Specialised Courts and the Reporting of Intimate Partner Violence: Evidence from Spain*

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Abstract

This paper assesses the effect of the creation of specialised intimate partner violence (IPV) courts on the reporting of these crimes and on the incidence of IPV homicides. We find that the opening of a specialised IPV court decreases the time to resolution of IPV cases in the judicial district by 65% and increases the reporting of IPV by approximately 24%. On the other hand, we do not find any effect of these specialised courts on the incidence of IPV homicides within the period studied.

Keywords: Intimate partner violence, reporting of intimate partner violence, specialised intimate partner violence courts.

JEL Codes: J12, J16, K14, K38

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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)[1], which has harmful 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).[2] 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 remains unreported, which hinders the enforcement of legal sanctions and the implementation of many 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 every five IPV episodes in Spain is 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 its pernicious consequences, governments and international organizations have implemented various policies aimed at promoting and facilitating the reporting of IPV, including information campaigns, specialised help-lines, women-only police stations, specialised units within the police and the creation of specialised IPV courts (WHO, 2005; OECD, 2013).

This paper examines the effectiveness of one of these policies to increase the reporting of

[1]The prevalence among partnered women varies across countries. Data from the World Health Organization shows that 30% of women aged between 15 and 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.

[2]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.

IPV and protect victims, namely the creation of specialised IPV courts. These are courts that are endowed with specific resources and specialised 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[3] have been created in multiple settings, including Australia, 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 specialised IPV courts on the reporting of IPV in Spain and on the incidence of IPV homicides.

But why should IPV courts affect the reporting of IPV? Specialised courts are designed to reduce the length of the judicial process and receive specific resources aimed at facilitating and making the judicial process less hazardous for IPV victims. Commonly mentioned in surveys and qualitative studies by victims as an important barrier for reporting IPV (Broidy et al., 2016; Silván et al., 2015), the opening of a specialised 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 underlying the potential effect of the creation of IPV courts on reporting is that victims of IPV have updated information about the duration and degree of the 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 offence. In this context, it does not seem unreasonable to think that an important share of victims of IPV have updated information and react to changes in the *cost* of reporting IPV.

However, IPV courts may not only affect the reporting but also the incidence of the most severe forms 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 adopting more timely protection measures (Golestani et al., 2021). Given that severe IPV offences often start with verbal abuse before escalating over time (Aizer and Dal Bó, 2009), the increase in the reporting of less

[3]Domestic violence courts not only deal with IPV offences but also with other forms of violence that occur within the household.

severe IPV offences caused by the creation of IPV courts may lead to an increase in the timely adoption of judiciary measures, 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 men and women perceive that these courts facilitate the reporting of IPV, the improvement in women's bargaining power within violent marriages could lead to a reduction in the incidence of IPV (Tur-Prats, 2019; Sanin, 2022).[4]

To assess the causal effect of the creation of IPV courts, we apply a difference-in-differences strategy using two-way fixed effects (TWFE) and the doubly robust estimator developed in Callaway and Sant'Anna (2020) 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 an IPV court reduces the length of time to disposition in IPV cases by 65%. Our analysis also reveals that the creation of IPV courts increase the reported number of IPV cases in the district by 24%, mainly driven by a rise in the reported number of less severe IPV cases. The fact that the proportion of IPV cases that are dismissed in court[5] remains unchanged suggests that the increase in the number of reported IPV cases resulting from the creation of IPV courts is not driven purely by a rise in the number of false reports. While we cannot disentangle which specific element of these courts is driving the effect, we show suggestive pointing to improvements in celerity as an important mechanism.

On the other hand, we do not observe any significant effect of IPV courts on the number of IPV homicides in the district, the most severe form of IPV for which we do not expect misreporting. 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 form of IPV within the time period studied. Consistently, we do not find that the opening of these courts increased the number of protection orders, which are typically issued by judges to protect IPV victims and their families in risky situations. We test this hypothesis further using survey and administrative data and find no effects of these courts on the survey-measured incidence of different forms of IPV, neither on

[4]The literature is however not homogeneous and some studies found that improvements in bargaining power among women lead to larger levels of IPV (Erten and Keskin, 2018, 2021a). We test this hypothesis in Appendix C and do not find evidence supporting this hypothesis.

[5]In Spain, IPV cases cannot be dismissed by the police. The police cannot turn away victims reporting an IPV crime and they can only open an IPV case in the court. The process is explained in detail in the following section.

the number of hospitalizations and homicides of women. Taken together, these results reassure the hypothesis that the rise in the number of reported cases of IPV is driven by an increase in the reporting and not in the incidence of IPV.

This study falls within the growing body of evidence at the intersection of the economics of crime, the judicial system and gender (Erten and Keskin, 2021a, 2018, 2021b; Chin and Cunningham, 2019; Cunningham and Shah, 2018; Doleac, 2020). More specifically, this study contributes to various strands of the literature. First, this is the first rigorous empirical analysis of the causal effect of the creation of specialised IPV courts on the reporting of this form of violence and on the incidence of IPV homicides. Previous studies published across different disciplines have explored the links between the creation of specialised courts and judicial outcomes (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 Pascual and Juan S., 2015; Garoupa et al., 2009; Gutierrez et al., 2017). They suggest that the creation of domestic violence courts is usually followed by an increase in arrests (Gover et al., 2003; Angene, 2000), with no differences in recidivism (Gutierrez et al., 2017). On the other hand, they show mixed evidence on whether specialised courts issue protection orders and jail sentences differently (Cissner et al., 2013, 2015). The results presented in the studies cited above should nonetheless be interpreted with caution because they are typically based in pre-post designs that monitor the outcomes of interest before and after the creation of specialised 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 quasi-random random variation in the assignment of misdemeanour domestic violence cases where defendants did not post-bond to either domestic violence courts or ordinary courts, this study shows strong and rigorous evidence that specialised 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 specialised courts on the reporting of IPV.

Second, our study contributes to the literature that explores the effectiveness of policies and strategies[6] that aim to increase the low levels of reporting of IPV (Iyer et al., 2012; Amaral et al.,

[6] Among others, existing evidence has showed that women-only police stations or women centres increase the reporting of IPV.

