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## Specialized Courts and the Reporting of Intimate Partner Violence: Evidence from Spain

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DECEMBER 2021

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## ABSTRACT

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# Specialized Courts and the Reporting of Intimate Partner Violence: Evidence from Spain\*

This paper assesses the effect of the creation of specialized intimate partner violence (IPV) courts on the reporting and incidence of these crimes. To achieve this goal, we exploit the sequential roll-out of specialized IPV courts throughout Spain by applying a difference-in-differences strategy over a sample of treatment and matched control districts. We find that the opening of a specialized IPV court decreases time to disposition of IPV cases by 61% and increases the reporting of IPV in the judicial district by approximately 22%. However, we do not see any effect of the courts on the incidence of IPV homicides, neither on the reported number of aggravated IPV offenses, for which misreporting is arguably smaller. The latter results suggest that despite the positive effects on the reporting of less severe IPV offenses, the creation of these courts failed to reduce the incidence of the most severe IPV offenses within the time period studied.

**JEL Classification:** J12, J16, K14, K38

**Keywords:** intimate partner violence, reporting of intimate partner violence, specialized intimate-partner violence courts

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# I Introduction

The World Health Organization estimates that one out of three women worldwide have experienced intimate partner violence (IPV) at some point during their life (WHO, 2013)<sup>1</sup>, which has harmful and lasting consequences not only for the welfare of IPV victims but also for their families and society (Aizer, 2011; Devries et al., 2013; Carrasco and Alonso-Borrego, 2019; OECD, 2013).<sup>2</sup> Often highlighted as one of the primary citizen concerns in surveys, reducing the prevalence of IPV is indeed at the top of the policy agenda of many governments and international organizations (Ramsay et al., 2005). However, the effectiveness of policy efforts is often challenged by the fact that an important share of these crimes remain unreported, which hinders the enforcement of legal sanctions and the implementation of some social policies that target IPV offenders or victims (Miller and Segal, 2018; Carrell and Hoekstra, 2012). For example, despite various campaigns and protection measures for victims that have been developed in the last few years, a recent report from the Spanish Ministry for Equality (2019) reveals that only one out of five IPV episodes in Spain are reported to the police. The limited reporting of these crimes in Spain is not an exception and surveys conducted in multiple settings reveal that severe under-reporting of IPV is a stylized fact in both developed and developing countries (European Union Agency for Fundamental Rights, 2015; Palermo et al., 2014; Joseph et al., 2017; Hadi, 2018). Well-aware of the importance of raising the report of IPV in the fight against this form of violence, governments and international organizations have implemented various policies aimed at promoting and facilitating the reporting of IPV, including information campaigns, specialized help-lines, women-only police stations, specialized units within the police and the creation of IPV specialized courts (WHO, 2005; OECD, 2013).

This paper examines the effectiveness of one of these policies to increase the reporting of IPV and protect victims, namely the creation of IPV courts. These are courts that are endowed with specific resources and specialized staff to provide a less hazardous experience for the victim and faster and timely judicial decisions, and where judges only deal with IPV cases. In the last few decades, IPV and domestic violence courts<sup>3</sup> have been created in multiple settings, including Aus-

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<sup>1</sup>The prevalence among partnered women varies across countries. Data from the World Health Organization shows that 30% of women among 15-69 years old worldwide suffered IPV, but this percentage ranges from 23% in high-income countries to 37.8% in low and middle-income countries of South-East Asia and East-Mediterranean countries.

<sup>2</sup>WHO defines IPV as the physical, psychological, or sexual harm that is committed by a current or former partner. While IPV also exists in same sex marriages or from women to men, most of them are committed by men to former or current female partners. The Spanish IPV courts only cover IPV that is committed by men against a current or former female partner.

<sup>3</sup>Domestic violence courts not only deal with IPV offenses but also with other forms of violence that occur within the

tralia, Canada, UK, USA and Spain, among other countries. However, there is a lack of rigorous evidence on the broader effects of these courts. We address this gap in the literature by documenting the causal effects of the creation of specialized IPV courts on the reporting of IPV in Spain. We also provide plausible causal evidence about how the creation of these courts affects the incidence of the most severe forms of IPV.

But why should IPV courts affect the reporting of IPV? Specialized courts are designed to speed up the judicial process and receive specific resources aimed at facilitating and making the judicial process less hazardous for IPV victims, and addressing the specific needs that they may have due to the psychological aspects of this type of violence. Commonly mentioned in surveys and qualitative studies by victims as an important barrier for reporting IPV to the police ([Broidy et al., 2016](#); [Silván et al., 2015](#)), the opening of a specialized court may improve the experience in the court for IPV victims. This may lead to a decrease in the share of these crimes that remain unreported and to an increase in the number of cases being judged. One assumption underlaying the potential effect of the creation of these courts on reporting is that victims of IPV have updated information about the duration and degree of arduousness of the judicial process. While we cannot test this hypothesis empirically, existing evidence from Spain suggests that, approximately, 77% of IPV victims have shared their experience with close friends or family ([Spanish Ministry for Equality, 2019](#)). A non-negligible share of victims also consulted with lawyers (24%), social services (15%) and other support organizations (10%) ([European Union Agency for Fundamental Rights, 2015](#)) before reporting the offense. In this context, it does not seem unreasonable to think that an important share of victims of IPV could react to changes in the *cost* of reporting IPV.

But IPV courts may not only affect the reporting but also the incidence of IPV. The specialization of judges and court personnel, and the overall lower volume of work in these courts may lead to judges making better decisions and more timely protection measures ([Golestani et al., 2021](#)). Given that this form of violence has a cyclical nature and often starts with verbal abuse, and then escalates to the most severe forms over time ([Aizer and Dal Bó, 2009](#)), the increase in the reporting of less severe IPV offenses caused by the creation of IPV courts may lead to an increase in the adoption of judiciary measures and in the issue of protection orders, which may help to stop the escalation of IPV through deterrence and incapacitation effects on potential offenders. Furthermore, IPV courts may also decrease the prevalence of IPV even if they do not increase reporting. If individuals perceive that these courts boost women's outside option, the improvement in women's

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household.

bargaining power within marriage could lead to a reduction in the incidence of IPV ([Farmer and Tiefenthaler, 1996](#); [Sanin, 2021](#); [Tur-Prats, 2019](#)).<sup>4</sup>

To assess the causal effect of the creation of IPV courts, we apply a difference-in-differences strategy over a sample of treatment and matched control districts that exploits the staggered roll-out of IPV courts throughout Spain. Our results show that the creation of IPV courts reduce the length of the judiciary process in these courts by 61%. Our analysis also reveals that the creation of IPV courts increase the reported number of IPV cases in the district by 22%. The fact that the proportion of IPV cases that are dismissed in court remains unchanged suggests that the increase in the number of IPV cases in court resulted from the creation of IPV courts is not driven purely by a rise in the number of false reports. The increase in the number of reported IPV cases is driven by an increase in the reported number of less severe IPV offenses. Meanwhile, we do not observe any significant change in IPV homicides neither in the reported number of IPV offenses that requested medical surgery or prolonged medical attention. Because strict police and hospital protocols make unreporting of these severe IPV offenses less frequent, we interpret the lack of a reduction in the reported number of these crimes as indicative that the IPV courts did not decrease the incidence of the most severe forms of IPV within the time period studied. Consistently, we do not find that the opening of these courts affected the number of protection orders, which are typically issued by judges to protect IPV victims and their families in risky situations.

This study contributes to various strands of the literature. First, we add to the body of evidence that assesses the impacts on judicial outcomes of specialized courts ([Coviello et al., 2014](#); [Golestani et al., 2021](#); [Miller and Curry, 2013, 2009](#); [Hansford, 2011](#); [Kesan and Ball, 2010](#); [Howard, 2005](#); [Palumbo et al., 2013](#); [Doménech and Mora-Sanguinetti, 2015](#); [Garoupa et al., 2009](#)) and, more specifically, the effects of specialized IPV courts. Two reviews of the literature on the effects of IPV and domestic violence courts are provided in [Moore \(2009\)](#) and [Gutierrez et al. \(2017\)](#). These reviews show mixed results, reflecting the existing controversy on the efficacy of specialized courts ([Palumbo et al., 2013](#)). Using data from the US, [Gover et al. \(2003\)](#) and [Angene \(2000\)](#) show that the creation of domestic violence courts is followed by increases in arrests and by improvements in different measures of judicial efficacy. Less clear is the evidence on whether or not specialized judges use jail sentences and protection orders differently, with mixed results from different studies ([Cissner et al., 2013, 2015](#)). Recidivism outcomes are also not statistically different in specialized

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<sup>4</sup>The literature is however not homogeneous and other studies found that improvements in bargaining power among women lead to larger levels of IPV ([Erten and Keskin, 2021, 2018](#)).

and non-specialized courts in most studies ([Gutierrez et al., 2017](#)). The evidence analyzed in these reviews should nonetheless be interpreted with caution because it is typically based in pre-post designs that monitor the outcomes of interest before and after the creation of specialized domestic violence or IPV courts. An exception to this literature is the work by [Golestani et al. \(2021\)](#). Using data from two counties in Tennessee and random variation in the assignment of misdemeanor domestic violence cases where defendants did not post-bond to either domestic violence courts or ordinary courts, this study shows strong evidence that specialized courts are less likely to convict individuals but, conditional on convicting, they are more likely to assign incarceration sentences. However, the specific features of the judicial system in the counties examined in the latter study impede the assessment of the effect of these specialized courts on the reporting of IPV. We contribute to this literature though providing the first rigorous empirical analysis on the effect that the creation of specialized IPV courts has on the reporting of this form of violence and on whether these courts can lead to a reduction in the incidence of the most severe forms of IPV.

Second, our study contributes to the literature that explores the effectiveness of policies and strategies that aim to increase the low levels of reporting of IPV ([Iyer et al., 2012](#); [Amaral et al., 2019](#); [Sviatschi and Trako, 2021](#); [Miller and Segal, 2018](#); [Iyengar, 2009](#); [Chin and Cunningham, 2019](#)), which is a crucial challenge in the fight against IPV. This study documents that specialized IPV courts can be effective instruments to raise the reporting of IPV, providing insights for the design of effective policies that target this goal.

