# Female Officers, Gender Violence 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, intra-household bargaining, and investment in children? This paper provides evidence from Peru on all-women's justice centers (WJCs), specialized institutions that mostly employ female officers and provide police and legal services to reduce gender-based violence. Examining the gradual rollout of WJCs across districts/villages, we find that the opening of a center increases reporting of gender-specific crimes by 40% and reduces the incidence of gender-based violence measured by domestic violence, femicides and hospitalizations due to mental health by about 10%. We find, moreover, that a decrease in the exposure of women 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.

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# 1 Introduction

Gender-based violence is a worldwide social problem that affects 30% of women each year (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 threat to prevent violence against them. High rates of under-reporting of gender-based violence and low arrest rates for crimes against women imply unequal access to the law, particularly for women (Eswaran, 2018). Evidence from India finds that only three percent of women have ever had contact with the police, even though gender violence is quite high (Banerjee et al., 2012). 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).[1] Moreover, 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 of access to justice.[2]

In this paper, we provide evidence that, in Peru, improving women's access to justice reduces gender-based violence and consequently improves children's outcomes. We exploit the impact of an innovative form of access and representation on justice: all-women's justice centers (WJCs). WJCs are specialized state institutions that primarily employ female officers in efforts to reduce gender-based violence by 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.[3]

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 attorneys about femicides, female deaths due to aggression, and mental health, (ii) administrative reported-crime data from police and WJC complaints, and (iii) self-reported domestic violence from household surveys. Second, we examine the intergenerational effects of increasing women's access to justice, 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 become more aligned with women's preferences when violence against women declines.

[1] In cases of family violence in rural Peruvian communities, for example, women are often assumed to have a certain level of blame in a conflict (Revilla, 1999). In many cases, police ignore domestic violence complaints, reasoning that "domestic disputes" are not a police matter. Moreover, traditional methods of justice based on local customs are also often discriminatory towards women (Franco and González, 2009).

[2] 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.

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

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. 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% (WHO, 2012).[4] 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 WJCs are a large-scale national program that covers the whole 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 WJC: the presence of a WJC center within 1 kilometer of a household/school and the presence of a WJC center 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 the proximity of 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, we can disentangle the effects from the reporting bias that is usually present in crime data.

Our first finding is that improving access to justice for women reduces domestic violence, femicides, and female deaths due to aggression. We also find that it improves women's mental health. In particular, we find that after a WJC opens, women who live in a 1-kilometer radius are significantly less likely to experience physical and emotional violence from their spouse. 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, suggesting that there is no overall improvement in law and enforcement conditions or policy changes other than WJCs that is driving our results.

WJCs have several innovative features that can explain the reduction in gender violence. WJCs may generate a credible threat to offenders through greater reporting, criminal penalties, or issuing 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

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

the steps of their complaints in a single office.[5] In addition, female officers may increase the likelihood that other women will approach the police and other government institutions to seek help.[6] For example, female officers may be more responsive to gender violence by being more engaged to the challenges female victims face when initiating such complaints.[7]

Consistent with these mechanisms, we find evidence that after a WJC opens in a district, women increase the reporting of gender-based violence cases. In particular, 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 concluded the complaint process against the aggressor, compared to 10% of those who went to a traditional police station.[8] In addition, the effects are driven by those locations with a larger share of female officers, suggesting that female representation in law enforcement can be an important factor to increase the reporting of gender violence.[9]

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 it are significantly more likely to be enrolled, attend school, pass a grade, have better national test scores, and be less likely to drop out of school. These results are robust to using different datasets that measure educational outcomes. Moreover, we find that the main results for children are driven by 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 ability to take care of 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;

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

[6]Female victims may feel more confident about reporting such crimes to female officers. Relatedly, Iyer et al. (2012) find that as women increase their representation in politics, a higher percentage of victims report crimes against them.

[7]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 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.

[8]Press release issued by the Peruvian Ministry for Women and Vulnerable Populations on January 8th, 2018. https://www.mimp.gob.pe/salaprensa/nota-prensa.php?codigo=2662

[9]While arresting and punishing offenders could create an incapacitation effect, we cannot test this mechanism because arrest data are not available.

McElroy and Horney, 1981; Manser and Brown, 1980).[10] Similarly, the threat point for women may increase when they have access to justice and when support services are more helpful. WJCs may allow women to credibly threaten to involve the police or 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.

The main threat to our identification strategy is 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 find no effects on non-gender-specific complaints such as complaints for property crimes.[11] In addition, we find no effects on children's education for historically non-violent households (using the status of the grandmother). 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 the police presence in these areas. Second, 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. Third, we show that WJC placement was not anticipated by changes in gender-based violence and schooling.[12] Finally, we limit the samples 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 is related to several literatures. 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

[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 here 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 and drug crimes.

[12] A central issue in our analysis is the fact that WJCs are not placed randomly. Conversations with policymakers 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 locating them based on anticipated increases in violence and schooling or increases in previous years.

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 having female officers 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 integral 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 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 using unreported measures of violence. In a further step, we provide evidence of intergenerational effects showing that children's education improves.

This paper is also related to the literature on the economics of crime, which analyzes the role of police deployment in deterring crimes. Di Tella and Schargrodsky (2004) and Draca, Machin and Witt (2011) find a reduction in crime when focusing on exogenous increases in the supply of police in specific areas in the wake of terrorist attacks. Mello (2016) and Machin and Marie (2011) reach the same conclusion when looking at the effect of providing extra resources to certain police districts. There is also evidence that hot-spot police patrolling can deter crime (Blattman et al., 2017). We complement this literature by focusing on gender-based violence where there is very little evidence regarding the role of police management in the prevention of these types of crimes. Related to this paper, Eswaran (2018) theoretically analyzes how traditional police beliefs about the falsity of sexual assault claims can lower the probability of conviction.

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 that women get an equal share of resources within their households will alleviate gender-based violence, recent research has shown 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 deterrence of crimes committed against women may be perceived to be low, allowing perpetrators to threaten

[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 improving their outside options and bargaining status in their relationship, thus decreasing their exposure to violence (Farmer and Tiefenthaler, 1996; Stevenson and Wolfers, 2006; Aizer, 2010; Hidrobo and Fernald, 2013). On the other hand, increasing the resources available to women may strengthen men's incentives to threaten or use violence to control these newly obtained resources or to regain decision-making power within the household. As a result, women may become more vulnerable to mistreatment (Bobonis, González-Brenes and Castro, 2013; Eswaran and Malhotra, 2011; Bloch, Rao and Desai, 2004).

violence without repercussions.

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, both find that domestic violence has a negative impact on women's pregnancies and infant health. We complement these papers by providing new causal evidence on the relationship between gender-based violence and children's education. Moreover, we provide new evidence of this kind in the context of developing countries where gender-based violence is high and justified by households.

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 the introduction of WJCs affects domestic violence and 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 "Belem do Pará" Convention on the "Prevention, Punishment, and Eradication of Violence against Women" significantly expanded Latin America's definition of domestic and sexual violence. As a consequence, many countries in the region modified or enacted new legislation incorporating those issues into their political agenda. In particular, Peru altered the jurisdiction of its police and justice system to encompass domestic and sexual violence complaints. This new legal framework paired with the government's awareness of the country's high levels of domestic violence led in 1999 to the creation of the 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

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

through an interdisciplinary approach that includes legal, social, and psychological dimensions. Basically, incoming victims receive a service designed to integrate all of 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, as much as possible, the time required to file the complaint and to follow the legal procedures of the corresponding court of justice. 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 in the first year to 226 by the end of 2014, covering 100% of the 24 regions of Peru and 96% of the provinces (188 of 196 provinces). 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/year; from 2006 to 2014 this rate climbed to 22 WJCs/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, by 2014 most of the WJCs had been concentrated in Metropolitan Lima and Lima Provinces (31 WJCs); in the Callao region there are four WJCs; the rest of the coastal region has 46; in the sierra region there are 117 and in the jungle region there are 28 (Figure 2). Given the above-mentioned strong ties to local justice and health institutions, WJCs are heavily concentrated in urban areas.

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 are from women between 25 and 45 years old, children and teenagers (0–17 years old) constitute the second largest group – 30%. Additionally, a 2006–2008 survey administered by MIMP on 51 WJCs revealed that for the majority of the women (75%) who visited a WJC, domestic violence stopped during or after the program intervened. In the remaining 25% of cases, however, violence persisted even after having the WJC visit (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,

[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).

[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

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 women's and their children's outcomes, we rely on microdata from the Peruvian Demographic and Health Survey (DHS), which is collected over 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 Peruvian DHS collects primarily demographic and health information from women aged 15 to 49 years old that includes their fertility, weight, marital status, employment status, household decision making and socio-economic characteristics, among others. In addition to this, it also includes some 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 which asks eligible women if they had 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 to 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 never married or never cohabited 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:

[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 is collected quarterly instead of 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 spouses 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. There are three security and ethical precautions increasingly mandated by the DHS program for the collection of data on domestic violence. The first requires that the interviewer not continue with the questions on domestic violence if privacy cannot be ensured. In general, the interviewers are women trained to elicit trust from the respondents. The second requires that only one eligible woman in each selected household is to be administered the module questions. In sample households where more than one woman is eligible for the DHS survey, the domestic violence module is administered to only one randomly selected woman. By interviewing only one woman in each household, possible security breaches, due to other persons in the household knowing that information on domestic violence was given, are minimized. The third requires that the domestic violence questions should be only administered to previously or currently married or cohabiting women, even though the DHS sample includes all women age 15–49. Only 1% of the eligible women were not interviewed because privacy was not made 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 employ several different outcomes to measure violence against women: femicides and female deaths due to aggression.

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 when the woman has experienced 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, 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 we do not observe attendance rates with the School Census, we use the Peruvian DHS to estimate the share of children in primary and secondary grades who are enrolled 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 he/she is involved in any child labor.

Panel B of Tables A-1 and A-2 provides summary statistics on women's characteristics and on 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 declared to have experienced 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 in 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% of the students in this educational level.

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 presence of WJCs in the district of residence but also proximity to the WJC. Although this data was collected yearly, in this study we were able to obtain the GPS cluster locations only for the 2000, 2004–2008, 2009–2011 and 2014 Peruvian DHS Surveys. Since the DHS does not disclose the name 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.

[20] More specifically, the DHS defines *moderate physical violence* as when the woman experiences 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 when the woman has experienced 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 when the woman has experienced 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.

