Gender Violence, Enforcement, and Human Capital: Evidence from All-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 investment in children? This paper provides evidence from Peru's all-women's justice centers (WJCs), specialized institutions that employ mostly female officers and provide police 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 increases reporting and prosecutions for gender-specific crimes by 40%; it also reduces the incidence of gender-based violence, as measured by domestic violence, femicide, and hospitalizations due to mental health, by about 10%. We find, moreover, that this decrease in women's exposure to violence has intergenerational effects: WJCs substantially increase human capital investments in children, raising enrollment, attendance, and test scores. These results are consistent with a bargaining model in which women's access to justice determines the threat point.

JEL Classification: J12, J16, I25, K38
Keywords: gender-based violence, access to justice, children, household bargaining

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 (Borker, 2017) and their children (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] In addition to all this, despite the fact that women's representation in politics has shown positive effects on the public goods provisions that women prefer (e.g. Chattopadhyay and Duflo, 2004; Beaman et al., 2009), the persistent lack of female representation in law and enforcement exacerbates inequalities in access to justice.[3] Might this low enforcement of gender-based violence generate substantial costs for women and children?

In this paper, we provide evidence that in Peru, increasing the enforcement of 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: all-women's justice centers (WJCs). WJCs are specialized state institutions that employ primarily female officers in efforts 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 developing countries in recent years, yet little is known about their effectiveness.[4]

This paper explores two questions about the relationship between WJCs, gender-based violence, and human capital investments. First, we ask whether WJCs improve the reporting and deterrence of gender-based violence, which we measure using (i) administrative data from hospitals and district attorney offices about femicides, female deaths due to aggression, and female mental health, (ii) administrative reported-crime data from police station and WJC complaints, (iii) administrative incarceration data, and (iv) self-reported domestic violence from household surveys. Second, we examine the intergenerational effects of increasing women's access to justice,

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[1]Evidence from India finds that only three 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 toward women (Franco and González, 2009).

[3]For instance, according to the Peruvian National Census of Police Stations (*Censo Nacional de Comisarías*) for 2017, only 13.3% of the 47,265 police officers who work at police stations are female.

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

focusing on investments in children's human capital using (iv) school census data and household surveys. These data provide insight into whether household investments in children increase when violence against women declines.

To estimate the causal impact of improving women's access to justice, we exploit the gradual rollout of WJCs across locations in Peru from 2006 to 2014. The problem of violence against women is particularly acute in Peru, which has the third-highest rate of intimate partner sexual violence in the world, at 46.6%, and also one of the highest rates of violence against women in the region (WHO, 2012; United Nations, 2015).[5] To address this endemic problem, the Peruvian Ministry for Women and Vulnerable Populations created the WJCs in 1999 as part of the National Program against Sexual and Family Violence. The opening of WJCs has been a large-scale national program implemented across the entire country. During the period of analysis, the number of WJCs grew from 13 in the first year to 226 in the last, covering all of the country's 24 regions and 96% of its provinces.

Given this setting, we use a difference-in-differences strategy that exploits time variation in the opening of WJCs and spatial variation in the exposure of a household or school to a WJC, together with province-by-year fixed effects. We geo-match households and schools with detailed data on WJC locations and founding years in order to construct two different measures of exposure to a center: the presence of a WJC within 1 kilometer of a household/school and the presence of a WJC in a household or school's district. 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").

To observe these sources of variation, we build a detailed panel using multiple geocoded datasets during the period 2006–2014. Our panel comprises individual and household-level survey data, administrative school-level data, administrative crime data, femicides, female deaths due to aggression, and female hospitalizations for mental health problems. 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., femicides and hospitalizations), we can disentangle the effects of the reporting bias usually present in crime data.

Our first finding is that improving access to justice for women reduces domestic violence and female deaths due to aggression. We also find that it 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 in the district is associated with a 7% reduction in female deaths due to aggression and a 10% decline in mental health hospitalizations. We find no effects for men over the same period,

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[5]This rate is well above the Latin American and Caribbean averages, which range from 5% to 15%.

suggesting that no overall improvement in law and enforcement conditions or policy changes is driving our results, only the opening of the WJCs.

WJCs have several innovative features that can explain the reduction in gender violence. WJCs pose a credible threat to offenders by ensuring more thorough and frequent reporting, enforcing criminal penalties, or offering the issue of restraining orders in gender-based violence cases. In particular, WJCs are likely to improve the quality of service provision and effectiveness by allowing women to pursue all the steps of their complaints in a single office.[6] In addition, the presence of female officers may increase the likelihood that women will feel comfortable approaching the police and other government institutions to seek help.[7] For example, female officers may be more responsive to gender violence because they are more aware of the challenges female victims face when initiating such complaints.[8]

Consistent with these mechanisms, 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%. 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.[9] 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 inter-generational 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, pass a grade, and have better national test scores, and they are less likely to drop out of school. These results are robust to using different datasets that measure educational outcomes. Moreover,

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[6]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.

[7]Female victims may feel more confident about reporting such crimes to female officers. On a related note, Iyer et al. (2012) find that as women increase their representation in politics, a higher percentage of female victims report crimes in their areas of jurisdiction.

[8]According to qualitative evidence from the United States, female police officers are more likely to be engaged in domestic violence cases (Bureau of Justice, 2000). For example, a study done in Washington, D.C., found that female officers were less likely than male officers to dismiss or ignore victims who had made repeated calls to the police (Lonsway, 2000). Women are also known to be less corrupt and less violent, and they are proven to have more pro-social traits and better interpersonal skills (Brollo and Troiano, 2016; Schacht, Rauch and Mulder, 2014; Eckel and Grossman, 2008; Nowell and Tinkler, 1994), which may predispose them to better handle gender-based violence once in office. According to recent surveys (Pew 2017), there is also a significant gender gap in attitudes on policing. Female officers are less likely than male officers to agree that aggressive tactics are necessary. These differences in preferences, traits, and sensitivity toward gender-based violence suggest that female police officers will behave differently than their male counterparts when dealing with complaints of violence against women.

[9]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

we find that the main results for children are driven by those from potentially 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.

We next examine further the mechanisms driving the results for children. 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).[10] Similarly, the threat point for women may increase when they have access to justice and support services are more helpful. WJCs may allow women to credibly threaten offenders with police involvement or to decrease offenders' incentives to use violence given the higher probability of criminal penalties.

While we cannot disentangle the specific mechanisms driving the results for children, 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 joint decisions with their husband. This mechanism may operate by changing women's bargaining power within the household, as well as improving their health by reducing their exposure to violence. While we cannot disentangle these effects, we find evidence that both mechanisms may be important.

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. First, we look at the data for non-gender-specific complaints, such as property crimes, and find no difference.[11] In addition, we find no effects on children's education for historically non-violent households and for households where the father is not present. These results, combined with the fact that we find a reduction only in the incidence of violence against women and not men, help rule out other confounding factors, such as an improvement in police presence or investments in education in these areas. Second, 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. Third, we show that WJC placement was not anticipated by changes in gender-based violence and schooling.[12] Finally, we limit the samples

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[10]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, Pollak and Wales, 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.

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

[12]A central issue in our analysis is the fact that WJCs are not placed randomly. Conversations with policy-

to areas that are most comparable to those with WJCs—urban schools and urban clusters of households—since WJCs are more likely to be located in more densely populated areas. We further examine the results by limiting the sample to all districts that ever had a WJC.

This paper can inform several strands of literature. It complements the literature on minority representation in politics and public goods provision (Chattopadhyay and Duflo, 2004; Pande, 2003; Clots-Figueras, 2012; Brollo and Troiano, 2016; Beaman et al., 2009) by providing evidence on the role of female representation in another sphere—law enforcement—that may have a significant impact on reducing crimes against women. In this regard, our paper is closely related to a nascent literature exploring the effects of female officers' presence at police stations on the reporting of gender-specific crimes in India (Amaral, Bhalotra and Prakash, 2018) and the US (Miller and Segal, 2018). This paper complements this literature 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, 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. Moreover, while contemporaneous evidence in India and the US mainly focuses on whether there is a change in *reporting* gender-based violence, we complement this work by showing that *actual* violence against women also declines after the introduction of WJCs. This may be due to the complementary services offered at WJCs, such as legal and medical support, that help these cases succeed. For example, Amaral, Bhalotra and Prakash (2018) find that although reporting increases after the introduction of female officers, the arrest rates and incidence of violence do not change, likely due to lack of evidence. WJCs, in contrast, help decrease actual violence by offering tools to increase the likelihood that cases succeed and offenders face consequences.

This paper is also related to the literature linking economic conditions 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, recent research has shown that this is not always the case (Bobonis, González-Brenes and Castro, 2013; Eswaran and Malhotra, 2011; Bloch, Rao and Desai, 2004).[13] A potential explanation for this result could be that the enforcement of jus-

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makers and WJC managers suggest that they choose where to locate them based primarily on population density, level of infrastructure, and proximity to certain institutions. There was no mention of choosing their locations based on anticipated or recent increases in violence or schooling. All results are robust to including pre-existing characteristics interacted with year fixed effects. Moreover, in order to control for the non-random placement of WJCs, we include a province-by-year fixed effect that controls for any characteristics that may vary at the province and year level. By using province-by-year fixed effects, our identification assumption is that treatment schools/households would otherwise have changed in the same ways, on average, as control schools/households in the same province.

[13]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

tice around crimes committed against women may be perceived to be low, allowing perpetrators to threaten violence without repercussions. Our data on WJCs offers an alternate scenario, in which offenders are punished for their crimes.

Finally, this paper provides new causal evidence on the effects of gender-based violence on children. The results are in line with previous research in developed countries that 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, Buckley and Whelan, 2008; Baldry, 2003; Carlson, 2000; Currie, 2006; Black, Sussman and Unger, 2010). To the best of our knowledge, the closest related papers that rely on a quasi-experimental approach are Aizer (2011), Agüero (2013), and Currie, Mueller-Smith and Rossin-Slater (2018). These papers, using different approaches and datasets, all find that domestic violence has a negative impact on women's pregnancies and infant health. We complement these findings by providing new causal evidence on the relationship between gender-based violence and children's education. Moreover, we provide this new evidence in the context of developing countries, where gender-based violence is high and, in many households, perceived as commonplace and justified.

The remainder of this paper is organized as follows. Section 2 presents a brief background on the prevalence of domestic violence in Peru and on the WJC intervention. 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

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

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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, González-Brenes and Castro, 2013; Eswaran and Malhotra, 2011; Bloch, Rao and Desai, 2004).

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.[14]

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. 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 reduce the emotional toll by making the process easier after a traumatic experience. WJCs are thus frequently located a short distance from partner establishments, such as prosecutors' offices and health facilities.[15]

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). Figure 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.

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 (Figure 2). Given the above-mentioned strong ties to local justice and health institutions, WJCs tend to be heavily concentrated in urban areas throughout these regions.

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). Whereas 40% of reported cases concern women between 25 and 45 years old, children and teenagers (0–17 years old) constitute the second largest group (30%). 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 (75%) of women who visited a WJC, domestic violence

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[14]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

[15]These centers are staffed by representatives of government institutions, such as police officers, prosecutors, counsellors, psychologists, and social workers, whose objective is to help the victims of domestic abuse (MIMDES, 2007).

stopped during or after the program intervention (MIMDES, 2009).[16]

3   The Data

This paper makes use of three different types of datasets, which provides variation across geographical regions and time at different levels of aggregation: (1) individual- and household-level data, (2) school-level data, and (3) administrative data on WJCs, crime complaints, femicides, female deaths due to aggression, and female hospitalizations for mental health problems at the district level.

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.[17] 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 other 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.[18] 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.[19] The DHS captures four different types of domestic violence:

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[16]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

[17]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.

[18]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.

[19]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

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.[20] Since the last measure is less visible and more difficult to measure, 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 or involvement in child labor) 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 and secondary grades who are enrolled in and attending school.[21] This survey also allows us to measure children's school performance (e.g., passed a grade, repeated a grade, dropped out) and whether the child is involved in any child labor.

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.[22] According to the Peruvian DHS, the data indicates that 39% of ever-partnered Peruvian women disclosed experiencing abuse from their spouse during the previous 12 months, 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, which is almost universal. The school attendance rate at the secondary level is also quite high (89%) and very similar between genders. Given that secondary school is not compulsory, the drop-out rate reaches 9% at this educational level.

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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 age 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 femicides and female deaths due to aggression.

[20]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.

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

[22]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.

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.[23] 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 substantively 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 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. Finally, we also limit our analysis to urban areas, because in these locations the range of displacement is less than in rural areas.

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 and secondary 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

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

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 area/district of 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) and 12,811 secondary schools (grades 1–5).[24]

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, while the average secondary school has 175 students. The proportion of primary schools is higher in rural areas, while urban areas tend to have a higher number of secondary schools per capita. 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. In contrast, a large proportion of secondary schools (40%) are private, and in almost all secondary schools the language of instruction is Spanish.

A final important issue with the School Census data is that it measures the total number of children enrolled, not enrollment/attendance rates. This may lead to the concern that our results reflect changes in population, instead of the proportion of children enrolled or attending. We discuss this issue in greater detail in Section 4. As an additional robustness check, we also use the Peruvian DHS to estimate the share of children who are attending school.

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[24]The primary-school sample covers between 4.1 and 3.5 million students each year, whereas the secondary school sample covers between 2.3 and 2.7 million students.

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 knowledge of 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. Figure 1 shows a histogram of WJC founding dates and also illustrates the evolution of the opening of WJCs from 1999 to 2016. Figure 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.

Data on the number of femicides at the district level was obtained from the Peruvian Crime Observatory at the Ministry of Public Affairs, and it covers the period 2009–2015. In Peru, femicides are classified in two categories: (1) *intimate femicide*, when the homicide is committed by the woman's partner, ex-partner, or other family member, and (2) *non-intimate femicide*, when the homicide is committed by a stranger, neighbor, friend, or in the case of sex workers, a client (INEI, 2017). This data is recorded by each district attorney office in the country. Unfortunately, this data is only available at the district level and is not geocoded. In this analysis, we only consider cases of intimate femicide in order to maintain consistency with the DHS data. This captures the majority of cases: Of the 852 femicides in Peru from 2009 to 2015, 762 (90%) were intimate and 90 (10%) were non intimate (see Figure A-3).