Finally, our study contributes to the scarce literature that investigates the link between the reporting of IPV, and the prevalence of these offenses and other outcomes ([Iyengar, 2009](#); [Miller and Segal, 2018](#); [Carrell and Hoekstra, 2012](#)). These studies show that interventions that have an impact on the reporting of IPV (e.g. mandatory arrest laws or female police officers) lead to changes in the number of IPV homicides, documenting beneficial effect of higher levels of IPV reporting on the reduction of IPV homicides. In a related study, [Carrell and Hoekstra \(2012\)](#) show that the reporting of domestic violence has beneficial spillover effects. Specifically, the latter paper shows that the reporting of domestic violence contribute to minimize its negative effects on the schooling outcomes of the peers of maltreated children. Unlike [Iyengar \(2009\)](#) and [Miller and Segal \(2018\)](#), we do not see that the increase in the reporting of IPV resulted from the creation of the courts lead to any reduction on homicides. Rather, our results suggest that the link between the reporting of IPV and IPV homicides is not mechanical and interventions that increase the reporting of softer forms of IPV do not necessarily lead to a reduction in the most severe forms of IPV, at



least in the short-term. We discuss various hypotheses that can explain this result in section [VI](#).

The rest of this paper is structured as follows. The next section discusses contextual factors related with IPV in Spain and the creation of the specialized IPV courts. Section [III](#) describes the data. Then, Section [IV](#) presents the identification strategy and Section [V](#) the main empirical results. Section [VI](#) concludes.

## **II Institutional framework**

### **II.a Gender-based violence in Spain**

Recent data reveal that nearly 3 million women (14.2% of women aged 16 or older) have suffered physical or sexual violence in Spain ([Spanish Ministry for Equality, 2019](#)) and 55 of them were murdered by their partner or former partner just in 2019. With the number of intra-partner homicides in Spain slightly increasing over the last 15 years, IPV is currently perceived by the Spanish population as one of the country's main concerns ([CIS, 2019](#)).<sup>5</sup> However, despite the implementation of several campaigns and policies to raise awareness and promote the reporting of these crimes, the share of IPV offenses in Spain that are reported to the police still remain low. A recent survey conducted by the [Spanish Ministry for Equality \(2019\)](#) shows that only 21.7% of them are either reported to the police, who have protocols to initiate the judicial action, or have been claimed directly in courts.<sup>6</sup>

As in many other countries, the fight for gender equality and against IPV is currently at the top of the policy agenda of the Spanish government ([WHO, 2013](#); [Spanish Government, 2019](#)). Since the mid-2000s, Spain has been implementing integrated programs to fight IPV, including, among others, awareness campaigns, the creation of a Ministry for Equality, and the creation of specialized IPV courts. Yet, rigorous empirical evidence evaluating the effectiveness of these interventions is nonexistent, to the best of our knowledge.

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<sup>5</sup>In the Spanish Barometer of November 2019, violence against women was ranked as the 12th problem of concern and a 5.7% of the people mentioned it as one of the three main problems, increasing this importance from previous years ([CIS, 2019](#)).

<sup>6</sup>The proportion of cases reported to either the police or directly to the court varies depending on whether the aggressor is the current or the former partner of the victim. It is estimated that only 5.4% of the IPV cases where the aggressor is the current partner of the victim are reported, whereas the percentage of IPV cases reported is nearly five times larger (25%) when the aggressor is the former partner. The share of reported cases is slightly larger (32%) for IPV cases where the victim suffered physical or sexual violence. Young women aged between 16 and 24 years old are the demographic group with the lowest share of IPV crimes reported (14%) ([Government Delegation against Gender-Based Violence, 2021](#)).

## II.b The functioning of specialized IPV courts

In December 2004, the Spanish parliament passed the Law of Measures of Integral Protection Against Gender-Based Violence.<sup>7</sup> The flagship measure of the law was the creation of specialized courts on violence against women. The first specialized IPV courts were created in 2005 in 16 judicial districts, mainly big cities, and then expanded throughout the country until 2011. In total, there are specialized IPV courts in 76 out of the 431 Spanish judicial districts. A map with the place and time of opening of specialized IPV courts in Spain is provided in Figure 1. The allocation of judicial districts throughout the country was decided by the national government after consultation with the regional governments and the General Council of the Judiciary.<sup>8</sup>

There are three different paths to start an IPV trial. The first one is through a police report. If a victim reports an IPV offense to the police or the police are aware or suspect of an IPV case (e.g., they assist a victim, they receive a report from a hospital, etc.), then they have the obligation to report it to the court, which opens the judicial process. Secondly, victims can also report an IPV offense directly to the court rather than to the police. Finally, a third person who is aware of IPV cases can also report them directly to the court or to the police, thus opening the legal procedure. This may happen, for example, when doctors in hospitals or medical centers become aware of an IPV case, where protocols obligate them to report potential IPV cases to the court. The vast majority of the IPV cases are nonetheless initiated via victim's report to the police (i.e., 70% of claims in 2019), while the initiation via a direct victim report in the court or through a third person report is typically less frequent.<sup>9</sup> It is important to note that every report of IPV regardless of whether it is made to the police or directly to the court and whether this is done by the victim or a third person, opens an IPV case in the court. The registration of the IPV case in the court is conducted immediately after the victim or a third person report it to the police or to the court.

The next step of the judicial process in an IPV case is the investigation phase. In those districts in which an IPV court exists, all IPV cases are automatically transferred to the specialized IPV court for the investigation phase of the trial. IPV courts are primarily investigation courts. The judges in these courts lead the investigations, decide on whether to dismiss the cases and also

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<sup>7</sup>The law is named in Spanish *Ley Orgánica 1/2004 de Medidas de Protección Integral contra la Violencia de Género*.

<sup>8</sup>The General Council of the Judiciary is an autonomous institution, mostly consisting of judges, that performs competences related to the government of the judiciary with the goal of guaranteeing their independence in the exercise of the judicial function

<sup>9</sup>In 2019, the percentage of IPV cases in which the victim directly report the offense in the court was only 1.9% and a third person (including the victim's relatives) was about a 3.6% of the total number of IPV cases ([Spanish Ministry for Equality, 2019](#)).

issue protection orders. If there is no IPV court in the district, the investigation phase of the IPV cases is conducted in ordinary investigation courts.<sup>10</sup> Once the investigation phase of the trial is concluded and if the case is not dismissed, the case is transferred from the IPV court or the ordinary investigation court to the relevant criminal court for the oral trial phase of the judicial procedure, which leads to either the acquittal or the conviction of the defendant. The oral trial phase is typically conducted in a different court than the investigation phase although there are two exceptions. Judges in IPV or ordinary investigation courts will also lead the oral trial phase and pass sentences for (a) minor IPV cases such as insults and for (b) IPV cases in which the offender recognized during the judicial investigation being the crime perpetrator.

Specialized IPV courts were designed with the explicit goal of speeding up the judicial process of IPV cases and providing resources to address the specific needs that victims may have due to the psychological aspects of this type of violence. These courts are typically equipped with special resources, such as a curtain to provide a partition when the victim testifies or the possibility to testify through online calls. In some of them, specialized psychologists and social workers are also available to support and inform the victim from the very early stages of the judicial procedure. While the judgment stage is typically executed in the criminal court in most IPV cases regardless of whether there is or not a specialized IPV court in the district, the investigation stage is a crucial stage in IPV judicial processes and involve gathering judicial evidence, issue protection orders and the potential dismissal of the case. It is also the most onerous for the victim because it starts immediately after the report and can involve several interactions. Indeed, while the average duration of an IPV case in an IPV court is approximately 50 days, the length of an IPV case in an ordinary investigation court is approximately 110 days. The victims in IPV courts receive a direct audience in a very short time. In addition, while judges in ordinary investigation courts deal with a wider set of criminal offenses, judges in IPV courts only deal with IPV cases, which allows judge specialization and reduces the workload in these courts.

The Law of Measures of Integral Protection Against Gender-Based Violence also introduced a national plan of awareness and prevention of gender-based violence and changed the Criminal Code redefining IPV offenses. The law introduced schedule flexibility in labor conditions, emergency social services support, special health treatment and protocols, and free legal support and counseling service to access information for victims that report IPV offenses. These additional dispositions were introduced at the same time throughout the country, regardless of whether or not

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<sup>10</sup>These courts are called in Spain *Juzgados de Instrucción*.

a specialized IPV court exists in the district. Therefore, the introduction of these additional dispositions are arguably absorbed by the vector of year fixed effects in our specification and would only threaten our identification strategy if they differently affect treatment and matched control districts, and operate only after IPV courts were created in the treatment districts. We believe that this is unlikely in our setting because these additional dispositions of the law are deployed nationwide and their deployment started immediately, while the creation of IPV courts was localized and staggered over the territory. The fact that the effect of the IPV court coincides with the year in which the IPV court is created in the district, which varies throughout the country, reassures our confidence in the attribution of the observed effects to the creation of the IPV court.

### III Data

We use information from judicial records gathered by the General Council of the Judiciary for the period 2005-2018.<sup>11</sup> We build a longitudinal dataset of the 431 judicial districts in Spain with yearly information on the number of total IPV cases registered in the court. Because every report of IPV regardless of whether this is made directly in the court or in the police station by the victim or a third person generates immediately an IPV case in the court, the number of total IPV cases in the court is a measure for the total number of IPV cases reported in the judicial district in the same year. The judicial records also include information on the yearly number of IPV offenses registered in the court by type of IPV offense (i.e., less severe, severe IPV offenses and homicides). Severe IPV offenses include those that request medical surgery or prolonged medical attention, cause an abortion or an injury to the fetus. Due to existing protocols in hospitals, we expect the degree of under-reporting of these offenses to be smaller than for less severe IPV offenses. Worth to mention, IPV cases and total number of IPV offenses are not equivalent since many IPV cases include multiple offenses. Finally, we also use recorded information on the average time of the judiciary process in the IPV court/ordinary investigation court or time to disposition<sup>12</sup>, on the share

<sup>11</sup>The data is publicly available at the following link: <https://www.poderjudicial.es/cgpj/es/Temas/Estadistica-Judicial/>

<sup>12</sup>This is measured in days. The judicial procedure in the IPV court/ordinary investigation court includes the investigation phase of the trial and, for the two cases described earlier in the paper, also the oral trial phase that concluded the judicial procedure. We do not know however the average full duration of the trials for IPV cases in the judicial district because the oral trial phase of many of them occurs in other criminal courts and we lack information at the case level. However, it is reasonable to expect that the shorter duration of the judicial process in IPV courts leads to a shorter duration of the total judicial process. To construct a variable measuring the average number of days to disposition, and since information is aggregated at the judicial district-year level, we follow the standard proce-

of cases dismissed and on the number of protection orders issued by the judges. Unfortunately, we lack information at the case level or about the characteristics of the victims and offenders.