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 can substantively affect the results of the analysis (Burgert et al., 2013).

Nevertheless, Perez-Heydrich et al. (2013) propose several recommendations 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 the resource facilities decreases, the probability that a DHS cluster is linked to the correct closest WJC increases for all types of locations (urban and rural). In Peru, there are a total of 226 WJCs by 2014, which means that the spatial density of the WJCs is quite low and, thus the measurement error has been significantly reduced. Second, the authors recommend to study the effect of the service within a reasonable buffer distance, rather than using the closest distance to the resource facility. For this reason, we are going to measure exposure to the WJC through different groups of Euclidean distance buffers. Finally, we are also going to limit the 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, which covers the universe of schools in Peru during the period 1998–2014. This dataset is collected on a yearly basis by the Peruvian Ministry of Education, with the exception of the year 2003, and it contains a rich set of information at the school level.

More specifically, the School Census collects comprehensive data on the total number of enrolled students by age, grade, and gender. This data is designed to reflect enrollment (not attendance) statistics corresponding to the months of May–July. The School Census also collects data on school characteristics such as language of instruction, public or private, urban or rural area, and other physical plant characteristics (i.e., electricity, piped water, and so on). 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 language (Spanish) 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 school. The geographic coordinates of the schools allow us to combine these data with the WJC's locations, in order to see whether the area/district of the school is located near a WJC and is 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 this dataset is not a balanced panel, because during the period of study some schools have closed, while others have opened. In addition, as mentioned above, there is no data available for the year 2003, since data for this year was not collected. 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 one through six) and 12,811 secondary schools (grades one through five).[23]

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 secondary schools are more likely to be found in urban areas. 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 of them the language of instruction is Spanish.

A final important issue of 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. We discuss this issue in greater detail in Section 4. In addition, as a robustness check we also use the Peruvian DHS to estimate the share of children who are attending school.

### 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 to 2014. By using the administrative locations and addresses provided in the directory of the MIMP, we were able to geocode all the WJCs, which allows us to have 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

[23]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.

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 to 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 a client who murders a sex worker (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 it is not geocoded. In this analysis, we only consider the cases of intimate femicides in order to be consistent with the DHS data. Of the 852 murders of women in Peru from 2009 to 2015, 762 (90%) were intimate femicides and 90 (10%) were non-intimate femicides (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 according to 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 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 to 2017, and is available at the district level.[24]

### 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 1-kilometer Euclidean buffer of the DHS

[24]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

cluster/school and (ii) WJC in the district of the DHS cluster/school.

The first measure uses the GPS coordinates of the DHS clusters/schools in order 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 and 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's/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.

Our preferred measure is the one that uses the Euclidean buffer 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 buffer is not only because we believe that these centers have a very localized effect, but also because the measure of exposure, using a five-kilometer Euclidean buffer, seems to be very similar to the one that uses the presence of a WJC in the district. We present the results using both measures of exposure to a WJC principally for our main outcomes of interest.

# 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. Even though our analysis will take advantage of variation over time, which will account for any fixed differences across districts and schools, it still remains important to understand what drives placement since placement decisions may not be orthogonal to other factors that could affect women's 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 from 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, on several occasions political representatives had a certain autonomy in deciding the order in which districts received the program. There is also anecdotical evidence from the authorities that the placement of WJCs was primarily developed by taking population density into account, but not 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 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.[25] 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 address the concern about the non-random placement of WJCs is that we are able to evaluate this endogenous placement statistically using our data. To do this we estimate, at the district level, (a) the determinants of having a WJC by the end of the sample in 2014 and (b) the determinants of adding a WJC during 2006–2014, the period when the program grew substantially. We focus on several variables at the district level cited by Peruvian policymakers, including the number of justice courts, district attorney offices, police stations, and health establishments. We also control for district population at baseline and department fixed-effects. Moreover, in order to verify that education patterns before the program began do not predict where the WJCs are introduced, we also control for pre-program changes in primary and secondary school enrollment at the district level. Unfortunately, we are unable to perform the same test for 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 interests 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. Districts with more police stations, more district attorney offices, more health estab-

[25] 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.

lishments and more densely populated are more likely to have WJCs by 2014 and more likely to add them during 2006–2014. Clearly, urban areas with more infrastructure development are more likely to have these specialized centers for women. In addition, pre-program changes in primary and secondary district school enrollment do not seem to have any impact. Neither coefficient is statistically significant and both are very small. Similarly, domestic violence does not 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 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 of 2005, right at the time when the WJCs started to be implemented more intensively.[26] In addition, *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 percentage of villages affected by violence during the civil conflict. The aim of *Juntos* was to reach some of the most vulnerable and marginalized segments of the population; it focused particularly on rural areas with high poverty rates and limited access to State services.[27] 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.[28]

[26] 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. There are two large expansions of the of the CCT *Juntos* program, first in 2007 and then in 2012.

[27] *Juntos* is targeted to 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. This program is also explicitly seen as a way to tackle the special vulnerability of populations who were most affected by the political violence that was prevalent in Peru between 1980 and 2000. Most of the victims of this conflict were poor populations living in rural areas and Quechua speakers.

[28] 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.

The second concern related to WJC placement is that if we estimate the impact of WJCs on all areas, our results might be identified of rural areas which are not "at risk" of having a WJC and these may not be 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's and children's outcomes. We exploit the variation created by the differential timing in the opening of the 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's and children's outcomes, the following specification is used:

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

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's/child's household or in the district of residence of woman/child $i$ in year $t$, $(\alpha_d)$ is a district fixed-effect, $(\lambda_{pt})$ is a province-by-year fixed effect, $(X'_{idt})$ is a vector of individual-level characteristics for woman/child $i$ depending on the sample of interest and $(\varepsilon_{idt})$ 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 account for possible time-invariant unobserved characteristics at the district level, such as cultural differences or attitudes towards the role of women/children. This, however, does not account for any differential trends in woman's/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 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.

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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.

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 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 rural-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, 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, and rural-urban residence.

The coefficient of interest is $\gamma_1$, 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 their 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 femicides, female deaths due to aggression and female hospitalizations for mental health problems at the district level:

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

where $(y_{dt})$ refers to alternative domestic violence metrics (e.g. femicides by intimate partner, female deaths due to aggression) 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 center, $(\alpha_d)$ is a district fixed-effect, $(\lambda_{pt})$ is a province-by-year fixed-effect, $(X_{dt}^{'})$ represents time-varying district level covariates (e.g. district population), and $(\varepsilon_{dt})$ 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 they are not geo-coded. For this specification, the dependent variables are defined using the logarithm (instead of the level).

This is a standard fixed-effects model, where identification is derived from changes in domestic violence/mental health outcomes correlated to changes in the introduction of WJC centers 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 domestic violence/mental health outcomes are uncorrelated with the rollout of the WJC centers within their 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, $(\alpha_s)$ is a school fixed effect, $(\lambda_{pt})$ 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, presence of electricity, presence of piped water, school language (Spanish), urbanization, and public school dummy) and $(\varepsilon_{st})$ 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 $(\beta_1)$, 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 the proximity of 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 $(\lambda_{pt})$ 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.

Nevertheless, we are concerned about the possibility that the results are driven by time-varying variables that might influence both the opening of the WJCs and school enrollment.

A related issue is the possibility that WJC managers consciously decide to introduce centers where school enrollment is increasing. To address both of these issues, we use the panel nature of the school data in order to construct a placebo treatment based on the timing of the WJCs introduction. We estimate whether *future* WJCs predict current enrollment using equation 4 below:

$$Y_{st} = \beta_0 + \beta_1 WJC_{st} + \beta_2 WJC_{st+1} + \beta_3 WJC_{st+2} + \beta_4 WJC_{st+3} + \alpha_s + \lambda_{pt} + \gamma_t X_s^{'} + \varepsilon_{st} \quad (4)$$

where $(WJC_{st+1})$, $(WJC_{st+2})$ and $(WJC_{st+3})$ are indicator variables that take the value of one if the school has a WJC within one kilometer of or within the district of the school starting from the year $t+1$, $t+2$ and $t+3$. If $\beta_2 > 0$, $\beta_3 > 0$ and $\beta_4 > 0$ are positive and significant, this would indicate that WJCs are being introduced in areas where schooling is increasing more rapidly. While, if $\beta_2 = \beta_3 = \beta_4 = 0$ this would indicate that WJCs are introduced in areas in which school enrollment is growing for other reasons.[29] Therefore, the coefficients $\beta_2, \beta_3$ and $\beta_4$ effectively capture the effect of future openings for areas that are not covered by the WJCs in $t$. Our hypothesis for the placebo regression is that total enrollment in schools that do not have a WJC within one kilometer of or within the district should *not* be affected by the fact that a WJC may open in the future in the proximity of these schools.

# 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. From 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

[29] This technique has already been used to address this concern by La Ferrara, Chong and Duryea (2012) and Oster and Steinberg (2013).

[30] The full sample of women in the Peruvian DHS surveys consists of 210,847 respondents aged 15 to 49 over the period 2000 to 2014. However, this sample is reduced to 121,404 eligible women since we only include partnered women who are eligible for the domestic violence module. When we run estimations using the geocoded cluster locations during the period 2006 to 2014, this sample is reduced even further, to 64,366 observations of women.

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 which are 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 overall sample (2.9 percentage points) and highly significant. Finally, column 4 limits further, to areas that have had a WJC (including those that change and those that have always had a center). 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: 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 compared to those living in districts that do not have this type of institution. 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 mistreatment emotional behavior outcome. For instance, we find that proximity to a WJC can be associated to a lower likelihood that an intimate partner threatening to take the children from the 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 number of WJCs 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 the proximity of the 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 their weight compared to those living further away. These results show some suggestive evidence of an improvement in women's health.

### 5.1.1 Mechanisms: WJCs and the Reporting of Gender-Based Violence

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 by having a more integral approach to handle gender-specific crimes. In other words, improving women's access 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. Table 4 presents the results from estimating equation 2. Column 1 shows that after the opening of a WJC, the number of complaints for gender violence increase by 35%, suggesting that women report more of these types of crimes after the introduction of WJCs.[31]

We also test whether results are driven by the presence of female officers. To do so, we interact the main treatment effects by the share of female officers per district per year. We find that the interaction is positive and significant (see Column 2). These results provide suggestive evidence that having a female officer in the WJC can be an important mechanism to increase the reporting of gender violence.