2019; Sviatschi and Trako, 2021; Miller and Segal, 2018; Iyengar, 2009; Chin and Cunningham, 2019; Sukhtankar et al., 2022), which is a crucial challenge in the fight against this type of violence. This study documents that specialised 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, the prevalence of IPV homicides and other outcomes (Iyengar, 2009; Miller and Segal, 2018; Carrell and Hoekstra, 2012). These studies show that interventions that increase (decrease) the reporting of IPV (e.g. mandatory arrest laws or female police officers) reduce (augment) the number 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 contributes to minimising 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 leads to any reduction in 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 within the period studied. We however cannot conclude that reporting the IPV does not help to prevent future IPV homicides. Among the potential hypotheses to explain these results are that the effects of reporting might be operating over a longer term or that the women that now reported IPV in response to the creation of the IPV courts are not those that otherwise would have been murdered. These hypotheses are discussed in detail 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 specialised IPV courts. Section III describes the data. Then, Section IV presents the identification strategy and Section V the main results. Section VI concludes.

II Institutional framework

II.a Intimate partner 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 major problems facing the country (CIS, 2019).[7] However, despite the implementation of several campaigns and policies to raise awareness and promote the reporting of these crimes, the share of IPV offences in Spain that are reported to the police remains 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.[8]

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 Gender Equality, or the creation of specialised IPV courts.[9] Yet, to the best of our knowledge, rigorous empirical evidence evaluating the effectiveness of these interventions is non-existent.

[7] 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).

[8] The proportion of cases reported to either the police or directly to the court varies depending on whether the perpetrator is the current or the former partner of the victim. It is estimated that only 5.4% of the IPV cases where the perpetrator is the current partner of the victim are reported, whereas the percentage of IPV cases reported is nearly five times larger (25%) when the perpetrator 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).

[9] The potential confounding effects of these interventions on our estimates of the impact of specialised IPV courts are examined and ruled out in Section V.

II.b The functioning of specialised IPV courts

In December 2004, the Spanish parliament passed the Law of Measures of Integral Protection Against Gender-Based Violence.[10] The flagship measure of the law was the creation of specialised courts on violence against women. The first specialised 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 specialised IPV courts in 76 out of the 429 Spanish judicial districts. A map with the place and time of opening of specialised IPV courts in Spain is provided in Figure 1. The allocation of specialised IPV courts throughout the country was decided by the national government after consultation with the regional governments and the General Council of the Judiciary.[11]

There are three different paths to start an IPV trial. The first one is through a police report. If a victim reports an IPV offence 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. This process is not revoked even if the woman retracted the report. Indeed, policemen cannot dismiss cases or turn away victims, and all police stations include an IPV unit.[12] Secondly, victims can also report an IPV offence 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 centres 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.[13] It is important to note that every report of IPV regardless of whether it is made to the

[10] The law is named in Spanish *Ley Orgánica 1/2004 de Medidas de Protección Integral contra la Violencia de Género*.

[11] The General Council of the Judiciary is an autonomous institution, mostly consisting of judges, that performs powers related to the government of the judiciary with the goal of guaranteeing their independence in the exercise of the judicial function.

[12] All stations from Policia Nacional, Guardia Civil, Policia Foral and Mossos d'Esquadra include an IPV unit served at least by one policeman. This is the case both in districts with and without specialised IPV courts.

[13] In 2019, the percentage of IPV cases in which the victim directly report the offence 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).

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. Furthermore, the classification of a case as an IPV case is determined by whether the perpetrator is the intimate partner or a former intimate partner of the victim which is asked following strict standard protocols which do not vary throughout the country. Therefore, and unlike in other contexts, the categorization of a case as an IPV case in Spain is not an arbitrary decision of any policeman.

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 specialised 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 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.[14] 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 offences such as minor threats or coercion and for (b) IPV cases in which the offender recognized during the judicial investigation being the crime perpetrator.

Specialised IPV courts were designed with the explicit goal of speeding up the investigation phase of 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, specialised 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 specialised IPV court in the district, the investigation stage is a crucial stage in IPV judicial processes and involves gathering

[14]These courts are called in Spain *Juzgados de Instrucción*.

judicial evidence, issuing 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. 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 offences, 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 offences. The law introduced schedule flexibility in labour conditions, emergency social services support, special health treatment and protocols, and free legal support and counselling service to access information for victims that report IPV offences. These additional dispositions were introduced at the same time in all the national territory, regardless of whether or not a specialised 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 courts.

Finally, it is worth mentioning that the functioning of the police in respect to IPV cases follows the same protocols regardless of whether there is an IPV court or not in the district. No specific team or additional resource to the police or the municipality is assigned in response to the creation of an IPV court in the judicial district.

III Data

We use information from judicial records gathered by the General Council of the Judiciary for the period 2005-2018.[15] For the main analysis, we build a longitudinal dataset of the 429 judicial districts in Spain with yearly information on the number of total IPV cases registered in the court per 100,000 inhabitants, the number of IPV homicides per 100,000 inhabitants, and the average length of the judiciary process or time to disposition of IPV cases in the specialised/ordinary investigation court.[16] 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 of the total number of IPV cases reported in the judicial district in the same year

To better understand the effects of IPV specialised courts, we complement our analysis with two additional sets of information. First, we use the judicial records to build a panel for the period 2005-2018 with yearly information on the number of IPV offences registered in the court per

[15]The data is publicly available at the following link: https://www.poderjudicial.es/cgpj/es/Temas/Estadistica-Judicial/

[16]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 procedure 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.

100,000 inhabitants by type of IPV offence (i.e., minor[17], less severe[18], severe[19], sexual[20] and other IPV offences[21]). Worth to mention, IPV cases and total number of IPV offences are not equivalent since many IPV cases include multiple offences.

Secondly, we use yearly information facilitated by the General Council of the Judiciary for the period 2005-2018 on judicial decisions in IPV cases both in specialised courts and ordinary investigation courts. Specifically, we build a variable that measures the number of protection measures issued by the court per 100,000 inhabitants, the share of IPV cases dismissed by the IPV or the ordinary investigation court, the share of cases in IPV or ordinary investigation courts that ended with a conviction,[22] and the share of cases that are transferred to be judged in a different court.

[17] Minor IPV offences include minor threats and coercion, defamation and unjust vexations. In these offences both the investigation and the oral trial phases are held in the specialised IPV court or in the ordinary investigation court if there is no IPV court in the district.

[18] Less severe IPV offences group the offences described in articles 153 and 173 of the Spanish criminal code. They include episodes of physical and psychological maltreatment that do not require medical surgery or prolonged medical attention. They also include offences against the family and victim's honour integrity and intimacy. Unlike minor IPV offences, these crimes are investigated in specialised IPV courts or in ordinary investigation courts if there is no IPV court in the district, but the oral trial is held in the criminal court.