We complement this information with 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, age, and gender. For the purpose of this analysis, we use female hospitalizations for mental health problems. 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. This information is recorded by health facilities such as hospitals and is only available at the district level. The number of registered cases in health facilities includes women between the ages of 18 and 59 and covers the period 2006 to 2015. Figure A-3 shows the number of female deaths due to aggression and female hospitalizations for mental health problems over time in Peru.

Finally, we 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*

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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.[25]

3.4   Measuring Exposure to WJCs

In order to be able to match the data on WJCs with the outcomes of interest, 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 of the DHS cluster of households/school.

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

The second measure matches the presence of a WJC in the district, based on its date of opening and location, with the DHS cluster/school's district. For instance, a DHS cluster/school in the district of Lima with a WJC introduced in 2006 is coded as having a WJC in the district of Lima since the year 2006.

The Euclidean buffer is our preferred measure, since we want to estimate the impact of having a WJC in the neighborhood of the school/household. 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. 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. The main reason for our choice of a one-kilometer distance buffer instead of a larger one is that once it increases the radius is similar to district level. For example, the measure of exposure using a five-kilometer Euclidean buffer looks very similar to the measure of the presence of a WJC in the district. We present the results using both measures of exposure to a WJC for our main outcomes of interest.

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[25]Typology of crimes according to 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

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 poorer 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. 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.[26] Moreover, our conversations with Peruvian policymakers suggest that educational considerations, particularly enrollment rates and school performance, were never factored into program placement decisions.

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

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[26]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.

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 femicides, 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.

The results from these regressions are shown in Table 1. In general, the results corroborate the evidence we collected from our conversations with Peruvian policymakers and WJC managers. In general, districts that contain more police stations, district attorney offices, and health establishments and are more densely populated are more likely to have WJCs by 2014 and more likely to add them during the 2006–2014 period. However, none of the coefficients except population are statistically significant. Clearly, urban areas with more infrastructure development are more likely to have these specialized centers for women. 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 that might also have an impact on education outcomes. An obvious candidate is the CCT program *Juntos*, which was launched in September 2005, just as WJCs started to be implemented more intensively.[27] *Juntos* integrates two broad objectives. In the short run, it aims to reduce poverty by providing households with cash transfers. 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

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[27]See Figure A-4 on the presence of both programs at the district level and Figure A-5 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.

limited access to state services.[28] By 2014, about 1,142 districts have CCTs and 225 districts have WJCs. However, more than half of the districts with WJCs (123 districts) are 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.[29]

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. As a further robustness check, we will also limit our samples to districts in which a WJC was opened during the sample period.

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:

$$y_{idt} = \gamma_0 + \gamma_1 WJC_{idt} + \alpha_d + \lambda_{pt} + \delta X'_{idt} + \varepsilon_{it} \tag{1}$$

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

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[28]*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.

[29]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 where 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.

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. We also use a set of outcomes for women's health/nutritional status, such as anemia status, weight, body mass index, etc. 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.

4.3 District-Level Specification

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

$$y_{dt} = \gamma_0 + \gamma_1 WJC_{dt} + \alpha_d + \lambda_{pt} + \delta X'_{dt} + \varepsilon_{dt} \tag{2}$$

where (y_{dt}) refers to alternative domestic violence metrics (e.g. femicides by intimate partner, 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 1km 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.

4.4 School-Level Specification

Finally, using the same identification strategy, we study the overall effect of WJCs on education outcomes at the school level by using the following regression equation:

$$Y_{st} = \beta_0 + \beta_1 WJC_{st} + \alpha_s + \lambda_{pt} + \gamma_t X'_s + \varepsilon_{st} \tag{3}$$

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 or in the district of the school, (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.

The coefficient of interest is (β_1), which captures the average change in enrollment in schools that are located near WJCs or in districts with WJCs to the average change in enrollment in schools that did not have a WJC. The identification assumption is that treatment schools located in proximity to, or in districts with, WJCs would otherwise have changed similarly, on average, to those control schools that are not exposed to the services of a WJC. In practice, by controlling for province-by-year fixed effects (λ_{pt}) and by variables that drive WJC placement, the identification assumption is that treatment schools would otherwise have changed similarly, on average, to control schools within their same province and with similar initial characteristics. Throughout this analysis, we cluster our standard errors at the school level. We also estimate this regression including district-specific time trends.

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 equation 1 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 or within the district (after controlling for several covariates, district fixed effects, district-specific time trends, and province-by-year fixed effects).

Panel A of Table 2 shows our domestic violence estimates when exposure to the program is measured through the presence of a WJC within a one-kilometer Euclidean buffer. Column 1 presents our results using the entire sample of women.[30] 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. Finally, column 4 limits further to areas that have ever had a

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[30]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.

WJC (including those that have always one and those that change). The coefficient in this case is still negative and similar in magnitude but not statistically significant, which may be due to the sample-size restriction.

In Panel B of Table 2, we explore the impact of WJCs on domestic violence by using an alternative measure of exposure: the presence of a WJC in the district. We use this alternative explanatory variable as a robustness check and also to explore whether the opening of a WJC matters in broader surroundings. These findings also show that women living in a district with a WJC are significantly less likely to suffer from physical violence by their spouse than those living in districts without one. The magnitude of the coefficients is relatively similar to the ones in Panel A. These results are robust to including district-specific trends and to limiting the sample to urban clusters and districts in which a WJC has been located at any time. 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. Finally, in Table A-6 of the Appendix, we also show that these results are driven by older and more educated women, who are more likely to have better outside options.

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. Since empirical work on gender-based violence generally suffers from measurement issues, in order to corroborate our results, we also use administrative district-level data on femicides and female deaths due to aggression as alternative outcomes of violence against women. Tables 3 presents the results of regressing the logarithm of femicides and female deaths due to aggression against the presence of a WJC in the district, respectively (equation 2). These findings provide evidence of a reduction in femicides and female mortality due to aggression. More precisely, the coefficients indicate that the opening of a WJC in the district can be associated with a statistically significant reduction in femicides and female hospitalizations for assault. The largest effect is found for women aged 20 to 39 years old, which is reassuring in light of the results found with the self-reported domestic violence data.

We also explore whether an improvement in access to justice for women has an impact on their health. Table 3 also shows the effects of WJCs on female hospitalizations due to mental health problems using district and year variation in the openings. 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. We do not find these effects for men. Table A-7 shows the effect of introducing a WJC in proximity to residence on a set of women's health outcomes using the Peruvian DHS. In particular, women living within one kilometer of the WJC experience an increase in weight compared to those living further away. These results suggest evidence of better nutrition and an overall improvement in women's health.

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 4 presents the results of estimating equation 2. Column 1 shows that after the opening of a WJC in the district, the number of gender violence complaints increase by 40%, suggesting that women report more of these types of crimes after the introduction of WJCs.[31] Columns 2-8 present the results for other types of crimes. We find no effect for complaints that are not related to gender violence, such as economic or property crimes. This 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.

Next, 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, with no effects on non-gender-specific crimes. 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 Table A-8 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. 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, interaction with women police officers is positive and significant (see Column 2). This suggests that having a female officer in the WJC can be an important mechanism to increase the reporting of gender violence, consistent with previous evidence in India and the US. Moreover, we also find that the cost of enforcement for gender-based violence only increased for the WJCs that offered all the complementary services. These results suggest 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

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

Column 3).

Overall, these results provide evidence that when a WJC opens in the district, 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 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 and attendance status. Table 5 summarizes the estimated effects of WJCs on children's school attendance at the primary and secondary level by estimating equation 1 for the sample of children. Table 6 presents the results for children's attendance status (e.g., passed grade, repeated, dropped out).

First, Panel A of Table 5 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 and also in the district of residence.

These results are robust to using the different measures of exposure to the program. The magnitude of the findings in Table 5 could be considered very large given the primary school attendance rate of 97%. In order to better interpret these results, in Table A-11 of the Appendix we analyze domestic violence in the household through children's primary-level school attendance status. Effectively, we find that domestic violence is higher among households that do not send their children to primary school and that this difference is driven by urban areas. In addition, 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-12 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.

Second, in Table 5 we also find a positive and statistically significant impact of WJCs on secondary school attendance for those children living within one kilometer of a center. These estimates range between two to three percentage points. However, this effect is no longer significant when we use the presence of a WJC in the district as a measure of exposure. Due to the GPS displacement issue in the Peruvian DHS data, we also estimate the impact of WJCs using two additional Euclidean buffers: three kilometers and five kilometers. Results in Tables A-13 and A-14 show that when we analyze the effect of the WJC in broader surroundings, we do not

find a significant impact for both primary and secondary school attendance rates.

Finally, the impact of WJCs on school attendance status—grade advancement conditional on staying in school, repeating grade, recent drop-out and old drop-out is also estimated using the same method as reported for school attendance. Results in Table 6 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 nor on having left school more than two years before the year of the survey. These results are robust to using different samples of children (i.e., children of the women selected for the domestic violence module).

What we find, overall, is that investments in children's human capital, especially those at the primary level, are affected positively by the introduction of the WJCs.

5.3   Impact of WJCs on School Enrollment

The evidence above suggests that overall primary school attendance increases in response to the introduction of a WJC. This section analyzes the impact of the WJCs on education outcomes at the school level as an additional robustness check. By estimating equation 3, Tables 7 and 8 present estimated impacts of WJCs on average enrollment in primary schools and secondary schools, respectively. Table 9 presents the impact of WJCs on standardized test scores for second-grade students at the primary level.

Panel A of Table 7 shows our primary school enrollment estimates when exposure to the program is measured through the presence of a WJC within a one-kilometer Euclidean buffer. The coefficient on a WJC within one kilometer in column 1 is positive and statistically significant. This result indicates that the 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 in the year 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. After controlling for district population, the impact of WJCs on primary school enrollment is even larger (3.3%) and statistically significant. Our preferred specifications are shown in columns 4 and 5, in which we limit the sample to only urban schools and districts that have ever had a WJC. Although this restricts the sample significantly, the coefficient for urban schools in column 4 is also larger in magnitude than the overall sample (3.2%) and highly significant. Finally, the impact for districts that have ever had a WJC is a bit smaller in magnitude (2.4%) but still significant, despite the fact that we restrict the sample size even further.

In Panel B of Table 7 we explore the impact of WJCs on primary school enrollment by using an alternative measure of exposure: the presence of a WJC in the district. Panel B shows that introducing a WJC in the district also has a positive and significant effect, but the coefficient is

a bit lower (1%), indicating that the effect probably decreases with distance. Focusing on our preferred specifications in columns 4 and 5, we find that adding a WJC in the district increases the total number of children in primary school between 1.2% and 1.9%. These results are also similar in magnitude to the results found with the individual-level data, which reassures us of their validity.

Table 8 shows the impact of WJCs on secondary school enrollment. We also find a positive impact on the number of children enrolled in secondary school (2.9%) when we use the entire sample, but the effect is not robust to controlling for district-specific trends and to limiting the sample to districts that have ever had a WJC. The specification with urban schools is the only one that yields a positive and significant coefficient of 3.4% for secondary school enrollment.

Lastly, consistent with previous results, we also find some suggestive evidence of a positive effect on standardized test scores for primary school children located in schools near a WJC. Table 9 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 all the different specifications, they are positive and highly significant for urban schools.

All these findings suggest a strong connection between the presence of WJCs and total number of children in primary school. In Table A-10 of the Appendix, we also show these effects broken down by gender and grade. We find that the effects are similar for boys and girls, although they seem to be driven mostly by girls. We also find that the impact is equally distributed among the different grades.

5.3.1 Mechanisms: WJCs, Intra-Household Bargaining, and Exposure to Violence

In this section we provide some evidence on the mechanisms that might potentially drive the effects on children. In the context of Peru, the presence of WJCs can reduce domestic violence and thus children's outcomes by several mechanisms. First, WJCs may improve women's intra-household bargaining power and thus investments on children.[32] Second, they improve children's outcomes by directly reducing their exposure to violence in the home. Third, they may improve outcomes by potentially removing perpetrators from the household.

First, we test whether the presence of a WJC in proximity to a household may allow women to send a signal to their husbands about their outside options. The availability of easier access to justice may thus generate a more credible threat to potential offenders by increasing the chance of women demanding police involvement and criminal penalties. Several economic theories of household bargaining power suggest that policies that aim to increase the outside options of a spouse in an abusive relationship may also affect intra-household distribution of resources through changes in their relative bargaining positions (Farmer and Tiefenthaler, 1996; McElroy

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[32]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, 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.

and Horney, 1981; Manser and Brown, 1980). In other words, women's threat points increase when they have alternatives and when support services are more helpful.

Central to this analysis is the relationship between household decision making or bargaining power and the introduction of a WJC. In order to test this, we use the Peruvian DHS, which records who has the final say on a variety of household decisions. 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-16 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. We also analyze whether WJCs have an effect on women's labor force participation. Results in Table A-17 indicate that the women's labor supply does not seem to be affected by the opening of these centers.[33] This result may seem a bit puzzling, but one possible interpretation is that WJCs might allow women to be more productive in their jobs by supplying more hours and therefore increasing their income.

To better understand why empowering women would help promote school attendance in the context of Peru, we next analyze the impact of WJCs on child labor. Table A-15 reports regression results of the impact of WJCs on child labor for children aged 6–14 years old. These findings show that proximity to a WJC is associated with a statistically significant reduction in child labor, especially for young girls.[34] This result rules out that WJCs' effects on children are driven solely by reducing violence against children.

Second, to shed light on the extent to which the reduction in gender based violence is driving the results of the schooling effects on children, 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. We find that most of the effects are driven by these types of households, showing that the opening of

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[33]In addition, we find that WJCs have no effect on civil status and fertility outcomes, suggesting that there is no selection into the domestic violence module (results upon request).

