium (PBE). Without loss of generality, we focus on the unique equilibrium where under indifference women choose to report and men choose low levels of violence. We start analyzing the reporting decision of women after observing V_H:

Proposition 1 (*Women's Reporting Strategy*)**.** *After observing V_H, women choose to report if and only if $c \leq \bar{c}$, where*

$$\bar{c} = \frac{p}{\delta}\left[U_W^P - U_W(V_H)\right] \tag{9}$$

Proposition 1 states that only women with low idiosyncratic cost report domestic violence. A corollary of Proposition 1 is that the fraction of women reporting is $F_c(\bar{c})$. As discussed in Section 2, WJCs affect incentives to report by reducing the institutional cost of reporting (lower δ), and by increasing the efficiency of the judicial system (higher p). Proposition 2 summarizes this insight.

Proposition 2. *WJCs increase the proportion of women who are willing to report high levels of violence, $F_c(\bar{c})$, by increasing the probability of prosecution, p, and decreasing the institutional cost of reporting, δ.*

It is important to note that Proposition 2 does not state that with the introduction of WJCs we are expected to observe more reporting, but rather women are more willing to do it *after* V_H. In order to understand how a WJC affects reporting, we must also account for its effects on the prevalence of domestic violence. Men's decision depends on how likely women are to report V_H, and on how likely a report is to end in prosecution. For the model to have a bite, in Assumption 5 we state that men have incentives to use violence absent the cost of prosecution.

Assumption 5 (*Incentives For Violence*)**.** Both before and after the introduction of WJCs, absent the cost of prosecution (i.e. $k = 0$), men prefer to use violence:

$$U_M(V_L) < \left[1 - pF_c(\bar{c})\right] U_M(V_H) + pF_c(\bar{c}) U_M^P \tag{10}$$

Finally, Proposition 3 reveals that men will use violence if the expected gains can compensate for the possible cost of prosecution.

Proposition 3 (*Men's Violence Strategy*)**.** *Men will choose V_H if and only if $k < \bar{k}$, where*

$$\bar{k} = \frac{\left[1 - pF_c(\bar{c})\right] U_M(V_H) + pF_c(\bar{c}) U_M^P - U_M(V_L)}{pF_c(\bar{c})} \tag{11}$$

The equilibrium is characterized by correct beliefs about types ($F_c(\cdot)$, $F_k(\cdot)$), and by Propositions 1 and 3. In the remaining of this section, we will study the effects of WJCs on observed reporting, violence, and children's educational outcomes.

2.4. Comparative statistics

In this subsection, we study the effects WJCs using comparative statistics. As explained above, WJCs have two effects: they decrease the idiosyncratic cost of reporting ($d\delta < 0$), and they increase the efficiency of the legal system ($dp > 0$). As per convention, in the following analysis we use the notation f_c and f_k to refer to the PDFs of c and k, respectively.

We start by studying the effects of WJCs on willingness to report in Proposition 4.

Proposition 4. *The introduction of WJcs increase the probability of reporting conditional on violence:*

$$dF_c(\bar{c}) = f_c(\bar{c})\left(\frac{\delta(dp) - p(d\delta)}{\delta}\right)\left[U_W^P - U_W(V_H)\right] > 0 \tag{12}$$

Propositions 3 and 4 allows us to state the first observational lesson of the model, which is that there are two mechanisms by which WJCs reduce domestic violence. First, men are less willing to use violence if after reporting the probability of being prosecuted increases. Second, men are further discouraged by women's increased willingness to report them.

Lemma 1. *WJCs decrease violence prevalence:*

$$dF_k(\bar{k}) = -f_k(\bar{k})\left(\frac{F_c(\bar{c})(dp) + p(dF_c(\bar{c}))}{pF_c(\bar{c})}\right)\left[U_W(V_H) - U_W(V_L)\right] < 0 \tag{13}$$

The effects on observed reporting are not as straightforward. Using Bayes Rule, we can divide the probability of reporting between the likelihood of reporting conditional on violence ($F_c(\bar{c})$) and the unconditional probability of violence ($F_k(\bar{k})$). Ambiguity comes from Proposition 4 stating that the former will increase with WJCs, while at the same time Lemma 1 states that the latter will decrease. In which way these conflicting forces are resolved in equilibrium is an empirical question. In the next sections, we find that WJCs do reduce the overall frequency of reported violence. Note that this finding should be taken as an underestimate of the effects on the willingness of women to report violence.

Lemma 2. *WJSs have an ambiguous effect on reported violence:*

$$d(F_k(\bar{k})F_c(\bar{c})) = F_c(\bar{c})d(F_k(\bar{k})) + F_k(\bar{k})d(F_c(\bar{c})) \lesseqgtr 0 \tag{14}$$

The final focus of this paper is on the effect of WJCs on educational outcomes. As with violence prevalence, we can identify two channels by which WJCs can affect children. First, violence enters the educational outcomes function directly. Second, a reduction in violence indirectly increase children's outcomes by increasing women's bargaining power, which in turn leads to more resources being allocated to them. These insights are summarized in Lemma 3.

[18] For simplicity, we assume that men differ in their cost of prosecution. Equivalently, we could fix this cost and assume that their type is related to the bargaining gains from violence ($\beta(V_H) - \beta(V_L)$).

[19] Since women play second and men's type does not affect their payoff, their beliefs about k are not relevant in this model.

Lemma 3. *WJCs increase children's educational outcomes directly and indirectly through an increase in resources:*

$$dS = dF_k(\bar{k})\left[S(C_{CH}(V_L), V_L) - S(C_{CH}(V_H), V_H)\right] > 0 \quad (15)$$

3. Data

3.1. Individual- and household-level data

To study the impact of WJCs on outcomes for women and their children, we rely on microdata from the Peruvian Demographic and Health Survey (DHS), which has been collected for the period 2000–2014.[20] These surveys are cross sections designed to be representative at the national and regional (second administrative) levels. The DHS employs a stratified random cluster sampling procedure in which the country is divided into several primary sampling units (in this case, districts) and clusters of households are randomly selected.

The survey collects primarily demographic and health information from women aged 15 to 49 years old, including data on their fertility, weight, marital status, employment status, household decision making, and socio-economic characteristics, among other things. Additionally, it includes demographic and socioeconomic characteristics for each of the women's household members (e.g., husband and children), which we exploit in our analysis.

In addition to the standard survey, the Peruvian DHS also includes a domestic violence module that asks eligible women if they have experienced physical, sexual, or emotional abuse from their current or previous partner in the previous 12 months.[21] While all women between the ages of 15 and 49 are asked to participate in the standard survey, only one woman in each household who has been or is married or partnered is randomly selected to complete the domestic violence module. Women who are not married or cohabiting are excluded from the sample.

This selection process is undertaken by the DHS program in order to minimize underreporting of domestic violence events.[22] The DHS captures four different types of domestic violence: moderate physical violence, severe physical violence, sexual violence, and emotional violence. These domestic violence categories are defined by the DHS as ex-post classified questions.[23] In this study we define exposure to a domestic violence event as a woman's experiencing any type of moderate or severe physical or sexual violence during the previous 12 months.

One advantage of using this household survey is that we can link children's outcomes (e.g., school attendance status) with their mother's and grandmother's self-reported domestic violence. This information is crucial in order to be able to understand the mechanisms behind the results. Since attendance rates are not accounted for in the School Census, we use the Peruvian DHS to estimate the share of children in primary grades who are enrolled in and attending school.[24] This survey also allows us to measure children's school performance (e.g., passed a grade, repeated a grade, dropped out).

Panel B of Tables A-1 and A-2 provides summary statistics on women's characteristics and children's school attendance status during 2006–2014, respectively.[25] According to the Peruvian DHS, the data indicates that 39% of ever-partnered Peruvian women disclosed experiencing abuse from their spouse, which is remarkably high. As for children's education outcomes, the school attendance rate at the primary level is 97% for both boys and girls.

In addition, the Peruvian DHS also records GPS coordinates for every cluster of households in a certain district, which allows us to measure not only the presence of WJCs in the district of residence but also proximity to the closest WJC.[26] Although this data was collected yearly, in this study we were able to obtain the GPS cluster locations for only the 2000,2004–2008, 2009–2011, and 2014 Peruvian DHS Surveys. Since the DHS does not disclose the names of the villages (*centros poblados*) where the clusters are located, the final sample is a repeated cross section of individuals (women and children) in which the lowest geographical unit we can condition on is the district.

One potential concern with this database is linked to the fact that the GPS locations of the sampled DHS clusters of households are displaced before public release to preserve the confidentiality of respondents. The GPS displacement is randomly carried out so that urban clusters are uniformly displaced up to two kilometers and rural clusters are displaced up to five kilometers, with 1% of the rural clusters displaced up to 10 kilometers. In addition, the displacement is restricted so that the points stay within the second administrative level, which is the province. Therefore, the GPS displacement procedure introduces a random error, which could affect the results of the analysis (Burgert et al., 2013).

Thus, we follow several recommendations proposed by Perez-Heydrich et al. (2013) for reducing distance measurement errors. First, they suggest that the amount of measurement error depends on the spatial density of the resource facilities. As the density of resource facilities decreases, the probability that a DHS cluster is correctly

[20] The *Encuesta Demografica y de Salud Familiar* (ENDES) is the Peruvian version of the Demographic and Health Surveys (DHS). These surveys are available for the following years: 2000,2004–2008, and 2009–2014. The Peruvian DHS is a continuous survey, which means that the data has been collected quarterly (as opposed to every five years) since 2004.

[21] It should be noted that although this is an important measure of domestic violence, it does not report the various forms of gender-based violence that affect women beyond spousal and inter-family relationships.

[22] The domestic violence module of questions is implemented only to a subsample of the women selected for the Peruvian DHS sample. In general, the interviewers are women trained to elicit trust from the respondents. There are three security and ethical precautions increasingly mandated by the DHS program for those collecting data on domestic violence. The first requires that the interviewer not continue with the questions on domestic violence if privacy cannot be ensured. The second requires that only one eligible woman in each selected household is to be administered the module questions, even if more than one is eligible. Interviewing only one woman in each household allows for the minimization of possible security breaches caused when others in the household discover that information on domestic violence was given. The third requires that the domestic violence questions only be administered to previously or currently married or cohabiting women, even though the DHS sample includes all women ages 15–49. Only 1% of eligible women were not interviewed because privacy was not possible in the household. Despite the selection measures taken by the DHS program, this empirical analysis may still suffer from measurement issues due to under-reporting. In order to account for this, we also study alternative outcomes to measure violence against women, including female deaths due to aggression.