Columns 3-8 presents the results for other type of crimes. We find no effect for complaints that are not related to gender violence, such as economic and property crimes. This is consistent with a survey done in 2017 which shows that 75% of women who went to a WJC issued a police complaint for gender violence, compared to 10% of those who went to a traditional police station.

Another mechanism that we are not able to explore is to what extent female officers at WJCs have better performance than male officers at traditional police stations. Unfortunately, we do not have data on performance indicators, such as whether the officer started a formal investigation or whether the complaint led to an arrest.

Overall, these results provide evidence that when a WJC opens in the district, women report more gender-specific crimes, increasing the cost for perpetrators of violence against women. Thus, after the opening of a WJC, these institutions become a credible threat to prevent violence since women are then more likely to report these types of crimes.

[31] This includes sexual harassment, rape, and domestic violence.

### 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 their attendance status. Table 5 summarizes the estimated effects of WJCs on children's school attendance at the primary and secondary level, from 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 opening of the centers. 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. From estimating equation 3, Tables 7 and A-8 present estimated impacts of WJCs on average enrollment in primary schools and secondary schools, respectively. Table 8 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 an increase of 2.8% 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 is 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 the alternative measure of exposure: 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 is reassuring.

Table A-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.

Finally, 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 8 shows that test scores of children in schools located in the proximity of 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. They also indicate that these findings are localized to within a few kilometers and they are mostly driven by urban areas. In Table A-10 of the Appendix, we also show these effects broken down by gender and grade. We find that these effects are similar for boys and girls, even though they seem to be driven mostly by girls. We also find that the impact is equally distributed among the different grades.

#### 5.3.1 Placebo Regression: *Future* WJCs

As mentioned earlier, one of the main threats to this identification strategy is the possibility that WJCs were rolled out in response to changes in enrollment, rather than causing them. This is strongly linked to the issue of endogenous WJC placement. Even though we account for characteristics that are constant over time through school fixed effects, one concern that remains is the possibility that WJCs are placed in areas where enrollment is increasing more rapidly, since center managers or policymakers are targeting more densely populated areas. Another concern is posed by time-varying unobservables correlated with both the timing of the WJCs and school enrollment. For example, it could be that areas reached by WJCs are also hit by a positive economic shock or that there are improvements in public welfare programs at the time when the WJCs are opening. We already account for this by controlling for province-by-year fixed effects.

However, another way to address the concern that WJCs are located in areas that are changing in other ways that we do not observe is by constructing a placebo treatment based on the timing of the WJC openings. We estimate analogous regressions to the ones in Tables 7 and A-8 (our baseline school-level specification), but instead of only looking at the effects of opening a WJC on current enrollment, we also look at the effects of *future* openings. The idea is that if *future* WJC openings predict current enrollment, this would suggest that WJC placement anticipates changes in schooling, rather than causing them. Table A-16 and A-18 show the results for this falsification exercise for primary and secondary school enrollment, respectively. We find that the effect of future WJCs is virtually zero and not statistically precise, suggesting no strong evidence of pre-trends. In addition, the inclusion of future WJCs does not affect our estimate of the impact of current WJCs on school enrollment.

### 5.3.2 Mechanisms: WJCs and Intra-Household Bargaining

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 improving women's intra-household bargaining power and well-being.

First, we argue that the presence of a WJC in proximity of a household may allow women to send a signal to their husbands regarding their outside options. The availability of easier access to justice thus may generate a more credible threat to potential offenders through greater chances that women may demand police involvement and criminal penalties. Several economic theories of household bargaining power suggest that policies that aim to affect a spouse's outside options from an abusive relationship may also affect intra-household distribution through changes in their relative bargaining positions (Farmer and Tiefenthaler, 1996; McElroy 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 in the household 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 money husband earns?*" Responses include: respondent only, jointly with partner, and 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 one is also a score that ranges from zero to one and counts the share of decisions made jointly with a partner. The third one 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 9 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 joint 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 the WJCs have an effect on women's labor force participation. Results in Table A-19 indicate that women's labor supply does not seem to be affected by the opening of these centers.[32] 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 shed light on the extent to which the reduction in gender-based violence is driving the

[32]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).

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 household without this characteristic. Previous literature suggests that having a mother subject to violence makes women more prone to be subject to violence in their own household. We find that most of the effects are driven by these types of households, showing that this effect is having an effect on children by affecting the households that are most vulnerable to violence (see Table A-15 in the Appendix).

Finally, we also test the effects on child labor. In the context of Peru, if a woman suffers from domestic violence and thus probably from health problems, the burden of household chores falls automatically on her daughters since sons are not expected to get involved in such activities. To better understand why empowering women would help promote school attendance in the context of Peru, we analyze the impact of WJCs on child labor. Table A-17 reports regression results of the impact of WJCs on child labor for children aged 6 to 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.

Overall, the results suggest that the intervention of WJCs in households with abuse may change the behavior of offenders and victims. In other words, opening WJCs might be a powerful tool to reduce the incentives of the spouse to choose domestic violence through an improvement of the bargaining power of women in the household and/or an increase in institutional trust, which, in turn, might also improve women's health and their ability to take care of their children.

Furthermore, our paper shows that the impact of WJCs is not limited to the direct recipients of gender violence. We find strong evidence of positive externalities in terms of their children's human capital investment. In the context of Peru, empowering and supporting women through the presence of WJCs can contribute to children's education outcomes by improving women's welfare. Women affected by domestic violence may be limited in their ability to take care of their children. Children growing up in households where there is violence among intimate partners can suffer from behavioral and emotional problems, which may lead them to drop out of school and even engage in child labor.

# 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 order of the opening of the WJCs and the trends in domestic violence and education patterns before the rollout of the program. Basically, 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. To address this concern, we first argue that pre-program changes in domestic violence and education patterns are not correlated with the timing of the future introduction of WJCs. Second, 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.

In order to test this, 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:

$$\Delta Log(Y_{st}) = Log(Y_{st-1}) - Log(Y_{st}) = \gamma + \alpha_t + \sum_{k \geq t} \delta_k I(WJCyear_{<1km,s} = k) + \varepsilon_{st} \quad (5)$$

The dependent variable, $\Delta Y_{st}$, 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 $\alpha_t$. 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 check 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:

$$\Delta y_{dt} = y_{dt-1} - y_{dt} = \gamma + \alpha_t + \sum_{k \geq t} \delta_k I(WJCyear_d = k) + \varepsilon_{dt} \quad (6)$$

In Tables A-20 and A-21 of the Appendix, we report the results of estimating Equation (5) and (6) 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 to 2014. In particular, results in Table A-20 indicate that opening a WJC within one kilometer of the school does not significantly explain pre-program changes in primary and secondary school enrollment between 1998 and 2005. Similarly, results in Table A-21 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-22 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-23 reports the results of estimating Equation 6 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.[33]

In conclusion, 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/within one kilometer. The pre-program patterns for each relevant outcome of interest are also depicted by Figure 4.

### 6.2 Accounting for the Dynamic Impact of WJCs

We next exploit the fact that we have information from prior to the introduction of the WJC, since the rollout was done gradually each year, in order to conduct an additional formal test of whether pre-trends in the outcomes of interest are correlated with the launching of the WJC program in Peru. This test also allows us to better understand the dynamics of WJC introduction and to disentangle the effect over time (for example, how quickly school enrollment or attendance rates increase after the opening of a WJC and whether this impact accelerates, stabilizes or mean

[33]Unfortunately, we are unable to perform this test for other women's outcomes due to lack of data availability for the pre-program period. Official data on femicides in Peru started being recorded in 2009 and female hospitalizations in 2006–2007 .

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 and school level, respectively:

$$y_{idt} = \gamma_0 + \sum_{i=-3}^{4} WJC_d * \beta_i I(\tau_{dt} = i) + \alpha_d + \lambda_{pt} + \delta X_{it}^{'} + \varepsilon_{idt} \tag{7}$$

$$Y_{st} = \beta_0 + \sum_{i=-3}^{4} WJC_s * \beta_i I(\tau_{st} = i) + \alpha_s + \lambda_{pt} + \gamma X_{st}^{'} + \varepsilon_{st} \tag{8}$$

where $\tau_t$ denotes the event year, defined so that $\tau = 0$ for the year the WJC was introduced within one kilometer of or within the district of the household $i$/school $s$, $\tau = 1$ for one year after the WJCs began to operate, and so on. For $\tau \leq -1$, school and households 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 three years before implementation and zero to four years after implementation.[34] For each outcome, we expect that coefficients on dummies for years –3 and –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.

Figures 5, 6 and 7 plot 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 from estimating Equation 7 for each of the women's outcomes. Similarly, Figure 8 plots the coefficient of the interaction for the leads and lags of the introduction of a WJC from estimating Equation 8 for the education outcomes at the school level.

For women's outcomes (e.g., domestic violence, emotional violence, female deaths due to aggression, joint decision-making, labor supply, and complaints), 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. In particular, we find that women located in districts with a WJC present lower propensity to experience domestic violence since the year of the opening of the WJC. 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 females deaths due to aggression, effects are seen four years after the opening.

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 in-

[34] Of these seven indicator variables, note that $\tau = -3$ is a dummy that takes the value one for more than three years before the WJC was introduced. The next five 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.

creases 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 is also accompanied by a significant improvement in school performance and by a decrease in drop-out rates in the same year.

Similarly, in the year of the opening, primary school enrollment increases substantially, by 1.8%, for schools located within one kilometer of the WJC, after which this increment fluctuates around 2% over the subsequent three years. For standardized test scores, there is also no difference in pre-program trends between schools located near WJCs and those that are farther away. Indeed, the graphs show an absence of a strong pre-trend and evidence of a trend break after the WJC opened within one kilometer of or inside the district. For secondary school enrollment, we find that schools exposed to a WJC have a lower enrollment and attendance prior to the opening of the WJC. However, this decline is opposite to the direction we observe after the WJC introduction and is not statistically significant.

Overall, for schools and households located in districts 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 domestic 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, their trust in the institutional system, and also 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 the WJCs, on the prevalence of gender-based violence and education outcomes.

We deal with the potential endogeneity in the WJC placement by exploiting the variation generated by the rollout of the women's justice centers in Peru. Basically, in order 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 the WJCs and also 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 their 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. 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, to pass a grade and less likely to drop out of school. We also show that introducing a WJC within one kilometer of a school 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. Most of these effects are localized within a few kilometers and they are mostly driven by urban areas.