[19] Severe IPV offences include those that request medical surgery or prolonged medical attention, cause an abortion or injure to the fetus. Due to existing protocols in hospitals, we expect the degree of under-reporting of these offences to be smaller than for less severe IPV offences. These crimes are investigated in specialised IPV courts or in ordinary investigation courts if there is no IPV court in the district, but the oral trial is held in the criminal court.

[20] Sexual IPV offences include all offences against sexual freedom and indemnity. These include severe crimes such as rapes and less severe crimes such as sexual harassment. These crimes are investigated in specialised IPV courts or in ordinary investigation courts if there is no IPV court in the district, but the oral trial is held in the criminal court.

[21] Other IPV offences include offences against freedom, breach of sentence or protection order and other IPV offences. These crimes are investigated in specialised IPV courts or in ordinary investigation courts if there is no IPV court in the district, but the oral trial is held in the criminal court.

[22] The share of conviction is calculated over the total number of cases where the oral trial is held in these courts because in IPV cases, the oral trial phase is typically conducted in a different court than the investigation phase. In other words, the passing of sentences in IPV cases is typically issued by criminal courts and not by IPV specialised courts or ordinary investigation courts. There are however two exceptions: Judges in IPV or ordinary investigation courts will also lead the oral trial phase and pass sentences for (a) minor IPV offences such as minor threats or coercion and for (b) IPV cases in which the offender recognized during the judicial investigation being the crime perpetrator. We

Additionally, we use in Appendix C 6 rounds of the gender-based violence survey conducted by the Spanish government in 1999, 2002, 2006, 2011, 2015 and 2019. The survey collects information at the individual level from a nationally representative sample of women on the current incidence of 4 types of IPV: psychological, physical, sexual and economic. Finally, we also use in Appendix C information available at the province year level from the Ministry for Gender Equality, the Home Office and the Ministry for Health on the number of calls to the IPV helpline, homicides, homicides of women, suicides, suicides of women, hospitalizations and hospitalizations of women. These data are used to rule out the hypothesis that the IPV courts are increasing the incidence of IPV and to provide suggestive evidence on additional hypotheses about the broader effects of these specialised courts.

During the period 2005-2018, an average of 158,270 IPV cases reached the courts every year, of which 4,837 were severe IPV offences. In total, 15,334 individuals are convicted every year in Spain for IPV offences. 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 specialised IPV court opened within the period studied.[23] This table shows that specialised IPV courts were opened in larger districts with more per capita IPV cases. While districts with specialised IPV courts have on average nearly 457 yearly IPV cases per 100,000 inhabitants, districts without specialised IPV courts have approximately 256 cases per 100,000 inhabitants every year. Furthermore, judges in districts with specialised IPV courts not only deal with more IPV cases, but they also issue more protection orders per capita and the judiciary processes in these courts are on average shorter. On the other hand, the share of cases dismissed, elevated or that ended with a conviction are very similar in districts with and without an IPV court opened during the period studied. Because the opening of IPV courts may impact some of these variables, we present the descriptive statistics in Table A2 in Appendix A but focusing only on the year 2005, before the opening of the IPV courts in treatment districts included in the analytical sample, which started from 2006. The table reveals that the differences between districts with IPV and without IPV courts were remarkable already

only have information for IPV sentences passed in IPV courts and ordinary investigation courts, but not in criminal courts.

[23]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 judicial districts is reported in Table A1 in the Appendix.

before the creation of the IPV courts.

IV Empirical strategy

The main goal of this study is to estimate the effects of opening a specialised IPV court in a judicial district. The first IPV specialised courts started operating in 2005 in 16 judicial districts. They then expanded throughout the country over the following 6 years. In total, specialised 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 429 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 specialised IPV court was opened in 2005 include all of the main Spanish cities for which appropriate control districts are not available.[24] Consequently, including these districts in our analyses would threaten the validity of our empirical design and make the main identification condition 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 specialised 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 different variables between 2005 and 2018 for districts without specialised 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 are larger in population, have a higher number of IPV offences per capita, IPV cases are resolved faster, 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

[24]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.

standard difference-in-differences models might not yield causal parameters because the parallel trends condition is unlikely to hold.

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 an IPV court was never opened. Following Lara-Ibarra et al. (2019), we use a nearest neighbour 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, number of IPV cases in the district, the population size of the district and the unemployment rate in the province.[25] 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 and of IPV cases 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 matched control districts. We then conduct the same analysis for those districts in which an IPV court was opened 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 and of IPV cases in the district 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 matched 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

[25] 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.

the pre-IPV court differences between treatment and control judicial districts, as shown in Table 2. While some statistically significant differences between treatment and matched controls remain for the number of protection orders issued and the number of aggravated IPV offences, Figures 3 and 4 reveal that treatment and matched control districts were on parallel trends before the opening of the specialised IPV court for all the main outcomes of interest and for most of the additional outcome variables. 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 our identification strategy.

Once the samples of treatment and matched controls are 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:

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

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.[26] 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 specialised IPV court. The parameter β yields the effect of the creation of a specialised IPV court in the judicial party on the outcome of interest.[27]

[26] 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 dummy variables *Year* vary across time and the vector of dummy variables *Cohort* vary across waves but is fixed over time.

[27] Because in some cases more than one treatment district is matched to the same control district, dummies identifying matched pairs cannot be included in the regression. This is however not problematic since standard errors are clustered at the district level and main outcomes in treatment and matched controls are similar both in values and

The main identification assumption of the analysis is that in the absence of the opening of a specialised 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 by examining the evolution of the outcomes of interest in treated and matched 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.

One potential concern with the results is that other anti-IPV interventions implemented around the same time could be confounding the estimated effects of IPV courts. For example, the Spanish ministry launched information campaigns during the period studied. However, these information campaigns and nation-wide policies were launched at the same time in all districts. Thus, they would only confound the estimates of interest if they affect differently the reporting of IPV in treatment and matched control districts and only after the creation of the IPV court. The results of the event studies rule out the latter concern: They show that, despite IPV courts were introduced in different years in different districts, the effect of the IPV court is evident from the very first year. This makes unlikely that these effects could be driven by interventions that were implemented at the same time in all judicial districts. But what about local governments implementing anti-IPV interventions? The judicial districts are not administrative units with the power to implement policies.[28] The smaller geographical and political units with relevant policy power in issues related with IPV are the regional governments. Again, the clear effects from the first year of the IPV courts make unlikely the existence of confounding effect caused by policies implemented by regional

trends before the opening of the specialised courts.