During the period 2005-2018, an average of 158,270 IPV cases reached the courts every year, of which 4,837 were severe IPV offenses. In total, 15,334 individuals are convicted every year in Spain for IPV offenses. Table 1 and Figure 2 present descriptive statistics and the evolution of different variables at the judicial district level for districts with and without a specialized IPV court opened between the period studied.<sup>13</sup> This table shows that specialized IPV courts were opened in larger districts with more IPV cases. While districts with specialized IPV courts have on average nearly 841 IPV cases per year, districts without specialized IPV courts have approximately 168 cases per year. Judges in districts with specialized IPV courts not only deal with more IPV cases, but they also issue more protection orders and the judiciary processes in these courts is on average shorter. On the other hand, the share of cases dismissed is very similar in districts with and without an IPV court opened during the period studied.

## IV Empirical Strategy

The main goal of this study is to estimate the effects of opening a specialized IPV court in a judicial district. The first IPV specialized courts started operating in 2005 in 16 judicial districts. They then expanded throughout the country over the following 6 years. In total, specialized IPV courts were created in 76 judicial districts. To estimate the effect of these courts, we exploit their sequential roll-out between 2006 and 2011 in 60 out of the 431 judicial districts. We exclude the 16 judicial districts where an IPV court was opened in 2005 from the analysis for two reasons. First, we lack information about outcomes of interest before 2005, and therefore it is not possible to examine the pre-trends of these *always treated* judicial districts. Second, the judicial districts where a specialized IPV court was opened in 2005 include all of the main Spanish cities for which appropriate control districts are not available.<sup>14</sup> Consequently, including these districts in our analysis would threaten the validity of our empirical design and make the identification condition

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dures described by the Italian National Institute of Statistics (ISTAT) and calculate time to disposition as the ratio between pending cases at the beginning of the year and at the end of the year over new cases plus resolved cases and multiplied this number by 365 days.

<sup>13</sup>The table excluded the 16 districts where an IPV court opened in 2005, which are excluded from the analytical sample. Descriptive statistics including these excluded municipalities is reported in Table A5 in the Appendix.

<sup>14</sup>The judicial districts where an IPV court was opened in 2005 are Madrid, Barcelona, Sevilla, Valencia, Bilbao, Málaga, Palma, Alicante, Granada, Las Palmas de Gran Canaria, Santa Cruz de Tenerife, Murcia, San Sebastián, Vitoria, San Bartolomé de Tirajana and Arona.

not testable.

The main challenge for the identification of the causal effect of opening an IPV court is that, even when the districts where an IPV cohort was opened in 2005 are removed from the sample, judicial districts where specialized IPV courts were opened in the following years were on average very different from judicial districts in which an IPV court was never opened. Figure 2 displays the evolution of outcomes of interests between 2005 and 2018 for districts without specialized IPV courts and for districts where an IPV court was opened after 2005. This figure reveals that judicial districts with and without IPV courts are very different in levels and trends for most of the outcomes examined. Before the opening of an IPV court, these districts have a higher number of IPV offenses (both severe and less severe), are resolved faster, the share of cases dismissed is smaller and the judges in these districts issue more protection orders. The differences in values and, most importantly, in trends between districts with and without these courts already before the opening of the IPV court suggest that standard difference-in-differences models might not yield causal parameters because the parallel trends condition is unlikely to be satisfied.

To overcome this limitation, we combine matching techniques with a difference-in-differences strategy. The first step is to identify a relevant control group among those judicial districts in which a judicial court was never opened. Following [Lara-Ibarra et al. \(2019\)](#), we use a nearest neighbor algorithm propensity score matching approach to find the most similar non-IPV court district for every district in which an IPV court was opened between 2006 and 2011. Our main approach to estimate the propensity score includes as matching variables only pre-treatment values of the outcome variable, the population size of the district and the unemployment rate in the province.<sup>15</sup> We estimate the matching exercise separately for every wave of judicial districts where an IPV court was created in a given year. We start by identifying for every judicial district in which an IPV court was opened in 2006, the closest control district in terms of the value of the outcome of interest in 2005, the population of the district in 2005 and the unemployment rate of the province in 2005. For this 2006 sample of treatment and matched controls we define a *Treatment* variable that is equal to 1 for treatment districts and 0 for the closest controls selected, a *Post* variable that is equal to one for years from 2006, an interaction term for the latter two variables and a variable *Cohort* that indicates the wave of the sample, in this case 2006 for both treatment and control districts. We then conduct the same analysis for those districts in which an IPV court was opened

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<sup>15</sup>Unemployment rate is not available at the judicial district level and the smaller geographical level at which this information is available is at the province level.

in 2007. We first select the closest control district for every district in which an IPV court was opened in 2007 using as matching variables the values of the outcome of interest in 2005 and 2006 and the baseline levels of unemployment and population. For this 2007 sample of treatment and matched controls we also define a *Treatment* variable that is equal to 1 for treatment districts and 0 for the closest controls selected, a *Post* variable that is equal to one for years from 2007, an interaction term for the latter two variables and a variable *Cohort* that indicates the wave of the sample, in this case 2007 for both treatment and control districts. We then conduct this matching exercise also for the waves of districts in which an IPV court was opened in 2008, 2009, 2010 and 2011, which was the last year in which IPV courts were created in Spain. The matching exercise drastically reduced the pre-IPV differences between treatment and control municipalities, as shown in Table A4 in the Appendix. While some statistically significant differences between treatment and matched controls remain for the number of protection orders issued and the number of aggravated IPV offenses, Figure 3 reveals that treatment and matched control districts were on parallel trends before the opening of the specialized IPV court for all of the variables that we considered with the exception of severe IPV offenses. In section V we examine this condition more comprehensively using a leads and lags estimation. Overall, our results suggest that the matching exercise is successful and validate the difference-in-differences strategy.

Once the samples of treatment and matched controls is built for every wave, the next step is to estimate a difference-in-differences regression. Following Machin and Sandi (2020), all the waves of treatment-control districts are pooled and the following difference-in-differences analysis is estimated over the resulted sample:

$$Y_{ict} = \beta IPV Court \times POST_{ict} + Year_t + Judicial District_i + Cohort_c + Post_{ct} + u_{it} \quad (IV.1)$$

where sub-indices  $c$ ,  $i$  and  $t$  indicate the cohort, the judicial district and year observed. *Cohort* is a vector of dummy variables that indicate the wave of treatment-control of the district. This indicates the year in which an IPV court is opened in the district for treatment districts. For control districts, these variables indicate the year in which the IPV court is opened in the matched treated district. *Post* is a binary variable that equals 1 after an IPV court is opened in the district for treatment districts or in the matched treated district for control districts. The specification also includes



*Year* and *Judicial District* fixed effects.<sup>16</sup> The variable of first interest in the regression is the interaction term of the variables  $IPV\ Court \times POST$ , which is equal to 1 if the judicial district  $i$  in year  $t$  from cohort  $c$  has a specialized IPV court. The parameter  $\beta$  yields the effect of the creation of a specialized IPV court in the judicial party on the outcome of interest. The regressions are estimated using a Poisson regression when the outcome variable is an integer count variable that does not take negative numbers<sup>17</sup> and using an OLS regression when the outcome variable is either a share<sup>18</sup>, a dichotomous outcomes<sup>19</sup> or a continuous number with decimal points.<sup>20</sup> Standard errors are clustered at the judicial district level.

We examine the sensitivity of the results to the use of alternative matching variables to build our control group of judicial districts. Specifically, we test whether the results of the study vary relevantly when the control group is selected based on a richer set of covariates beyond the outcome of interest, population and unemployment. The set of matching covariates in this robustness check is defined using a Stepwise regression to optimize the prediction of treatment status. The results of this empirical exercise are reported in Appendix A and, overall, they reveal consistent results.

The main identification assumption of the analysis is that in the absence of the opening of a specialized IPV court in treatment judicial districts, the evolution of the outcomes of interest would have been the same in treatment and matched control judicial districts. We assess the feasibility that this parallel trends condition is met in our context through examining the evolution of the outcomes of interest in treated and control judicial districts before the opening of IPV courts. If the outcomes of interest followed the same trend in treated and control judicial districts before the opening of an IPV court, then it would be reasonable to expect that they would have followed the same trend over the full period of interest if the IPV had never been opened. To examine the existence of parallel trends in treatment and control judicial districts before the opening of an IPV court, we estimate a leads and lags regression (Autor, 2003), which yields information on the dynamic of the effect and on whether differential trends between treatment and control judicial districts could have pre-existed the opening of IPV courts. The results of this exercise are examined in detail in the next section.

<sup>16</sup>Note variables *Post* and vectors of binary variables *Cohort* and *Year* do not generate multicollinearity as the variable *POST* varies across time and waves, the vector of variables *Year* vary across time and the vector of variables *Cohort* vary across waves but is fixed over time.

<sup>17</sup>These variables include the number of IPV cases in the judicial districts, the number of protection orders, the number of severe IPV offenses, and the number of less severe IPV offenses, and the number of protection orders.

<sup>18</sup>The share of cases dismissed.

<sup>19</sup>Whether there is an IPV homicide in the district.

<sup>20</sup>The average number of days to disposition or time to disposition of IPV cases in the district.