From a public policy standpoint, our analysis implies that providing better representation 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 program.

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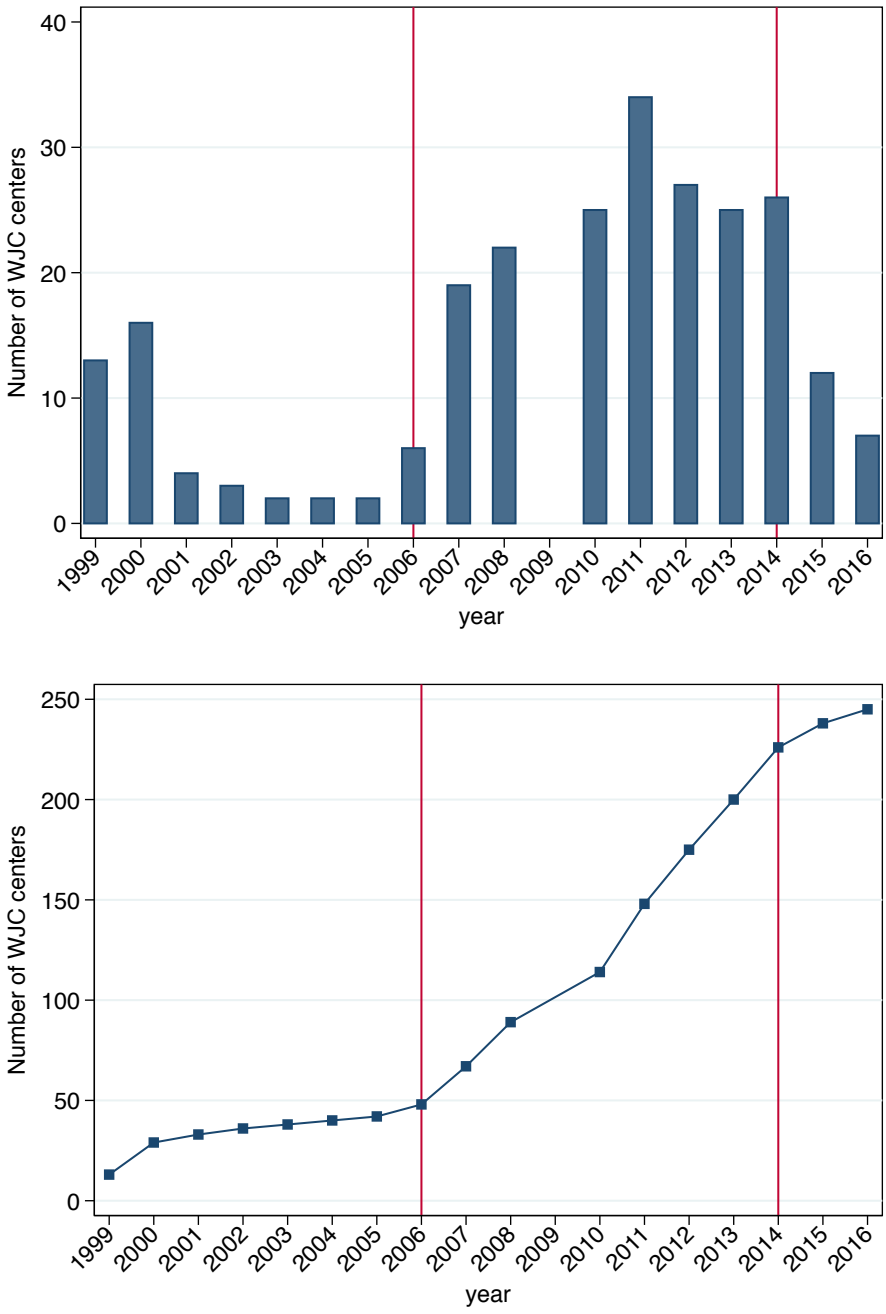
Figure 1: Distribution and Growth of the Openning of the *women's justice centers* (WJCs) by Year - Peru (1999-2016)




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

Figure 2: Rollout of the 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

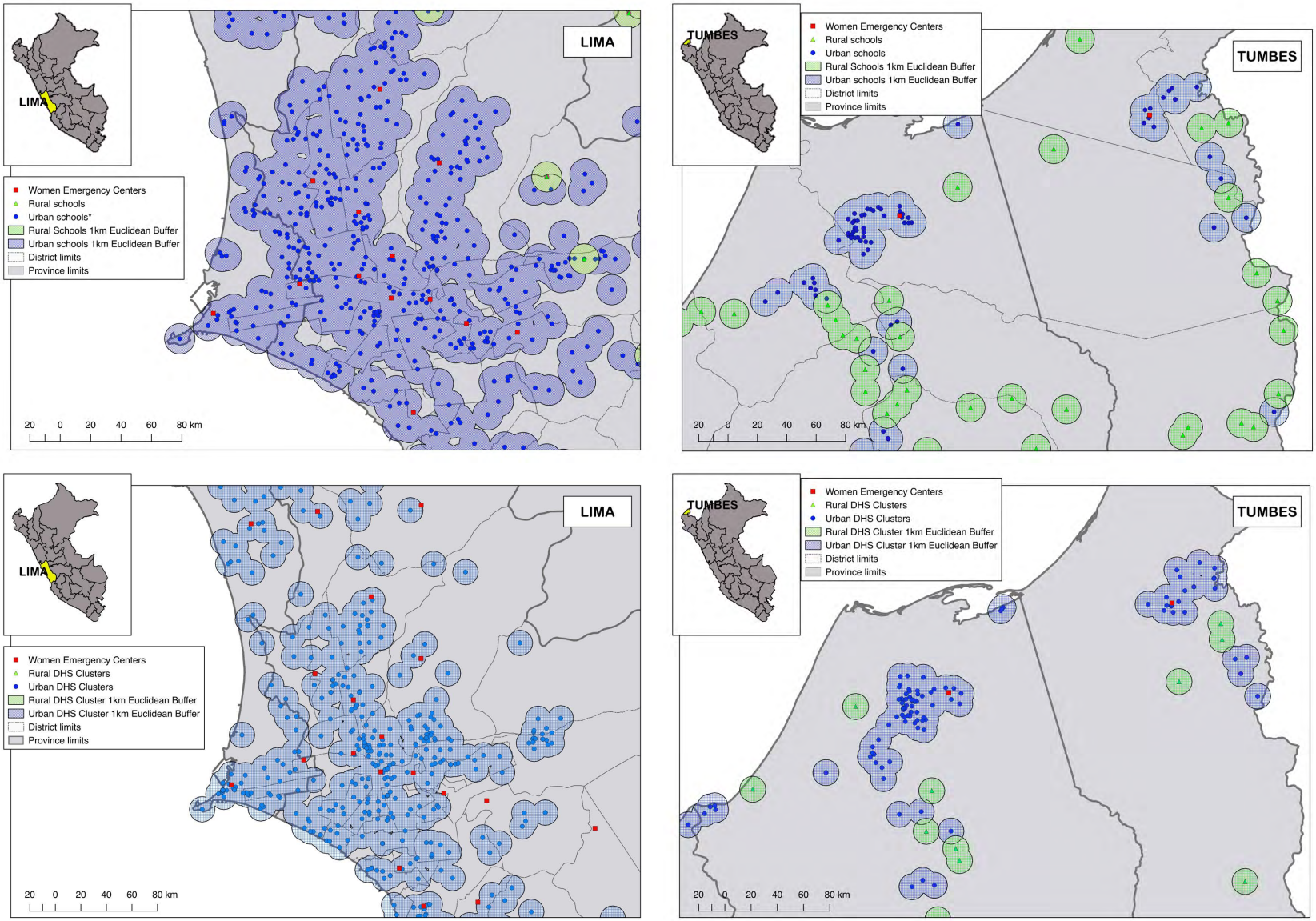


Table 1: Placement of WJC Centers in the District

| 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) |
| △ Primary Enrollment, (1998-2005) | | | 0.0001 | | 0.0003 | 0.0002 |
| | | | (0.0003) | | (0.0004) | (0.0003) |
| △ Secondary Enrollment, (1998-2005) | | | | -0.0001 | -0.0002 | -0.0001 |
| | | | | (0.0001) | (0.0002) | (0.0001) |
| Domestic Violence, 2000 | | | | | 0.1065 | |
| | | | | | (0.0839) | |
| CCT *Juntos* in the district | | -0.0605** | | | | -0.0451* |
| | | (0.0242) | | | | (0.0249) |
| # Households with CCT *Juntos*, 2014 | | 0.0000 | | | | 0.0000 |
| | | (0.0000) | | | | (0.0000) |
| Observations | 1,843 | 1,838 | 1,843 | 1,843 | 700 | 1,838 |
| R-squared | 0.3671 | 0.3708 | 0.1635 | 0.1638 | 0.1555 | 0.1670 |
| Department FE | YES | YES | YES | YES | YES | YES |

*Notes:* *** p<0.01, ** p<0.05, * p<0.1. This table shows the effects of district characteristics on WJC center's placement. The left hand side variable in Columns 1 and 2 is the 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 Domestic Violence (2006-2014)

| Dep. variable | **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 centers in the 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 centers in the 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.

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

| Dep. variables | **Log(# Complaints)** | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Gender violence | Gender violence | Family | Economic | Finance | Public | Property | Drugs |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| WJC in the district | 0.416*** | 0.171* | 0.046 | 0.004 | -0.026 | 0.037 | 0.040 | 0.026 |
| | (0.072) | (0.089) | (0.062) | (0.020) | (0.035) | (0.060) | (0.075) | (0.029) |
| $FemaleOfficer_{d,t}$ | | 0.369*** | | | | | | |
| | | (0.099) | | | | | | |
| $WJC_{d,t} \times FemaleOfficer_{d,t}$ | | 1.461*** | | | | | | |
| | | (0.287) | | | | | | |
| Observations | 12,823 | 10,984 | 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 | 1,832 |
| 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. 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.