[28] Judicial districts can include one or more than one municipality. While in some cases judicial districts cover one municipality, the competences of local governments in municipalities to implement related policies are very limited.

government. We however test further for potential confounding effects of regional policies through the inclusion of Year-Region specific fixed effects in our specification with reassuring results.

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 judicial districts, the parameter of interest β can be thought of as an estimate of the pooled effect of six 2×2 difference-in-differences, one per cohort examined. Nonetheless, we examine the robustness of the results to the use of the doubly robust estimator developed in Callaway and Sant'Anna (2020) for staggered difference-in-differences.

In Appendices A and B, we report the results of two additional empirical checks aiming to test the robustness of our results to our matching approach and to the estimation method. First, 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 that includes every outcome variable analysed, population of the district and unemployment rate in the province. Specifically, we select the matching covariates using a Stepwise regression to optimize the prediction of treatment status. In this approach, the estimation of the impact on every outcome of interest is implemented using the same sample of districts. While the parallel trends condition does not seem to hold for some of the outcomes analysed when this matching procedure is implemented, this empirical exercise, reported in Appendix B, reveals consistent results. Second, we re-estimate the results using the synthetic difference-in-differences method developed in a recent paper by Arkhangelsky et al. (2021). The results of this analysis are reported in Appendix A and show largely consistent estimates.

V Results

We start the analysis by estimating the effect of opening a specialised IPV court on time to disposition. The results are reported in Column 1 of Table 3 and they reveal that the opening of an IPV court reduced the number of days to disposition by 65%. The estimates are similar in magnitude

and significance when the regression is estimated using the methodology 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 offences? To address this question, we estimate the effect of the opening of IPV courts on the 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. Furthermore, policemen cannot dismiss cases or turn away victims, their protocols do not vary across judicial districts and the categorization of an IPV case is exclusively determined by whether the reported perpetrator currently has or had in the past an intimate relation with the victim. The estimates are displayed in Column 2 of Table 3 and they show that the opening of an IPV court increases the yearly number of reported IPV cases by 18%-24%, 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.

In our setting, we can only expect a reduction in the *cost* of reporting to actually increase reporting if women have updated information about the *costs* of reporting IPV when they make the decision about whether to report IPV. Is this assumption reasonable? Existing surveys and reports show that, in Spain, before deciding to report IPV, 77% of IPV victims have shared their experience with close friends or family (Spanish Ministry for Equality, 2019), 24% consulted with lawyers, 15% with social services and 10% with other support organizations (European Union Agency for Fundamental Rights, 2015). These results show that it is likely that a non-negligible share of women that experience IPV have updated information about the judicial procedure. Furthermore, data from the 2011 gender-based violence survey conducted by the Spanish Ministry for Equality show many of the victims of IPV violence are exposed to this type of violence frequently.[29] Although we do not know the share of these cases that ended up in court, it does not seem un-

[29]The survey shows that among women that ever experienced physical violence, 68% of them reported suffering this type of IPV at least sometimes and 30% frequently. Among women that ever experienced psychological violence, 81% of them reported suffering this type of IPV at least sometimes and 52% frequently. Among women that ever experienced sexual violence, 67% of them reported suffering this type of IPV at least sometimes and 19% frequently. Among women that ever experienced economic violence, 76% of them reported suffering this type of IPV at least sometimes and 36% frequently.

reasonable to believe that, though repeated exposure, some of these women may have updated information about the judiciary process.

The results reported in Column 3 show no effects of the opening of specialised IPV courts on IPV homicides, for which we expect none or very little misreporting. The magnitude of the coefficient, ranging between a non-significant reduction in the number of IPV homicides of 5.6% and an increase of 1% depending on the estimation method, suggests that the rise in reporting caused by the IPV courts does not lead to a decrease in IPV homicides within the period studied. One potential concern is that the low prevalence of IPV homicides makes the identification of statistically significant effects difficult. In the matched sample used in the estimation of the effects of the opening of IPV courts on the prevalence of IPV homicides, there is a total of 360 IPV homicides, which yields an average of 0.3 homicides per year in each judicial district or 0.17 per 100,000 inhabitants. While we might lack enough variation to detect very small impacts, our results rule out the existence of meaningful effects.[30] We discuss in Section VI what factors might be explaining the lack of effect of IPV courts on homicides.

In Table 4 we explore how the opening of IPV courts affects the reporting of IPV by type of offence and judicial decisions. Columns 1 to 5 show that IPV courts seem to increase the reporting of every IPV offence, but the effect is particularly striking for less severe IPV offences as well as for the category other IPV offences, which include IPV offences against freedom, breach of sentence or protection orders and other unclassified IPV offences. Because the breach of sentence or protection orders implied a previous experience at the court, the large effect on this category is consistent with more informed women reacting to the decrease in the *cost* of reporting IPV. One additional question is whether the increase in reporting, particularly of less severe IPV crimes, could lead to different judicial outcomes. While we lack information on judicial decisions by specific type of IPV offence, we can examine the effect of the opening of an IPV court on judicial decisions in IPV cases as a whole. The results of this analysis are reported in Columns 6 to 9 of Table 4 and show that, despite the increase in the reporting of IPV cases (particularly of less severe offences), the share of cases dismissed, elevated to the criminal court and that ended with a conviction in the district do not change with the opening of the IPV court. The small and statistically insignificant coefficient of the parameter that measures the effect of the opening of

[30]The minimum detectable effect size in the difference in difference estimation is 0.21.

an IPV court on the share of cases dismissed 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. Finally, we do not observe any positive effect on the number of protection orders issued when specialised courts are opened, suggesting that the increase in the number of the IPV offences reported caused by the opening of the IPV specialised court does not seem to be driven by cases perceived by judges as risky. Although the results on judicial decisions should be interpreted with caution due to IPV courts examining a higher number of less severe IPV cases relative to other cases, the lack of effects of IPV courts on the share of IPV cases that ended in dismissal, elevation or conviction is consistent with the hypothesis that the effect on reporting is not driven by judges making very different judicial decisions in IPV specialised courts.

Figures 5 and 6 show the results of a leads and lags estimation for all the outcomes analysed, providing further insights into the dynamics of the effects. The results of these analyses show on the first place that treatment and matched control districts were on parallel trends for most of the variables examined in the paper. Furthermore, the figures show that the impact on the outcomes for which an effect is found is evident in the first year and increases over time.