[34]In the context of Peru, if a woman suffers from domestic violence, the burden of household chores falls automatically on her daughters, since sons are not expected to get involved in such activities.

WJCs has an effect on children by positively affecting the households that are most vulnerable to violence (see Table 10).

Third, 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 and test whether there is an increase in divorces. We find that all the effects are driven by households where the father is present, suggesting that effects are not driven by incapacitation effects but rather by a reduction in violence of potential perpetrators. Moreover, we find no effects on divorces.

Overall, 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 domestic 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 domestic violence and education outcomes. However, several pieces of evidence from the previous analysis suggest that this is likely not the case. First, we find no effects on non-gender-specific complaints and prosecutions, such as complaints for property crimes. Second, we find no effects on children's education for historically non-violent households or households where the father is not present. Third, we show that WJC placement was not anticipated by changes in gender-based violence and schooling. These results, combined with the fact that we find a reduction only in the incidence of violence against women and not men, help rule out other confounding factors, such as overall improvement in the police presence or simultaneous investments in education in these areas.

Nevertheless, to address this concern, 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, in the next section, 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:

$$\Delta Log(Y_{st}) = Log(Y_{st-1}) - Log(Y_{st}) = \gamma + \alpha_t + \sum_{k \geq t} \delta_k I(WJCyear_{<1km,s} = k) + \varepsilon_{st} \quad (4)$$

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/secondary 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 women's 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, secondary school attendance rate) on yearly indicators of the introduction of a WJC in the district:

$$\Delta y_{dt} = y_{dt-1} - y_{dt} = \gamma + \alpha_t + \sum_{k \geq t} \delta_k I(WJCyear_d = k) + \varepsilon_{dt} \quad (5)$$

In Tables A-18 and A-19 of the Appendix, we report the results of estimating Equation (4) and (5) 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 and secondary 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 indicates 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 Equation 5 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).[35] 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 Figure 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.

6.2 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

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

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:

$$y_{jdt} = \gamma_0 + \sum_{i=-5}^{4} WJC_d * \beta_i I(\tau_t = i) + \alpha_d + \lambda_{pt} + \delta X'_{jdt} + \varepsilon_{jdt} \tag{6}$$

$$\varphi_{dt} = \rho_0 + \sum_{i=-5}^{4} WJC_d * \beta_i I(\tau_t = i) + \alpha_d + \lambda_{pt} + \delta X'_{dt} + \emptyset_{dt} \tag{7}$$

$$Y_{st} = \beta_0 + \sum_{i=-5}^{4} WJC_s * \beta_i I(\tau_t = i) + \alpha_s + \lambda_{pt} + \gamma X'_{st} + \varepsilon_{st} \tag{8}$$

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.[36] 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.

Figure 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 Equation 6 and 7 for each of the women's outcomes at the individual and district level, respectively. Similarly, Figure 6 and 7 plot the coefficient of the interaction for the leads and lags of the introduction of a WJC by estimating Equation 6 and 8 for the education outcomes at the individual and school level, respectively.

For women's outcomes, 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 by 20%, and complaints increase by 40%. For female deaths due to aggression, effects are seen starting four years after the opening.

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[36]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.

For primary school enrollment and attendance, we find that the treated schools and households did exhibit a rising trend (relative to the control group) prior to the WJC implementation, but this difference is not statistically significant. In particular, primary school attendance increases by two percentage points two years after the opening of a WJC in the district. This increase reaches its peak in the third year after the WJC is introduced, and it is also accompanied by a significant improvement in school performance and a decrease in drop-out rates for the same year (Figure 6). To further check that schooling effects are not driven by any other policy change apart from the WJCs, we also analyze trends in expenditures on education. We find no evidence of any changes on district level expenditures due to the WJCs. Moreover, we find no evidence of pre-trends.

Similarly, primary school enrollment increases by one percentage point two years after the opening of a WJC in the district. For standardized test scores, there is also no difference in pre-program trends between schools located in district with a WJC and those that lack one. Indeed, the graphs show an absence of a strong pre-trend and evidence of a trend break after the WJC opened in the district. Second-grade test scores also increase two years after the opening of a WJC in the district, after which this increment fluctuates over the subsequent two years (Figure 7).

Overall, for households, districts, and schools with a WJC presence, the greatest impact on primary school enrollment, primary school attendance, and standardized test scores is found two years after the opening of the centers, which coincides with the negative impact on women's self-reported domestic violence. The similar timing of the effects on education and gender-based violence provides further evidence that improving access to justice for women might be an important mechanism for allowing women to take better care of their children (i.e., investing in their human capital) by increasing their threat point in intra-household bargaining power and their trust in the institutional system, as well as by improving their health.

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. To the best of our knowledge, this is the first quantitative analysis that attempts to explore the impact of an unexamined dimension of institutional intervention that provides better representation and access to justice for women—namely, women's justice centers—on the prevalence of gender-based violence and education outcomes.

We deal with the potential endogeneity in women's justice center placement by exploiting the variation generated by the rollout of WJCs in Peru. To ensure that our results are not driven by selection or time-varying unobservables, we use a difference in-differences strategy that exploits variation created by the differential timing in the opening of WJCs and the spatial

variation in the exposure of a school or household to a WJC together with province-by-year fixed effects. We provide evidence in support of the identifying assumptions and account for two key time-varying confounders: the fact that WJC introduction might anticipate changes in the outcomes of interest and unobservable changes in variables that might affect both the timing of the WJCs and gender-based violence and education outcomes.

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, femicides, and female deaths due to aggression—and consequently improve women's mental health. These results may be driven by improving women's 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 intergenerational positive effects: We find that children in primary school living in potentially abusive households located near a WJC are significantly more likely to attend school and pass a grade and less likely to drop out. We also show that introducing a WJC causes an increase of 3% in the total number of children enrolled in primary schools, which reinforces our previous results. Moreover, primary school second-graders have better test scores in reading and mathematics. Consistent with the results for education, we also find that young girls are less likely to be working after the opening of the WJCs.

From a public policy standpoint, our analysis implies that providing better representation in and access to justice for women can be a powerful tool to reduce gender-based violence and increase human capital investment in children, suggesting a positive inter-generational benefit of the women's justice center program.

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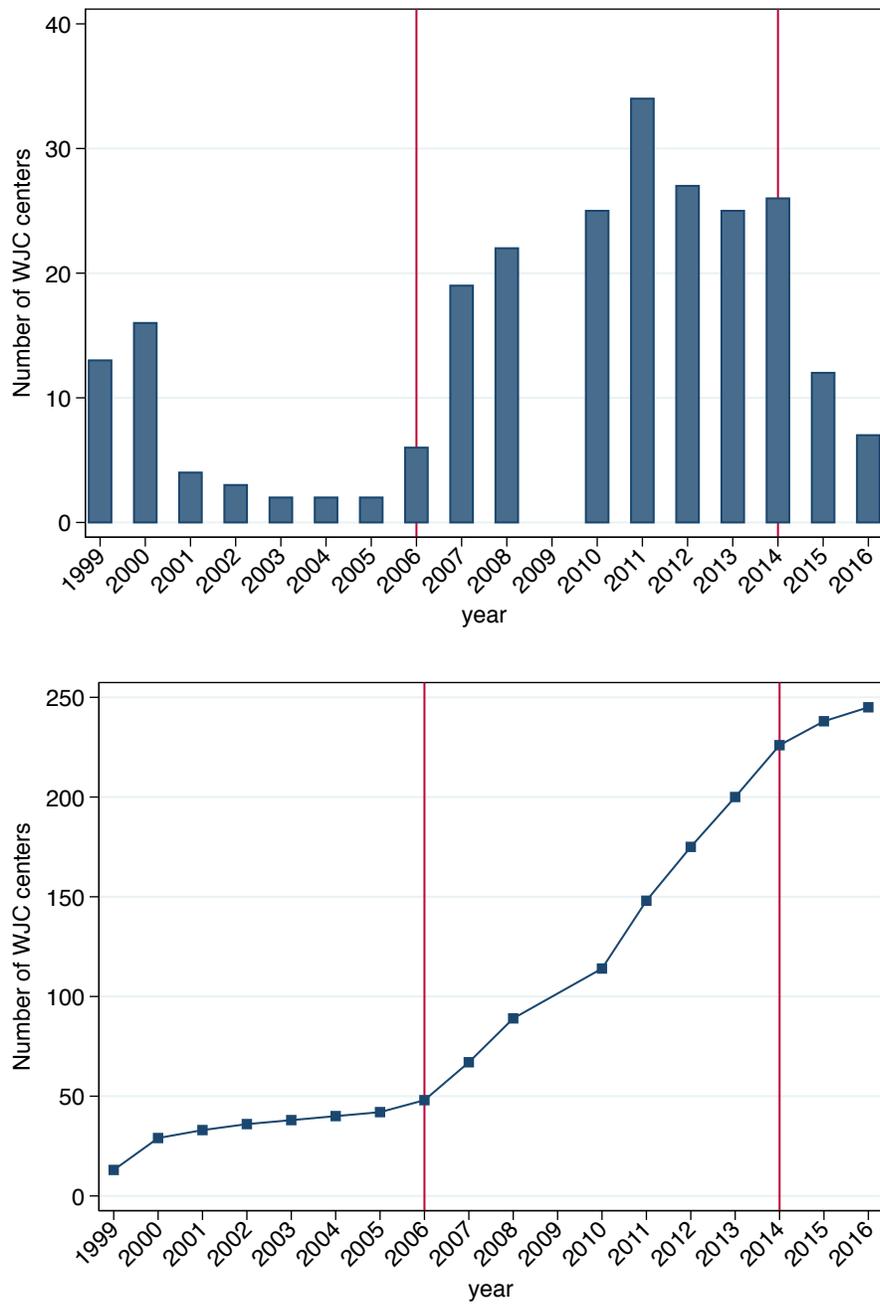
Notes: Author's estimates based on WJC centers data from the Peruvian Ministry for Women and Vulnerable Populations (MIMP).

Figure 2: Rollout of WJCs Across Time and Space (1999-2014)

a. WJC centers in 2000

b. WJC centers in 2006

c. WJC centers in 2011

d. WJC centers in 2014

Notes: Author's estimates based on WJC centers data from the Peruvian Ministry for Women and Vulnerable Populations (MIMP).

Figure 3: Euclidean Distance Buffers and WJC Centers (Schools and DHS Clusters of Households) – Lima and Tumbes
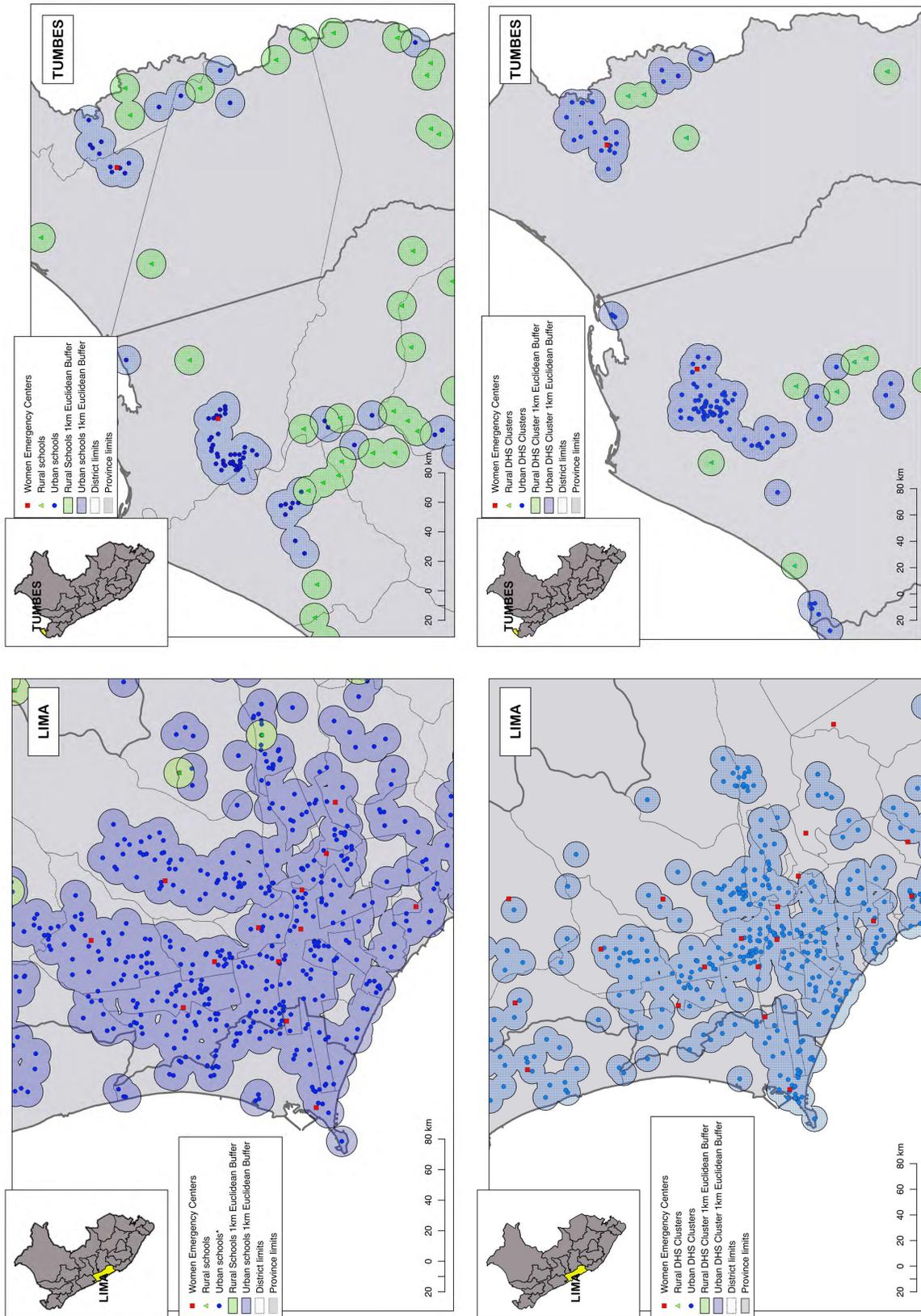
Table 1: Placement of WJC Centers in the District

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)
$\triangle$ Primary Enrollment, (1998-2005)			0.0001		0.0003	0.0002
			(0.0003)		(0.0004)	(0.0003)
$\triangle$ 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 number of WJC centers in the district by 2014; in Columns 3 to 6 it is whether any centers were added during the sample period 2006-2014. Standard errors are in parentheses, clustered at the district level. Source: MIMP (*Ministerio de la Mujer y Poblaciones Vulnerables*)

Table 2: The Effect of WJC Centers on Self-Reported Domestic Violence (2006-2014)

Dep. variable	**Self-Reported Domestic Violence in last 12 months**			
Sample	All women	All women	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)

Panel A: WJC center within a distance buffer from the cluster of residence

WJC within 1km	-0.022**	-0.018*	-0.029***	-0.017
	(0.010)	(0.011)	(0.010)	(0.012)
Observations	64,363	64,363	38,395	27,996
Number of districts	1,167	1,167	485	215
Mean dep. var	0.390	0.390	0.399	0.397

Panel B: WJC center in the district of residence

WJC in district	-0.024**	-0.060***	-0.023*	-0.032*
	(0.011)	(0.020)	(0.014)	(0.018)
Observations	96,560	96,560	58,579	42,393
Number of districts	1,293	1,293	531	225
Mean dep. var	0.387	0.387	0.397	0.394
District 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 a dummy indicating whether the women suffered any type of domestic violence (less severe, severe, or sexual violence) during the last 12 months. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the women's cluster of residence and presence of WJC center in the women's district. 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. Source: Peru DHS 2006-2014.