[23] Specifically, the DHS defines *moderate physical violence* as a woman experiencing at least one of the following acts from her spouse or partner: (a) the spouse has pushed, shaken, or thrown anything; (b) the spouse has slapped the respondent; (c) the spouse has punched the respondent with his fist or something harmful; (d) the spouse has kicked or dragged the respondent. *Severe physical violence* is defined as a woman experiencing at least one of the following acts: (e) the spouse has tried to strangle or burn the woman; (f) the spouse has threatened the woman with a knife, gun, or other weapon; (g) the spouse has attacked the woman with a knife, gun, or other weapon. *Sexual violence* is defined as a woman experiencing at least one of the following acts: (h) the spouse has physically forced sex when not wanted; (i) the spouse has forced other sexual acts on the woman when not wanted; (j) the spouse has twisted the woman's arm or pulled her hair.

[24] For the children's school attendance analysis, we also use the 1996 Peruvian DHS in order to assess the validity of the identification strategy.

[25] We focus our analysis on the middle of the rollout period,2006–2014, for which identifying assumptions are likely to hold. We discuss this choice in more detail in Section 6.

[26] In the Peruvian DHS (2000–2014), there are on average 25 households per cluster, which may range from 1 to 45 households.

linked to the closest WJC increases for all types of locations (urban and rural). In Peru, there are a total of 226 WJCs by 2014; this means that the spatial density of the WJCs is quite low, and thus the measurement error is likely to be significantly reduced. Second, the authors recommend studying the effect of the service within a reasonable buffer distance, rather than using the closest distance to the resource facility. For this reason, we measure exposure to the WJC through different groups of Euclidean distance buffers. Moreover, we also limit our analysis to urban areas, because in these locations the range of displacement is less than in rural areas. Finally, as robustness we validate the results using school geocoded data.

3.2. School-level data

We use two school-level datasets: the Peruvian School Census (*Censo Escolar*, CE) and the Census Evaluation of Students (*Evaluacion Censal de Estudiantes*, ECE). The Peruvian School Census is a large-panel dataset on primary school enrollment that covers the universe of schools in Peru during the period 1998–2014. This dataset has been collected on a yearly basis by the Peruvian Ministry of Education (with the exception of the year 2003), and it contains a rich set of information at the school level.

More specifically, the School Census collects comprehensive data on the total number of enrolled students by age, grade, and gender. This data is designed to reflect enrollment (not attendance) statistics corresponding to the months of May–July. The School Census also collects data on school characteristics such as language of instruction, public or private, urban or rural area, and other physical plant characteristics (e.g., electricity or piped water). We complement this data with the Census Evaluation of Students, which contains the standardized test scores of a national exam administered every year to all primary school students in second grade during the period 2007–2014. This exam has two portions: math and (Spanish) language skills.

Each school in these datasets is given a unique ID number, which allows us to follow schools over time. In addition, one of the main advantages of these school datasets is that they are geocoded, which means that we can observe the exact location of the schools. We can then combine these data with the data on the geographic location of WJCs to see whether the school is located near a WJC and thus affected by the opening of these centers.

Panel A of Table A-3 shows the years of data coverage and the number of schools by rural/urban region. In order to be consistent with the individual-level data, for this analysis, we also use data that covers the period 2006–2014. In the later years, the dataset covers a larger share of schools. It is important to note that during the period of study, some schools closed and others opened; additionally, as mentioned above, no data was collected for the year 2003. Although this means we do not have a balanced panel, by including school fixed effects, we ensure that we compare the same schools over time. The main analysis, then, draws on a nine-year unbalanced panel dataset of 36,994 primary schools (grades 1–6).[27]

Panel C of Table A-3 provides some summary statistics on school enrollment and school characteristics. The average primary school in our sample has 95.9 students. Around one third of primary schools in Peru are not equipped with electricity and piped water. The majority of primary schools are public and teach in Spanish, but there is also a small proportion that teach in Quechua and other native languages.

3.3. District-level data

Information on the rollout of the WJCs was provided by the Peruvian Ministry for Women and Vulnerable Populations (MIMP) and consists of a directory of WJCs across all of Peru. This directory contains the name of the WJCs, their founding dates (date-month-year), their administrative locations (district-province-department), and their addresses during the period 1999–2014. By using the administrative locations and addresses provided in the directory of the MIMP, we were able to geocode all the WJCs, which allows us to obtain not only the district where they are located but also their exact GPS location.

This data collection project resulted in a dataset of 226 WJCs from 1999 to 2014. Fig. 1 shows a histogram of WJC founding dates and also illustrates the evolution of the opening of WJCs from 1999 to 2016. Fig. 2 maps the rollout of the WJCs at the national level, which allows one to visualize the extensiveness and national scope of the program. From both figures, we can clearly see a substantial growth in the number of centers over time, with 81% of them being founded after the year 2005.

We complement this information with confidential data on female deaths due to aggression and female hospitalizations for mental health problems, which were obtained from the Peruvian Ministry of Health — National Institute of Statistics and Informatics (INEI). This database contains the number of registered cases of hospitalizations by type of illness and gender. For the purpose of this analysis, we use female hospitalizations for mental health problems. This information is recorded by health facilities such as hospitals and is only available at the district level and covers mostly urban areas. The number of registered cases in health facilities includes women between the ages of 18 and 59 and covers the period 2006 to 2015.[28] It also records the number of hospitalizations that resulted in deaths for different types of causes. The main cause of female mortality that is relevant to this analysis is death due to aggression since about 90% of them include femicides, that is homicides committed by a family member. For the case of female deaths due to aggression, there is only data available for the years 2007, 2012–2014.[29]

We also use information on complaints of crimes registered in the Police Reporting System of the National Police of Peru (*Sistema Informático de Denuncias Policiales*, SIDPOL) and the National Registry of Complaints of Crimes and Misdemeanors of the INEI (*Registro Nacional de Delitos en las Dependencias Policiales*). This database contains the number of crimes according to type of crime and place of registration for the period 2011–2017, and this data is available at the district level.[30] Finally, from the National Penitentiary System, we obtained data on the number of prosecutions related to femicide, rape and sexual assault for the period 2006–2014.

3.4. Measuring exposure to WJCs

In order to be able to match the data on WJCs with the outcomes of interest, given that our outcome data is at different levels of aggregation, we construct two measures of exposure to the program: (i) WJC within a 1-kilometer Euclidean buffer of the DHS cluster of households/school and (ii) WJC in the district.

The first measure uses the GPS coordinates of the DHS clusters/schools to measure a 1-kilometer Euclidean distance buffer from every DHS cluster/school location. For this method, the Euclidean buffer of one kilometer is first centered on each DHS cluster/school, then each DHS cluster/school is linked to a WJC if the WJC falls within the buffer, without consideration of district administrative borders. For instance, a DHS cluster/school located within one kilometer of a WJC

[27] The primary-school sample covers between 3.5 million and 4.1 million students each year.

[28] This is the only period that data was available.

[29] Unfortunately, the data was provided at the district-year level and thus, we are unable to separate the other 10% of the cases. We also obtained data on femicides from the National Police of Peru, however the only data available covers the last years of our sample and contains very few observations.

[30] The typology of crimes follow the Penal Code. For more detail see the Penal Code (Legislative Decree No. 635), Title II. http://spij.minjus.gob.pe/content/publicaciones_oficiales/img/CODIGOPENAL.pdf.

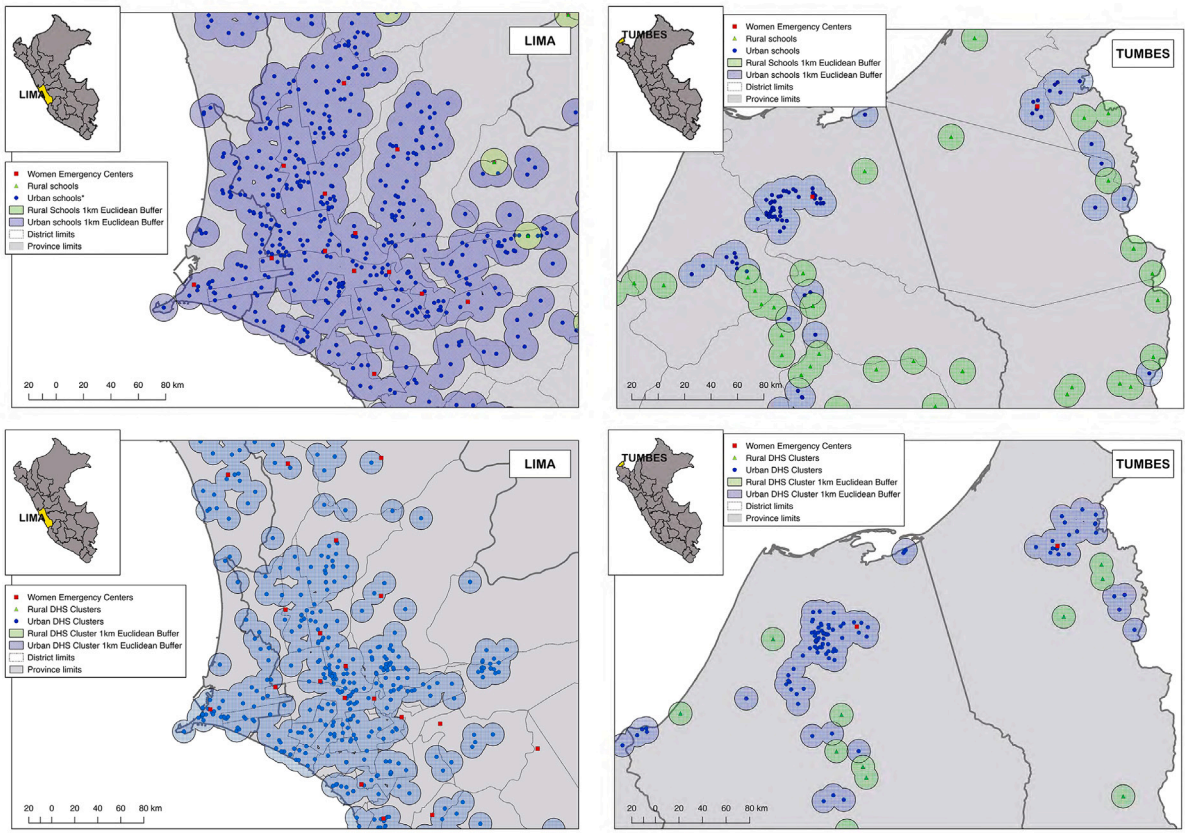

Fig. 3. Euclidean distance buffers and WJC centers (Schools and DHS Clusters of Households) – Lima and Tumbes.

founded in 2008 is coded as having a WJC within one kilometer of the DHS cluster/school since 2008. Fig. 3 shows a visual representation of the Euclidean buffers for two specific regions in Peru: Lima and Tumbes.