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 | Repeated grade | Dropped out | Left school +2 years ago | Passed grade | Repeated grade | Dropped out | Left school +2 years ago |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Sample A: All Children* | | | | | | | | |
| WJC within 1km | 0.020** | -0.004 | -0.018** | 0.001 | 0.020* | -0.000 | -0.017* | -0.002 |
| | (0.010) | (0.005) | (0.009) | (0.001) | (0.013) | (0.005) | (0.012) | (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*** | -0.006 | -0.019*** | 0.001 | 0.030** | -0.007 | -0.018 | -0.003 |
| | (0.008) | (0.005) | (0.007) | (0.001) | (0.013) | (0.005) | (0.012) | (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 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 |

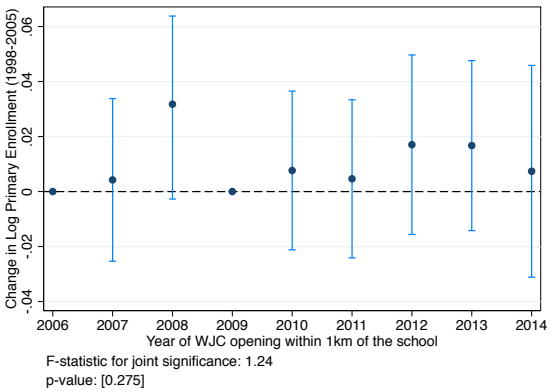
*Notes:* *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the average of the standardized reading and math test scores for 2nd grade of primary school. The independent variables measures the number of WJC centers within a 1km Euclidean buffer from the school and presence of WJC center in school's 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 9: Mechanisms: Impact of WJC centers on Decision Making and Bargaining Power in the Household (2006-2014)

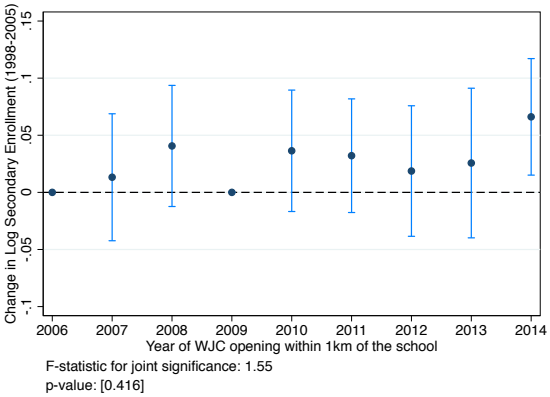
| Dep. variable | **Joint decision-making** | | |
|---|---|---|---|
| | score (0-6) | score (0-1) | dummy(0/1) |
| | (1) | (2) | (3) |
| *Sample: Married or cohabiting women 15-49 years old* | | | |
| WJC within 1km | 0.040 | 0.007 | 0.017* |
| | (0.047) | (0.008) | (0.009) |
| Observations | 72,009 | 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.

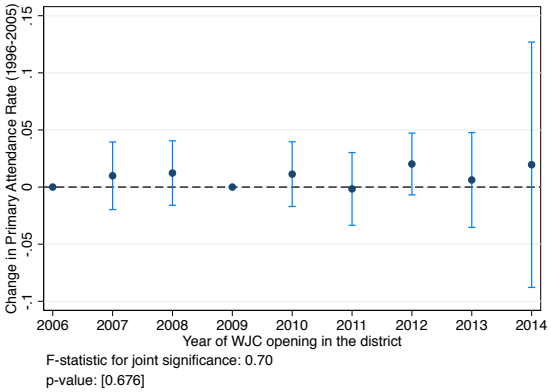
Figure 4: Effect of WJC center rollout on changes in pre-program outcomes



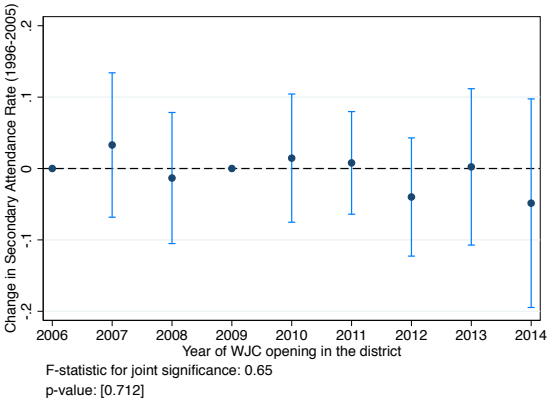

a. Primary School Enrollment



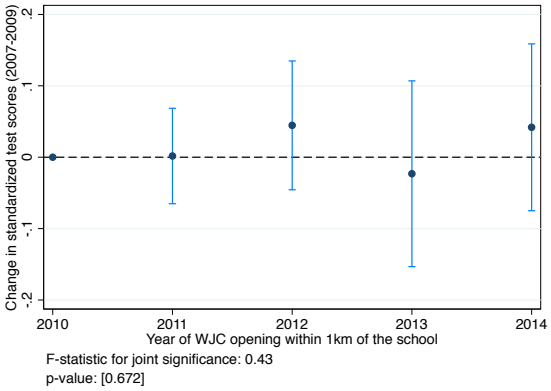

b. Secondary School Enrollment



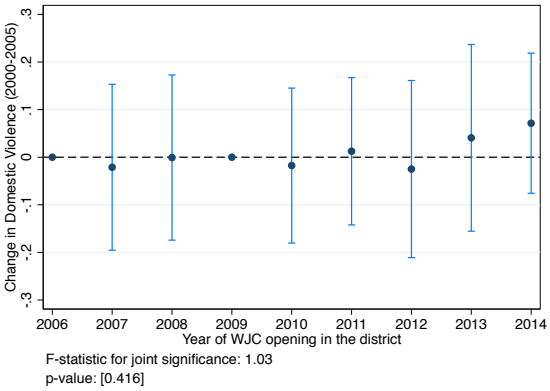

c. Primary School Attendance Rate




d. Secondary School Attendance Rate



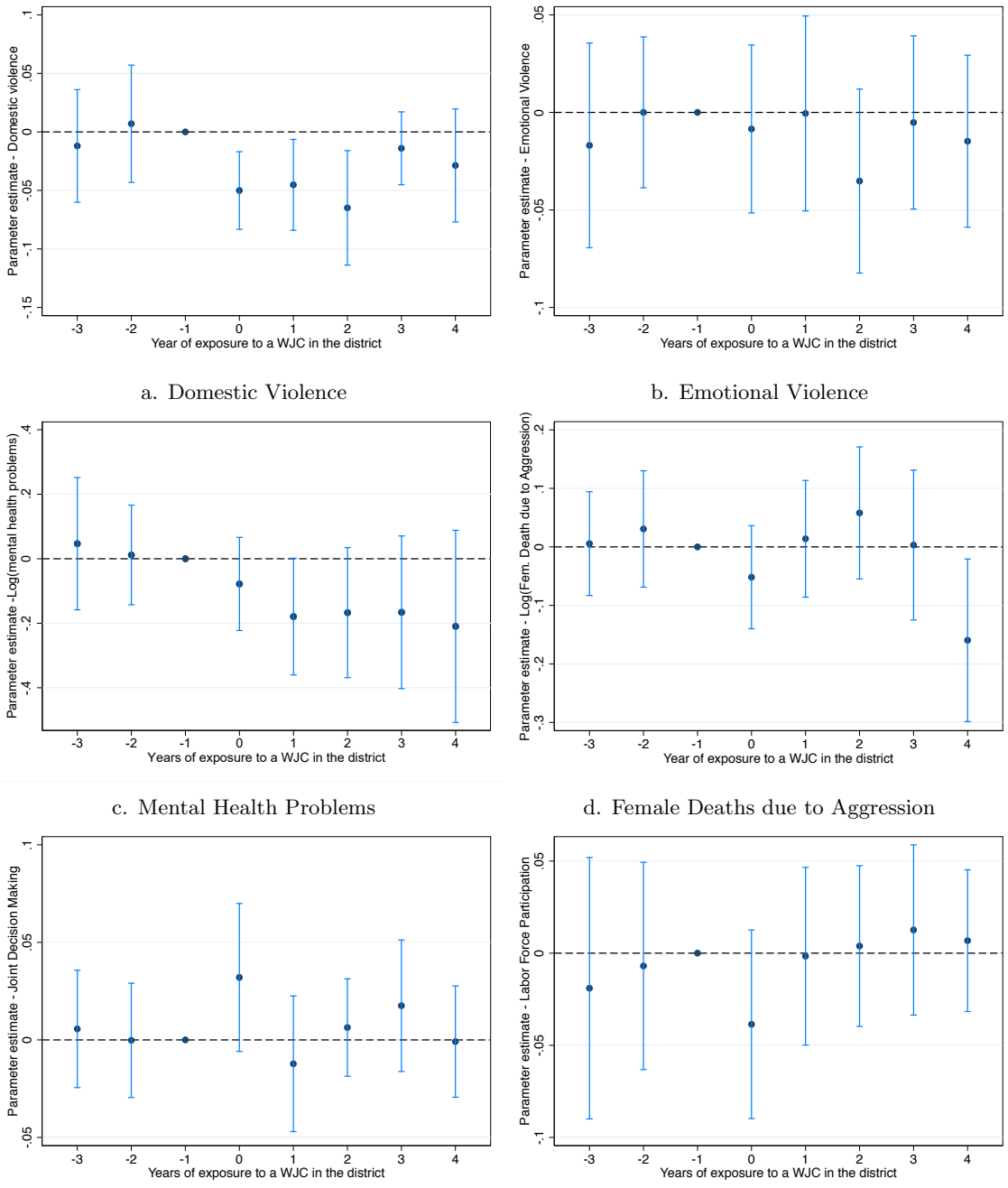

e. Test Scores (2nd Grade)




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's Outcomes




a. Domestic Violence

b. Emotional Violence

c. Mental Health Problems

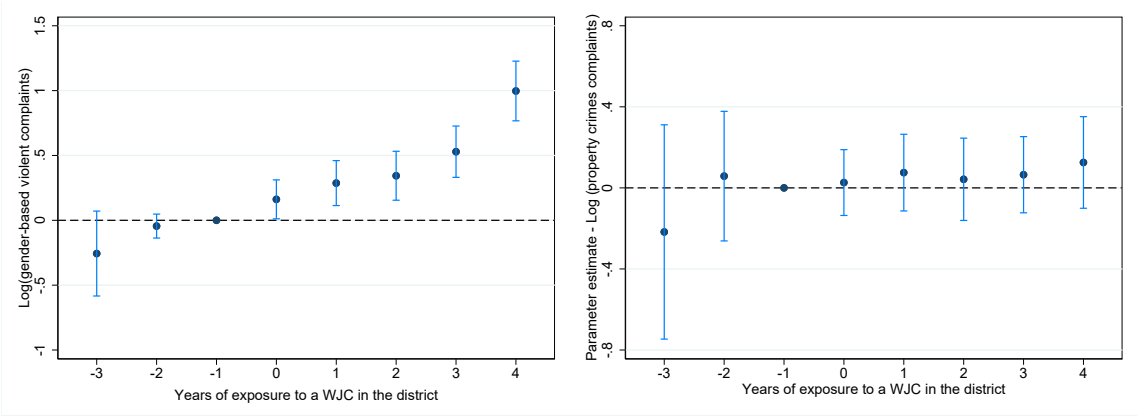
d. Female Deaths due to Aggression

e. Joint Decision Making

f. Labor Supply

*Notes:* These graphs plot the coefficient obtained from a regression of the outcomes on the interation between presence of WJC within 1km/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.