One additional concern with the results are other interventions affecting the incidence or the reporting of IPV that are implemented in treatment districts around the same time that the court opened (e.g., awareness campaigns). While the allocation of judicial districts throughout the country was decided by the national government after consultation with regional governments and the General Council of the Judiciary, the judicial districts are not administrative units with policy competences.<sup>21</sup> The smaller geographical unit with relevant policy competences in issues related with IPV are the regional governments. To test for potential confounding effects of other policies, we examine the robustness of our results to the inclusion of Year-Region specific fixed effects in our specification with reassuring results. Furthermore, the fact that the effect of the IPV court is evident from the year in which the IPV court is created, which varies in every judicial district, reassures our confidence in the attribution of the observed effects to the creation of the IPV court.

Recent literature highlights a number of important challenges when the timing of the treatment varies across treated units in difference-in-differences and event study settings ([Goodman-Bacon, 2021](#); [Borusyak and Jaravel, 2021](#)). Our empirical approach attenuates these concerns. Since our specification stacks six different cohorts of treatment with their matched control municipalities, the parameter of interest  $\beta$  can be thought of as an estimate of the pooled effect of six  $2 \times 2$  difference-in-differences, one per cohort examined. Nonetheless, we examine the robustness of the results to the re-estimation of the effect of interest using the difference-in-differences estimator developed in [Callaway and Sant’Anna \(2020\)](#).<sup>22</sup>

## V Results

We start the analysis by estimating the effect of opening a specialized IPV court on time to disposition. The results are reported in Column 1 of Table 2 and they reveal that the opening of an IPV court reduced the number of days to disposition by 61%. The estimates are similar in magnitude

<sup>21</sup>Judicial districts can include one or more than one municipalities. Some big municipalities such as Madrid or Barcelona are formed by more than one judicial district. While in some cases judicial districts cover one municipality, the competences of local governments in municipalities to implement related policies are very limited.

<sup>22</sup>When Callaway and Sant’Anna’s difference-in-differences is estimated using either the number of IPV cases in the judicial district, time to resolution, the number of protection orders, the number of severe IPV offenses and the number of less severe IPV offenses as an outcome variable, then the outcome variable is logged. Because variables that take a value of 0 cannot be logged without losing observations, we calculate the inverse hyperbolic sine (IHS) for the variables severe IPV offenses, for which some observations take a value of 0. The IHS transformation avoids the loss of observations when the value is 0 and is calculated as  $IHS(x) = \ln(x + \sqrt{x^2 + 1})$ . The interpretation of the coefficients of variables where the IHS transformation is conducted is equivalent to the interpretation of coefficients of logged variables.

and significance when the regression is estimated using the DID estimator developed in [Callaway and Sant'Anna \(2020\)](#).

However, is this reduction in the time to disposition in IPV cases accompanied by an increase in the reporting of IPV offenses? To address this question, we estimate the effect of the opening of IPV specialized courts on the total number of IPV cases registered in courts in the district. Because every report of IPV in the police or directly in the court generates the registration of an IPV case in the court, this variable measures the reported incidence of IPV in a given year. The estimates are reported in Column 2 of Table 2 and they show that the opening of these courts increase the yearly number of reported IPV cases by 16%-22%,<sup>23</sup> depending on the estimation method used. Because the registration of the IPV case in the court is done within a few days following the police report or the direct report to the court, this result is not mechanically driven by IPV courts simply registering the cases earlier or ordinary courts failing to register IPV cases within the year. While IPV courts increase the reported number of IPV cases, the last column of the table indicates that the percentage of cases that are dismissed remained the same. The coefficient of the parameter measuring this effect is consistently small in magnitude and statistically insignificant at conventional confidence levels. This result is aligned with the hypothesis that the effect on the reported number of IPV cases observed is not driven by an increase in the number of false reports of IPV.

Figure 4 reports the results of a leads and lags estimation, providing further insights into the dynamics of the effects. The results of these analysis show on the first place that treatment and matched control districts were on parallel trends for the three variables examined in Table 2. Furthermore, Figure 4 shows that the impact on both the reported number of IPV cases and disposition time is evident in the first year and increases over time.

The next step is to assess the report of which specific forms of IPV offenses increase in response to the opening of a specialized IPV court. Columns 1, 2 and 3 of Table 3 present the results of the effect of the opening of specialized IPV courts on the reporting of less severe IPV offenses, severe IPV offenses and IPV homicides. Severe IPV offenses are defined as those that resulted in medical surgery or prolonged medical treatment. Due to strict protocols in hospitals and in police stations, we expect the degree of misreporting of these IPV offenses to be relatively small when compared with the less severe forms of IPV. The estimates reported in Column 1 reveal that the increase in the number of reported IPV cases caused by the opening of specialized IPV courts is mainly driven by an increase in the reporting of less severe IPV offenses, which increases by 42%. Meanwhile,

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<sup>23</sup>Coefficients in Poisson regressions should be interpreted as a  $e^{\beta} - 1$  percentual change in the outcome of interest.

the coefficients measuring the effect of the opening of an IPV court on the reporting of severe IPV offenses and IPV homicides, as reported in columns 2 and 3, are smaller and statistically indistinguishable from 0 at conventional confidence levels. Because the misreporting of these crimes is arguably small, we interpret these results as suggestive that specialized IPV courts do not help to prevent the incidence of homicides and severe IPV offenses. In line with the latter interpretation, we do not observe in column 4 of Table 3 any effect of the opening of IPV courts on the total number of protection orders issued by judges, suggesting that the number of severe IPV offenses remained relatively stable.

The leads and lags analysis reported in Figure 4 for these variables show that, with the exception of severe IPV crimes, the rest of outcomes were on parallel trends before the opening of IPV courts, validating the estimations reported above. While the lack of parallel trends require to interpret with caution the magnitude of the difference-in-difference estimation on the number of severe IPV offenses, the pre-existing differential trends for this outcome suggest that, if anything, the true effect of the IPV courts on the number of aggravated IPV offenses is arguably less negative, which does not affect the main conclusions of the study.

In Appendix A, we examine the robustness of our results to different empirical exercises. First, we show in table A2 that our results are robust to the inclusion of region-year FE in the specification. Since regional governments in Spain are the main administrative level below the central government at which policies are implemented, these results reassure our confidence that our estimates are not confounded by policies implemented around the same time affecting the reporting of IPV. Second, we examine the robustness of the results to the inclusion of unemployment rate as a control variable. While the direction of the link seem to the depend on the context, unemployment, which raised dramatically in Spain from year 2008, is a crucial determinant of IPV (see for example [Anderberg et al. 2016](#), [Bhalotra et al. 2021](#), [Tur-Prats 2021](#) or [Erten and Keskin 2021](#)). The results of this analysis are reported in table A1 and show that the inclusion of unemployment as a control variable in our specification does not affect relevantly our main estimates. Third, we check the robustness of the results to defining the outcome variables as number of cases, offenses and protection orders per 100,000 inhabitants. The results, reported in table A3, show consistent results. Finally, we examine whether or not the use of a richer set of matching covariates for the selection of the control group affect our conclusions. Although the results should be taken with caution because the fulfillment of the parallel trends condition in this robustness exercise is in some cases questionable, tables A6 and A7 and figures A1 and A2 show overall consistent results

in terms of the direction of the effects and statistical significance.

## VI Conclusions

Intimate partner violence is one of the worst expressions of gender inequality, with tremendous negative consequences for the well-being of victims, their families, and society. A crucial limitation for the design of effective policies and enforcement of legal sanctions is that only a small portion of IPV offenses ended up being judged. This paper investigates the effect of the creation of specialized IPV courts in Spain, which is the flagship policy of the Spanish government to raise the reporting of IPV. These specialized IPV courts led to judge specialization and were endowed with specific resources aiming to make the legal procedure less arduous for IPV victims.

The results of this study suggest that the creation of these courts reduced the length of the procedure of IPV trials, arguably contributing to reduce the perceived cost of reporting IPV offenses for victims. Consistently, we find that the creation of these courts increased the number of reported IPV cases by 22% in our preferred specification. This increase in the number of reported IPV cases is mainly driven by an increase in the reporting of less severe IPV offenses. However, within the time period studied, we did not observe any effect of specialized courts on IPV homicides, neither on the reported incidence of severe IPV offenses, which are less likely to be misreported. This suggests that, within the few years that followed its opening, specialized courts did not help to reduce the incidence of the most severe forms of IPV. Our results document the crucial role that the functioning of the judiciary system and the perceived cost for victims have on the reporting decisions of victims and illustrate that an increase in the reporting of IPV did not necessarily lead to a reduction in the incidence of the most severe IPV offenses, at least in the short term.

Do our results mean that the judiciary system and associated institutions are failing and the reporting of IPV does not help to prevent the most severe forms of IPV? Not necessarily. Indeed, previous evidence show that other interventions affecting the reporting of IPV have important effects on IPV homicides ([Iyengar, 2009](#); [Miller and Segal, 2018](#)). Then, what alternative hypothesis could explain our results? Firstly, the beneficial effects of increasing the report of less severe cases on preventing the escalation of IPV might only operate in the very long-term and effects might not be relevant within the time period studied (2005-2018). Secondly, this result is also consistent with the hypothesis that the men that typically commit severe IPV offenses might not be the ones denounced for committing less severe IPV offenses as a consequence of the creation of the special-

ized courts. This might be the case for example if those men committing feminicides or severe IPV offenses do not typically commit less severe forms of IPV before or if men that are more prone to commit homicides do also commit less severe IPV offenses but are not denounced. More research is needed over a longer period of time and with individual and case specific data to disentangle the different hypothesis that could explain why the increase in the report of IPV does not lead to a reduction in the incidence of the most severe forms of IPV in our analysis.