In Appendix C, we use survey data and province level data to examine the potential effect of IPV courts on the incidence of different types of IPV. First, we use 6 rounds of the gender-based violence Spanish survey to show that a higher exposure to IPV courts is not associated with a higher incidence of IPV. Second, we use administrative information at the province level to show that a higher exposure to IPV courts is not associated with an increase in the number of hospitalizations of women or in the number of women murdered. These results are consistent with the estimates presented earlier on in the paper showing no effects of IPV courts on the number of IPV homicides. Jointly, they rule out the hypothesis that the effect of IPV courts on the reported number of IPV cases is driven by IPV courts increasing the *true* incidence of IPV rather than the reporting of these cases. The appendix also shows no association between a higher exposure to IPV courts and the number of suicides, the number of women suicides or the number of calls to the IPV helpline. Because reporting IPV through this helpline does not lead to the opening of an IPV case in the court, the *cost* of reporting IPV using the helpline is not affected by the opening of an IPV court. Thus, the lack of an effect on these calls is consistent with the hypothesis that the effect of IPV courts on the reporting of IPV is driven by the reduction in the *cost* of the judicial process for IPV

victims.

In Appendices A and B, we examine the robustness of our results to different empirical exercises. First, we show in Table A3 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 effect seems to depend on the context, unemployment, which raised dramatically in Spain from 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 2018). The results of this analysis are reported in Table A4 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 the use of the synthetic difference-in-differences method developed in Arkhangelsky et al. (2021). The results are reported in Table A5 and the estimated effects are very similar to those reported in the main analysis of the paper. Finally, we examine whether the use of a richer set of matching covariates selected using a Stepwise regression to optimize the prediction of treatment status affects our conclusions. Although the results should be taken with caution because the fulfilment of the parallel trends condition in this robustness exercise is in some cases questionable, the estimates reported in Table B1 show overall consistent results in terms of the direction of the effects and statistical significance.

V.a Discussion of potential mechanisms

In the previous section we show that IPV courts increase the reporting of IPV. But what is the exact constraint released by these specialised courts? We hypothesise the effects of IPV courts could be driven by three broad mechanisms: IPV courts decreasing the *cost* of the judicial process through providing quicker judicial decisions, other targeted resources (e.g. availability of child care in the court, specialised social workers in the court, etc) decreasing the *cost* of the judicial process through making the process less burdensome for the victims, and judges in these courts issuing on average sentences and protection measures that better match the expectations of the victims.

The paper evaluates the effect of IPV courts as a *package*. Unfortunately, we lack the necessary data to disentangle conclusively the extent to which the different mechanisms are contributing to the effect.[31] However, we can provide some suggestive evidence on the proposed mechanisms. First, the fact that IPV courts do not affect the proportion of cases that ends with a dismissal or conviction neither the number of protection orders (with similar levels of incidence and reporting of severe IPV) is consistent with the hypothesis that the effects of IPV courts on reporting are unlikely driven by judges in IPV courts and ordinary investigation courts making very different decisions. To test the celerity mechanism, we exploit the opening of 7 ordinary investigation courts in the years 2007, 2009 and 2011 in districts where there is no IPV court. While these additional ordinary investigation courts are typically not endowed with specific resources targeting IPV victims, they are likely to reduce the length of the judiciary process because, if there is no specialised IPV court, the number of IPV cases is split between the total number of ordinary investigation courts in the district. To estimate the effect of the opening of an additional ordinary investigation court, we use the synthetic difference-in-differences method developed in Arkhangelsky et al. (2021). The latter paper shows that the method performs better than traditional matching approaches when the number of treatment observations is very low as in this case. The results of this empirical exercise are reported in Table D1 in Appendix D. While they are not statistically significant and the lack of statistical power require to take them with caution, the coefficients for time to disposition and number of IPV cases have the expected sign and the magnitudes are meaningful, suggesting that the celerity plays an important role in explaining the results of specialised IPV courts.

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 implementation of effective anti-IPV policies and the enforcement of legal sanctions is that only a small portion of IPV offences ends up in court. This paper investigates the effect of the creation of specialised IPV courts in Spain, which was the flagship policy of the Spanish

[31] There is no available information about the resources available at the court level. Furthermore, although sentences issued by judges are public, the systematic web scraping of these sentences is illegal and punished by the Spanish law.

government to raise the reporting of IPV. These specialised IPV courts were endowed with specific resources aiming to improve the celerity and to make the legal procedure less arduous for IPV victims.

The results of this study suggest that the creation of IPV courts reduced the length of IPV trials, arguably contributing to reducing the perceived *cost* of reporting IPV offences for victims. Consistently, we find that the creation of IPV courts increased the reporting of IPV cases by nearly 24% in our preferred specification. However, within the time period studied, we did not observe any effect of specialised courts on IPV homicides, which are unlikely to be misreported. 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 does not necessarily lead to a reduction in IPV homicides, at least in the short- and medium-term.

Do our results mean that the judiciary system and associated institutions are failing and the reporting of IPV does not help to prevent homicides? Not necessarily. Previous evidence shows 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 IPV offences 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 men that typically commit IPV homicides might not be the ones now reported for committing less severe IPV offences as a consequence of the creation of IPV courts. This might be the case for example if those men committing IPV homicides do not typically commit less severe forms of IPV before murdering or if men that are more prone to commit homicides do also commit less severe IPV offences but are not those reported more in response to the creation of IPV courts. Unfortunately, we would need inexistent individual and case-specific data to disentangle the different hypotheses that could explain why the increase in the report of IPV does not lead to a reduction in the incidence of IPV homicides in our setting.

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

	Judicial districts with IPV court (N=60 districts)			Judicial districts without IPV court (N=353 districts)			
	N	Mean	SD	N	Mean	SD	Diff
Total IPV cases per 100,000 inhab	840	456.892	329.511	4,942	256.415	159.018	200.48***
IPV homicides per 100,000 inhab	840	0.189	0.407	4,942	0.131	0.643	0.06**
Time to resolution (days)	840	65.248	49.290	4,936	118.025	102.277	-52.78***
Sh convicted	840	0.759	0.189	4,585	0.790	0.233	-0.03*
Sh elevated	840	0.238	0.141	4,935	0.232	0.168	0.01
Sh dismissal	840	0.358	0.155	4,935	0.363	0.194	-0.00
Protection orders per 100,000 inhab	840	98.908	74.444	4,942	68.845	52.430	30.06***
Minor IPV offences per 100,000 inhab	840	26.596	23.946	4,942	18.388	23.530	8.21***
Less severe IPV offences per 100,000 inhab	840	278.212	229.689	4,942	146.195	109.964	132.02***
Severe IPV offences per 100,000 inhab	840	14.267	37.155	4,942	5.735	16.309	8.53***
Sexual IPV offences per 100,000 inhab	840	3.149	6.391	4,942	1.216	5.991	1.93***
Other IPV offences per 100,000 inhab	840	104.167	141.697	4,942	42.011	49.516	62.16***
Unemployment rate	840	17.147	7.706	4,942	18.338	8.217	-1.19*
Population	840	202,499.800	72,908.555	4,942	57,047.045	38,696.637	145,452.75***