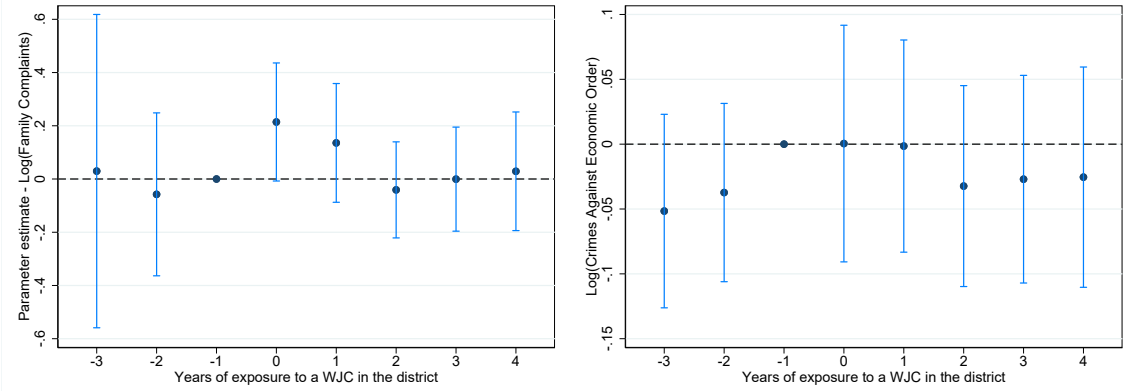
Figure 6: Event Study: Pre-WJC and Post-WJC Trends in Complaints




a. Gender-Specific Crimes




b. Property Crimes



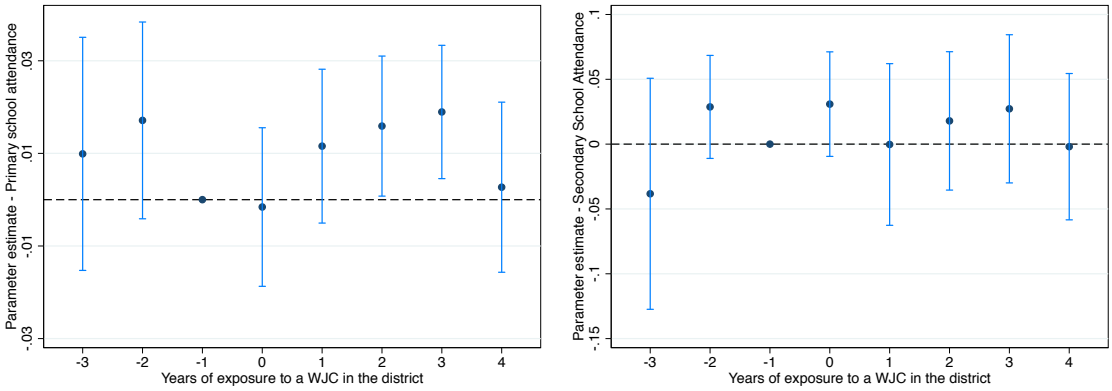

c. Crimes Against Family




d. Crimes Against Economic Order

*Notes:* These graphs plot the coefficient obtained from a regression of the outcomes on the interation between presence of WJC within 1km/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.

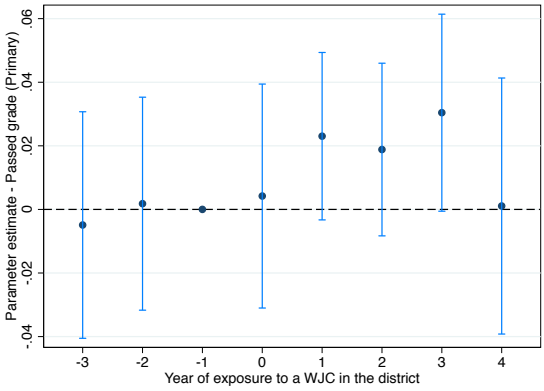
Figure 7: Event Study: Pre-WJC and Post-WJC Trends in Children's School Attendance



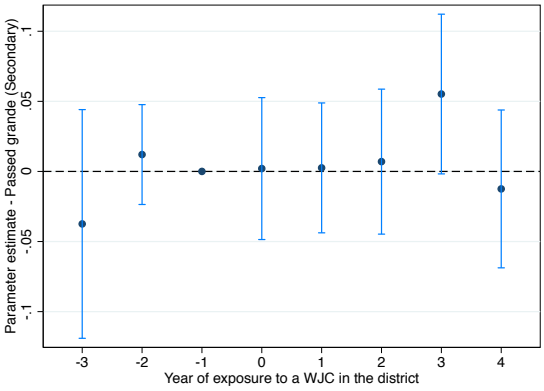

a. School Attendance (Primary Level)



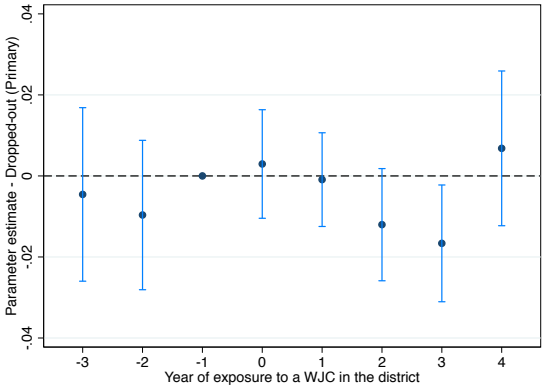

b. School Attendance (Secondary Level)



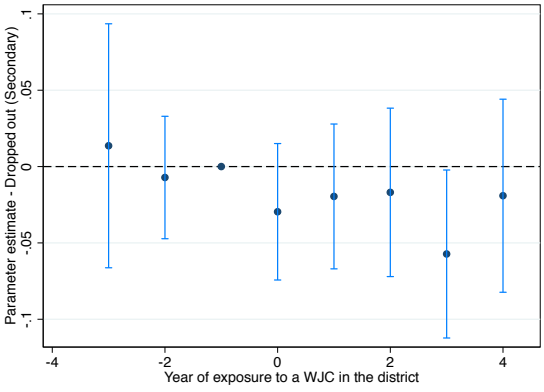

c. Passed Grade (Primary Level)




d. Passed Grade (Secondary Level)



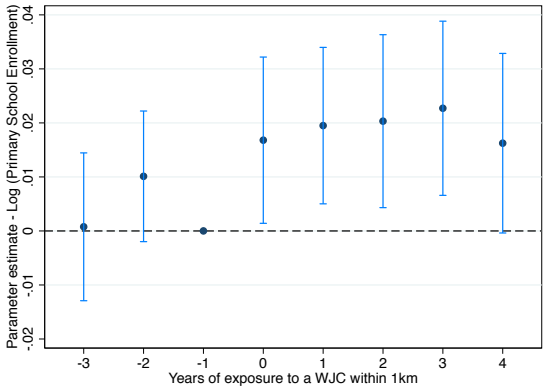

e. Dropped Out (Primary Level)



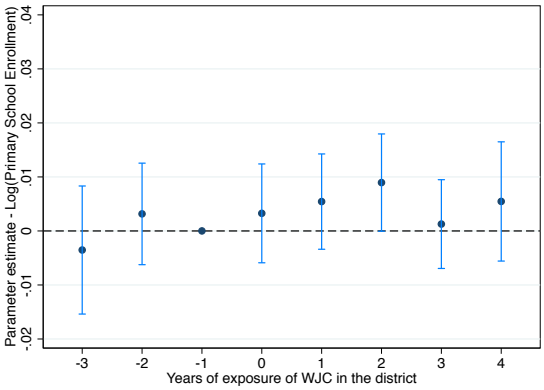

f. Dropped Out (Secondary Level)

*Notes:* These graphs plot the coefficient obtained from a regression of the outcomes on the interation between presence of WJC within 1km/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.

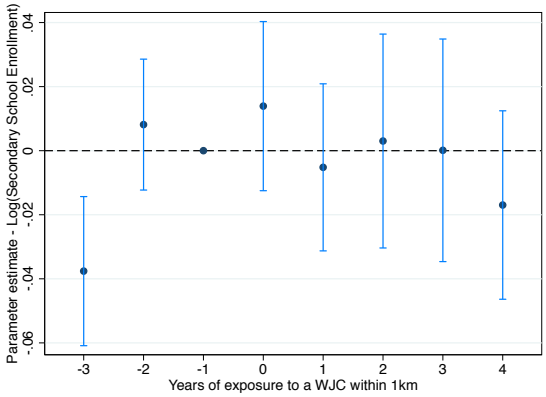
Figure 8: Event Study: Pre-WJC and Post-WJC Trends in School Enrollment and Test Scores