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Table 1: Descriptive Statistics: Judicial districts with and without IPV courts (full sample)

	Judicial districts with JVM (N=60 Munic.)			Judicial districts without JVM (N=355 Munic.)			Diff
	N	Mean	SD	N	Mean	SD	
Total IPV cases	840	840.852	404.278	4,970	167.827	171.776	673.03***
Time to disposition (days)	840	65.248	49.290	4,970	117.814	102.074	-52.57***
Sh dismissal	840	0.358	0.155	4,970	0.363	0.194	-0.01
IPV cases	840	490.249	280.820	4,970	92.610	106.798	397.64***
Aggravated IPV	840	33.377	54.833	4,970	4.684	12.993	28.69***
Homicide	840	0.261	0.439	4,970	0.066	0.248	0.19***
Protection orders	840	180.142	89.210	4,970	43.022	49.205	137.12***

*Note:* Differences are estimated using univariate regression analysis with standard errors clustered at the judicial district level. The sample of judicial districts with JVM does not include the 16 judicial districts excluded from the analysis because they were opened in 2005. The districts excluded are: Madrid, Barcelona, Sevilla, Valencia, Bilbao, Málaga, Palma, Alicante, Granada, Las Palmas de Gran Canaria, Santa Cruz de Tenerife, Murcia, San Sebastián, Vitoria, San Bartolomé de Tirajana and Arona. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1.

Table 2: Effect of opening an IPV court in the judicial district on IPV cases reported, time to disposition and share of cases dismissed

	(1) Time to disposition	(2) Total IPV cases	(3) Sh dismissal
<i>Estimation method (A): TWFE</i>			
IPV Court $\times$ POST	-0.611*** (0.161)	0.202*** (0.066)	0.007 (0.032)
Year FE	YES	YES	YES
Judicial district FE	YES	YES	YES
Cohort FE	YES	YES	YES
Observations	1,302	1,246	1,260
Regression type	OLS	Poisson	OLS
Variable logged/IHS	Yes	No	No
Mean	77.35	757.2	0.369
<i>DiD estimation method (B): Callaway and Sant'Anna</i>			
ATT	-0.603*** (0.133)	0.158** (0.064)	-0.005 (0.037)
Variable logged/IHS	Yes	Yes	No

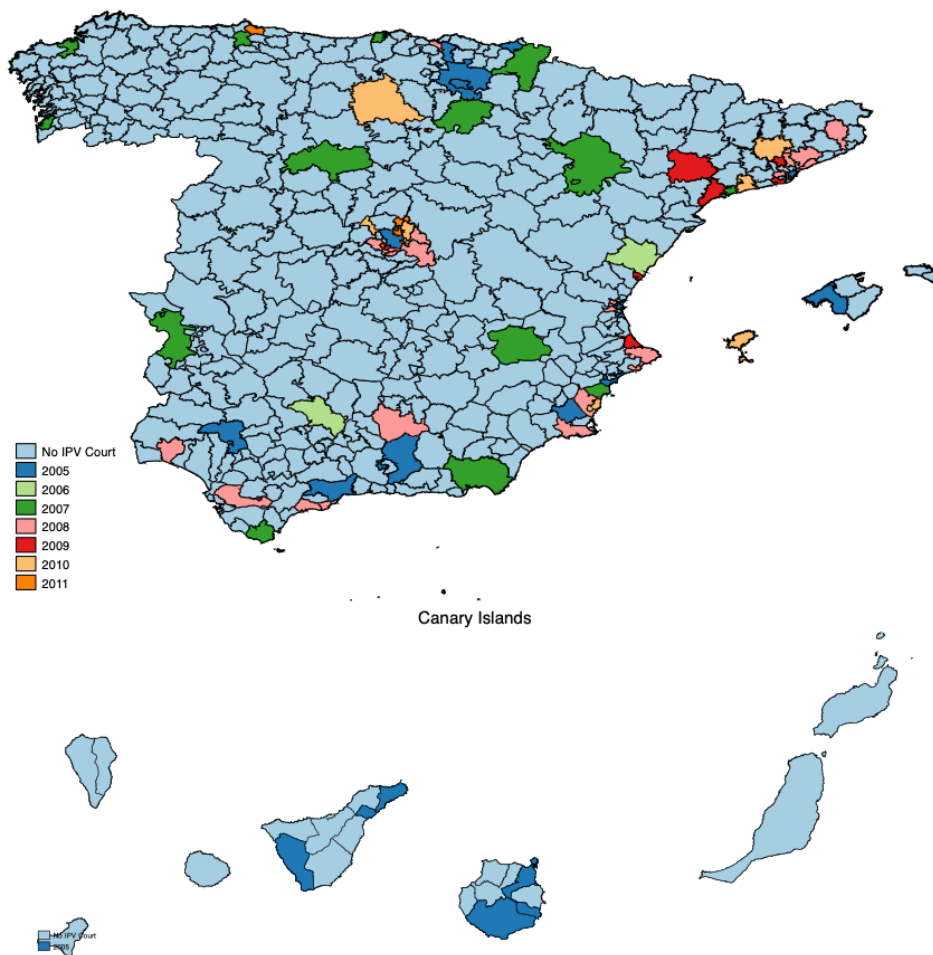
*Note:* ATT stands for average treatment on the treated difference-in-differences estimator developed in Callaway and Sant'Anna (2021). Matched controls are selected based on the pre-IPV court levels of the outcome variable, population of the judicial district in 2005 and unemployment rate in 2005 in the province. The results are reported using two estimation methods: (A) Standard two-way fixed-effects and (B) the Difference-in-Differences estimator developed in Callaway and Sant'Anna (2021). The TWFE estimation is conducted using Poisson regressions when the outcome is a count integer variable (Total IPV cases) and an OLS when the outcome is either a share or a continuous outcome with decimal numbers (time to disposition and share of cases dismissed). The dependent variable in specifications (B) is the Ln of the variable when the outcome is an integer or a continuous variable (Total IPV cases and Time to disposition) and the value of the variable when the variable is a share (Sh dismissal). In all regressions, standard errors are clustered at the judicial district level. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table 3: Effect of opening an IPV court on reported IPV offenses and issue of protection orders

	(1) Less severe IPV offenses	(2) Aggravated IPV offenses	(3) IPV Homicide (0/1)	(4) Protection orders
<i>Estimation method (A): TWFE</i>				
IPV Court $\times$ POST	0.351*** (0.092)	-0.046 (0.309)	-0.021 (0.055)	-0.050 (0.128)
Year FE	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Observations	1,288	1,204	1,274	1,176
Regression type	Poisson	Poisson	OLS	Poisson
Variable logged/IHS	No	No	No	No
Mean	422.2	27.25	0.228	168.8
<i>DID Estimation method (B): Callaway and Sant'Anna</i>				
ATT	0.488*** (0.126)	0.160 (0.385)	0.014 (0.066)	0.043 (0.115)
Variable logged/IHS	Yes	Yes	No	Yes

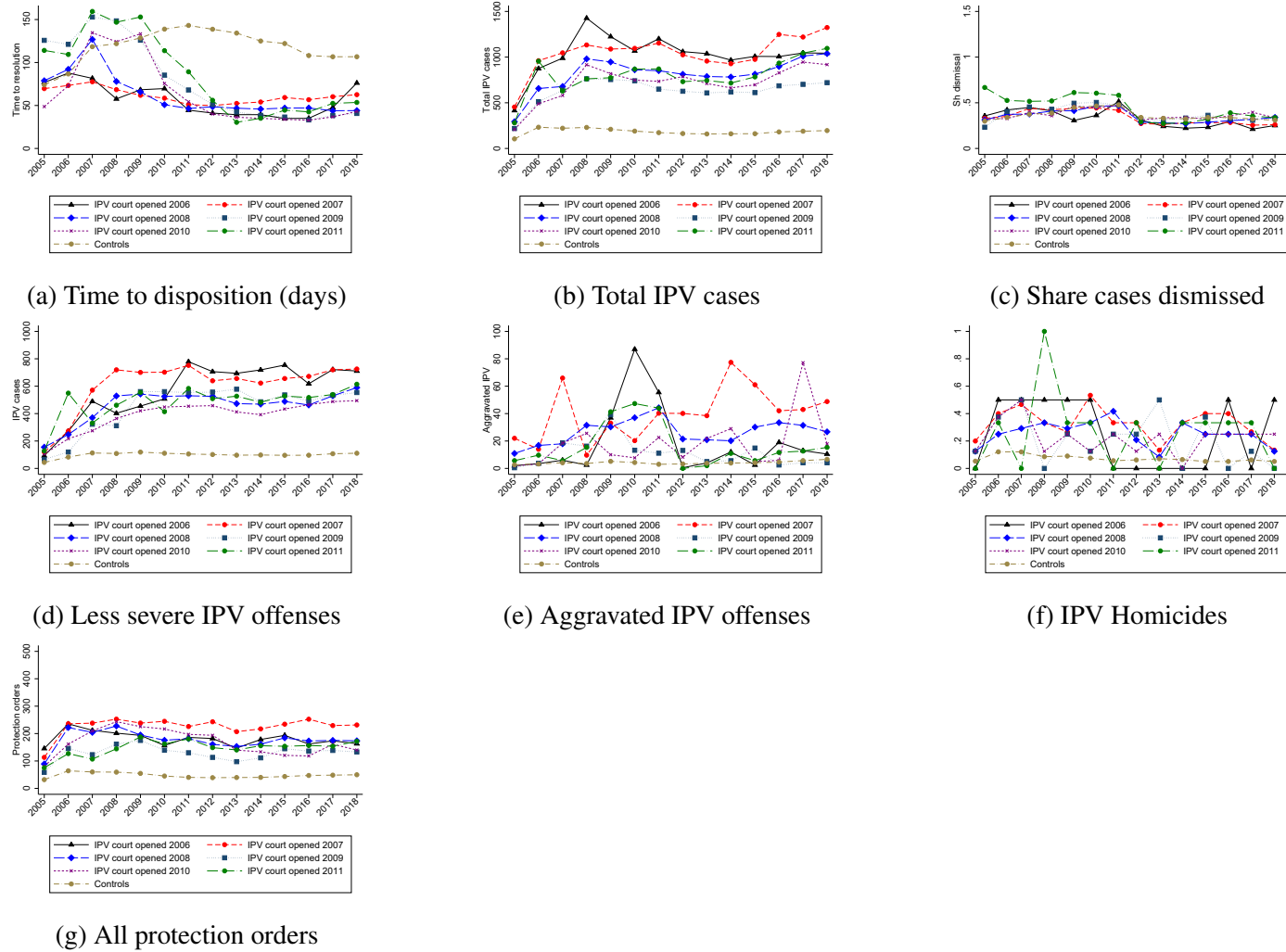
*Note:* ATT stands for average treatment on the treated difference-in-differences estimator developed in Callaway and Sant'Anna (2021). Matched controls are selected based on the pre-IPV court levels of the outcome variable, population of the judicial district in 2005 and unemployment rate in 2005 in the province. The results are reported using two estimation methods: (A) standard two-way fixed-effects and (B) the difference in differences estimator developed in Callaway and Sant'Anna (2021). The TWFE estimation is conducted using Poisson regressions except for homicide, which is conducted using a LPM because outcome is a dummy variable. The dependent variable in specification (B) is the Ln of the variable when the outcome is the number of protection orders or the number of less severe IPV offenses and the inverse hyperbolic sine of the value when the variable is the number of aggravated IPV offenses. The inverse hyperbolic sine transformation is used with these variables because in some observations the variable takes the value of 0 and the use of the Ln would lead to a loss of observations. In all regressions, standard errors are clustered at the judicial district level. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