The Euclidean buffer is our preferred measure, since based on our conversations with Peruvian policymakers, access to the WJC services may decline with distance. Therefore, for geocoded outcomes we measure exposure based on how far the centers are from respective households, such that individuals residing at different points in the same district may have different levels of exposure to the WJCs.[31] Panel A of Tables A-1 and A-2 and Panel B of Table A-3 show descriptive statistics of exposure to the WJCs at the individual (women and children) and school level.

4. Empirical strategy

4.1. Placement of WJCs

A central methodological issue in our analysis is the fact that WJCs are not placed randomly across the country. Although our analysis will take advantage of variation over time, which will account for any fixed differences across districts and schools, it remains important to understand what drives placement, since this type of decision may not be orthogonal to other factors that could affect women and children's outcomes of interest.

We address this concern in a number of ways that lead us to believe that the link between the opening of the WJCs and the outcomes of interest is causal. First, we had several discussions with Peruvian policymakers and WJC managers about the location choices. From the foundation of the first WJC in 1999 to the end of 2005, the primary criteria they cited when deciding where to locate were population density and level of infrastructure at the regional level. In this stage, they prioritized capitals and large cities for WJC placement. Starting in 2006, after the decentralization process that transferred the responsibility of the WJCs to local governments (districts), Peruvian policymakers decided to open new WJCs at the district level. To do so, they incorporated additional criteria such as proximity to police stations, district attorney offices (known as *fiscalias*), and health establishments. Even though program guidelines suggested that priority should be given to districts with sufficient judicial and medical infrastructures to support a WJC, on several occasions political representatives had a certain autonomy in deciding the order in which districts received the program.

There is also anecdotal evidence from the authorities that the placement of WJCs was primarily developed by taking population density into account, without considering the incidence of violence against women and the location of other services. This is likely due to a lack of reliable data on domestic violence or femicides for all the districts in Peru prior to the opening of the centers. Official data on femicides in Peru started to be recorded only after 2009, and several ministerial reports have documented the fact that WJCs failed to consider the rate of incidence of violence against women in program placement.[32] Moreover, our conversations with Peruvian policymakers suggest that educational considerations, particularly enrollment rates and school performance, were never factored into program placement decisions.[33]

[31] Even though the hospital data is at the district level, it covers mostly patients that are from urban centers that have access to these facilities.

[32] See, for instance, Ombudsman Office, *Informe Defensorial N 144. Centros de Emergencia Mujer: Supervisión de los servicios especializados en la atención de víctimas de violencia familiar y sexual*, July 2009, Ministerio de la Mujer y Desarrollo Social, *Investigacion operativa: "Eficacia de la intervencion de los Centros Emergencia Mujer"*, August 2009.

[33] Importantly, while it may be the case that WJCs opened in places where there were pre-existing public services such as health establishments, none

Table 1
Placement of WJC centers in the district.
Source: MIMP (*Ministerio de la Mujer y Poblaciones Vulnerables)*.

Dependent variables	WJC in district, by 2014		Added WJC in district during 2006–2014			
	(1)	(2)	(3)	(4)	(5)	(6)
# Police Stations	0.0093	0.0064	−0.0098	−0.0105	−0.0446**	−0.0128
	(0.0123)	(0.0125)	(0.0130)	(0.0130)	(0.0205)	(0.0132)
# of Health Establishments	0.0024	0.0024	0.0001	0.0000	0.0005	0.0001
	(0.0016)	(0.0016)	(0.0013)	(0.0013)	(0.0012)	(0.0013)
# Criminal Attorney Offices	0.0267	0.0250	0.0030	0.0027	−0.0100	0.0016
	(0.0289)	(0.0284)	(0.0251)	(0.0249)	(0.0206)	(0.0246)
# Family Attorney Offices	0.0122	0.0128	0.0160	0.0162	0.0069	0.0168
	(0.0518)	(0.0512)	(0.0488)	(0.0485)	(0.0431)	(0.0482)
# Courts	0.0236	0.0235	0.0147	0.0145	0.0122	0.0144
	(0.0145)	(0.0144)	(0.0135)	(0.0135)	(0.0110)	(0.0134)
Log. Population, 2000	0.0744***	0.0723***	0.0740***	0.0717***	0.1167***	0.0714***
	(0.0112)	(0.0119)	(0.0102)	(0.0101)	(0.0189)	(0.0111)
△ Primary Enrollment, (1998–2005)			0.0001		0.0003	0.0002
			(0.0003)		(0.0004)	(0.0003)
△ Secondary Enrollment, (1998–2005)				−0.0001	−0.0002	−0.0001
				(0.0001)	(0.0002)	(0.0001)
Domestic Violence, 2000					0.1065	
					(0.0839)	
CCT *Juntos* in the district		−0.0605**				−0.0451*
		(0.0242)				(0.0249)
# Households with CCT *Juntos*, 2014		0.0000				0.0000
		(0.0000)				(0.0000)
Observations	1,843	1,838	1,843	1,843	700	1,838
R-squared	0.3671	0.3708	0.1635	0.1638	0.1555	0.1670
Department FE	YES	YES	YES	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table shows the effects of district characteristics on WJC center's placement. The left-hand side variable in columns 1 and 2 is the presence of a WJC in the district by 2014; in columns 3 to 6 it is whether any center was added during the sample period 2006–2014. Standard errors are in parentheses, clustered at the district level.

A second way to address the concern about the non-random placement of WJCs is that we are able to evaluate this endogenous placement statistically using our data. To do this we estimate, at the district level, (a) the determinants of having a WJC by the end of the sample in 2014 and (b) the determinants of adding a WJC during 2006–2014, the period when the program grew substantially. We focus on several variables at the district level cited by Peruvian policymakers, including the number of justice courts, district attorney offices, police stations, and health establishments. We also control for district population at baseline and department fixed effects. Moreover, in order to verify that education patterns before the program began do not predict where the WJCs are introduced, we also control for pre-program changes in primary and secondary school enrollment at the district level. Unfortunately, we are unable to perform the same test for the hospitalization data, due to a lack of pre-program data on these variables for all the districts in Peru. We do, however, control for baseline (self-reported) domestic violence at the district level by using the 2000 Peruvian DHS, which contains a representative sample of 700 districts in Peru. Moreover, in the next section we perform an event study for all the variables of interest exploiting variation in the years when data is available before the opening of WJCs.

The results from these regressions are shown in Table 1. The results corroborate the evidence we collected from our conversations with Peruvian policymakers and WJC managers. Districts that contain more police stations, district attorney offices, and health establishments and the more densely populated ones are more likely to have WJCs by 2014 and also more likely to add them during the 2006–2014 period. However, none of the coefficients except population are statistically significant.[34] In addition, pre-program changes in districts' primary and secondary school enrollment do not seem to have any impact. Neither coefficient is statistically significant, and both are very small. Similarly, domestic violence does not appear to have any impact on WJC placement. These findings suggest that WJC placement between 2006–2014 does not seem to have been based either on pre-program changes in schooling or on baseline levels of domestic violence.

Finally, we note two additional concerns that might threaten the validity of our research design. First, one might be worried that another shift (e.g., a government program or policy change) might have been rolled out during the same period and in the same places as the WJCs, which might also have an impact on education outcomes. An obvious candidate is the CCT program *Juntos*, which was launched in September 2005.[35] *Juntos* integrates two broad objectives. In the short run, it aims to reduce poverty by providing households with cash transfers. While in the long run, it aims to break the intergenerational transmission of poverty by promoting human capital through improving access to education and health services.

In spite of this, several reasons lead us to believe that *Juntos* is not a confounding factor in our empirical strategy. Districts were selected for program participation based on an index that includes poverty rate and the percentage of villages affected by violence during the 1980–2000 civil conflict. The aim of *Juntos* was to reach some of the most vulnerable and marginalized segments of the population; it focused particularly on rural areas with high poverty rates and limited access

of these services were opened after the opening of a WJC. We test this by analyzing whether the opening of a hospital or health facility predicts a WJC opening and we find no evidence of it. Moreover, if effects were driven by these other services such as general police stations or health facilities we would also observe effects on other types of crimes or illnesses. In the next section, we show that results are only concentrated on gender-specific crimes.

[34] We also test the joint significance of the relationship between WJC placement and each characteristic. We do not find evidence to reject the null hypothesis (p-value=0.4896).

[35] See Figure A-3 on the presence of both programs at the district level and Figure A-4 on the timing of CCT *Juntos* and the implementation of the WJC program. Two large expansions of the CCT *Juntos* program took place, first in 2007 and then in 2012.

to state services.[36] By 2014, about 1,142 districts had CCTs and 225 districts had WJCs. However, more than half of the districts with WJCs (123 districts) were not covered by the CCT *Juntos* program. This evidence clearly suggests that while WJCs were more likely to be implemented in urban areas, the CCT program was more likely to cover dispersed populations in the poorest rural areas. We test this assumption more directly by analyzing whether WJC placement at the district level was correlated with the CCT *Juntos* implementation. Columns 2 and 4 in Table 1 indicate that the placement of WJCs was not determined by the rollout of the CCT *Juntos* program.[37]

Another potential confounding program is *Cuna Más* (formerly *Wawa Wasi*). However, the program's objectives and site locations do not align with those of WJCs, making it unlikely that *Cuna Más* significantly influences our estimates. The program aims to foster the holistic development of vulnerable children (6–36 months) through daycare and family support visits. It targets districts in urban and rural areas facing poverty and extreme poverty, with rural sites serving *Juntos* beneficiaries and urban sites focusing on districts with poverty rates at or above 19.1%. In contrast, WJCs prioritize capital and large cities, not specifically targeting impoverished communities.

Crucially, it is important to note that the program has never considered the history of domestic violence as part of eligibility criteria or used it to prioritize a child's access to services. This further supports the argument that *Cuna Más* is unlikely to introduce confounding elements to our study's estimates.

The second concern related to WJC placement is that if we estimate the impact of WJCs on all areas, our results might include rural areas that do not qualify for a WJC in the first place, due to low population or a lack of infrastructure, and thus may not provide an accurate comparison for those areas that get a WJC. Given this, we will focus our analysis on a specification in which we limit the sample to urban areas (urban schools and households), which are the ones more likely to receive a WJC placement.[38]

In Section 6 we also document the absence of pre-trends. Moreover, these results are confirmed by the event study analysis. In that section, we also estimate the main regression models, but use various district-level time-varying characteristics as placebo outcomes and we find no significant effect. This lack of significant correlation between the presence of a WJC and other observable district-level characteristics can help assuage concerns about potential omitted variables bias on unobservables. Finally, we also follow the techniques developed by Altonji et al. (2005) and Oster (2019) to understand to what extent unobserved variation is likely to explain the results.