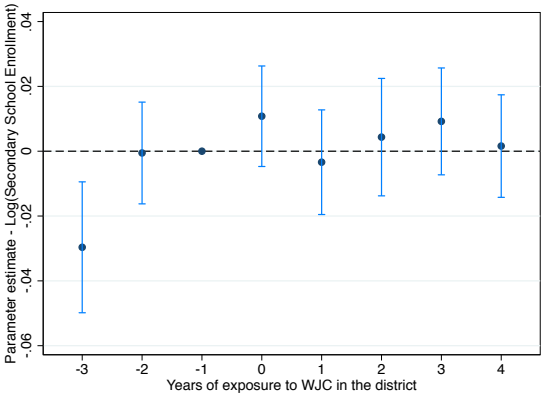

a. Primary School Enrollment - WJC <1km



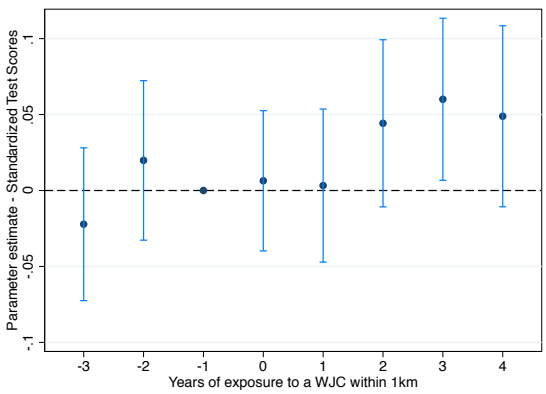

b. Secondary School Enrollment - WJC in the district



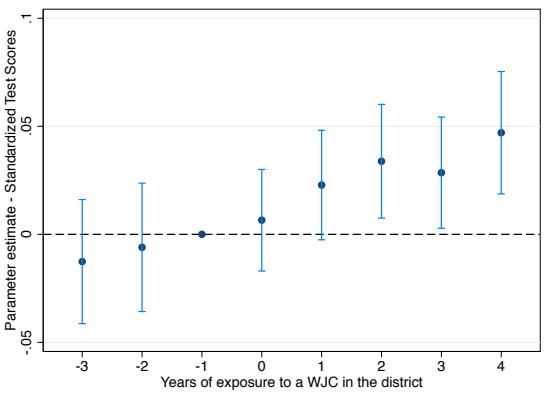

c. Secondary School Enrolment - WJC <1km




d. Secondary School Enrollment - WJC in the district




e. Test Scores (2nd Grade) - WJC <1km




f. Test Scores (2nd Grade) - WJC in the district

*Notes:* These graphs plot the coefficient obtained from a regression of the outcomes on the interation between presence of WJC within 1km/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

Figure A-1: Total Number of Persons Attended in WJC Centers by Year (2002-2016)

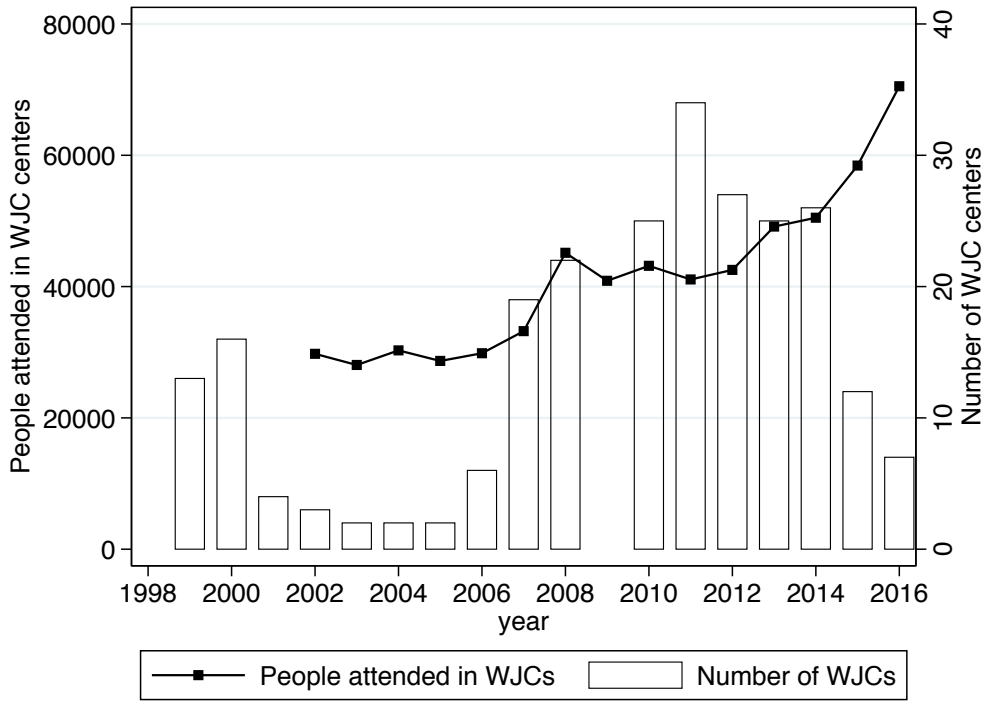
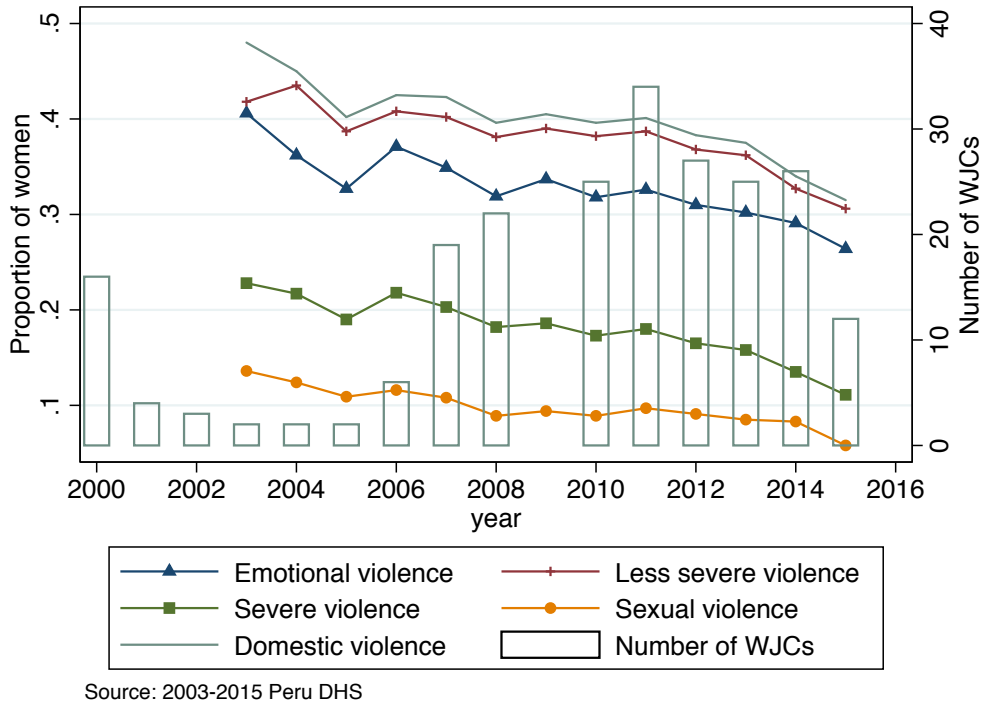


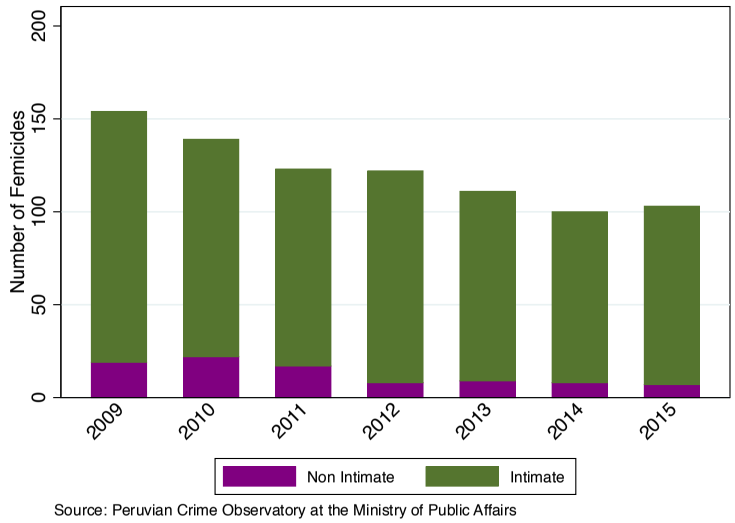

Figure A-2: Domestic Violence in Peru (2003-2015)

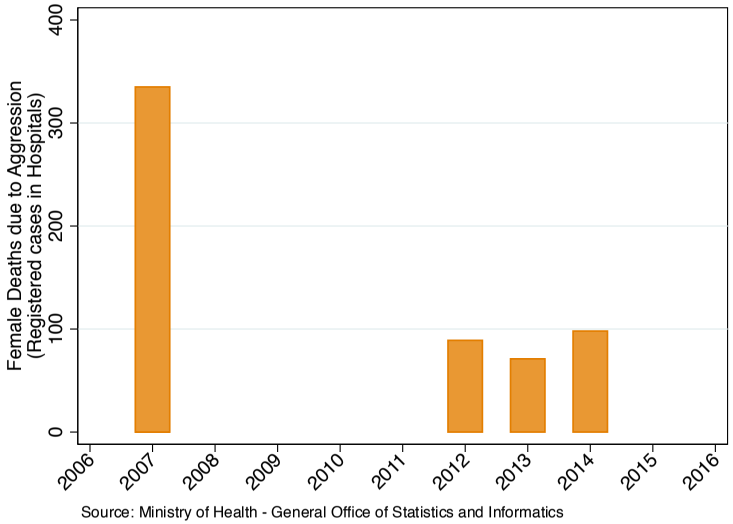


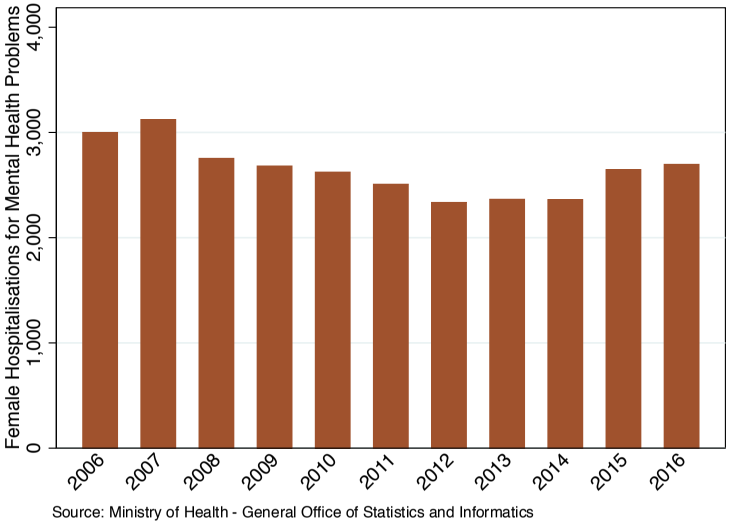

Source: 2003-2015 Peru DHS

*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 Dealths 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