Table 3: WJC centers and Gender-Based Violence at the District Level

Dep. var	Log(# Femicides)					
	2009-2015					
Sample	All	All	Age 0-19	Age 20-39	Age 40-59	Age 60+
	(1)	(2)	(3)	(4)	(5)	(6)
WJC in district	-0.008	-0.008	0.012	-0.021*	0.003	0.002
	(0.015)	(0.015)	(0.008)	(0.012)	(0.008)	(0.001)
Log (population)		0.023	-0.015	0.017	0.015	0.012*
		(0.036)	(0.022)	(0.026)	(0.018)	(0.007)
Observations	12,915	12,894	12,894	12,894	12,894	12,894
Number of districts	1,845	1,842	1,842	1,842	1,842	1,842
Mean dep. var	0.058	0.058	0.010	0.035	0.010	0.001
District FE	YES	YES	YES	YES	YES	YES
Province-Year FE	YES	YES	YES	YES	YES	YES

Dep. var.	Log(# Female Deaths due to Aggression)			Log(# Female Mental Health Problems)		
	2007-2014			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)
Log (population)		-0.057	-0.060		0.685***	0.724***
		(0.051)	(0.051)		(0.189)	(0.192)
Observations	7,384	7,368	7,368	4,529	4,529	4,529
Number of clusters	1,846	1,842	1,842	844	844	844
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
Male deaths/mental			YES			YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable number of femicides at the district level was obtained from Peru's Crime Observatory at the Ministry of Public Affairs. Female deaths due to aggression at the district level was 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. Robust standard errors (in parentheses) are clustered at the district level.

Table 4: Mechanisms: WJCs, the Reporting of Gender-Based Violence (2011-2017) and Prosecution (2006-2015)

	Complaints						
Dep. variables	Gender violence	Family	Economic	Finance	Public	Property	Drugs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
WJC in the district	0.416***	0.046	0.004	-0.026	0.037	0.040	0.026
	(0.072)	(0.062)	(0.020)	(0.035)	(0.060)	(0.075)	(0.029)
Observations	12,823	12,823	12,823	12,823	12,823	12,823	12,823
Number of districts	1,832	1,832	1,832	1,832	1,832	1,832	1,832
	Prosecuted						
Dep. variables	Gender violence	Family[1]	Economic	Finance	Public	Property	Drugs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
WJC in the district	0.403***		-0.000	0.002	0.070	1.065	0.247
	(0.119)		(0.003)	(0.021)	(0.048)	(1.585)	(0.279)
Observations	18,420	18,420	18,420	18,420	18,420	18,420	18,420
Number of districts	1,842	1,842	1,842	1,842	1,842	1,842	1,842
District FE	YES	YES	YES	YES	YES	YES	YES
Province-Year FE	YES	YES	YES	YES	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates were made using information on complaints of crimes registered in the Police Reporting System of the National Police of Peru (SIDPOL-PNP) and the National Registry of Complaints of Crimes and Misdemeanors of the INEI. Gender violence includes sexual rape, sexual rape attempt, seduction, acts against pudor and other offenses of violation of sexual freedom; Family includes illegal marriages, crimes against marital status, omission of family assistance and attacks against parental authority; Economic includes abuse of economic power, illicit sale of merchandise, hoarding, speculation, adulteration, and others; Finance covers financial and monetary crimes; Public includes crimes against transportation, communication and other public services, crimes against public health, and others; Property includes theft, robbery, scam and other frauds, extortion, usurpation, among others; and finally Drugs includes crimes related to illicit drug trafficking. Robust standard errors (in parentheses) are clustered at the district level.
[1] No records for this type of crime.

Table 5: The Effect of WJC Centers on Children's Primary School Attendance (2006-2014)

Dep. variable	Currently Attending Primary Level			
Sample	All children 6-11 y.o	All children 6-11 y.o	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)

Panel A: WJC center within a distance buffer from the cluster of residence

WJC within 1km	0.019**	0.018*	0.027***	0.023***
	(0.008)	(0.009)	(0.009)	(0.008)
Observations	48,703	48,703	25,391	19,563
Number of districts	1,159	1,159	485	215
Mean dep. var	0.970	0.970	0.971	0.969

Panel B: WJC center in the district of residence

WJC in the district	0.005	-0.005	0.016**	0.022**
	(0.007)	(0.011)	(0.008)	(0.009)
Observations	71,866	71,866	38,330	29,051
Number of districts	1,286	1,286	531	225
Mean dep. var	0.970	0.970	0.970	0.967

Dep. variable	Currently Attending Secondary Level			
Sample	All children 12-16 y.o	All children 12-16 y.o	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)

Panel A: WJC center within a distance buffer from the cluster of residence

WJC within 1km	0.022*	0.027*	0.029**	0.027**
	(0.012)	(0.014)	(0.012)	(0.013)
Observations	33,519	33,519	18,266	13,570
Number of clusters	1,140	1,140	480	215
Mean dep. var	0.895	0.895	0.916	0.908

Panel B: WJC center in the district of residence

WJC in the district	0.012	0.039**	0.027	0.036
	(0.016)	(0.018)	(0.020)	(0.024)
Observations	49,461	49,461	27,482	20,275
Number of districts	1,270	1,270	528	224
Mean dep. var	0.896	0.896	0.913	0.904
District 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 a dummy indicating whether the child is currently attending primary/secondary school. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the child's cluster of residence and presence of a WJC center in the child's district. 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 and the sample of secondary level includes children between the ages of 12 and 16. 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. Source: Peru DHS 2006-2014.

Table 6: School Attendance Status and Proximity to a WJC center - (2006-2014)

Sample	Primary School Attendance Status Children: 6-11 years old				Secondary School Attendance Status Children: 12-16 years old			
Dep. variables	Passed grade (1)	Repeated grade (2)	Dropped out (3)	Left school +2 years ago (4)	Passed grade (5)	Repeated grade (6)	Dropped out (7)	Left school +2 years ago (8)
Sample A: All Children								
WJC within 1km	0.020** (0.010)	-0.004 (0.005)	-0.018** (0.009)	0.001 (0.001)	0.020* (0.013)	-0.000 (0.005)	-0.017* (0.012)	-0.002 (0.009)
Observations	64,921	64,921	64,921	64,921	53,378	53,378	53,378	53,378
Number of districts	1,165	1,165	1,165	1,165	1,161	1,161	1,161	1,161
Mean dep. var.	0.917	0.048	0.023	0.002	0.778	0.036	0.094	0.085
Sample B: Children of the women selected for the DV Module								
WJC within 1km	0.023*** (0.008)	-0.006 (0.005)	-0.019*** (0.007)	0.001 (0.001)	0.030** (0.013)	-0.007 (0.005)	-0.018 (0.012)	-0.003 (0.009)
Observations	48,213	48,213	48,213	48,213	30,380	30,380	30,380	30,380
Number of districts	1,155	1,155	1,155	1,155	1,135	1,135	1,135	1,135
Mean dep. var.	0.919	0.048	0.022	0.002	0.782	0.038	0.090	0.084
District FE	YES	YES	YES	YES	YES	YES	YES	YES
Province-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is a dummy indicating the school attendance status of the child. 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 for primary level includes children between the ages of 6 and 11 and the sample for secondary level includes children between the ages of 12 and 16. 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. Source: Peru DHS 2006-2014.

Table 7: The Effect of WJC Centers on Primary School Enrollment (2006-2014)

Dep. variable	**Log (Primary School Enrollment)**				
Sample	All schools	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard	Standard
	(1)	(2)	(3)	(4)	(5)

Panel A: WJC center within a distance buffer from the school

WJC within 1km	0.028***	0.027***	0.033***	0.032***	0.024**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)
Log (District Population)			0.443***	0.424***	0.415***
			(0.023)	(0.031)	(0.055)
Observations	315,221	315,221	315,221	119,232	103,662
Number of schools	36,947	36,947	36,947	14,405	12,413
Mean dep. var	95.9	95.9	95.9	177.8	127.7

Panel B: WJC center in the district of the school

WJC in the district	0.009*	0.002	0.005	0.012**	0.019**
	(0.005)	(0.004)	(0.005)	(0.006)	(0.009)
Log (District Population)			0.439***	0.417***	0.398***
			(0.023)	(0.031)	(0.056)
Observations	315,407	315,407	315,407	119,270	103,730
Number of schools	36,994	36,994	36,994	14,412	12,427
Mean dep. var	95.9	95.9	95.9	177.8	127.7
School FE	YES	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the logarithm of enrollment plus one. The independent variables measures the number of WJC centers within a 1km Euclidean buffer from the school and presence of WJC center in school's district. Standard errors (in parentheses) are clustered at the school 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).Source: Peruvian School Census 2006-2014.

Table 8: The Effect of WJC Centers on Secondary School Enrollment (2006-2014)

Dep. variable	Log (Secondary School Enrollment)				
Sample	All schools	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard	Standard
	(1)	(2)	(3)	(4)	(5)

Panel A: WJC center within a distance buffer from the school

WJC within 1km	0.029**	0.017	0.030**	0.034***	-0.005
	(0.012)	(0.014)	(0.012)	(0.013)	(0.019)
Log (District Population)			0.427***	0.426***	0.442***
			(0.038)	(0.043)	(0.082)
Observations	102,685	102,685	102,685	69,686	41,324
Number of schools	12,809	12,809	12,809	8,516	5,175
Mean dep. var	174.8	174.8	174.8	215.3	195.3

Panel B: WJC center in the district of the school

WJC in the district	0.023***	-0.004	0.014*	0.019**	-0.005
	(0.008)	(0.008)	(0.008)	(0.008)	(0.013)
Log (District Population)			0.420***	0.417***	0.448***
			(0.038)	(0.043)	(0.083)
Observations	102,691	102,691	102,691	69,692	41,324
Number of schools	12,811	12,811	12,811	8,518	5,175
Mean dep. var	174.8	174.8	174.8	215.3	195.3
School FE	YES	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is the logarithm of enrollment plus one. The independent variables measures the number of WJC centers within a 1km Euclidean buffer from the school and presence of WJC center in school's district. Standard errors (in parentheses) are clustered at the school 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). Source: Peruvian School Census 2006-2014.

Table 9: The Effect of WJC Centers on Primary-Level 2nd Grade Test Scores (2006-2014)

Dep. variable	**Standardized Test Scores (2nd Grade)**			
Sample	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)

Panel A: WJC center within a distance buffer from the school

WJC within 1km	0.028*	0.018	0.040**	0.027
	(0.017)	(0.019)	(0.018)	(0.021)
Observations	181,240	181,240	92,666	69,822
Number of schools	29,737	29,737	13,507	10,858
Mean dep. var	508.9	508.9	536.9	526.9

Panel B: WJC center in the district of the school

WJC in the district	0.026**	-0.020	0.050***	0.050***
	(0.011)	(0.016)	(0.013)	(0.016)
Observations	181,279	181,279	92,681	69,838
Number of schools	29,747	29,747	13,510	10,862
Mean dep. var	508.9	508.9	537.0	527.0
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 average of the standardized reading and math test scores for 2nd grade of primary school. The independent variables measures the number of WJC centers within a 1km Euclidean buffer from the school and presence of WJC center in school's district. Standard errors (in parentheses) are clustered at the school 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). Source: Peru ECE 2007-2014.

Table 10: Heterogeneity by Violent Households

	Enrolled	Passed grade	Repeated grade	Dropped out	Left school +2 years ago
	(1)	(2)	(3)	(4)	(5)

Sample A: Children of households where the grandmother was was subject to violence by grandfather

	Enrolled	Passed grade	Repeated grade	Dropped out	Left school +2 years ago
	(1)	(2)	(3)	(4)	(5)
WJC within 1km	0.025***	0.037***	-0.015**	-0.026***	0.003
	(0.009)	(0.011)	(0.006)	(0.009)	(0.002)
Observations	20,636	19,475	19,475	19,475	19,475
R-squared	0.164	0.154	0.135	0.188	0.089

Sample B: Children of households where the grandmother was was NOT subject to violence by grandfather

	Enrolled	Passed grade	Repeated grade	Dropped out	Left school +2 years ago
WJC within 1km	0.018*	0.014	-0.002	-0.015	0.000
	(0.010)	(0.011)	(0.009)	(0.009)	(0.001)
Observations	27,795	28,613	28,613	28,613	28,613
R-squared	0.148	0.117	0.094	0.151	0.071
District FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is a dummy indicating the school attendance status of the child. 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 for primary level includes children between the ages of 6 and 11 and the sample for secondary level includes children between the ages of 12 and 16. Covariates include 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, rural residence dummy, district fixed effect, and province-by-year fixed effect. Source: Peru DHS 2006-2014.