4.2. Individual-level specification

We use a difference-in-difference empirical strategy to estimate the impact of WJCs on women and children's outcomes. We exploit the variation created by the differential timing in the opening of WJCs and also the spatial variation in the exposure of a woman/child to a WJC. In order to estimate the impact of WJCs on women and children's outcomes, the following specification is used:

$$y_{idt} = \gamma_0 + \gamma_1 WJC_{idt} + \alpha_d + \lambda_{pt} + \delta X'_{idt} + \varepsilon_{it} \quad (16)$$

where (y_{idt}) represents the outcome of interest of woman i (or the child of woman i) at year t who resides in district d, (WJC_{idt}) is an indicator variable that takes the value of one if there is a WJC within one kilometer of the woman/child's household or in the district of residence of woman/child i in year t, (α_d) is a district fixed-effect, (λ_{pt}) is a province-by-year fixed effect, (X'_{idt}) is a vector of individual-level characteristics for woman/child i depending on the sample of interest, and (ε_{idt}) is a random error term. Standard errors are clustered at the district level, and we also include district-specific time trends. The inclusion of district fixed-effects accounts for possible time-invariant unobserved characteristics at the district level, such as cultural differences or attitudes toward the role of women/children. This, however, does not account for any differential trends in woman/children's outcomes associated with WJC placement. To address this, we allow the year fixed effects to differ by province. Province-by-year fixed effects rule out the concern that our results are driven by changes that vary by province and year, such as an increase in political corruption or a decrease in provincial resources.

There are two main measures of domestic violence to be used as dependent variables for the women's specification. The first is a measure of physical domestic violence, which is defined as a binary indicator that takes a value of one if the woman reports any moderate or severe physical abuse or any sexual abuse from an intimate partner during the previous 12 months. The second measure is a binary indicator for emotional violence, which is based on three questions that refer to behaviors or situations that are considered by experts to be strong indicators of mistreatment. The vector X'_{idt} includes a set of control variables for a woman's age, age at first marriage, number of children, years of education, number of household members, number of families in the dwelling, marital status, and whether it is a rural or urban residence.

Since our school-level data contains the number of students enrolled but not enrollment rates, we use the Peruvian DHS to estimate the impact of WJCs on children's school attendance status. The most relevant child outcome variable is a dummy variable indicating whether the child is attending school during the year of the survey. We also use additional school-performance outcomes, which are defined as a change in school attendance status between one year and the next, conditional on the child being enrolled in school. The dependent variable can therefore be classified as: (a) currently attending school, (b) having passed a grade, (c) having repeated a grade, (d) having dropped out, and (e) having left school more than two years prior. For the children's specification, we also include a set of control variables including age, gender, head of household's years of education, number of children in the household aged 0–18, number of children in the household aged 0–5, number of female adults, number of male adults, and whether it is a rural or urban residence. We also cluster the standard errors at the district level.

The coefficient of interest is γ_1, which compares the average change in outcomes of women and children who are located near WJCs or in districts with WJCs to the average change in outcomes of women and children who are not reached by a WJC. The identification assumption is that in the absence of WJCs, treatment households (women and children) would otherwise have changed similarly, on average, to control households within the same province. Note that in this specification we cannot control for individual fixed effects, because the Peruvian DHS databases of women and children are repeated cross sections. For

[36] *Juntos* targets the population living in poverty and extreme poverty: households with children under 14, pregnant women, widowed parents, and/or older adults. It is particularly focused on getting children out of poverty and improving their education, health, and nutrition. The program is also seen explicitly as a way to tackle the special vulnerability of populations who were most affected by the political violence prevalent in Peru between 1980 and 2000. Most of the victims of this conflict were poor populations living in rural areas and Quechua speakers.

[37] We also construct a panel database at the district level on WJC and CCT *Juntos* placement from 2005 to 2014, which allows us to better analyze whether program implementations were correlated over space and time. By using a fixed-effects model, we can control for any time-invariant locality factors at the district level and also year dummies. The results in Table A-4 corroborate the idea that the CCT *Juntos* is not a confounding factor in our research design.

[38] For outcomes that are only available at the district level, we control for population. Results do not change if we keep districts that contain a main city or town.

school level data, since schools are geocoded we are able to include school fixed effects.[39]

4.3. District-level specification

We then estimate the following equation to capture the impact of WJC centers on district-level outcomes:

$$y_{dt} = \gamma_0 + \gamma_1 WJC_{dt} + \alpha_d + \lambda_{pt} + \delta X'_{dt} + \varepsilon_{dt} \quad (18)$$

where (y_{dt}) refers to alternative domestic violence metrics (e.g. female deaths due to aggression), number of gender violence complaints, number of men prosecuted for gender-specific crimes (e.g., sexual assault, femicide, or rape), and hospitalizations for mental health problems aggregated at the district level in year t, (WJC_{dt}) is an indicator variable that takes the value of one starting in the first year in which district d offers a WJC, (α_d) is a district fixed-effect, (λ_{pt}) is a province-by-year fixed-effect, (X'_{dt}) represents time-varying district level covariates (e.g., district population), and (ε_{dt}) is a random error term. In this case, we are unable to use exposure to a WJC center within a 1 km Euclidean buffer as treatment, since the outcome variables are only available at the district level and are not geo-coded. For this specification, the dependent variables are defined using the logarithm (instead of the level) and standard errors are clustered at the district level.

This is a standard fixed-effects model, where identification is derived from changes in gender violence/mental health outcomes correlated to changes in the presence of a WJC in the district. This empirical strategy allows us to account for both time-invariant characteristics of districts and time-varying characteristics that are common between treatment and control districts. Therefore, the identification assumption is that any unobserved time-varying covariates that affect gender violence/mental health outcomes are uncorrelated with the rollout of the WJCs within the same province.

5. Results

5.1. Impact of WJCs on the incidence of gender-based violence

We begin by estimating the impact of the introduction of WJCs on the incidence of gender-based violence against women. By estimating Eq. (16) for the sample of women, Table 2 presents the results of regressing the likelihood of experiencing domestic violence (by an intimate partner) in the previous 12 months against the presence of a WJC within one kilometer of the household (after controlling for several covariates, district fixed effects, district-specific time trends, and province-by-year fixed effects).

Table 2
The effect of WJC centers on self-reported domestic violence (2006–2014).
Source: Peru DHS 2006–2014.

Dep. variable	Self-reported domestic violence		
Sample	All women	All women	Only urban clusters
Controls	Standard	District trends	Standard
	(1)	(2)	(3)
WJC within 1km	−0.022**	−0.018*	−0.029***
	(0.010)	(0.011)	(0.010)
Observations	64,363	64,363	38,395
Number of districts	1,167	1,167	485
Mean dep. var	0.390	0.390	0.399
District FE	YES	YES	YES
Province*Year FE	YES	YES	YES
Covariates	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is a dummy indicating whether the women suffered any type of domestic violence (less severe, severe, or sexual violence). The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the women's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample includes women between the ages of 15 and 49. Women who were never married or never cohabited are excluded from the sample. Covariates include age, age at first marriage, number of children, years of education, number of household members, number of households in the dwelling, marital status (married=1), rural residence dummy, district fixed-effects, and province-by-year fixed effects.

Column 1 in Table 2 presents the results using the entire sample of women.[40] Introducing a WJC within one kilometer of the woman's residence decreases domestic violence by 2.2 percentage points, which represents a 5.6% decrease in domestic violence. Column 2 shows this regression after including district-specific trends to address the concern that districts that have a WJC are trending differently than those that do not. This coefficient is slightly smaller (1.8 percentage points) but still significant. Our preferred specification is shown in Column 3, in which we limit the sample to just urban clusters, which means that control areas are most comparable to those affected by the introduction of a WJC. Even though this specification reduces the sample significantly, the coefficient is a bit higher in magnitude than the one for the overall sample (2.9 percentage points) and highly significant.[41]

One limitation of the Peruvian DHS data collected on domestic violence is that it is self-reported by women and therefore subject to recall bias, cultural values, and willingness to report domestic violence. Moreover, self-reporting of domestic violence could be affected by the new presence of the WJC in the district. One possibility is that the opening of a WJC made women realize that domestic violence is shameful and thus, feel stigmatized by reporting. If this is the case, women would under-report domestic violence and thus, we could be over-estimating the effects of WJCs in a district. However, large qualitative evidence and quantitative evidence in the next section suggest

[39] In particular, we estimate the following equation:

$$y_{dt} = \gamma_0 + \gamma_1 WJC_{dt} + \alpha_d + \lambda_{pt} + \delta X'_{dt} + \varepsilon_{dt} \quad (17)$$

where, (Y_{st}) is the education outcome (i.e., total number of children enrolled and standardized test scores) in school s at year t, (WJC_{st}) is an indicator variable that takes the value of one if the school has a WJC within one kilometer, (α_s) is a school fixed effect, (λ_{pt}) is a province-by-year fixed effect, $(\gamma_t X'_s)$ is a year-interacted vector of the school's initial characteristics (including initial school enrollment, the presence of electricity, the presence of piped water, school language, urbanization, and public school dummy), and (ε_{st}) is a random error term. The inclusion of school fixed effects accounts for any time-invariant characteristics at the school level. We also allow the year fixed effects to differ by province and by measures of the school's baseline enrollment and baseline infrastructure. Since schools that are initially different might be more likely to change differently, this empirical specification focuses on comparing changes in treatment and control schools with similar initial characteristics that might drive WJC placement.

[40] The full sample of women in the Peruvian DHS surveys consists of 210,847 respondents aged 15–49 over the period 2000–2014. However, this sample is reduced to 121,404 eligible women, since we only include women who are partnered and eligible for the domestic violence module. When we run estimations using the geocoded cluster locations during the period 2006–2014, this sample is reduced even further, to 64,366 observations of women. The reason for this reduction is that for the years 2011–2013 the DHS did not collect information on the geocoded location and thus, for those years we will not be able to estimate the effects of the WJCs using DHS surveys. Therefore, we complement the analysis using administrative data that also includes those years.

[41] In the Appendix, in Table A-5, we present the impact of the WJCs on different types of emotional violence. In general, we find a negative but not statistically significant effect, except for one emotional behavior outcome. For instance, we find that proximity to a WJC can be associated with a lower likelihood of an intimate partner threatening to take their children away from their spouse.

Table 3
WJC centers and gender-based violence at the district level.