Figure 1: Roll-out of specialized IPV courts throughout Spain



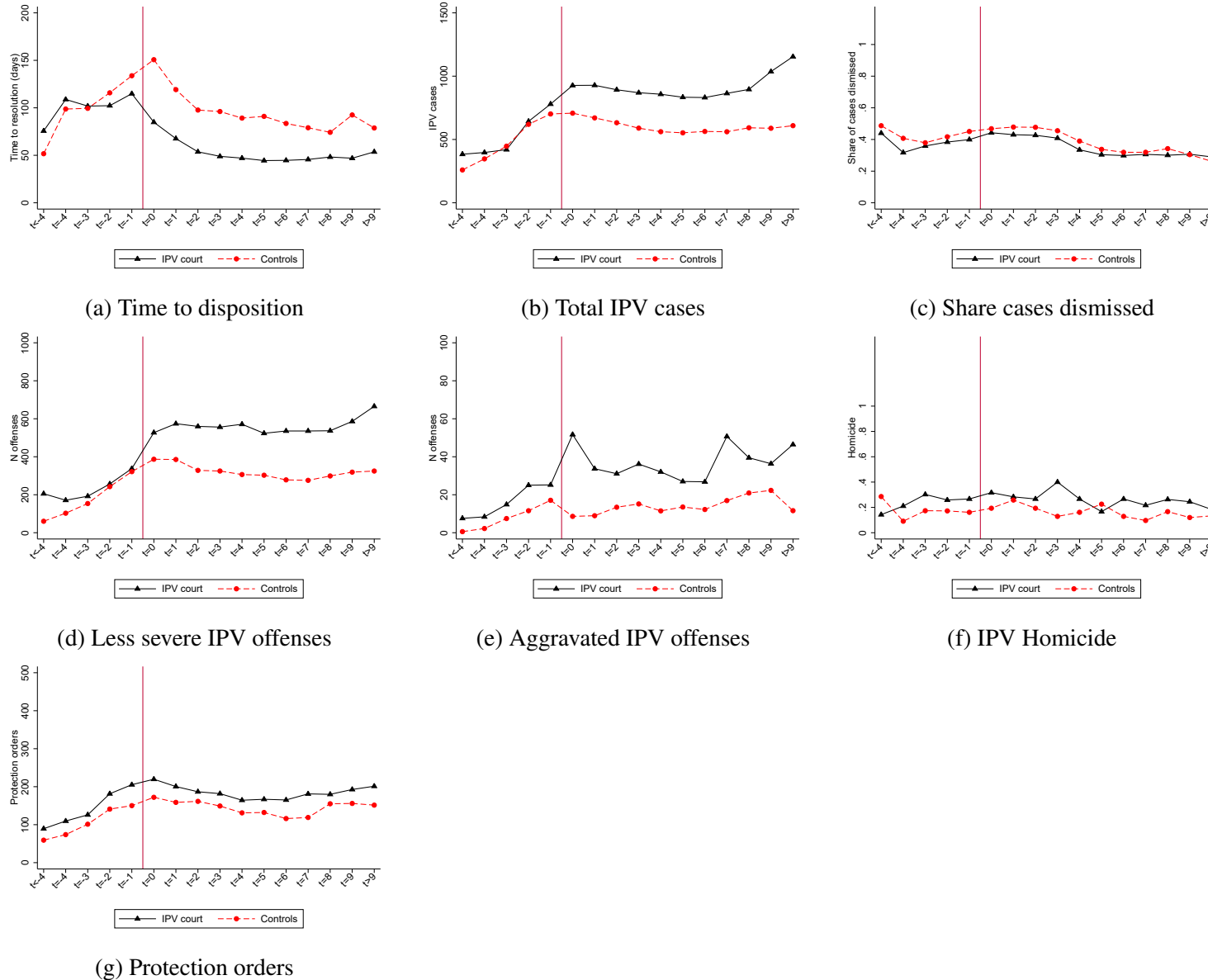
Note: These maps show which judicial districts in Spain have specialized IPV courts and their opening year.

Figure 2: Evolution of outcomes over time: districts with and without an IPV court



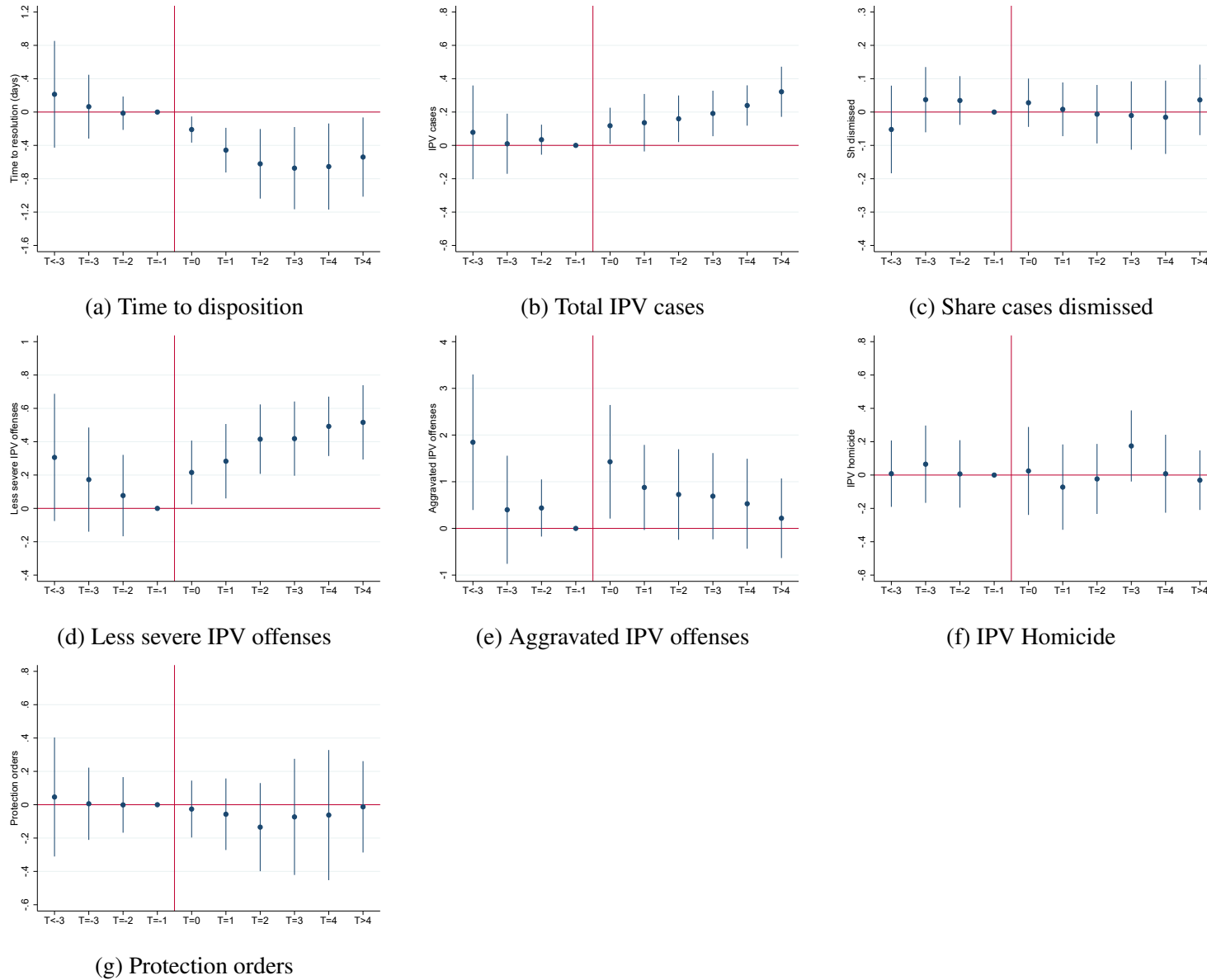
Note: This figure shows the evolution of the outcomes of interest for the period 2005-2018 for the different cohorts of judicial districts where an IPV court was opened and for the judicial districts where no IPV court was opened during the period studied.

Figure 3: Evolution of outcomes over time: treatment and matched control districts



Note: This figure shows the evolution of the outcomes of interest in judicial districts where an IPV court is opened and in matched judicial districts without an IPV court. Matched controls are selected based on the pre-IPV court levels of the outcome variable, population of the district in 2005 and the unemployment rate in the province in 2005.

Figure 4: Leads and lags estimation of the effect of IPV courts:



Note: This figure shows the results of a leads and lags estimation of the effect of opening an IPV on the outcomes of interest using as treatment judicial districts those with an IPV court opened within the period of interest and as control judicial districts those matched on pre-IPV levels of the outcome of interest, population of the district in 2005 and the unemployment rate in the province in 2005.