Table 11: Heterogeneity by Father Present in the Household

Sample A: Children of households where the father is present in the household

	Enrolled	Passed grade	Repeated grade	Dropped out	Left school +2 years ago
	(1)	(2)	(3)	(4)	(5)
WJC within 1km	0.022**	0.026***	-0.007	-0.018**	0.000
	(0.009)	(0.010)	(0.006)	(0.008)	(0.001)
Observations	34,972	34,274	34,274	34,274	34,274
R-squared	0.138	0.108	0.082	0.157	0.070

Sample B: Children of households where the father is NOT present in the household

	Enrolled	Passed grade	Repeated grade	Dropped out	Left school +2 years ago
WJC within 1km	0.019	0.024	-0.005	-0.029*	0.003
	(0.016)	(0.017)	(0.011)	(0.015)	(0.002)
Observations	13,530	13,761	13,761	13,761	13,761
R-squared	0.186	0.190	0.175	0.173	0.129
District FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is a dummy indicating the school attendance status of the child. 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 for primary level includes children between the ages of 6 and 11 and the sample for secondary level includes children between the ages of 12 and 16. 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. Source: Peru DHS 2006-2014.

Figure 4: Effect of WJC center rollout on changes in pre-program outcomes

a. Primary School Enrollment

b. Secondary School Enrollment

c. Primary School Attendance Rate

d. Secondary School Attendance Rate

e. Test Scores (2nd Grade)

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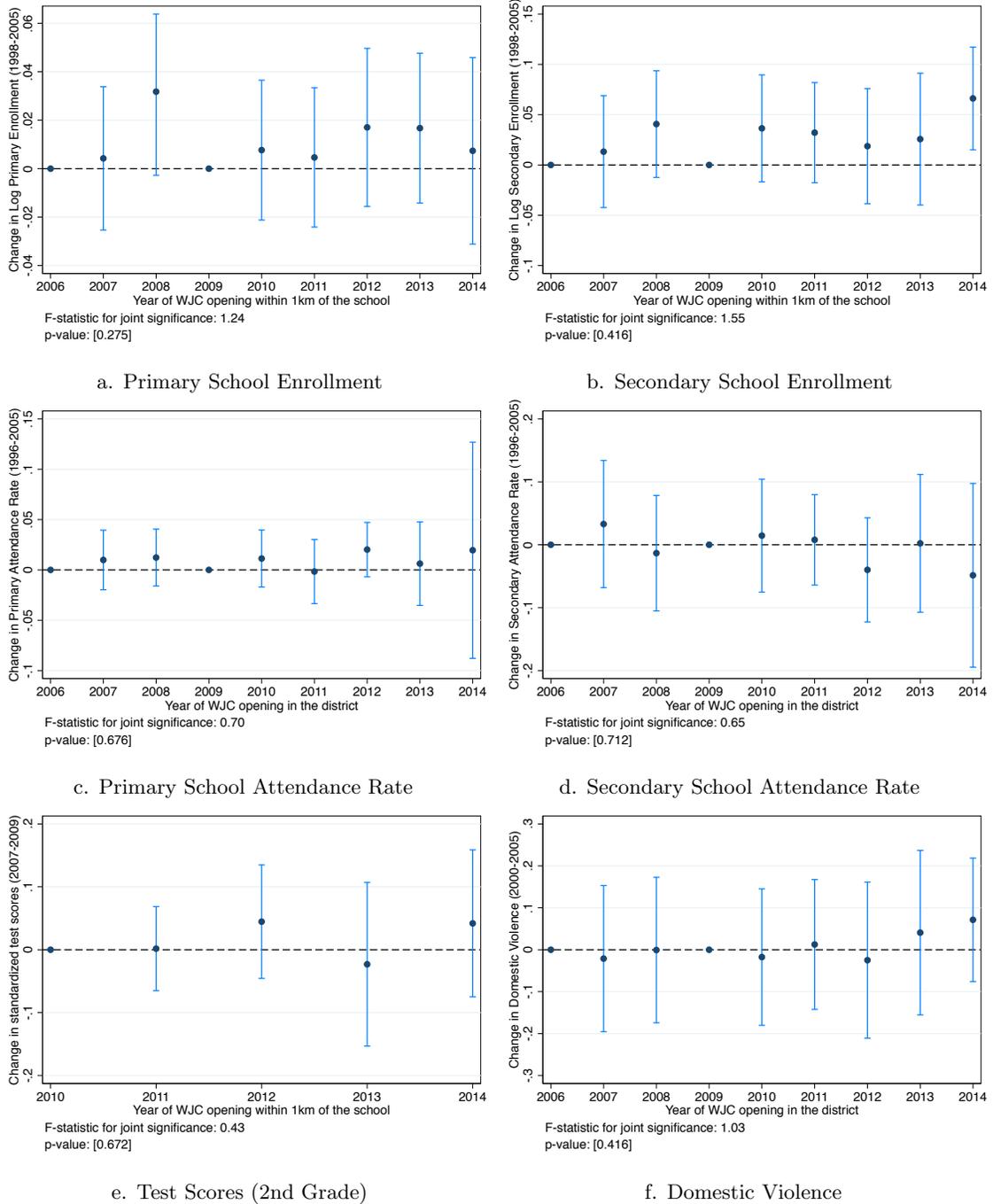
Notes: This figure shows coefficient estimates from changes in the outcomes of interest during pre-program periods (e.g. 1996-2005, 1998-2005, 2000-2005 depending on data availability) on year of WJC center introduction indicators (within 1km or in the district) and year fixed effects.

Figure 5: Event Study: Pre-WJC and Post-WJC Trends in Women' Outcomes

a. Self Reported Domestic Violence

b. Mental Health

c. Female Deaths due to Agression

d. Complaints for Gender-Specific Crimes

e. Prosecuted for sexual assault and rape

f. Prosecuted for femicide

Notes: These graphs plot the coefficient obtained from a regression of the outcomes on the interation 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.

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Figure 6: Event Study: Pre-WJC and Post-WJC Trends in Children's School Attendance (Household Survey Data)
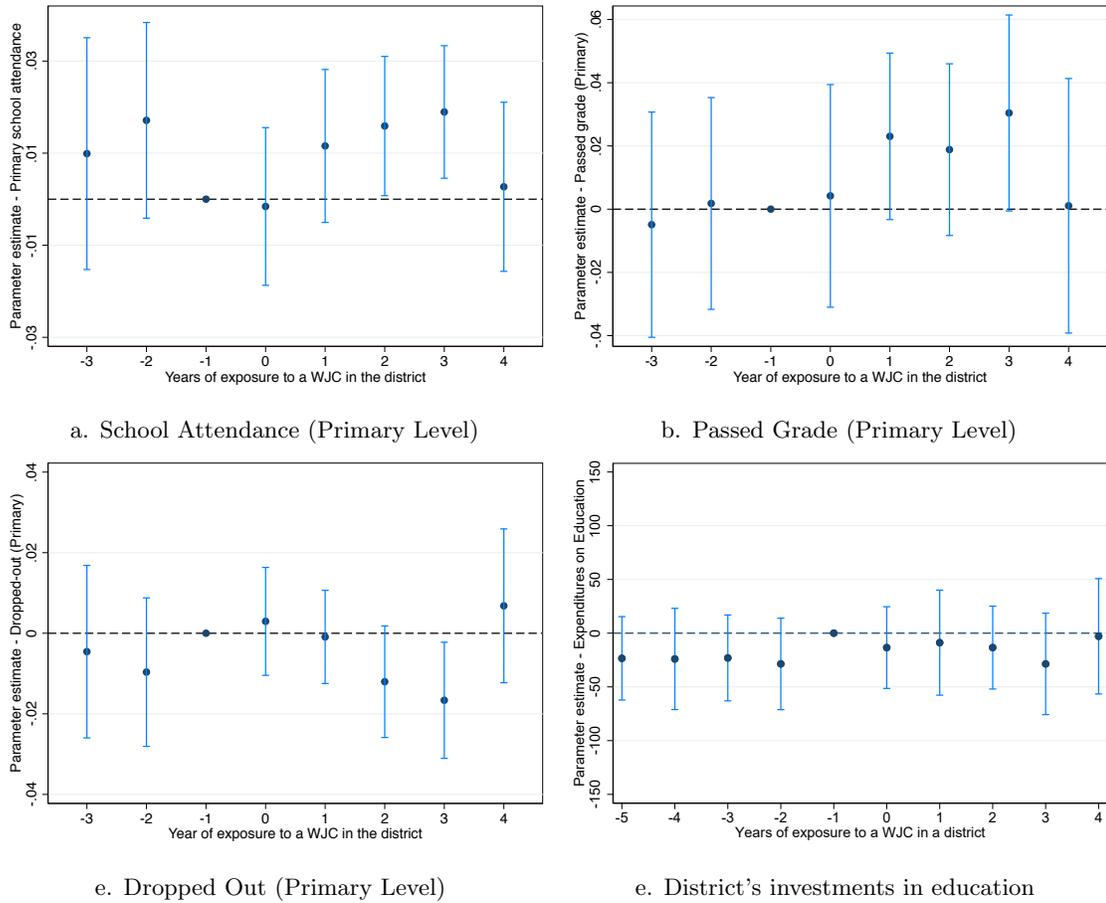
a. School Attendance (Primary Level)

b. Passed Grade (Primary Level)

e. Dropped Out (Primary Level)

e. District's investments in education

Notes: These graphs plot the coefficient obtained from a regression of the outcomes on the interation 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.

Figure 7: Event Study: Pre-WJC and Post-WJC Trends in School Enrollment and Test Scores (School Census Data)
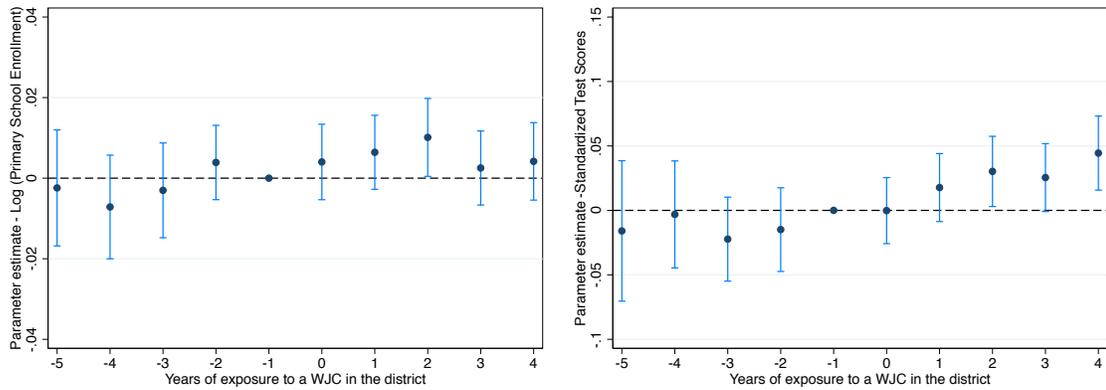
a. Primary School Enrollment   b. Test Scores (2nd Grade)

Notes: These graphs plot the coefficient obtained from a regression of the outcomes on the interation 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 school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year.

APPENDIX FOR ONLINE PUBLICATION

Figure A-1: Total Number of Persons Attended in WJC Centers by Year (2002-2016)
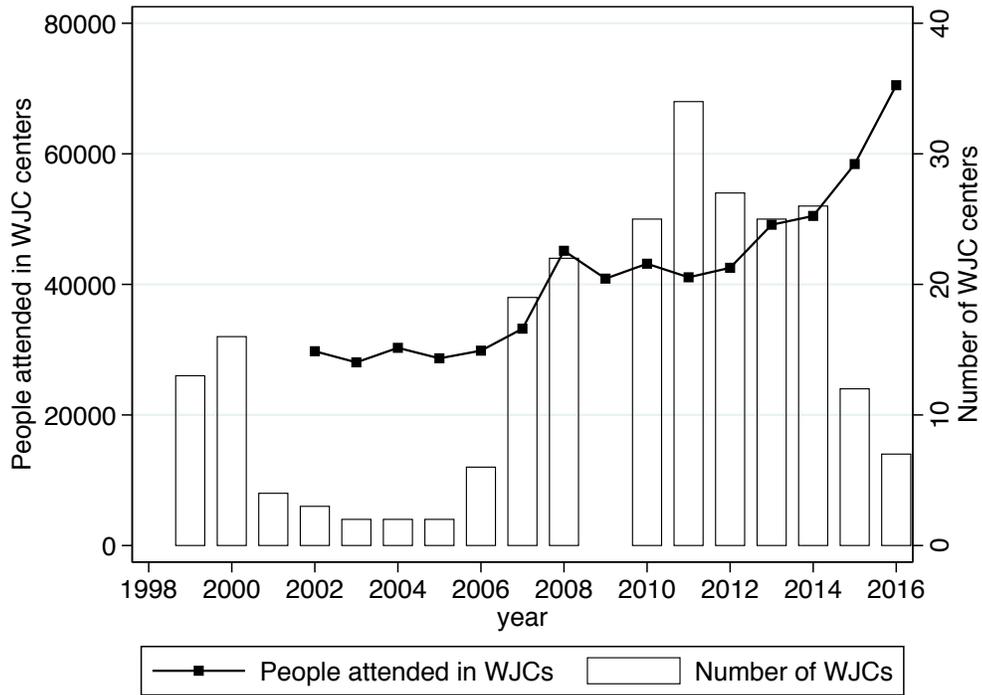
Figure A-2: Domestic Violence in Peru (2003-2015)
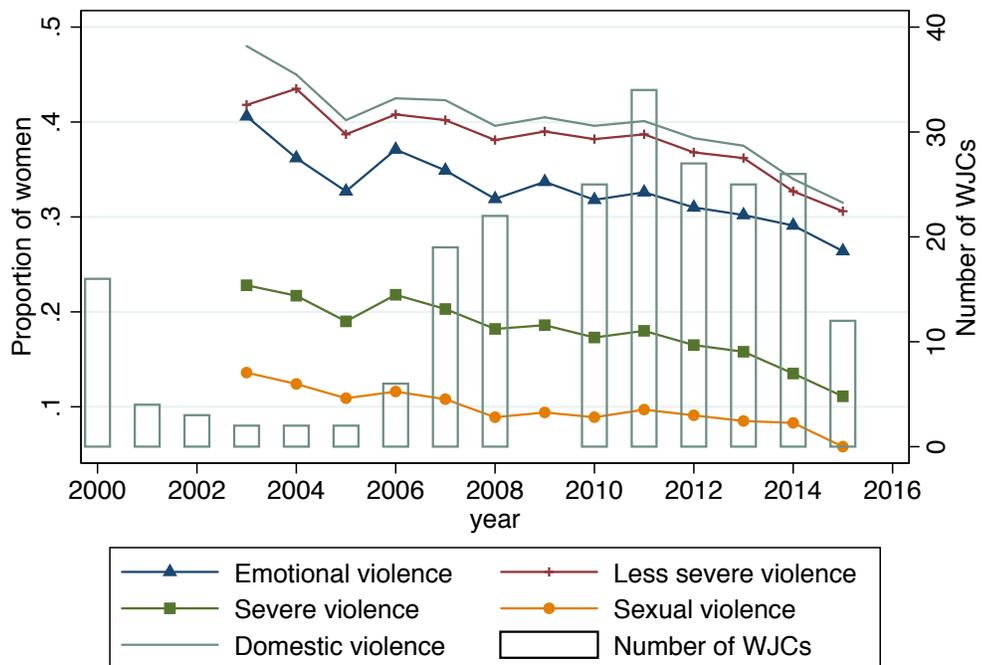
Source: 2003-2015 Peru DHS