Dep. var.	Log(# female deaths due to aggression) 2007, 2012–2014			Log(# female mental health problems) 2006–2016		
	(1)	(2)	(3)	(4)	(5)	(6)
WJC in district	−0.074**	−0.075**	−0.078**	−0.0781*	−0.0875**	−0.101**
	(0.031)	(0.031)	(0.031)	(0.043)	(0.043)	(0.045)
Observations	7,380	7,368	7,368	20,306	20,262	20,262
Number of districts	1,846	1,842	1,842	1,846	1,842	1,842
Mean dep. var.	0.080	0.080	0.080	5.25	5.25	5.25
District FE	YES	YES	YES	YES	YES	YES
Province-Year FE	YES	YES	YES	YES	YES	YES
Population		YES	YES		YES	YES
District trends			YES			YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Female deaths due to aggression and mental health problems at the district level were obtained from the Peruvian Ministry of Health and are all the registered cases in hospitals. The sample of female deaths due to aggression includes women between the ages of 18 and 59. The sample contains all the years that data was available. Results are robust to restricting to the years 2006–2014. Robust standard errors (in parentheses) are clustered at the district level.

that it is the opposite case: WJCs increase the reporting of gender violence since their opening made women realize that there were state services to help them. In this case, the estimation using DHS data could be an underestimate of the true effect. Given these issues, in order to corroborate our results, we also use administrative district-level data on female deaths due to aggression as alternative outcomes of violence against women. Table 3 provides evidence of a reduction in female mortality due to aggression. More precisely, the coefficients indicate that the opening of a WJC can be associated with a statistically significant reduction in female hospitalizations for assault. The fact that we also find a reduction in hospitalizations is consistent with previous work by Agüero and Frisancho (2022) that shows no difference between the level of domestic violence reported on average under a DHS-like questionnaire and the one estimated using a list experiment.[42]

We also explore whether an improvement in access to justice for women has an impact on their health. In particular, violence against women has been associated with worse health outcomes, such as depression and anxiety (Ahmadzad-Asl et al., 2016; Oram et al., 2017). Table 3 also shows the effects of WJCs on female hospitalizations due to mental health problems. We find that after the opening of a WJC in the district, women's mental health problems decline by 10% over the period of analysis.[43]

These results are robust to different specifications such as using the linear probability model (LPM) and transforming the dependent variable with the inverse hyperbolic sine (IHS). Appendix Tables A-6 and A-7 present the results of using different transformations of the dependent variable. First, we estimate the LPM by analyzing the effect on the probability of having at least one female death due to aggression and admission for a mental health problem in the district. We find that the opening of WJC significantly reduced these probabilities. Second, we apply the IHS transformation. The IHS is commonly used where there are fat tails. We find that results do not change, the estimated coefficients are statistically negative and larger.

Overall the decline in domestic violence is consistent with the formal model presented in Section 2.2. Lemma 1 states that domestic violence should decrease after the introduction of WJCs because of two reasons. First, men are discouraged from using violence since, conditional on women reporting, WJCs are more efficient in prosecuting than traditional methods. Second, WJCs increase women's incentives to report after a crime, which makes men even more reticent to use violence from fear of their partners' reaction. In the next section, we explore these mechanisms.

5.1.1. Mechanisms: WJCs, the reporting of gender-based violence, and prosecution

In this section we study the mechanisms behind this reduction in gender violence. WJCs may reduce the incidence of gender-based violence by increasing victims' reporting of crime and offering a more integral approach to handling gender-specific crimes. In other words, improving women's access to and representation in law enforcement through the presence of WJCs may generate a more credible threat to offenders through greater reporting, criminal penalties, or the issuing of restraining orders on gender-based violence cases.

We study this mechanism by looking at the impact of WJCs on complaints and prosecutions of men on charges related to gender specific crimes such as sexual violence or femicide. Table A-10 presents the results of estimating equation (18). Column 1 shows that after the opening of a WJC in the district, the number of gender violence complaints more than double, suggesting that women report more of these types of crimes after the introduction of WJCs.[44] This result is consistent with a survey done in 2017, which shows that 75% of women who went to a WJC completed the process of issuing a police complaint for gender violence, compared to 10% of those who went to a traditional police station.

This result is not obvious when taking into consideration the insights from our formal model. According to Lemma 2, WJCs are expected to have two opposing effects on the number of reported cases. On the one hand, both the reduction of institutional costs and the increase in the efficiency of prosecution encourage women to go to the authorities after suffering domestic violence. On the other hand, we have seen earlier that WJS disincentivizes men from using violence (Lemma 1). The fact that we do find an increase in reporting suggests that the first effect dominates, and thus the increase in willingness to report by women more than compensates for the decrease in overall violence. In light of this intuition, our findings can be interpreted as an underestimate of the increase in willingness to report.

[42] The results are robust to controlling for population and district trends. Given that this analysis is done at the district level, we cannot restrict the sample to urban areas. However, since this information comes from hospital data it covers mostly women from urban centers.

[43] It is important to note, that while the WJC also offered other services such as medical and legal advice, these other services are only given to victims of domestic violence and are complementary to the complaint. That is, they are only given to help victims to gather evidence against the perpetrator and thus, they are only provided to the ones who would like to fill in a complaint. For example, the medical services are given only at the time of the complaint and there are no follow up visits done at the WJC. Nevertheless to rule out this possibility, we check if there is an increase in hospitalization for illnesses that are not related to violence and we find no significant effect. We also check if men's health improved due to the opening of WJC and we find no evidence of it. These results are available upon request.

[44] Gender violence complaints include those for sexual harassment, rape, and domestic violence.

Table 4
The effect of WJC centers on children's primary school attendance (2006–2014).
Source: Peru DHS 2006–2014.

Dep. variable	Currently attending primary level		
Sample	All children 6-11 y.o	All children 6-11 y.o	Only urban clusters
Controls	Standard (1)	District trends (2)	Standard (3)
WJC within 1km	0.019**	0.018*	0.027***
	(0.008)	(0.009)	(0.009)
Observations	48,703	48,703	25,391
Number of districts	1,159	1,159	485
Mean dep. var	0.970	0.970	0.971
District FE	YES	YES	YES
Province*Year FE	YES	YES	YES
Covariates	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is a dummy indicating whether the child is currently attending primary school. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the child's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample of primary school level includes children between the ages of 6 and 11. Covariates include age, gender, household's head years of education, number of children in the household aged 0–18, number of children in the household aged 0–5, number of female adults, number of male adults, rural residence dummy, district fixed effect and province-by-year fixed effect.

Next, in columns 4–6 we analyze whether there is an increase in prosecutions of men for charges related to gender violence. We find that there is an increase in the probability of being prosecuted for sexual assault, rape, and femicide. These results provide further evidence that enforcement actually increased as a result of the WJC. Moreover, they imply that WJCs not only made gender-based violence less attractive (deterrence), but potentially increased the apprehension rate of offenders, leaving fewer of them around to commit these types of crimes (incapacitation). In the Appendix, we also look at the effectiveness of complaints by analyzing the impact on the share of complaints prosecuted (Table A-12). We find a significant increase, showing that WJC increase the propensity for a complaint to be prosecuted.

These results are robust to different specifications and controls. First, all results are robust to including district time trends and controlling by population. Second, Appendix Tables A-6 and A-7 present the results using different transformations of the dependent variable. We apply the IHS transformation of the dependent variable and we find very similar estimates as in our previous analysis. Another concern could be that the effect is driven by extreme values of the dependent variable. To address this issue, Table A-6 presents the LPM using a binary variable that takes a value of 0 when no gender specific crimes are prosecuted or reported, and 1 otherwise. The results do not change.

Finally, to rule out that effects are not driven by changes in police enforcement or other security changes at the district level, we analyze the effects on complaints and prosecutions that are not related to gender violence. Table A-11 present the results for other types of crimes. We find no effect for complaints and prosecutions that are not related to gender violence, such as economic or property crimes.

In Table A-13 in the Appendix, we test to what extent the program's innovative features could be driving the increase in reporting and the likelihood of prosecuting a perpetrator. First, we test whether the results are driven by the presence of female officers at the district level before the opening of the centers.[45] Second, we test to what extent the complementary services (legal, medical, and social services) could potentially explain the increase in reporting and prosecution. We find that for reporting these types of crimes, the interaction of the main treatment with the number of women police officers is positive and significant (see column 2). However, having more women does not increase the number of prosecutions. This suggests that having a female officer in the WJC can be an important mechanism to increase the reporting of gender violence but does not necessarily increase prosecutions. Moreover, we find that prosecution for gender-based violence only increased for the WJCs that offered all the complementary services. These results indicate that having all available services at a WJC can increase the effectiveness of the complaints and thus increase the probability of prosecution for crimes against women (see column 3).

Overall, these results provide evidence that when a WJC opens, women report more gender-specific crimes and the probability of being prosecuted for committing these crimes goes up, increasing the cost for perpetrators of violence against women. Thus, after the opening of a WJC, these institutions pose a credible threat to violent offenders, since women become more likely to report these types of crimes and perpetrators are more likely to be punished.

5.2. Impact of WJCs on children's school attendance

Given the reduction of gender-based violence, in this section we analyze whether there are positive spillover effects on children's outcomes. We start by analyzing the impact of WJCs on children's school attendance rates.

Table 4 indicates that children in primary school living in households located near a WJC are significantly more likely to attend school. More specifically, living in the proximity of a WJC increases children's school attendance by approximately two percentage points. Focusing on our preferred specifications in columns 3 and 4, we find a positive and statistically significant effect on children's primary school attendance after the opening of a WJC in the proximity of the household in urban areas.

The magnitude of the findings in Table 4 could be considered very large given the primary school attendance rate of 97%. In order to better interpret these results, we also analyze the impact of WJCs through the distribution of the primary school attendance. Information on primary school attendance is used to assign children into four distinct school attendance quintiles. Results in Table A-8 of the Appendix indicate that the effect of opening a WJC within one kilometer of a child's residence on primary school attendance is only statistically significant for those children located in areas with the lowest school attendance rates (about 90%).

As further robustness check, we analyze the impact of the WJCs on school enrollment using school-level data in Table 5. We find that the

[45] While one of the objectives of the program was to have all female officers at the WJCs, Peruvian policy makers highlighted that this was not feasible given that in some districts there were very few female officers available.

Table 5
The effect of WJC centers on primary school enrollment (2006–2014).
Source: Peruvian School Census 2006–2014.