Note: The table presents descriptive statistics for the judicial districts in the analytical sample. Therefore, judicial districts with an IPV court opened in 2005 are excluded from the table. 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 2: Descriptive Statistics: Judicial districts with IPV courts and matched controls before the opening of the IPV court

	Judicial districts with IPV court (N=60 districts)			Matched judicial districts without IPV court				
	N obs	Mean	SD	N obs	N districts	Mean	SD	Diff
Total IPV cases per 100,000 inhab	194	336.113	233.428	105	30	321.953	180.096	14.16
IPV homicides per 100,000 inhab	194	0.197	0.410	96	27	0.109	0.343	0.09
Time to resolution (days)	194	104.804	68.891	92	27	78.890	49.442	25.91*
Sh convicted	194	0.801	0.240	96	28	0.810	0.155	-0.01
Sh elevated	194	0.303	0.154	91	26	0.279	0.125	0.02
Sh dismissal	194	0.381	0.209	89	25	0.398	0.236	-0.02
Protection orders per 100,000 inhab	194	93.544	71.401	69	22	70.063	50.988	23.48*
Minor IPV offences per 100,000 inhab	194	21.058	22.835	90	26	26.415	33.773	-5.36
Less severe IPV offences per 100,000 inhab	194	165.684	121.401	80	24	174.915	198.847	-9.23
Severe IPV offences per 100,000 inhab	194	7.726	15.039	79	24	6.134	13.508	1.59
Sexual IPV offences per 100,000 inhab	194	2.769	6.936	91	27	1.957	7.288	0.81
Other IPV offences per 100,000 inhab	194	100.025	84.797	77	23	75.835	83.412	24.19*

Note: Judicial districts with IPV courts (treatment districts) includes the 60 judicial districts with an IPV court opened between 2006 and 2011. The number of districts in the matched controls group changes for every outcome because more than one treatment district can be matched to the same control district and the matching sample is conducted separately for every outcome. A common control group for the estimation of the effects of the IPV court on all the outcomes is reported in the Appendix as a robustness check with reassuring results. Note that unlike in Table 1, descriptive statistics in this table are only presented for the period before the opening of the IPV court, which helps to explain the reduced number of observations. 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 3: Effect of opening an IPV court in the judicial district on length of judiciary process, the reporting of IPV and homicides

	(1) Time to disposition	(2) Total IPV cases per 100,000 inhab	(3) IPV homicides per 100,000 inhab
Estimation method (A): TWFE			
IPV Court × POST	-0.654***	0.235***	-0.056
	(0.177)	(0.064)	(0.040)
Year FE	YES	YES	YES
Judicial district FE	YES	YES	YES
Cohort FE	YES	YES	YES
Observations	1,218	1,260	1,218
Estim with dep. var logged/IHS	Yes	Yes	Yes
Mean	68.45	422.6	0.17
Estimation method (B): Callaway and Sant'Anna			
ATT	-0.606***	0.180***	0.009
	(0.130)	(0.056)	(0.064)
Dependent var logged/IHS	Yes	Yes	Yes

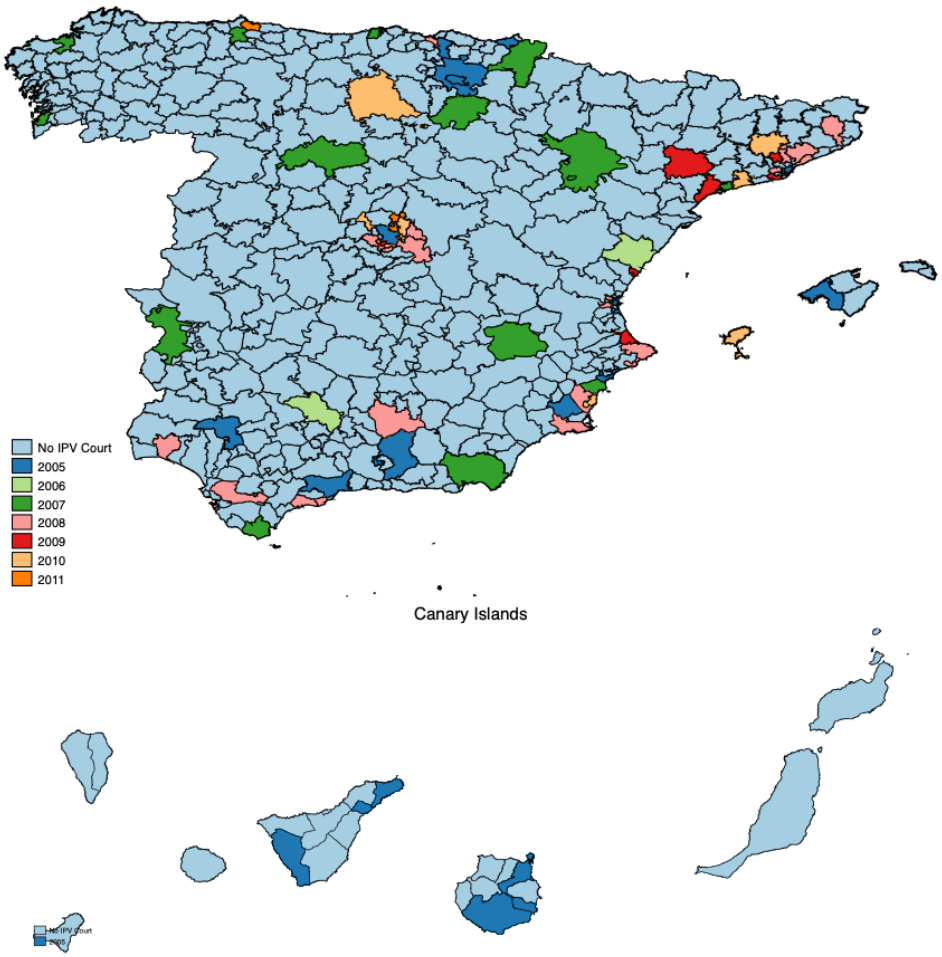
Note: ATT stands for average treatment on the treated difference-in-differences estimator developed in Callaway and Sant'Anna (2021). Matched controls are selected based on the pre-IPV court levels of the outcome variable, total IPV cases, the population of the judicial district in 2005 and the unemployment 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 dependent variable is the Ln of the variable because the outcome in all specifications is an integer or a continuous variable. When the value of the dependent variable is 0 in many observations, we use the inverse hyperbolic sine (IHS) transformation rather than taking Ln. IHS approximates the interpretation of the Ln and avoids the loss of observations. In all regressions, standard errors are clustered at the judicial district level. ***p<0.01;**p<0.05;*p<0.1