Notes: Author's estimates based on WJC centers data from the Peruvian Ministry for Women and Vulnerable Populations (MIMP) and the Peruvian Demographic Heath Survey (2003-2015).

Figure A-3: Femicides, Female Deaths due to Aggression and Female Hospitalizations for Mental Health Problems – Peru
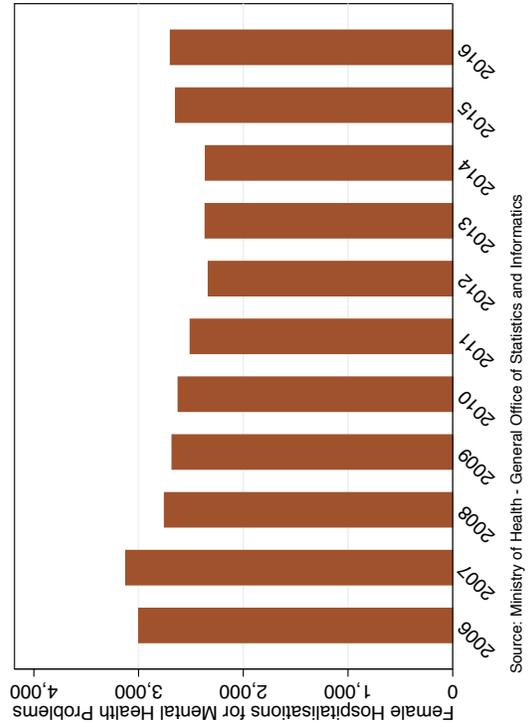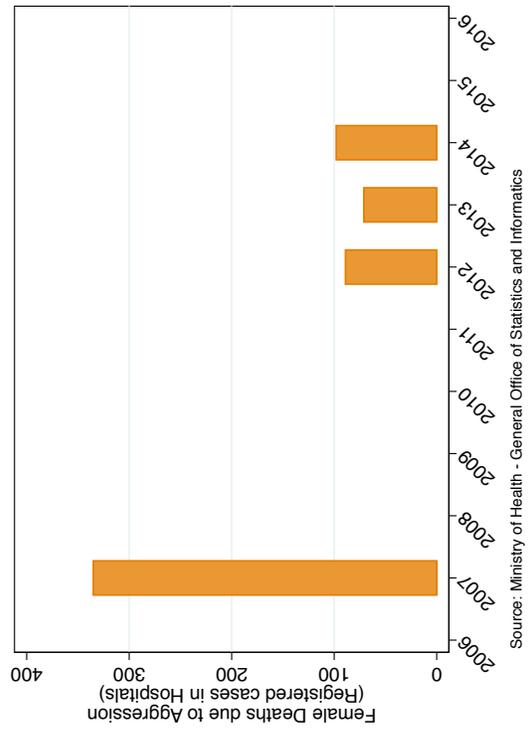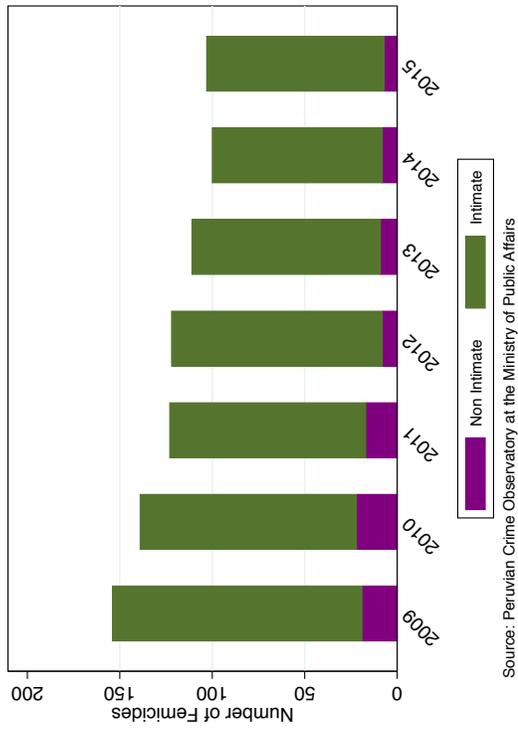
Notes: Author's estimates based on femicides data from the *Peruvian Crime Observatory at the Ministry of Public Affairs*, on female dealths data due to aggressions (cases registered in hospitals) and on female hospitalizations for mental health problems from the *Peruvian Ministry of Health – General Office of Statistics and Informatics.*

Figure A-4: WJC center and CCT *Juntos* presence in the district
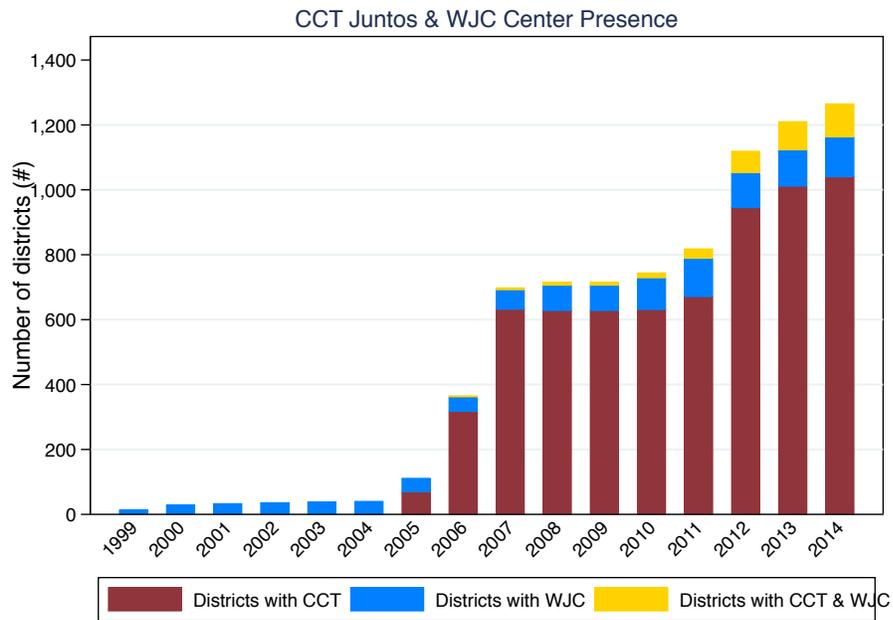
Figure A-5: WJC center and CCT *Juntos* entry in the district
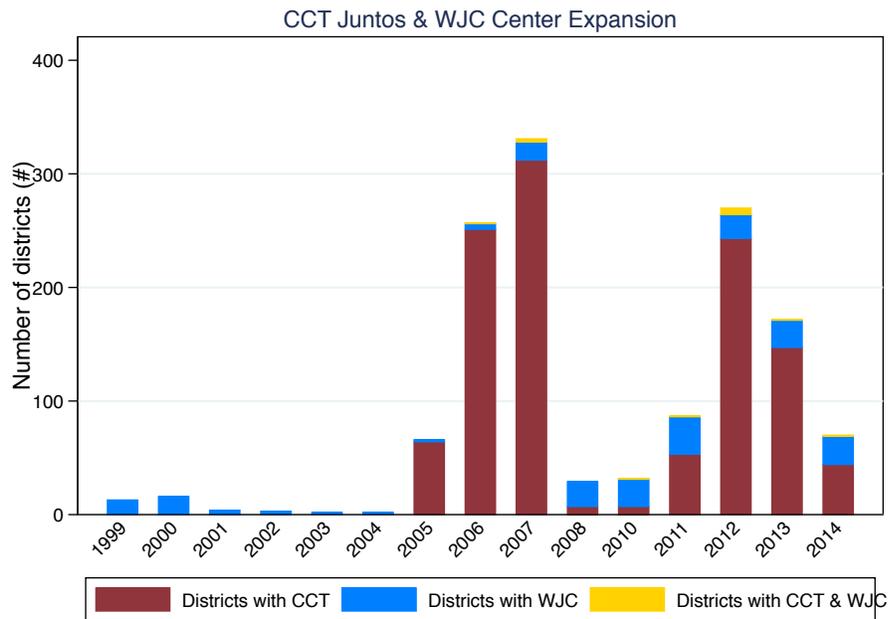
Table A-1: Women's Descriptive Statistics and WJC Center Exposure – DHS (2006-2014)

| | Women: 15-49 years old | | |
	All	Urban	Rural
Panel A.1: Number of women by exposure to a WJC center			
No WJC within 1km	55,323	29,432	25,891
WJC within 1km	9,040	8,965	75
No WJC within 5km	38,603	13,841	24,762
WJC within 5km	25,760	24,556	1,204
Total of women	64,363	38,397	25,966
Panel A.2: Number of women by exposure to a WJC center			
No WJC in the district	61,946	28,540	33,406
WJC in the district	34,614	30,041	4,573
Total of women	96,560	58,581	37,979
	Women: 15-49 years old		
	Obs	Mean	Std. Dev.
Panel B: Women's Summary Statistics			
Domestic violence last 12 months	64,363	0.390	0.487
Less severe violence	64,363	0.376	0.484
Severe violence	64,363	0.174	0.379
Sexual violence	64,363	0.093	0.291
Emotional violence	64,363	0.323	0.467
Anemic	57,540	0.220	0.414
Weight (kg)	59,460	61.57	11.10
BMI	59,460	26.80	4.416
Underweight	59,460	0.006	0.079
Overweight	59,460	0.511	0.499
Obese	59,460	0.208	0.406
Smokes	64,363	0.035	0.184
Age	64,363	33.93	8.336
Age at first marriage	64,363	20.14	4.739
# Total children ever born	64,363	2.811	1.993
# Years of education	64,363	8.577	4.481
# Household Members	64,363	4.626	1.818
Married	64,363	0.356	0.478
Living together	64,363	0.517	0.499
Widowed	64,363	0.007	0.089
Divorced/Not living together	64,363	0.118	0.319
Urban cluster	64,363	0.596	0.490
Currently working	64,363	0.684	0.464

Notes: The GPS data was not available for the years 2012 and 2013 in the Peru DHS. Source: Peru DHS (2006-2014)

Table A-2: Children's Descriptive Statistics and WJC Center Exposure – DHS (2006-2014)

	Primary Level (Children: 6-11 years old)			Secondary Level (Children: 12-16 years old)		
	All	Urban	Rural	All	Urban	Rural
Panel A.1: Number of children by exposure to a WJC center - (GPS data)						
No WJC within 1km	42,914	19,654	23,260	29,494	14,282	15,212
WJC within 1km	5,789	5,740	49	4,025	3,991	34
No WJC within 5km	32,066	9,706	22,360	21,691	7,087	14,604
WJC within 5km	16,637	15,688	949	11,828	11,186	642
Total of children	48,703	25,394	23,309	33,519	18,273	15,246
Panel A.2: Number of children by exposure to a WJC center - (All data)						
No WJC in the district	48,895	19,250	29,645	33,392	13,999	19,393
WJC in the district	22,971	19,084	3,887	16,069	13,490	2,579
Total of children	71,866	38,334	33,532	49,461	27,489	21,972

	Primary Level (Children: 6-11 years old)			Secondary Level (Children: 12-16 years old)		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Panel B: Children's Summary Statistics						
Currently Attending	48,703	0.970	0.169	33,519	0.895	0.305
Female Attendance	24,689	0.970	0.169	18,549	0.899	0.300
Male Attendance	24,014	0.970	0.169	14,970	0.891	0.311
Passed Grade	48,213	0.919	0.271	30,380	0.782	0.412
Repeated Grade	48,213	0.048	0.215	30,380	0.038	0.191
Dropped Out	48,213	0.022	0.146	30,380	0.090	0.287
Left School +2 years ago	48,213	0.002	0.047	30,380	0.084	0.278
Age	48,703	8.467	1.700	33,519	13.786	1.384
Head's Years of Education	48,703	8.602	7.159	33,519	8.348	7.025
Urban Cluster	48,703	0.521	0.499	33,519	0.545	0.497
# Female Adults in HH	48,703	1.219	0.532	33,519	1.218	0.541
# Male Adults in HH	48,703	1.101	0.611	33,519	1.120	0.669
# HH Members 0-18 years old	48,703	3.166	1.522	33,519	3.248	1.551