Dep. variable	Log (primary school enrollment)			
Sample	All schools	All schools	All schools	Only urban schools
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
WJC within 1km	0.028***	0.027***	0.033***	0.032***
	(0.008)	(0.008)	(0.008)	(0.008)
Log (District Population)			0.443***	0.424***
			(0.023)	(0.031)
Observations	315,221	315,221	315,221	119,232
Number of schools	36,947	36,947	36,947	14,405
Mean dep. var	95.9	95.9	95.9	177.8
School FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the logarithm of enrollment. The independent variables measures the presence of a WJC within a 1km Euclidean buffer from the school. Standard errors (in parentheses) are clustered at the district level. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (Spanish), urban, and public school dummy).

introduction of a WJC within one kilometer of a school is associated with a 2.8% increase in the number of children enrolled in primary school after the center was opened. The coefficient in column 2, after controlling for district-specific trends, is almost unchanged (2.7%) and still highly significant. In column 3, we include district population as a time-varying control in order to rule out the concern that our results might be driven by mechanical changes in population, especially due to the fact that our school data measure the number of students enrolled, not enrollment rates. Results do not change. In column 4 we limit the sample to only urban schools. Although this restricts the sample significantly, the coefficient is very similar to the baseline estimate. Moreover, these results are also similar in magnitude to the results found with the household-level data, which reassures us of their validity.

Finally, we also estimate the effects by distance buffers relative to the 1 km Euclidean distance (see Figure A-5). We find that enrollment and the likelihood of attending primary school decline with distance to the nearest WJC center. In particular, schooling outcomes of children located further away from a WJC center decline relative to those being less than 1 km away. We find a similar pattern for the prevalence of domestic violence which increases with distance to the nearest WJC center. Women living further away from the WJC center are more likely to experience domestic violence compared to those living less than 1 km away. All these findings support the qualitative evidence that effects are local.

5.3. Impact of WJCs on school achievement

We also look at whether the decline in gender violence led to better schooling outcomes by analyzing the effect of WJC on the scores at the Census Evaluation of Students administered at the national level to second grade students. Table 6 shows that test scores of children in schools located in proximity to a WJC are 0.02 to 0.05 standard deviations higher. Even though these results are not robust to including district time trends, in the next section we show using an event study design no pre-trends in this outcome.

In the Appendix, we also study the impact of WJCs on school attendance status conditional on staying in school, repeating grade, recent drop-out and old drop-out using the same method as reported for school attendance. Results in Table A-9 show that children located near a WJC are significantly more likely to pass a grade and are also significantly less likely to drop out of school. However, we find no effect on grade repetition. As placebo, we also analyze the effects on having left school more than two years before the year of the survey and we find no effects.

5.3.1. Mechanisms: Exposure to violence, bargaining power, incapacitation

In this section we provide some evidence on the mechanisms that might potentially drive the positive effects on children. In the context of Peru, the presence of WJCs can reduce gender violence and thus, increase children' schooling by the following mechanisms.

Exposure to household violence: WJC may improve children's outcomes by directly reducing their exposure to violence in the home. A large literature suggests that children exposed to violence are more likely to have psychological problems and this could affect their school performance. While we do not have data on mental health for children, we test whether schooling outcomes are driven by potentially violent households. We divide the sample between households in which the grandmother was subject to domestic violence by the grandfather and households without this characteristic. Previous literature suggests that having a mother who was subjected to violence makes women more prone to be subjected to violence in their own household (Bedi and Goddard, 2007, 2010; Gil-González et al., 2008; Pollak, 2004). We find that most of the effects are driven by these types of households, showing that the opening of WJCs has an effect on children by positively affecting the households that are most vulnerable to violence (see Table A-14). It could also be the case, that WJC reduce violence against children if women are more likely to report it. We test this possibility by analyzing the impact of a WJC on whether the fathers' use of violence to discipline children.[46] Table A-15 in the Appendix shows non significant effect suggesting that children's outcomes may be affected indirectly by their mothers.

Intra-household bargaining: it could also be the case that WJCs may improve women's intra-household bargaining power and thus investments on children.[47] In order to test this, we use the Peruvian DHS, which records who has the final say on a variety of household decisions.

[46] We define child violence as an indicator variable for households where fathers "discipline" their children by doing any of the following: (i) slapping them, (ii) depriving them of food, (iii) beating them, (iv) locking them up, (v) kicking them out of the house, (vi) throwing water on them, or (vii) taking off their clothes.

[47] Several studies done in Peru show that women spend more of their income on children's items, such as clothes, books, and uniforms (Veras-Soares *y* Silva,

Table 6
The effect of WJC centers on primary-level 2nd grade test scores (2006–2014).
Source: Peru ECE 2007–2014.

Dep. variable	Standardized test scores (2nd Grade)		
Sample	All schools	All schools	Only urban schools
Controls	Standard (1)	District trends (2)	Standard (3)
WJC within 1km	0.028*	0.018	0.040**
	(0.017)	(0.019)	(0.018)
Observations	181,240	181,240	92,666
Number of schools	29,737	29,737	13,507
Mean dep. var	508.9	508.9	536.9
School FE	YES	YES	YES
Province*Year FE	YES	YES	YES
Covariates	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the average of the standardized reading and math test scores for 2nd grade of primary school. The independent variables measures the presence of a WJC within a 1km Euclidean buffer from the school. Standard errors (in parentheses) are clustered at the district level. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (Spanish), urban, and public school dummy).

For example, a woman is asked "*Who makes the final decision on large household purchases?*" or "*Who makes the final decision on the money your husband earns?*" Responses include: by the respondent only, jointly with her partner, or by the partner only. For these categories, we construct three measures of equal decision making. The first one is a score that ranges from zero to six and counts the number of times the respondent makes decisions jointly with a partner. The second is a score that ranges from zero to one and counts the share of decisions made jointly with a partner. The third is a dummy that takes the value of 1 when at least one decision is made jointly with the partner. In addition to decision making, we also estimate the effect of WJCs on women's earnings relative to their husbands'.

Table A-17 in the appendix provides the estimates of the impact of WJCs on decision-making and bargaining power. We find suggestive evidence of an improvement in the bargaining power of women in the household. In particular, we find that women living near a WJC are more likely to make decisions jointly with their husbands. They are also less likely to earn less than their husband and more likely to earn as much as their husband.

Incapacitation: WJC may improve outcomes by potentially removing perpetrators from the household. To analyze whether incapacitation effects are driving the results, we divide the sample between children living in households where the father is present and those where the father is absent (see Table A-16). We find that there are also positive effects in households where the father is present, suggesting that effects are not only driven by incapacitation effects but also by a reduction in violence of potential perpetrators.[48]

Safety perception: WJC may have improved the perception of safety in the area and thus, increase the likelihood that children attend school. However, we can rule out this channel since in Table A-11, we show that the opening of a WJC does not reduce complaints or prosecutions for other crimes, suggesting that results cannot be driven by a decline in general crime. We also use data from the victimization and security module from the Peruvian National Household Survey (ENAHO), in which individuals are asked whether they were victims of any violent crime and whether they perceive crime and insecurity as a problem in their area (2006–2014). For all of these outcomes, we find no significant changes after the opening of a WJC.

Overall, while we cannot disentangle the exact mechanism, we find consistent evidence that the reduction in gender violence may be driving the results on children. Our results are consistent with Lemma 3 in our formal model: violence may have a positive effect on children through a direct and an indirect channel. As explained above, we cannot discard any of these two channels and both of them might in fact be contributing to the improvement in educational outcomes. Nevertheless, we are able to eliminate other alternative explanations, such as incapacitation and safety perception. Moreover, we find suggestive evidence that the opening of WJCs could be a powerful tool to reduce men's incentives to choose domestic violence, which can in turn improve women's status in the household and their investments in their children.

6. Robustness checks

6.1. Assessing the internal validity of the research design

In this section, we present several robustness checks that support the validity of the paper's identification assumption. Using the difference-in-difference approach, the identification relies on the assumption that the path of the outcome variables for the treatment and control households/schools should not be systematically different in the absence of WJCs. More precisely, this means that the introduction of WJCs should be the only factor that drives the treatment group to experience a change in an outcome variable, such as a relative reduction in domestic violence.

The main threat to this identification strategy is the correlation between the opening of WJCs and trends in gender violence and education patterns before the rollout of the program. In essence, the average effect of the WJCs would be biased if the timing of their creation was correlated with pre-program changes in gender violence and education outcomes. However, several pieces of evidence from the previous analysis suggest that this is likely not the case.

First, all results were robust to the inclusion of district specific time trends. Second, we find no effects on non-gender-specific complaints and prosecutions, such as complaints for property crimes. Moreover, we find no effects on children's education for historically non-violent households or households where the father is not present. Third, we

2010). We also use the ENAHO 2014 to validate this assumption, and we find significant differences in school expenditures by gender, indicating that women spend much more on their children.

[48] In addition, we find that WJCs have no effect on civil status, suggesting that the improvement in children's outcomes is not driven by women increasing their probability of divorce or separation after the opening (results upon request).

show that WJC placement was not anticipated by changes in gender-based violence and schooling. These results help rule out other confounding factors, such as overall improvement in the police presence or simultaneous investments in education in these areas.

Nevertheless, to test the presence of pre-trends, in the next section we provide the following analysis. First, we analyze whether pre-program changes in domestic violence and education patterns could possibly be correlated with the timing of the future introduction of WJCs. Second, we analyze the effects on district time-varying outcomes. Third, we conduct an event study to show that pre-program trends are not driving our results. We also use this analysis to provide a sense of the dynamic effect of WJCs.

6.1.1. Pre-program changes

In order to test pre-trends, we begin by estimating a regression of pre-program changes in school enrollment on indicators for the year the WJC was introduced within a one-kilometer radius of the school:

$$\Delta Log(Y_{st}) = Log(Y_{st-1}) - Log(Y_{st}) = \gamma + \alpha_t + \sum_{k \geq t} \delta_k I(WJCyear_{<1km,s} = k) + \varepsilon_{st} \tag{19}$$

The dependent variable, ΔY_{st}, is the change in education outcomes at the school level from year $t-1$ to year t (e.g., a change in the log of primary total school enrollment, a change in school test scores). The set of dummy variables ($WJCyear_{<1km} = k$) take the value of one in the year in which a WJC was opened within one kilometer of the school. Year fixed effects are denoted as α_t. The data for this test is derived exclusively from the School Census (CE) panel database, and the sample is restricted to schools that were reached by the program between 2006 and 2014. The reference group is the opening of a WJC in 2006. If ($WJCyear$) effects are jointly significant, it would indicate that year of WJC creation within one kilometer of the school was correlated with pre-program changes in total school enrollment.

Unfortunately, we cannot perform exactly the same test with the Peruvian DHS, since we do not observe the same clusters of households over time. This means that we cannot exploit the variation generated by proximity to the WJC through Euclidean buffers. However, we can still verify whether the timing of a WJC's introduction in the district is correlated with changes in domestic violence and children's school attendance rates in the district. For this case, we regress pre-program changes in the outcomes of interest for women and children at the district level (e.g., domestic violence, primary school attendance rate) on yearly indicators of the introduction of a WJC in the district:

$$\Delta y_{dt} = y_{dt-1} - y_{dt} = \gamma + \alpha_t + \sum_{k \geq t} \delta_k I(WJCyear_d = k) + \varepsilon_{dt} \tag{20}$$

In Tables A-18 and A-19 of the Appendix, we report the results of estimating Equation (19) and (20) on three different windows of pre-program changes in education outcomes at the school and district level, respectively. These findings show that pre-program changes in education at the beginning of the rollout might be correlated with the timing of the introduction of a WJC. The other two windows of pre-program education results indicate that the rollout year is not correlated with pre-program changes in education outcomes. For this reason, we focus our analysis on the middle of the rollout, that is, from 2006 to 2014, for which identifying assumptions are likely to hold.