Table 4: Effect of opening an IPV court in the judicial district on the reporting of different offences and on judicial decisions

	(1) Minor IPV offences per 100,000 inhab	(2) Less severe IPV offences per 100,000 inhab	(3) Severe IPV offences per 100,000 inhab	(4) Sexual IPV offences per 100,000 inhab	(5) Other IPV offences per 100,000 inhab	(6) Sh convicted	(7) Sh elevated	(8) Sh dismissal	(9) Protection orders per 100,000 inhab
Estimation method (A): TWFE									
IPV Court × POST	0.095	0.359**	0.140	0.192	0.620**	0.011	-0.009	-0.022	-0.053
	(0.176)	(0.140)	(0.482)	(0.246)	(0.247)	(0.047)	(0.023)	(0.040)	(0.160)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,193	1,160	744	791	1,122	1,231	1,204	1,190	1,148
Estim with dep. var logged/IHS	Yes	Yes	Yes	Yes	Yes	No	No	No	Yes
Mean	27.23	256	12.53	2.62	88.69	0.76	0.23	0.37	92.89
Estimation method (B): Callaway and Sant'Anna									
ATT	-0.093	0.377***	0.135	-0.208	0.727***	0.011	0.004	-0.023	-0.103
	(0.148)	(0.127)	(0.372)	(0.233)	(0.223)	(0.040)	(0.024)	(0.037)	(0.148)
Dependent var logged/IHS	Yes	Yes	Yes	Yes	Yes	No	No	No	Yes

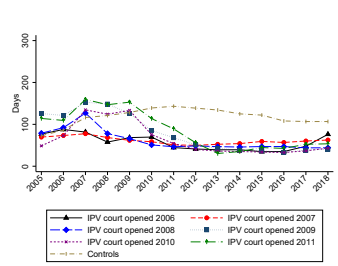
Note: ATT stands for average treatment on the treated difference-in-differences estimator developed in Callaway and Sant'Anna (2021). Matched controls are selected based on the pre-IPV court levels of the outcome variable, total IPV cases, the population of the judicial district in 2005 and the unemployment 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 dependent variable is the Ln of the variable when the outcome is an integer or a continuous variable (minor IPV offences, less severe IPV offences, severe IPV offences, sexual IPV offences, other IPV offences and protection orders). When the dependent variable is an integer or a continuous variable but the value is 0 in many observations, we use the inverse hyperbolic sine (IHS) transformation rather than taking Ln. The IHS approximates the interpretation of the Ln and avoids the loss of observations. The dependent variable is the value of the variable when the variable is a share (share of cases that ended with a conviction, share of cases elevated after investigation to be judged in the criminal court and the share of cases dismissed).
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 specialised IPV courts throughout Spain

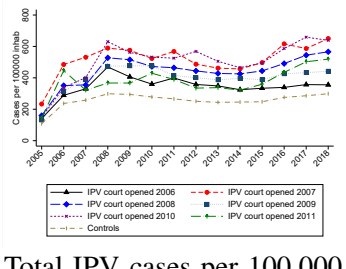


Note: These maps show which judicial districts in Spain have specialised IPV courts and their opening year.

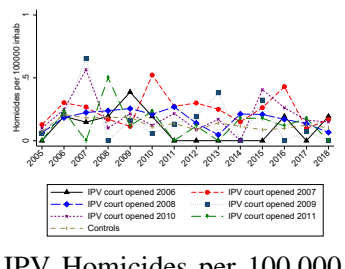
Figure 2: Evolution of outcomes over time: districts with and without an IPV court



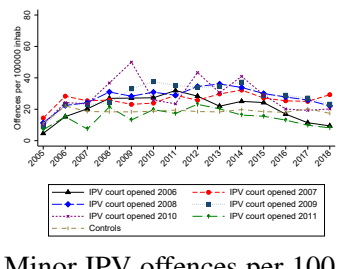
(a) Time to disposition (days)



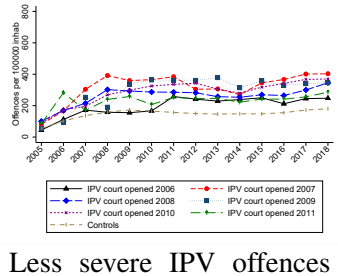
(b) Total IPV cases per 100,000 inhab



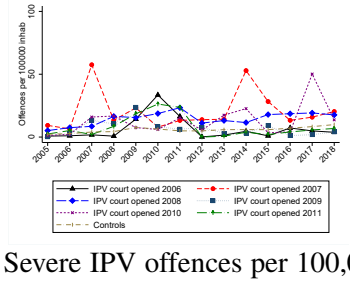
(c) IPV Homicides per 100,000 inhab



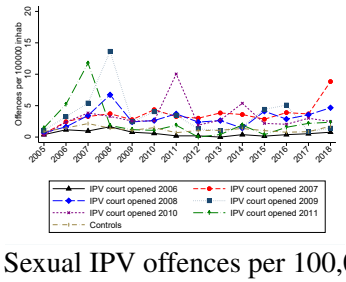
(d) Minor IPV offences per 100,000 inhab



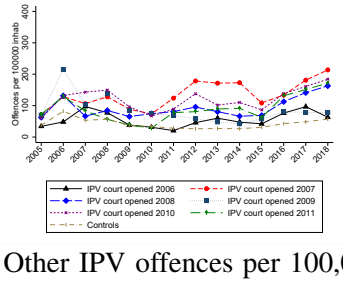
(e) Less severe IPV offences per 100,000 inhab



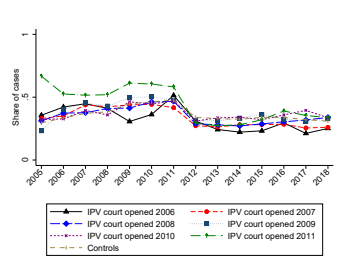
(f) Severe IPV offences per 100,000 inhab