## A Robustness checks and additional tables and figures

This appendix examines the robustness of the main results of the study to the use of an alternative strategy for the selection of the judicial districts that integrate the control group. Specifically, we explore the question of whether the results change when, rather than matching on pre-treatment levels of the outcome variable and on the population of the district and unemployment rate, we match on a wider set of variables selected using Stepwise regression methods to optimize the prediction of treatment status and imposing the outcome variable as one of them. The pool of additional matching variables are the pre-treatment values of the total number of IPV cases, time to disposition, the number of aggravated IPV offenses, unemployment rate and the population of the judicial district. The Stepwise and the matching procedures are conducted separately for every cohort of IPV districts, and are then stacked to conduct the difference-in-differences analysis using both the traditional TWFE and the DID estimator developed in [Callaway and Sant'Anna \(2020\)](#). While the use of a wider set of matching variables might make treatment and control municipalities more comparable in general terms, Figure [A1](#) suggests that they are less comparable in terms of each specific outcome variable and the fulfillment of the parallel trends is questionable for some of them.

The results of this analysis are reported in Tables [A6](#) and [A7](#); and are graphically reported in Figures [A1](#) and [A2](#). While the estimates should be taken with caution due to some concerns with the fulfillment of the parallel trends condition for some outcomes, the results of this robustness check are overall consistent in terms of the direction of the effect and statistical significance with those reported in the main analysis.

Table A1: Effect of opening an IPV court in the judicial district: Analysis includes unemployment rate in the province as a control variable

	(1) Time to disposition	(2) Total IPV cases	(3) Sh dismissal	(4) Less severe IPV offenses	(5) Aggravated IPV offenses	(6) Homicide (0/1)	(7) Protection orders
IPV Court $\times$ POST	-0.607*** (0.165)	0.202*** (0.066)	0.007 (0.033)	0.351*** (0.0916)	-0.0383 (0.309)	-0.0289 (0.0576)	-0.0498 (0.123)
Unemployment	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES
Observations	1,302	1,246	1,260	1,288	1,204	1,274	1,176
Regression type	OLS	Poisson	OLS	Poisson	Poisson	OLS	Poisson
Variable logged/IHS	Yes	No	No	No	No	No	No
Mean	77.35	757.2	0.37	422.2	27.25	0.228	168.8

*Note:* Matched controls are selected based on the pre-IPV court levels of the outcome variable, population of the judicial district in 2005 and unemployment rate in 2005 in the province. The results are reported using a two-way fixed-effects model. The TWFE includes as control variables the log of the yearly level of unemployment rate in the province, year FE, judicial district FE and cohort FE. The TWFE estimation is conducted using Poisson regressions when the outcome is a count integer variable (total IPV cases, number of less severe IPV offenses, number of aggravated IPV offenses and protection orders) and an OLS when the outcome is either a share, a dummy variable or a continuous outcome with decimal numbers (Time to disposition, share of cases dismissed or IPV homicide in the district). Values of the variable time to disposition are logged. In all regressions, standard errors are clustered at the judicial district level. \*\*\*p<0.01;\*\*p<0.05;\*p<0.1

Table A2: Effect of opening an IPV court in the judicial district: Analysis includes Year-Region FE

	(1) Time to disposition	(2) Total IPV cases	(3) Sh dismissal	(4) Less severe IPV offenses	(5) Aggravated IPV offenses	(6) Homicide (0/1)	(7) Protection orders
IPV Court $\times$ POST	-0.566*** (0.190)	0.161*** (0.0611)	-0.00312 (0.0293)	0.338*** (0.107)	0.101 (0.380)	-0.00544 (0.0582)	0.0967 (0.114)
Region $\times$ Year FE	YES	YES	YES	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES
Observations	1,301	1,246	1,260	1,288	1,204	1,274	1,176
Regression type	OLS	Poisson	OLS	Poisson	Poisson	OLS	Poisson
Variable logged/IHS	Yes	No	No	No	No	No	No
Mean	77.35	757.2	0.369	422.2	27.25	0.228	168.8

*Note:* Matched controls are selected based on the pre-IPV court levels of the outcome variable, population of the judicial district in 2005 and unemployment rate in 2005 in the province. The results are reported using a two-way fixed-effects model. The TWFE includes region-year FE, judicial district FE and cohort FE. The TWFE estimation is conducted using Poisson regressions when the outcome is a count integer variable (total IPV cases, number of less severe IPV offenses, number of aggravated IPV offenses and protection orders) and an OLS when the outcome is either a share, a dummy variable or a continuous outcome with decimal numbers (Time to disposition, share of cases dismissed or IPV homicide in the district). Values of the variable time to disposition are logged. In all regressions, standard errors are clustered at the judicial district level. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

Table A3: Effect of opening an IPV court in the judicial district: Outcome variables defined as cases per 100,000 inhabitants

	(1) IPV cases	(2) Less severe IPV offenses	(3) Aggravated IPV offenses	(4) Protection orders
<i>Estimation method (A): TWFE</i>				
IPV Court $\times$ POST	0.237*** (0.066)	0.510*** (0.163)	0.625 (0.402)	0.044 (0.123)
Year FE	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Observations	1,246	1,218	1,218	1,246
Regression type	OLS	OLS	OLS	OLS
Variable logged/IHS	Yes	Yes	Yes	Yes
Mean	424.1	251.2	14.74	98.94
<i>DiD estimation method (B): Callaway and Sant'Anna</i>				
ATT	0.174*** (0.057)	0.439*** (0.132)	0.362 (0.373)	-0.011 (0.109)
Variable logged/IHS	Yes	Yes	Yes	Yes

*Note:* ATT stands for average treatment on the treated difference-in-differences estimator developed in Callaway and Sant'Anna (2021). Matched controls are selected based on the pre-IPV court levels of the outcome variable, population of the judicial district in 2005 and unemployment rate in 2005 in the province. The results are reported using two estimation methods: (A) Standard two-way fixed-effects and (B) the Difference-in-Differences estimator developed in Callaway and Sant'Anna (2021). All variables are constructed as number of cases, offenses or protection orders per 100,000 inhabitants using the population of the district in 2005. In all regressions, standard errors are clustered at the judicial district level. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1

Table A4: Descriptive Statistics: Judicial districts with IPV courts and matched controls before the opening of the court

	Judicial districts with JVM			Judicial districts without JVM			Diff
	N	Mean	SD	N	Mean	SD	
Total IPV cases	194	593.005	323.163	105	530.381	304.892	62.62
Time to disposition (days)	194	104.804	68.891	114	109.560	91.158	-4.76
Sh dismissal	194	0.381	0.209	101	0.423	0.188	-0.04
Less severe IPV cases	194	255.378	184.432	110	213.045	167.334	42.33
Aggravated IPV	194	19.933	32.091	85	10.447	14.300	9.49**
Homicides	194	0.325	0.605	95	0.221	0.549	0.10
Protection orders	194	162.825	97.287	76	123.118	68.782	39.71**

*Note:* Differences are estimated using univariate regression analysis with standard errors clustered at the judicial district level.\*\*\*p<0.01;\*\*p<0.05;\*p<0.1.

Table A5: Descriptive Statistics: judicial districts with and without IPV courts (full sample)

	Judicial districts with JVM (N=76 Munic.)			Judicial districts without JVM (N=355)			Diff
	N	Mean	SD	N	Mean	SD	
Total IPV cases	1,064	1,298.567	1,798.502	4,970	167.827	171.776	1,130.74***
Time to disposition (days)	1,064	64.586	47.411	4,964	117.814	102.074	-53.23***
Less severe IPV offenses	1,064	822.650	1,203.236	4,970	92.490	106.714	730.16***
Severe IPV offenses	1,064	46.256	99.327	4,970	3.722	11.586	42.53***
Homicides	1,064	0.484	0.929	4,970	0.073	0.291	0.41***
Protection orders	1,064	269.102	378.085	4,970	43.022	49.205	226.08***

*Note:* Differences are estimated using univariate regression analysis with standard errors clustered at the judicial district level. The number of matched controls depends on the specific outcome. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1.

Table A6: Effect of opening an IPV court in the judicial district on the number of cases reported, time to disposition and share of individuals convicted (selection of matched control districts based on variables selected using Stepwise regression methods)

	(1) Time to disposition	(2) Total IPV cases	(3) Sh dismissed
<i>Estimation method (A): TWFE</i>			
IPV Court $\times$ POST	-0.809*** (0.139)	0.201** (0.079)	-0.044 (0.053)
Year FE	YES	YES	YES
Judicial district FE	YES	YES	YES
Cohort FE	YES	YES	YES
Observations	1,148	1,148	1,106
Regression type	OLS	Poisson	OLS
Variable logged/IHS	Yes	No	No
Mean	71.85	753.2	0.38
<i>Estimation method (B): Callaway and Sant'Anna</i>			
ATT	-0.689*** (0.138)	0.181*** (0.067)	-0.033 (0.044)
Variable logged/IHS	Yes	Yes	No

*Note:* ATT stands for average treatment on the treated difference-in-differences estimator developed in Callaway and Sant'Anna (2021). Matched controls are selected based on the pre-IPV court levels of the variables selected using Stepwise procedures to optimize prediction of treatment condition imposing the outcome variable as one of the matching variables. The results are reported using two estimation methods: (A) standard two-way fixed-effects and (B) the difference-in-differences estimator developed in Callaway and Sant'Anna (2021). The TWFE estimation is conducted using Poisson regressions when the outcome is a count integer variable (Total IPV cases) and an OLS when the outcome is either a share or a continuous outcome with decimal numbers (Time to disposition and share of IPV cases dismissed). The dependent variable in specification (B) is the Ln of the variable when the outcome is an integer or a continuous variable (Total IPV cases and Time to disposition) and the value of the variable when the variable is a share (Sh dismissed). In all of the regressions, standard errors are clustered at the judicial district level.