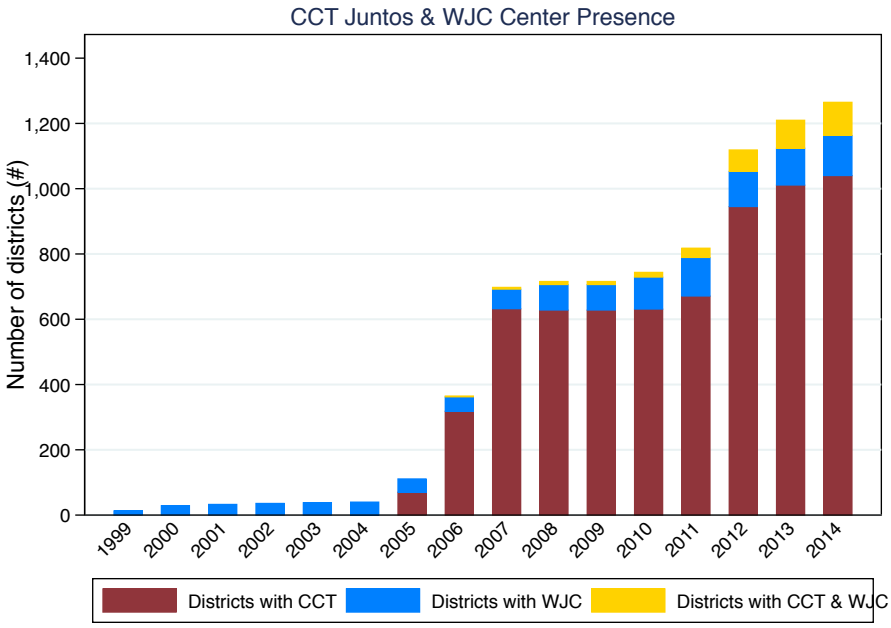



Figure A-5: WJC center and CCT *Juntos* entry in the district

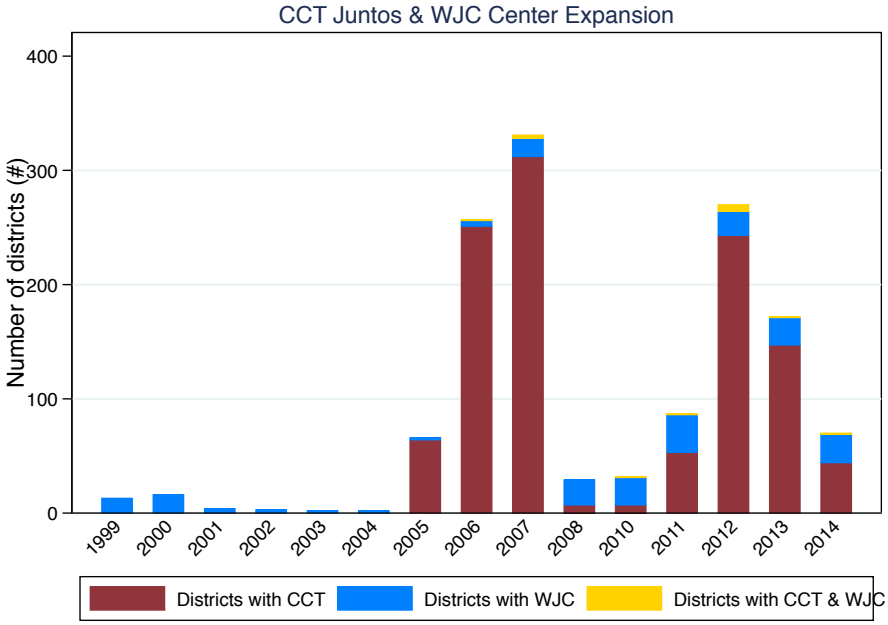


Table A-1: Women's Descriptive Statistics and WJC Center Exposure - DHS (2006-2014)

| | **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** | | | | | |
|---|---|---|---|---|---|---|
| | Obs. | Mean | WJC within 1km | Obs. | Mean | WJC in the 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: 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 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.** | | |
| | Obs. | Mean | WJC within 1km | Obs. | Mean | WJC within 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 | 23,573 | 0.002 | -0.0009 | 12,808 | 0.098 | -0.006 |
| +2 year ago | | | (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 | 24,543 | 0.002 | 0.001 | 17,358 | 0.074 | 0.009 |
| +2 year ago | | | (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: 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 subject to violence by grandfather* | | | | | |
| 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 not subject to violence by grandfather* | | | | | |
| 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, 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: Placebo regressions, Impact of Future WJC Centers on Primary School Enrollment

| Dep. variable | **Log (Primary School Enrollment)** | | | |
|---|---|---|---|---|
| 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, $t$ | 0.024*** | 0.023*** | 0.027*** | 0.019** |
| | (0.008) | (0.007) | (0.008) | (0.009) |
| WJC within 1km, $t+1$ | 0.004 | 0.005 | 0.003 | 0.006 |
| | (0.006) | (0.006) | (0.007) | (0.007) |
| WJC within 1km, $t+2$ | 0.002 | -0.002 | -0.003 | 0.002 |
| | (0.006) | (0.006) | (0.007) | (0.008) |
| WJC within 1km, $t+3$ | 0.004 | 0.011 | 0.004 | -0.002 |
| | (0.008) | (0.008) | (0.009) | (0.010) |
| Observations | 315,221 | 315,221 | 119,232 | 103,518 |
| Number of schools | 36,947 | 36,947 | 14,405 | 12,398 |
| P-value joint test | 0.987 | 0.493 | 0.831 | 0.767 |
| Mean dep. var | 95.9 | 95.9 | 177.8 | 127.7 |
| *Panel B: WJC center in the district of the school* | | | | |
| WJC in the district, $t$ | 0.008* | 0.000 | 0.017*** | 0.029*** |
| | (0.004) | (0.004) | (0.006) | (0.008) |
| WJC in the district, $t+1$ | 0.002 | -0.000 | 0.006 | -0.002 |
| | (0.004) | (0.004) | (0.005) | (0.006) |
| WJC in the district, $t+2$ | 0.003 | -0.001 | 0.001 | 0.015** |
| | (0.004) | (0.004) | (0.005) | (0.006) |
| WJC in the district, $t+3$ | -0.007 | -0.009** | -0.004 | -0.012 |
| | (0.005) | (0.004) | (0.006) | (0.008) |
| Observations | 315,407 | 315,407 | 119,270 | 103,586 |
| Number of schools | 36,994 | 36,994 | 14,412 | 12,412 |
| P-value joint test | 0.200 | 0.148 | 0.408 | 0.071 |
| Mean dep. var | 95.9 | 95.9 | 177.8 | 127.7 |
| 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. Robust standard errors in parentheses. The dependent variable is the logarithm of enrollment plus one. The independent variable measures the presence of a WJC center within 1km/in the district in year $t$ and controls for openings of future WJC centers in year $t+1$, $t+2$ and $t+3$. 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-17: The Effect of WJC Centers on Child Labor (2006-2014)

| Dep. variable | **Child Labor** | | | |
|---|---|---|---|---|
| Sample | All children 6-14 y.o | All children 6-14 y.o | Female | Male |
| | (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-18: Placebo regressions, Impact of Future WJC centers on Secondary School Enrollment

| Dep. variable | **Log (Secondary School Enrollment)** | | | |
|---|---|---|---|---|
| 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, $t$ | 0.033*** | 0.023* | 0.039*** | 0.006 |
| | (0.012) | (0.013) | (0.013) | (0.019) |
| WJC within 1km, $t+1$ | -0.017 | -0.017 | -0.020 | -0.032* |
| | (0.013) | (0.013) | (0.014) | (0.018) |
| WJC within 1km, $t+2$ | 0.010 | 0.008 | 0.004 | 0.008 |
| | (0.013) | (0.014) | (0.014) | (0.020) |
| WJC within 1km, $t+3$ | 0.014 | 0.011 | 0.023 | 0.013 |
| | (0.014) | (0.014) | (0.015) | (0.020) |
| Observations | 102,685 | 102,685 | 69,686 | 41,277 |
| Number of schools | 12,809 | 12,809 | 8,516 | 5,170 |
| P-value joint test | 0.162 | 0.215 | 0.073 | 0.163 |
| Mean dep. var | 174.8 | 174.8 | 215.3 | 195.3 |
| *Panel B: WJC center in the district of the school* | | | | |
| WJC in the district, $t$ | 0.026*** | 0.002 | 0.032*** | 0.015 |
| | (0.007) | (0.008) | (0.008) | (0.012) |
| WJC in the district, $t+1$ | -0.013* | -0.018** | -0.008 | -0.014 |
| | (0.007) | (0.007) | (0.008) | (0.011) |
| WJC in the district, $t+2$ | 0.008 | 0.002 | 0.003 | 0.009 |
| | (0.008) | (0.008) | (0.009) | (0.013) |
| WJC in the district, $t+3$ | 0.010 | -0.002 | 0.010 | 0.009 |
| | (0.009) | (0.008) | (0.011) | (0.015) |
| Observations | 102,691 | 102,691 | 69,692 | 41,277 |
| Number of schools | 12,811 | 12,811 | 8,518 | 5,170 |
| P-value joint test | 0.047 | 0.119 | 0.314 | 0.288 |
| Mean dep. var | 174.8 | 174.8 | 215.3 | 195.3 |
| 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. Robust standard errors in parentheses. The dependent variable is the logarithm of enrollment plus one. The independent variable measures the presence of a WJC center within 1km/in the district in year $t$ and controls for openings of future WJC centers in year $t+1$, $t+2$ and $t+3$. 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-19: Impact of WJC centers on Women's Labor Force Participation (2006-2014)

| Dep. variables | Currently working | Works for family | Works for someone else | Self-employed |
|---|---|---|---|---|
| | (1) | (2) | (3) | (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-20: Relationship between WJCs within 1km rollout and pre-program school enrollment

| | Schools matched to WJC within 1km, Pre-WJC period | | | | | |
|---|---|---|---|---|---|---|
| | **△ Log(Primary School Enrollment)** | | | **△ Log(Secondary School Enrollment)** | | |
| | (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-21: 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-22: 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-23: Relationship between WJCs in the district and four windows of pre-program domestic violence

| | Districts matched to WJC locations, Pre-WJC period | | | |
|---|---|---|---|---|
| | **△ Domestic violence in last 12 months** | | | |
| | (1) | (2) | (3) | (4) |
| | △2000-2005 | △2000-2008 | △2000-2010 | △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.