We do not find evidence that pre-program trends in education patterns are correlated with the order of WJC implementation during the period 2006–2014. In particular, the results in Table A-18 indicate that opening a WJC within one kilometer of a school does not significantly explain pre-program changes in primary school enrollment between 1998 and 2005. Similarly, results in Table A-19 show that the opening of a WJC in a district is not correlated with pre-program changes in district school attendance rates between 1996 and 2005. Results in Table A-20 also indicate that pre-program changes in standardized test scores at the school level are not correlated with the introduction of a WJC. In all cases, we are unable to reject the null hypothesis of the joint test. These findings strongly suggest that pre-program time trends for the education outcomes of interest are not correlated with the introduction of the WJCs between 2006 and 2014.

Moreover, Table A-21 reports the results of estimating Eq. (20) using women's self-reported domestic violence as an outcome variable. Column 1 shows that the timing of WJCs in the district is not significantly correlated with pre-program changes in district-level domestic violence, and the p-value for the joint test is 0.416. The lack of a significant correlation between the year a WJC was introduced in a district and changes in district-level domestic violence for different windows provides evidence that pre-program time trends in domestic violence were not correlated with the introduction of the WJC in the district. Unfortunately, we are unable to perform this test for other women's outcomes due to lack of data availability for the pre-program period (pre-2006).[49] Overall, we have presented evidence that pre-program changes in domestic violence and education patterns are not correlated with the timing of future WJCs' introduction in the district or within one kilometer. The pre-program patterns for each relevant outcome of interest are also depicted by Fig. 4.

6.1.2. Other district-time varying outcomes

As another test of the identifying assumption, we estimate the main regression models, but use various district-level time-varying characteristics as outcomes. Table A-22 shows that we find a lack of significant correlation between the presence of a WJC and other observable district-level characteristics. These results help assuage concerns about omitted variables bias on unobservables.

6.1.3. Accounting for the dynamic impact of WJCs

We next exploit the fact that we have access to information from prior to the introduction of the WJCs, since the rollout was done gradually each year in order to conduct additional formal testing on whether pre-trends in the outcomes of interest are correlated with the launching of the WJC program. This test also allows us to better understand the dynamics of WJC introduction and disentangle the effect over time (for example, how quickly school enrollment or attendance rates increase after the opening of a WJC and whether this impact accelerates, stabilizes, or mean reverts). To explore these dynamics, we conduct an event study analysis in which we analyze the impact of leads and lags in the introduction of WJCs. Formally, we estimate the following regressions at the individual, district, and school level, respectively:

$$y_{jdt} = \gamma_0 + \sum_{i=-5}^{4} WJC_d * \beta_i I(\tau_t = i) + \alpha_d + \lambda_{pt} + \delta X'_{jdt} + \varepsilon_{jdt} \tag{21}$$

where τ_t denotes the event year, defined so that $\tau = 0$ for the year the WJC was introduced in the district d (of household j or school s), $\tau = 1$ for one year after the WJCs began to operate, and so on. For $\tau \leq -1$, households, schools, and districts were untreated by the introduction of a WJC. The coefficients are measured relative to the omitted coefficient $\tau = -1$. In other words, we add indicator variables for up to five years before implementation and zero to four years after implementation.[50] For each outcome, we expect that coefficients on dummies for years –5 to –2 (the years prior to the WJCs opening) should not be significant, because if this were the case, the validity of the parallel trends assumption would be violated.

[49] Official data on femicides in Peru were recorded starting in 2009, and female hospitalizations in 2006–2007.

[50] Of these nine indicator variables, note that $\tau = -5$ is a dummy that takes the value one for more than five years before the WJC was introduced. The next seven dummies are equal to one only in the relevant year of the WJC opening, while the final variable $\tau = 4$ is equal to one in each year starting with the fourth year of adoption.

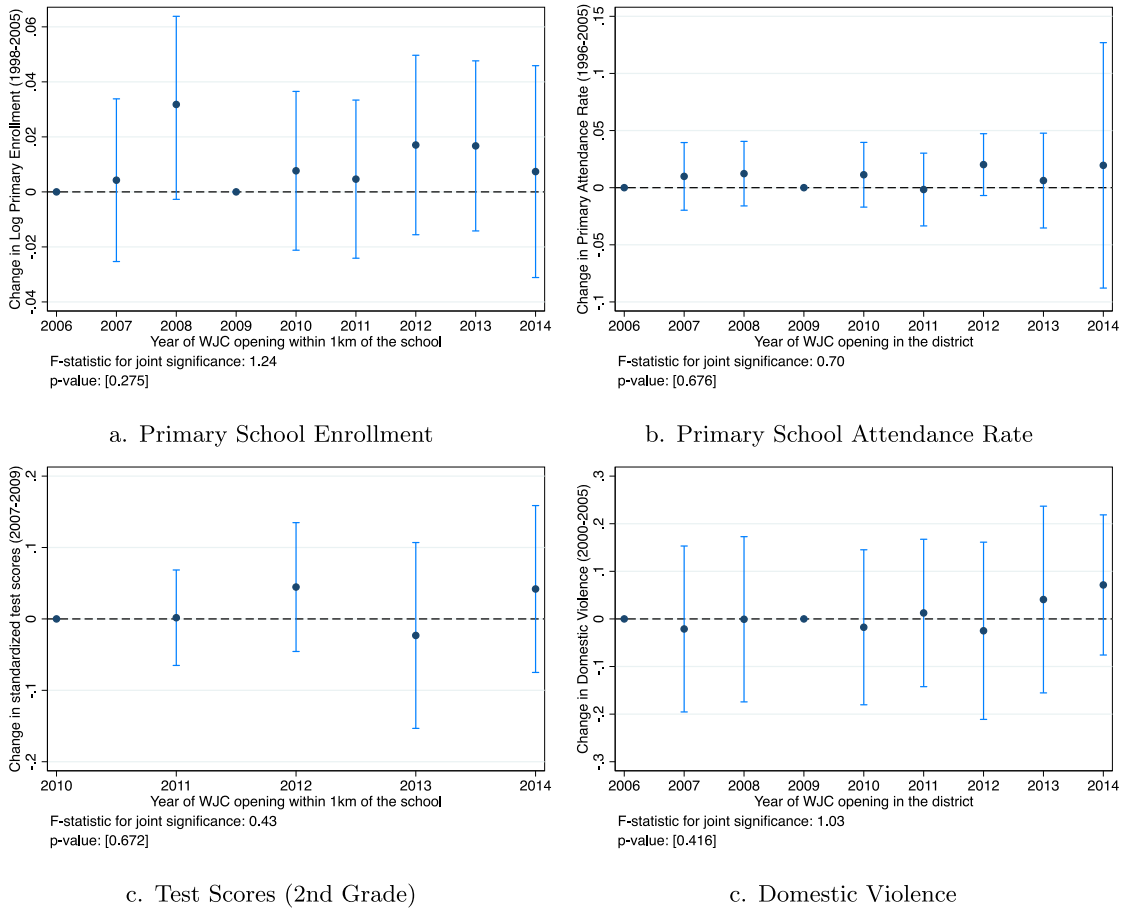

Fig. 4. Effect of WJC center rollout on changes in pre-program outcomes.
Notes: This figure shows coefficient estimates from changes in the outcomes of interest during pre-program periods (e.g. 1996–2005, 1998–2005, 2000–2005 depending on data availability) on year of WJC center introduction indicators (within 1km or in the district) and year fixed effects.

Fig. 5 plots the coefficient of the interaction for the years leading up to the opening of the WJCs and the years after the introduction of the WJCs by estimating Eq. (21) for each of the women's outcomes at the individual and district level, respectively. We find that the coefficients on the years leading up to the opening of the WJCs are close to zero and not significant, showing no evidence of an anticipatory response within districts about to introduce WJCs. Specifically, we find that women residing in districts with a WJC presence have a lower propensity of experiencing self-reported domestic violence beginning the year of the WJC's opening. This pattern of decline reaches its largest impact two years after the opening of the center. A similar pattern is found for gender violence complaints and mental health. One year after the opening of the WJC, hospital entries related to mental health problems decline, and complaints significantly increase. For female deaths due to aggression, effects are seen starting four years after the opening.[51]

For primary school attendance and test scores (Panel A and B), we find that treated schools and households did not exhibit any statistically significant rising trend (relative to the control group) prior to the WJC implementation. In particular, both primary school attendance and test scores increase one year after the opening of a WJC (Fig. 6). Indeed, the graphs show an absence of a strong pre-trend and evidence of a trend break after the WJC opened in the district.

To further check that schooling effects are not driven by any other policy change apart from the WJCs, as placebo we also analyze trends in expenditures on education in Panel C. We find no evidence of any changes on district level expenditures due to the WJCs. Moreover, we find no evidence of pre-trends.

Recent work shows that, in the presence of heterogeneous treatment effects, the coefficients of the treatment variable in an event study design might place negative weights on the average treatment effects for certain groups and periods (e.g., Borusyak and Jaravel, 2017; Goodman-Bacon, 2018; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2020). To test this concern, we compute the regression weights for each of our outcomes. We find the presence of negative weights for complaints and prosecutions. Thus to address this concern, we follow (de Chaisemartin and D'Haultfœuille, 2020) and use an alternative estimator which solves this issue by calculating the average of all these treatment effects. The results are presented in Figure A-6 in the Appendix. Similarly to the OLS estimation of the event study, these results indicate that complaints and prosecutions increase sharply after the opening of a WJC, whereas before the event, the effects are not distinguishable from zero.

6.1.4. *Variation in observables as a proxy for unobserved variation*

Lastly, building on Altonji et al. (2005) and Oster (2019), we assess whether unobserved variation is likely to explain the effects of WJC on women and children outcomes.

First, we construct the index of observables that is the best predictor of having a WJC at the district by regressing the presence of a WJC

[51] Importantly for female deaths due to aggression and complaints since the data covers few years before the introduction of the WJC we can only analyze the effects up to 4 years before the WJC opened.