Notes: The GPS data was not available for the years 2012 and 2013 in the Peru DHS. Source: Peru DHS (2006-2014)

Table A-3: School Descriptive Statistics and WJC Center Exposure – School Census (2006-2014)

	Primary Schools (1st - 6th Grade)			Secondary Schools (1st - 5th Grade)		
	All	Urban	Rural	All	Urban	Rural
Panel A: Years of coverage and number of schools						
Number of schools in						
First year of coverage (2006)	32,817	12,007	20,810	9,693	6,822	2,871
Last year of coverage (2014)	36,859	14,325	22,534	12,773	8,488	4,285
Panel B: Number of schools by exposure to a WJC center						
Never had WJC within 1km	34,372	11,883	22,489	11,287	7,018	4,269
WJC within 1km	2,575	2,524	51	1,522	1,504	18
Never had WJC within 5km	26,418	5,095	21,323	7,282	3,164	4,118
WJC within 5km	10,529	9,312	1,217	5,527	5,358	169
Total of schools	36,947	14,407	22,540	12,809	8,522	4,287
Never had WJC in the district	24,439	6,530	17,909	7,481	4,040	3,441
WJC in the district	12,555	7,884	4,671	5,330	4,484	846
Total of schools	36,994	14,414	22,580	12,811	8,524	4,287

	Primary Schools (1st - 6th Grade)			Secondary Schools (1st - 5th Grade)		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Panel C: School Summary Statistics						
Total Enrollment	315,221	95.9	142.5	102,685	174.8	206.8
Female Enrollment	315,221	46.9	73.6	102,685	84.4	114.9
Male Enrollment	315,221	49.0	75.4	102,685	90.4	113.1
Public School	315,221	0.797	0.402	102,685	0.636	0.481
Urban School	315,221	0.378	0.485	102,685	0.679	0.466
School Language (Spanish)	315,221	0.815	0.387	102,685	0.905	0.292
School Language (Quechua)	315,221	0.124	0.330	102,685	0.000	0.242
School with electricity	315,221	0.671	0.469	102,685	0.872	0.334
Schools with piped water	315,221	0.729	0.444	102,685	0.845	0.361
Reading test-scores (2nd grade)	181,240	510.18	73.08			
Math test-scores (2nd grade)	181,240	507.74	81.68			
Both test-scores (2nd grade)	181,240	508.9	73.44			

Notes: The GPS data was not available for 49 schools (47 primary schools and 2 secondary schools) in the Peruvian School Census. Source: Peru School Census (2006-2014)

Table A-4: Correlation between WJC center and CCT *Juntos* program implementation (2005-2014)

Dep. var.	WJC center entry$_d$		WJC center presence$_d$	
	(1)	(2)	(3)	(4)
CCT *Juntos* entry$_d$	0.002	0.005		
	(0.003)	(0.004)		
CCT *Juntos* presence$_d$			-0.027***	0.001
			(0.008)	(0.008)
Observations	18,390	18,390	18,390	18,390
Number of districts	1,839	1,839	1,839	1,839
District FE	NO	YES	NO	YES
Year FE	NO	YES	NO	YES

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors that allow for clustering at the district level level are reported in parentheses. Program (WJC or CCT) entry is equal to one only in the year of introduction in the district. Program presence is equal to one in every year beginning with the first year after the program entry.

Table A-5: Impact of WJC centers on Emotional Violence (2006-2014)

Dep. variables	Emotional violence	Spouse ever humiliated	Spouse ever threatened with harm	Spouse ever threatened to take children
	(1)	(2)	(3)	(4)
Sample A: All women 15-49 years old				
WJC within 1km	-0.010	-0.002	-0.003	-0.017*
	(0.010)	(0.009)	(0.006)	(0.010)
Observations	64,364	64,364	64,364	64,364
Number of districts	1,167	1,167	1,167	1,167
Mean dep.var.	0.323	0.229	0.119	0.206
Sample B: Only women in urban clusters				
WJC within 1km	-0.018	-0.009	-0.007	-0.024**
	(0.011)	(0.010)	(0.007)	(0.011)
Observations	38,396	38,396	38,396	38,396
Number of districts	485	485	485	485
Mean dep.var.	0.337	0.239	0.114	0.219
District 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 a dummy indicating whether the women suffered any type of emotional violence during the last 12 months. 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. Source: Peru DHS 2006-2014.

Table A-6: Domestic Violence Effects by Age, Education Level and Type of Domestic Violence
(2006-2014)

Dep. variable	**Domestic violence in last 12 months**					
			WJC within			WJC in the
	Obs.	Mean	1km	Obs.	Mean	district
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Results for Women by Age						
Women 15-33 years old	31,442	0.349	-0.004	47,136	0.355	-0.013
			(0.018)			(0.016)
Women 34-49 years old	32,886	0.402	-0.038***	49,380	0.418	-0.038***
			(0.019)			(0.018)
Panel B: Results for Women by Education Level						
No education	2,254	0.374	-0.102	3,380	0.374	0.134
			(0.110)			(0.119)
Primary Level	22,198	0.402	-0.035	32,844	0.390	-0.025
			(0.026)			(0.024)
Secondary Level	24,989	0.415	-0.018	37,834	0.394	-0.042**
			(0.015)			(0.016)
Higher Level	14,033	0.331	-0.029*	21,435	0.316	0.013
			(0.016)			(0.025)
Panel C: Results for Women by Type of Domestic Violence						
Less severe violence	64,366	0.376	-0.029***	96,560	0.373	-0.018
			(0.010)			(0.012)
Severe violence	64,366	0.171	-0.014*	96,560	0.171	-0.006
			(0.009)			(0.009)
Sexual violence	64,366	0.092	0.001	96,560	0.092	-0.007
			(0.006)			(0.007)
District FE			YES			YES
Province-Year FE			YES			YES
Covariates			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) during the last 12 months. 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. Source: Peru DHS 2006-2014.

Table A-7: The Effect of WJC centers on Women's Health Outcomes (2006-2014)

Dep. variables	Anemic (1)	Weight (kg) (2)	BMI (3)	Underweight (4)	Overweight (5)	Obese (6)	Smokes (7)
			Sample A: All women 15-49 years old				
WJC within 1km	-0.012	0.508**	0.105	0.001	-0.003	0.004	0.005
	(0.009)	(0.212)	(0.089)	(0.002)	(0.009)	(0.009)	(0.003)
Observations	57,540	59,460	59,460	59,460	59,460	59,460	64,363
Number of districts	1,134	1,134	1,134	1,134	1,134	1,134	1,167
Mean dep. var	0.261	61.72	26.86	0.006	0.511	0.208	0.037
			Sample B: Only women in urban clusters				
WJC within 1km	-0.008	0.576**	0.127	0.001	-0.005	0.002	0.007
	(0.011)	(0.259)	(0.107)	(0.002)	(0.011)	(0.011)	(0.005)
Observations	34,387	35,534	35,495	35,495	35,495	35,495	38,395
Number of districts	477	477	477	477	477	477	485
Mean dep. var	0.207	63.6	27.4	0.006	0.517	0.252	0.051
District FE	YES	YES	YES	YES	YES	YES	YES
Province-Year FE	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Source: Peru DHS 2006-2014.

Table A-8: Mechanisms: WJCs, the Reporting of Gender-Based Violence (2011-2017) and Prosecution (2006-2015)

Dep. variables	Complaints		
	Gender violence (1)	Gender violence (2)	Gender violence (3)
WJC in the district	0.416***	0.171*	-0.057
	(0.072)	(0.089)	(0.095)
$WJC_{d,t} \times$ Female Officer$_{d,t}$		1.461***	
		(0.287)	
$WJC_{d,t} \times$ All services$_d$			0.366***
			(0.106)
Observations	12,823	10,984	12,124
Number of districts	1,832	1,832	1,741
Dep. variables	Prosecuted		
	Gender violence (1)	Gender violence (2)	Gender violence (3)
WJC in the district	0.403***	0.335**	-0.039
	(0.119)	(0.137)	(0.233)
$WJC_{d,t} \times$ Female Officer$_{d,t}$		0.289	
		(0.414)	
$WJC_{d,t} \times$ All services$_d$			0.512*
			(0.268)
Observations	18,420	18,420	17,420
Number of districts	1,842	1,842	1,741
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. Estimates were made using information on complaints of crimes registered in the Police Reporting System of the National Police of Peru (SIDPOL-PNP) and the National Registry of Complaints of Crimes and Misdemeanors of the INEI. Gender violence includes sexual rape, sexual rape attempt, seduction, acts against pudor, and other offenses of violation of sexual freedom; Family includes illegal marriages, crimes against marital status, omission of family assistance and attacks against parental authority; Economic includes abuse of economic power, illicit sale of merchandise, hoarding, speculation, adulteration, and others; Finance covers financial and monetary crimes; Public includes crimes against transportation, communication and other public services, crimes against public health, and others; Property includes theft, robbery, scam and other frauds, extortion, and usurpation, among others; finally, Drugs includes crimes related to illicit drug trafficking.

Table A-9: School Enrollment Effects by Gender and Grade

Dep. variable	School Enrollment					
	Primary Schools			Secondary Schools		
	Obs.	Mean	WJC within 1km	Obs.	Mean	WJC within 1km
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Results for Schools by Gender						
Log(Female enrollment)	315,221	46.9	0.033***	102,685	84.42	0.009
			(0.010)			(0.017)
Log(Male enrollment)	315,221	49.9	0.021	102,685	90.40	0.067***
			(0.013)			(0.014)
Panel B: Results for Schools by Grade						
Grade 1 enrollment	315,221	15.57	0.019*	102,685	40.97	0.027**
			(0.010)			(0.014)
Grade 2 enrollment	315,221	17.08	0.030***	102,685	38.18	0.034**
			(0.009)			(0.014)
Grade 3 enrollment	315,221	16.55	0.026***	102,685	35.18	0.023
			(0.009)			(0.015)
Grade 4 enrollment	315,221	16.07	0.031***	102,685	31.84	0.043**
			(0.009)			(0.018)
Grade 5 enrollment	315,221	15.70	0.023**	102,685	28.64	0.044**
			(0.009)			(0.019)
Grade 6 enrollment	315,221	14.97	0.033***			
			(0.009)			
School FE			YES			YES
Province*Year FE			YES			YES
Covariates			YES			YES

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is the logarithm of enrollment plus one. The independent variables measures the number of WJC centers within a 1km Euclidean buffer from the school and presence of WJC center in school's district. Standard errors (in parentheses) are clustered at the school 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). Source: Peruvian School Census 2006-2014.

Table A-10: School Enrollment and Children's School Attendance Status Effects by Gender

Dep. variable	Currently Attending School					
	Primary Level Children 6-11 y.o.			Secondary Level Children: 12-16 y.o.		
			WJC within			WJC within
	Obs.	Mean	1km	Obs.	Mean	1km
	(1)	(2)	(3)	(4)	(5)	(6)
Sample: Female						
School attendance	23,973	0.970	0.020**	14,855	0.891	0.022
			(0.009)			(0.019)
Passed grade	23,573	0.917	0.033***	12,808	0.781	0.031
			(0.010)			(0.024)
Repeated grade	23,573	0.047	-0.010*	12,808	0.028	-0.020
			(0.005)			(0.009)
Dropped out	23,573	0.022	-0.025**	12,808	0.088	-0.003
			(0.010)			(0.018)
Left school +2 year ago	23,573	0.002	-0.0009	12,808	0.098	-0.006
			(0.001)			(0.014)
Sample: Male						
School attendance	24,646	0.970	0.015*	18,474	0.899	0.022
			(0.008)			(0.015)
Passed grade	24,543	0.919	0.012	17,358	0.784	0.023
			(0.009)			(0.021)
Repeated grade	24,543	0.050	-0.001	17,358	0.045	0.00007
			(0.008)			(0.008)
Dropped out	24,543	0.021	-0.012*	17,358	0.091	-0.032*
			(0.007)			(0.018)
Left school +2 year ago	24,543	0.002	0.001	17,358	0.074	0.009
			(0.001)			(0.011)
District FE			YES			YES
Province*Year FE			YES			YES
Covariates			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 or secondary 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 for primary level includes children between the ages of 6 and 11 and the sample for secondary level includes children between the ages of 12 and 16. 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. Source: Peru DHS 2006-2014.

Table A-11: Domestic Violence by Children's Primary Level School Attendance Status (2006-2014)

Primary Level (6-11 y.o.)	Children's School Attendance Status		
	Not Attending	Attending	Diff
	(1)	(2)	(3)
Domestic violence (All)	0.435	0.408	0.026**
	(0.010)	(0.001)	(0.010)
Observations	2,131	69,735	
Domestic violence (Urban Areas)	0.469	0.430	0.038***
	(0.014)	(0.002)	(0.014)
Observations	1,149	37,185	
Domestic violence (Rural Areas)	0.395	0.384	0.010
	(0.015)	(0.002)	(0.015)
Observations	982	32,550	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses.

Table A-12: The Effect of WJC Centers on Primary Level Attendance Quintiles (2006-2014)

Dep. variable	Currently Attending Primary Level			
	Quintile 1	Quintile 2	Quintile 3	Quintile 4
Sample: Children 6 to 11 years old				
WJC within 1km	0.067**	0.014	0.021	0.0002
	(0.029)	(0.010)	(0.014)	(0.003)
Observations	11,802	8,944	9,403	18,549
Number of clusters	171	139	109	740
Mean dep. var	0.917	0.969	0.985	0.998
% Rural	0.335	0.349	0.250	0.486
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	NO	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Source: Peru DHS 2006-2014.