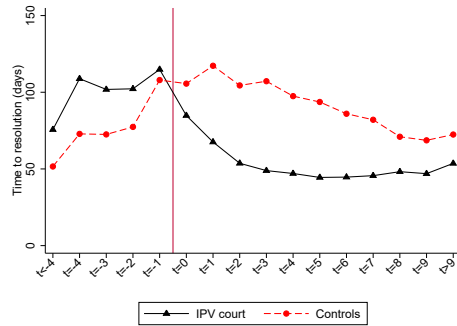
Table A7: Effect of opening an IPV court on reported IPV offenses and issue of protection measures (selection of matched control districts based on variables selected using Stepwise regression methods)

	(1) Less severe IPV offenses	(2) Aggravated IPV offenses	(3) IPV Homicide (0/1)	(4) Protection orders
<i>Estimation method (A): TWFE</i>				
IPV Court $\times$ POST	0.186* (0.113)	0.227 (0.356)	-0.038 (0.064)	0.038 (0.089)
Year FE	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Observations	1,134	1,176	1,106	1,148
Regression type	Poisson	Poisson	OLS	Poisson
Variable logged/IHS	No	No	No	No
Mean	443.5	27.38	0.234	163.2
<i>DID estimation method (B): Callaway and Sant'Anna</i>				
ATT	0.248 (0.179)	0.542 (0.391)	-0.004 (0.072)	0.039 (0.112)
Variable logged/IHS	Yes	Yes	No	Yes

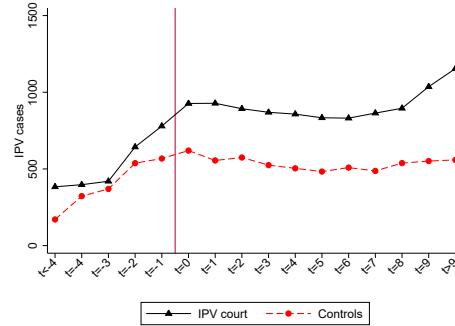
*Note:* ATT stands for average treatment on the treated difference-in-differences estimator developed in Callaway and Sant'Anna (2021). Matched controls are selected based on the pre-IPV court levels of the variables selected using Stepwise procedures to optimize prediction of treatment condition imposing the outcome variable as one of the matching variables. The results are reported using two estimation methods: (A) standard two-way fixed-effects and (B) the difference-in-differences estimator developed in Callaway and Sant'Anna (2021). The TWFE estimation is conducted using Poisson regressions when the outcome is an integer number (number of less severe IPV offenses, aggravated IPV offenses and protection orders issued) and a LPM when the outcome variable is a binary variable (whether there is an IPV homicide in the district). The dependent variable in specification (B) is the Ln of the variable when the outcome is protection orders and the number of less severe IPV offenses and the inverse hyperbolic sine of the value when the variable is aggravated IPV offenses. The inverse hyperbolic sine transformation is used with these variables because in some observations the variable takes the value of 0 and the use of the Ln would lead to a loss of observations. In all regressions, standard errors are clustered at the judicial district level. \*\*\*p<0.01; \*\*p<0.05; \*p<0.1



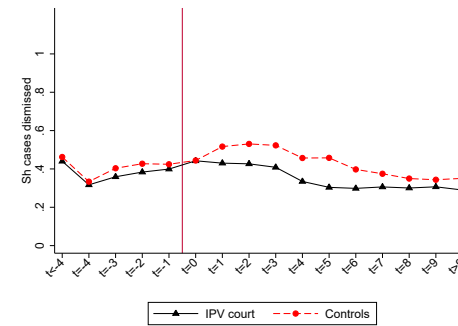
Figure A1: Evolution of outcomes over time: treatment and control districts matched using variables selected using Stepwise procedure



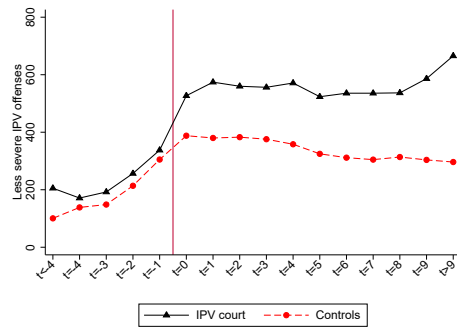
(a) Time to disposition



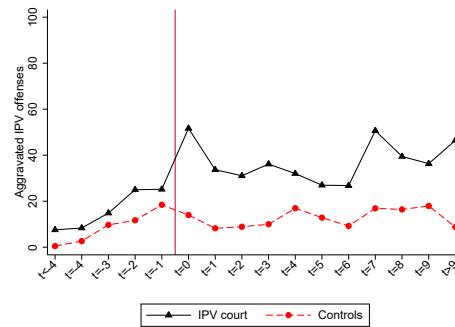
(b) Total IPV cases



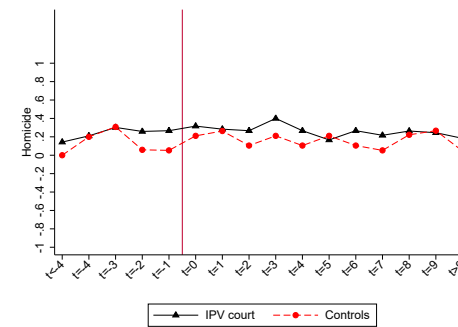
(c) Share cases dismissed



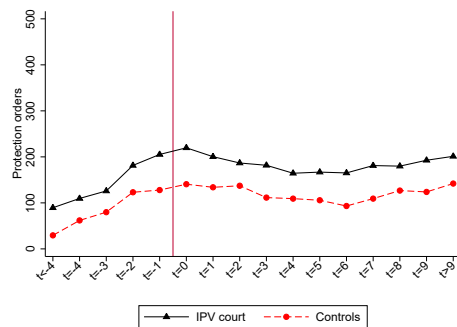
(d) Less severe IPV offenses



(e) Aggravated IPV offenses



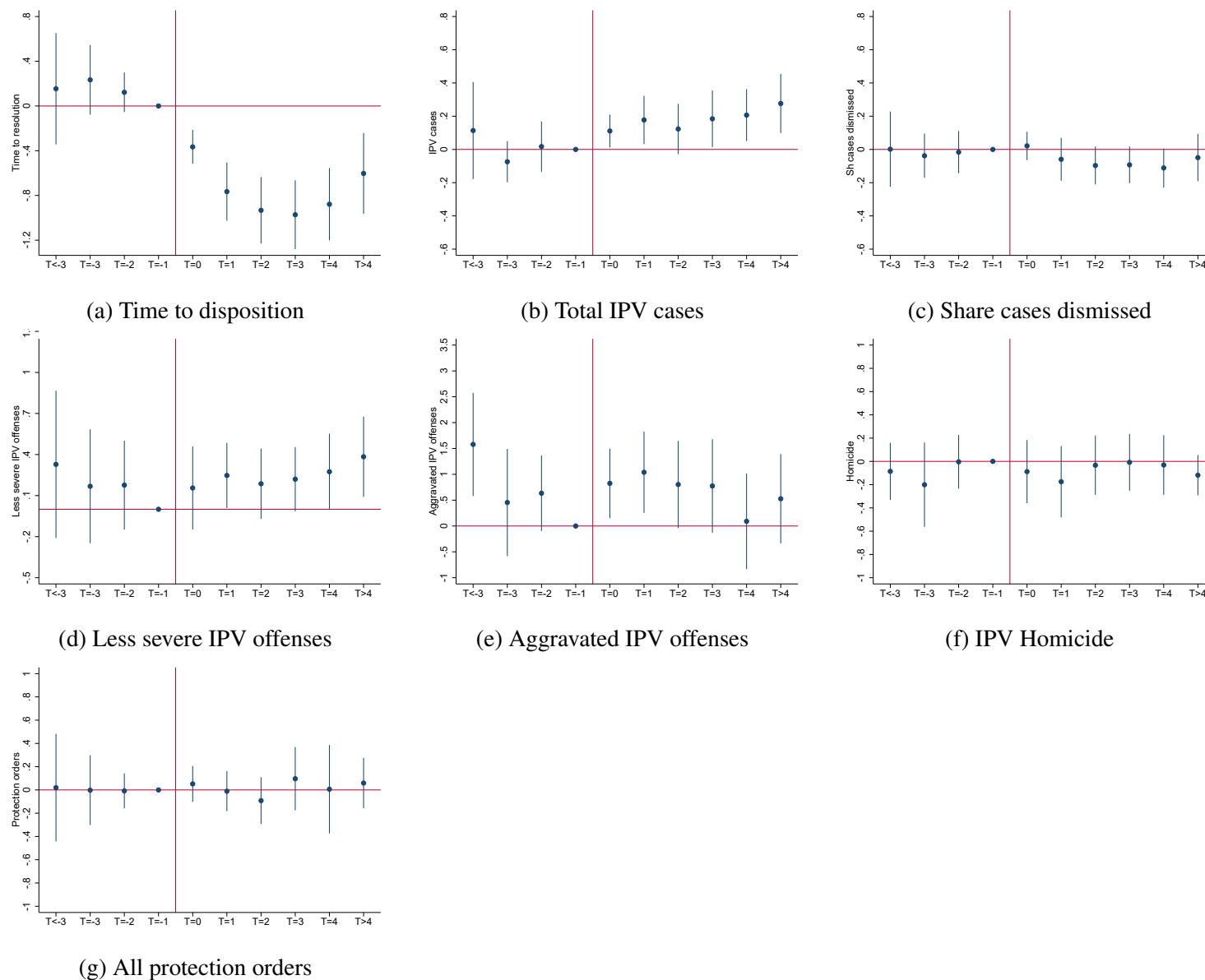
(f) IPV Homicide



(g) Protection orders

Note: This figure shows the evolution of the outcomes of interest in judicial districts where an IPV court is opened and in judicial districts without an IPV court. Matched controls are selected based on the pre-IPV court levels of the variables selected using Stepwise procedures to optimize prediction of treatment condition imposing the outcome variable as one of the matching variables.

Figure A2: Leads and lags estimation of the effect of IPV courts: treatment and control districts matched using variables selected using Stepwise procedure



Note: This figure shows the results of a leads and lags estimation of the effect of opening an IPV on the outcomes of interest using as treatment judicial districts those with an IPV court opened within the period of interest and as control judicial districts those matched on a set of variables selected using Stepwise procedures to optimize prediction of treatment condition imposing the outcome variable as one of the matching variables.