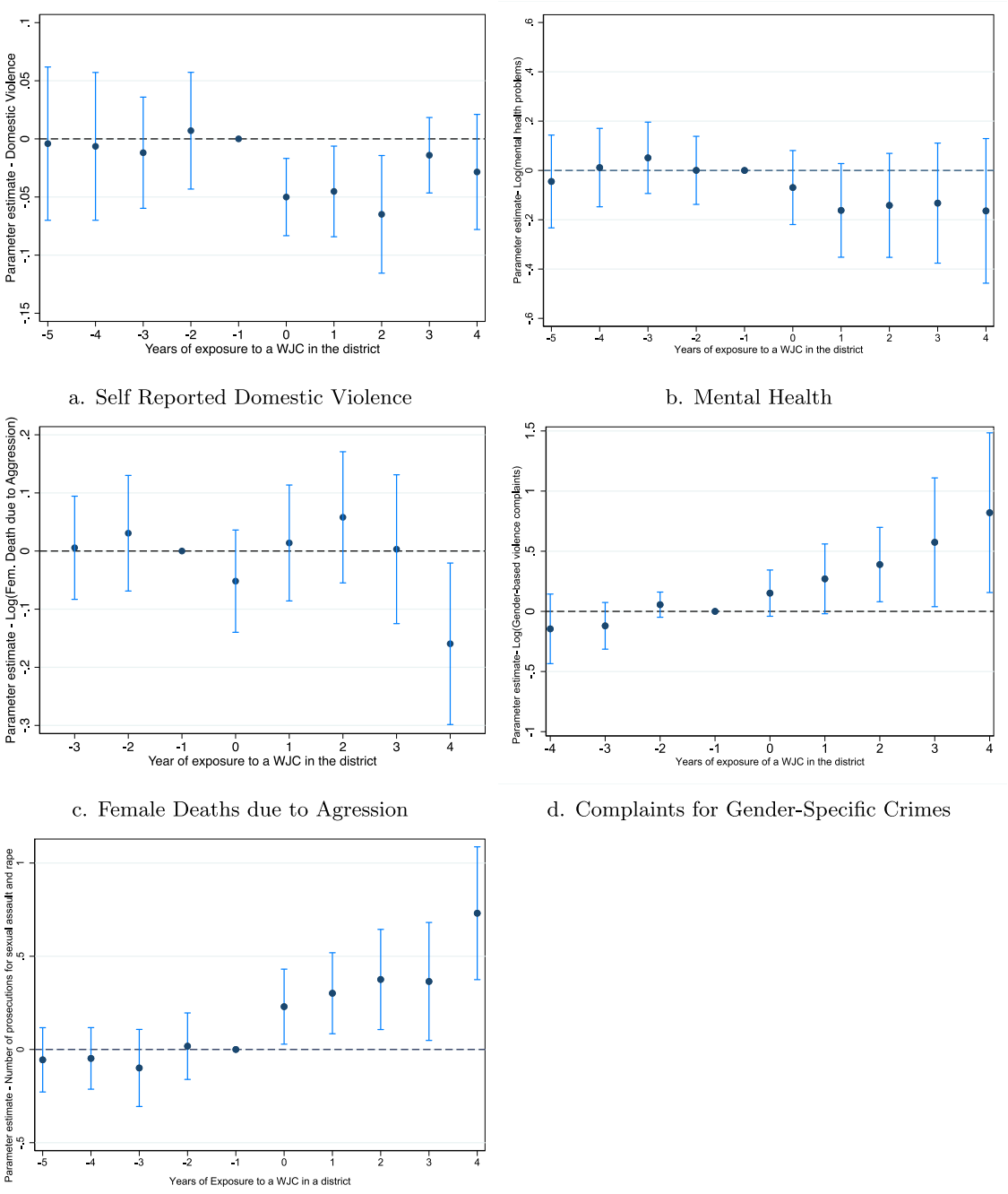

Fig. 5. Event study: Pre-WJC and Post-WJC trends in women' outcomes.
Notes: These graphs plot the coefficient obtained from a regression of the outcomes on the interaction between presence of WJC in the district and dummies for the years leading up to the opening of the WJC centers and years after the WJC introduction. Each bar represents the estimated coefficients and the capped, vertical line shows the estimated 95% confidence interval. Covariates include district fixed effects, year fixed effects, year-by-province fixed effects, and individual controls.

on all the controls and taking the fitted value. Those controls include the variables that are likely to predict the allocation of a WJC such as, the number of health establishments, courts, criminal and family attorneys, and population. Then, we regress our outcomes on this index of observables, controlling for province-by-year, district, and year fixed effects. The results are reported in Panel A of Table A-23 in the Appendix. We find that the predicted-from-observables WJC presence is not significantly related to any of the outcomes. Thus, under the assumption that these observables are representative of unobservables, these findings provide some evidence that a potential selection on unobservables is not driving the results.

Second, even if we do not find any evidence of selection on observables, it still could be the case that a potential selection on unobservables might bias our point estimates. To gauge the magnitude of this bias, we report (Oster, 2019)'s δ-statistic indicating how stronger the unobservables need to be to fully explain our results by omitted variable bias, relative to the observables. To do so, following (Oster, 2019), we set the R-squared from a hypothetical regression of the

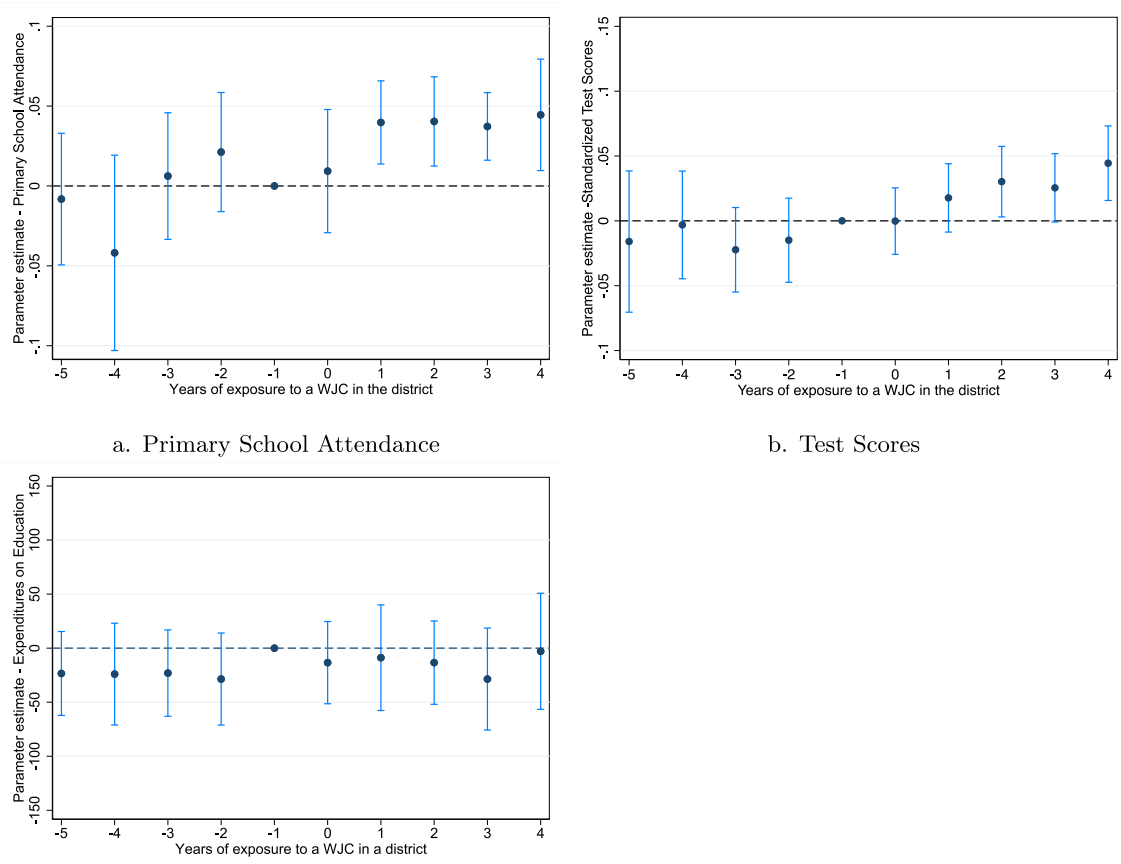

Fig. 6. Event study: Pre-WJC and Post-WJC trends in children's education.
Notes: Graph (a) plots the coefficient obtained from a regression of primary attendance at the individual level on the interaction between presence of WJC in the district and dummies for the years leading up to the opening of the WJC centers and years after the WJC introduction. Covariates include district fixed effects, year fixed effects, year-by-province fixed effects, and individual controls. Graph (b) plots the coefficient obtained from a regression of the test scores at the school level on the interaction between presence of WJC in the district and dummies for the years leading up to the opening of the WJC centers and years after the WJC introduction. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year. Graph (c) presents as placebo the coefficient obtained from a regression of the district's investments in education on the interaction between presence of WJC in the district and dummies for the years leading up to the opening of the WJC centers and years after the WJC introduction. Each bar represents the estimated coefficients and the capped, vertical line shows the estimated 95% confidence interval.

outcomes on the WJC and both observed and unobserved controls to be equal to 1.3 R^2, where R^2 is the R-squared from previous estimations. Panel B in Table A-23 shows that the selection on unobservables would need to be up to 18 times larger than the selection on observables to attenuate the effect of the WJC opening to zero. Moreover, since δ is greater than one, our results pass the suggested threshold.

7. Conclusion

In this paper, we argue that the opening of WJCs in Peru has a positive impact on women's status and their children's human capital investment, and that these impacts are concentrated in the very local areas around the WJC. In particular, we provide evidence on a novel intervention that provides both better representation and access to justice for women.

Our main finding is that women's status and investments in children's human capital are affected positively by the introduction of the WJCs. In particular, our results reveal first that providing better access to justice for women can reduce domestic violence, and female deaths due to aggression—and consequently improve women's mental health. These results may be driven by improving women's access and representation in law and enforcement, which encourages women to increase the reporting of gender-based violence, and by increasing the probability of prosecution. We also find evidence of inter-generational positive effects: we find that children living in potentially abusive households located near a WJC are significantly more likely to attend school.

From a public policy standpoint, our analysis implies that improving law and enforcement responses to gender-based violence can be a powerful tool to reduce violence against women and increase human capital investment in children, suggesting a positive inter-generational benefit.

CRediT authorship contribution statement

Maria Micaela Sviatschi: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Project administration, Validation, Visualization, Writing – original draft, Writing – review & editing. **Iva Trako:** Conceptualization, Formal analysis, Investigation, Methodology, Writing – original draft, Writing – review & editing.

Declaration of competing interest

I declare that I have no relevant or material financial interests that relate to the research described in this paper.

Data availability

The data that has been used is confidential.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2024.103262.

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