Table A-13: The Effect of WJC Centers on Children's Primary School Attendance (2006-2014) - Alternative Euclidean Buffers

Dep. variable	**Currently Attending Primary Level**			
Sample	All children 6-11 y.o	All children 6-11 y.o	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)

Panel A: WJC center within a distance buffer from the cluster of residence

WJC within 3km	0.007	0.004	0.015	0.010
	(0.011)	(0.012)	(0.014)	(0.016)
Observations	48,703	48,703	25,391	19,563
Number of districts	1,159	1,159	485	215
Mean dep. var	0.970	0.970	0.971	0.969

Panel B: WJC center in the district of residence

WJC within 5km	-0.007	-0.004	0.005	0.006
	(0.008)	(0.008)	(0.011)	(0.007)
Observations	48,703	48,703	25,391	19,563
Number of clusters	1,159	1,159	485	215
Mean dep. var	0.970	0.970	0.970	0.967
District 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 a dummy indicating whether the child is currently attending primary school. The independent variables measures the presence of a WJC within a 3km and 5km Euclidean buffer of the child's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample 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. Source: Peru DHS 2006-2014.

Table A-14: The Effect of WJC Centers on Children's Secondary School Attendance
(2006-2014) – Alternative Euclidean Buffers

Dep. variable	Currently Attending Secondary Level			
Sample	All children 12-16 y.o	All children 12-16 y.o	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)

Panel A: WJC center within a distance buffer from the cluster of residence

WJC within 3km	0.008	0.009	0.016	0.012
	(0.012)	(0.014)	(0.014)	(0.017)
Observations	33,519	33,519	18,266	13,570
Number of clusters	1,140	1,140	480	215
Mean dep. var	0.895	0.895	0.916	0.908

Panel B: WJC center in the district of residence

WJC within 5km	-0.011	-0.001	-0.001	-0.003
	(0.013)	(0.015)	(0.016)	(0.019)
Observations	33,519	33,519	18,266	13,570
Number of clusters	1,140	1,140	480	215
Mean dep. var	0.896	0.896	0.913	0.904
District 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 a dummy indicating whether the child is currently attending secondary school. The independent variables measures the presence of a WJC within a 3km and 5km Euclidean buffer of the child's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample includes children between the ages of 12 and 16. 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. Source: Peru DHS 2006-2014.

Table A-15: The Effect of WJC Centers on Child Labor (2006-2014)

Dep. variable	Child Labor			
Sample	All children	All children	Female	Male
	6-14 y.o	6-14 y.o		
	(1)	(2)	(3)	(4)

Panel A: All Children

WJC within 1km	-0.021***	-0.008*	-0.014**	-0.003
	(0.005)	(0.004)	(0.006)	(0.006)
Observations	97,933	97,933	48,108	49,816
Number of districts	1,169	1,169	1,162	1,164
Mean dep. var	0.070	0.070	0.064	0.075

Panel B: Children of the women selected for the DV module

WJC within 1km	-0.024***	-0.012**	-0.018**	-0.006
	(0.006)	(0.005)	(0.008)	(0.007)
Observations	71,410	71,410	35,162	36,215
Number of districts	1,163	1,163	1,145	1,147
Mean dep. var	0.065	0.065	0.059	0.070
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	NO	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 working. 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 includes children between the ages of 6 and 14 years old. 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. Source: Peru DHS 2006-2014.

Table A-16: Mechanisms: Impact of WJC centers on Decision Making and Bargaining Power in the Household (2006-2014)

Dep. variable	Joint decision making		
	score (0-6)	score (0-1)	dummy(0/1)
	(1)	(2)	(3)

Sample: Married or cohabiting women 15-49 years old

WJC within 1km	0.040	0.007	0.017*
	(0.047)	(0.008)	(0.009)
Observations	72,009	72,009	72,009
Number of clusters	1,168	1,168	1,168
Mean dep.var.	2.238	0.373	0.798

Dep. variable	Earnings compared to husband		
	Earns more than husband	Earns Less than husband	Earns the same as husband

Sample: Married or cohabiting women 15-49 years old

WJC within 1km	0.008	-0.034*	0.029**
	(0.011)	(0.018)	(0.014)
Observations	33,767	33,767	33,767
Number of districts	1,094	1,094	1,094
Mean dep.var.	0.125	0.676	0.189
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. In the DHS, women are asked who makes decisions on a variety of household issues. For instance, a women is asked "*Who makes the final decision on your own health care?*," "*Who makes the final decision on large household purchases?*," etc. Responses include: respondent only, jointly with partner, and partner only. From these replies, we construct three measures of equal decision making—that is, when the women makes decisions jointly with the partner. Robust standard errors (in parentheses) are clustered at the district level. The sample includes women between the ages of 15 and 49. 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. Source: Peru DHS 2006-2014.

Table A-17: Mechanisms: Impact of WJC centers on Women's Labor Force Participation (2006-2014)

Dep. variables	Currently working (1)	Works for family (2)	Works for someone else (3)	Self-employed (4)
Sample A: All women 15-49 years old				
WJC within 1km	-0.010	-0.004	-0.010	0.005
	(0.010)	(0.005)	(0.008)	(0.007)
Observations	113,785	113,786	113,786	113,786
Number of clusters	1,168	1,168	1,168	1,168
Mean dep.var.	0.646	0.211	0.305	0.236
Sample B: Married or cohabiting women selected for the DV module				
WJC within 1km	-0.009	-0.004	-0.024	0.017
	(0.014)	(0.009)	(0.017)	(0.011)
Observations	64,354	64,354	64,354	64,354
Number of districts	1,167	1,167	1,167	1,167
Mean dep.var.	0.684	0.209	0.269	0.300
District 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 a dummy indicating women's labor force participation during the last 12 months. 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. 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. Source: Peru DHS 2006-2014.

Table A-18: Relationship between WJCs within 1km rollout and pre-program school enrollment

	△ Log(Primary School Enrollment)			△ Log(Secondary School Enrollment)		
	Schools matched to WJC within 1km, Pre-WJC period					
	(1)	(2)	(3)	(4)	(5)	(6)
	△98-00	△98-05	△98-10	△98-00	△98-05	△98-10
WJC within 1km in 2002	0.028			0.060		
	(0.032)			(0.047)		
WJC within 1km in 2003	-0.016			0.042		
	(0.036)			(0.050)		
WJC within 1km in 2004	-0.021			-0.070		
	(0.035)			(0.054)		
WJC within 1km in 2005	-0.054			-0.207***		
	(0.156)			(0.066)		
WJC within 1km in 2006	-0.014			-0.048		
	(0.031)			(0.056)		
WJC within 1km in 2007	-0.011	0.004		-0.020	0.013	
	(0.029)	(0.015)		(0.046)	(0.028)	
WJC within 1km in 2008	-0.006	0.032		-0.032	0.041	
	(0.029)	(0.035)		(0.045)	(0.027)	
WJC within 1km in 2009	-	-		-	-	
WJC within 1km in 2010	-0.034	0.008		0.003	0.036	
	(0.028)	(0.015)		(0.045)	(0.027)	
WJC within 1km in 2011	-0.022	0.005		-0.052	0.032	
	(0.027)	(0.015)		(0.045)	(0.025)	
WJC within 1km in 2012	0.002	0.017	0.005	-0.016	0.019	0.000
	(0.035)	(0.017)	(0.009)	(0.043)	(0.029)	(0.013)
WJC within 1km in 2013	-0.029	0.017	0.009	-0.007	0.026	0.004
	(0.029)	(0.016)	(0.011)	(0.045)	(0.033)	(0.018)
WJC within 1km in 2014	-0.021	0.007	0.004	-0.003	0.066**	0.031*
	(0.031)	(0.020)	(0.011)	(0.043)	(0.026)	(0.016)
Observations	2,190	6,372	6,157	1,115	3,400	3,540
Number of schools	1,179	1,247	678	607	710	404
Year FE	YES	YES	YES	YES	YES	YES
P-value joint test	0.536	0.275	0.925	0.001	0.148	0.197

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors (in parentheses) that allow for clustering at the school level are reported in parentheses. The dependent variable in columns 1-6 is the change in the logarithm of school enrollment plus one. The observations correspond to three windows of pre-WJC center period for each school. All regressions include year fixed effects.

Table A-19: Relationship between WJCs in the district rollout and pre-program school attendance

	Districts matched to WJC locations, Pre-WJC △1996-2005					
	△ Primary School Attendance			△ Secondary School Attendance		
	(1)	(2)	(3)	(4)	(5)	(6)
	△96-00	△96-05	△96-10	△96-00	△96-05	△96-10
WJC in the district in 2002	0.002			-0.071		
	(0.036)			(0.060)		
WJC in the district in 2003	-0.056			0.032		
	(0.060)			(0.062)		
WJC in the district in 2004	-0.005			0.041		
	(0.036)			(0.082)		
WJC in the district in 2005	0.016			-0.051		
	(0.036)			(0.060)		
WJC in the district in 2006	-0.057			-0.078		
	(0.052)			(0.087)		
WJC in the district in 2007	-0.031	0.010		-0.065	0.033	
	(0.040)	(0.015)		(0.109)	(0.051)	
WJC in the district in 2008	-0.011	0.012		-0.008	-0.013	
	(0.039)	(0.014)		(0.098)	(0.046)	
WJC in the district in 2009	-	-	-	-	-	-
WJC in the district in 2010	-0.026	0.011	-0.009	-0.062	0.015	-0.013
	(0.040)	(0.014)	(0.008)	(0.071)	(0.045)	(0.028)
WJC in the district in 2011	-0.034	-0.002	-0.016	0.030	0.008	-0.029
	(0.041)	(0.016)	(0.009)	(0.067)	(0.036)	(0.024)
WJC in the district in 2012	0.012	0.020	0.006	0.022	-0.040	-0.052
	(0.039)	(0.014)	(0.008)	(0.076)	(0.042)	(0.041)
WJC in the district in 2013	-0.008	0.006	-0.012	0.055	0.002	-0.015
	(0.049)	(0.021)	(0.011)	(0.101)	(0.055)	(0.030)
WJC in the district in 2014	-0.073	0.020	-0.007	-0.152	-0.049	-0.030
	(0.076)	(0.054)	(0.038)	(0.125)	(0.074)	(0.054)
Observations	90	186	228	90	184	226
Number of districts	90	106	102	90	106	102
Year FE	YES	YES	YES	YES	YES	YES
P-value joint test	0.000	0.676	0.222	0.000	0.712	0.778

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (in parentheses) that allow for clustering at the district level are reported in parentheses. The dependent variable in columns 1-6 is the change in school attendance rate at the district level. The observations correspond to three windows of pre-WJC center period for each district. All regressions include year fixed effects.

Table A-20: Relationship between WJCs within 1km rollout and four windows of pre-program standardized test scores (2nd grade - Primary School)

	Schools matched to WJC within 1km			
	Pre-WJC period			
	△ **Standradized Test Scores**			
	(1)	(2)	(3)	(4)
	△07-09	△07-10	△07-11	△07-12
WJC within 1km in 2011	0.002			
	(0.034)			
WJC within 1km in 2012	0.045	-0.009		
	(0.046)	(0.029)		
WJC within 1km in 2013	-0.023	-0.029	-0.001	
	(0.066)	(0.038)	(0.034)	
WJC within 1km in 2014	0.042	-0.019	-0.009	-0.025
	(0.060)	(0.039)	(0.033)	(0.034)
Observations	1,565	1,675	1,068	734
Number of schools	821	600	292	168
Year FE	YES	YES	YES	YES
P-value joint test	0.670	0.895	0.828	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (in parentheses) that allow for clustering at the school level are reported in parentheses. The dependent variable in columns 1-4 is the change in standardized reading and math z-scores at the school level. The observations correspond to the pre-WJC center period for each school, it includes all schools which are located within 1km of a WJC center which opened between 2010-2014, 2011-2014, 2012-2014, and 2013-2014. All regressions include year fixed-effects.

Table A-21: Relationship between WJCs in the district and four windows of pre-program domestic violence

	Districts matched to WJC locations, Pre-WJC period			
	$\triangle$ **Domestic violence in last 12 months**			
	(1)	(2)	(3)	(4)
	$\triangle$2000-2005	$\triangle$2000-2008	$\triangle$2000-2010	$\triangle$2000-2013
WJC in the district in 2007	-0.021			
	(0.088)			
WJC in the district in 2008	-0.001			
	(0.087)			
WJC in the district in 2009	-	-		
WJC in the district in 2010	-0.018	-0.006		
	(0.082)	(0.035)		
WJC in the district in 2011	0.013	0.007	-0.026	
	(0.078)	(0.034)	(0.042)	
WJC in the district in 2012	-0.025	0.060	-0.011	
	(0.093)	(0.041)	(0.041)	
WJC in the district in 2013	0.041	0.013	0.005	
	(0.098)	(0.061)	(0.050)	
WJC in the district in 2014	0.071	0.119**	-0.036	-0.016
	(0.074)	(0.078)	(0.042)	(0.020)
Observations	105	161	239	128
Number of districts	78	99	83	38
Year FE	YES	YES	YES	YES
P-value joint test	0.416	0.103	0.433	-

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (in parentheses) that allow for clustering at the district level are reported in parentheses. The dependent variable in columns 1-4 is the change domestic violence at the district level. The observations correspond to the pre-program period of the WJC center rollout for each district, it includes all districts that ever had a WJC center which opened between 2006-2014, 2009-2014, 2010-2014 and 2013-2014. All regressions include year fixed-effects.

Table A-22: Relationship between WJCs in the district and district time varying economic variables

Dep. variables	Expenditure in education (1)	Income taxes (2)	Donations (3)	Income (4)	Transfers (5)	Current income (6)	Capital income (7)
WJC in the district	11.094 (13.908)	-8.647 (5.991)	-0.143 (0.103)	11.256 (7.658)	34.877 (26.620)	37.343 (30.023)	-37.076 (33.465)
Observations	32,985	31,045	31,045	31,045	31,045	31,045	31,045
District FE	YES	YES	YES	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES	YES	YES	YES
Number of districts	1833	1833	1833	1833	1833	1833	1833

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors that allow for clustering at the district level are reported in parentheses.