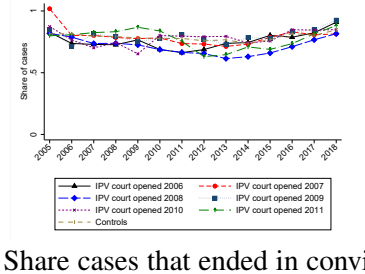
(g) Sexual IPV offences per 100,000 inhab



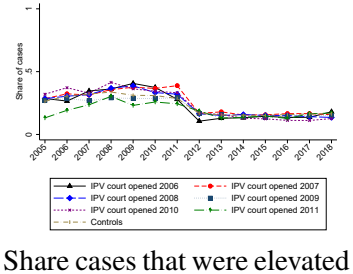
(h) Other IPV offences per 100,000 inhab



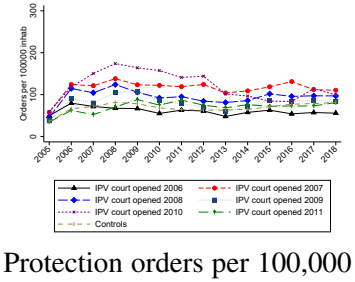
(i) Share cases dismissed



(j) Share cases that ended in conviction



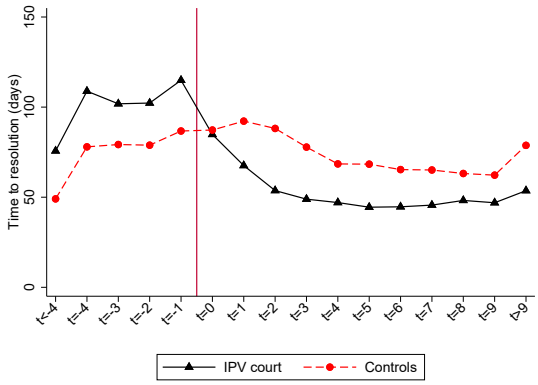
(k) Share cases that were elevated after investigation



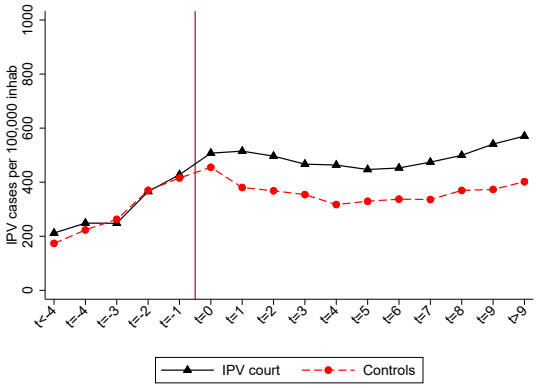
(l) Protection orders per 100,000 inhab

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.

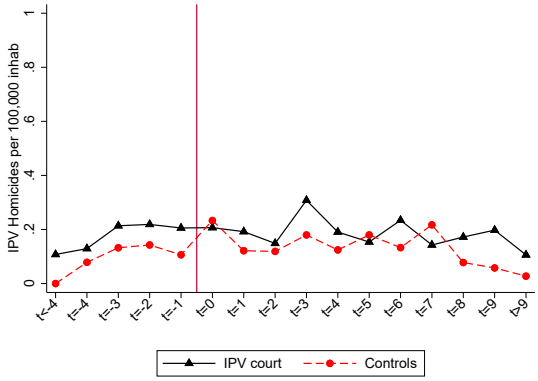
Figure 3: Evolution of the reporting of IPV cases, IPV homicides and time to disposition over time: treatment and matched control districts



(a) Time to disposition



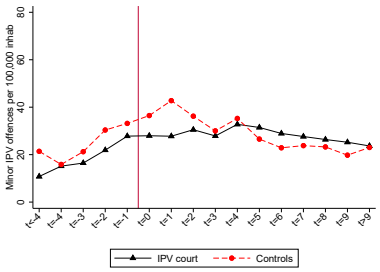
(b) Total IPV cases per 100,000 inhab



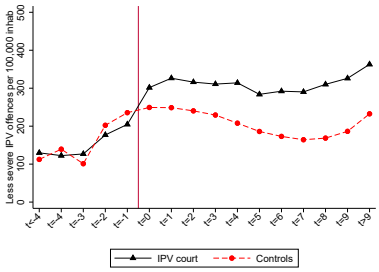
(c) IPV Homicides per 100,000 inhab

Note: This figure shows the evolution of the reported number of IPV cases, IPV homicides and time to disposition 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, IPV cases, population of the district in 2005 and the unemployment rate in the province in 2005.

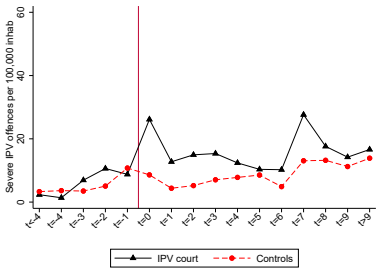
Figure 4: Evolution of the reporting of IPV offences by type of offence and judiciary decisions over time: treatment and matched control districts



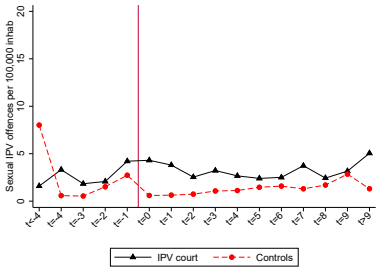
(a) Minor IPV offences per 100,000 inhab



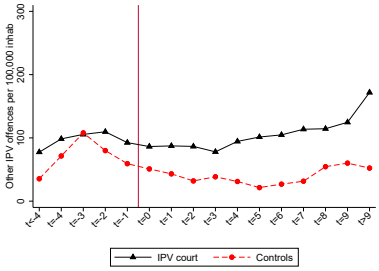
(b) Less severe IPV offences per 100,000 inhab



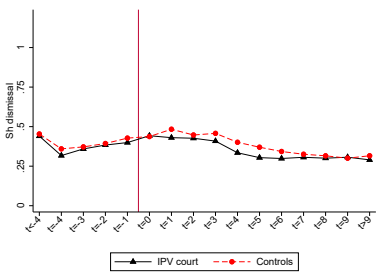
(c) Severe IPV offences per 100,000 inhab



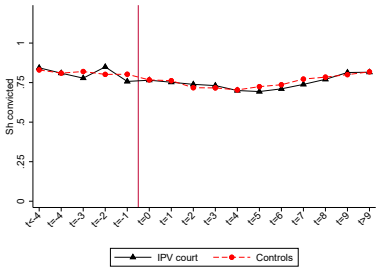
(d) Sexual IPV offences per 100,000 inhab



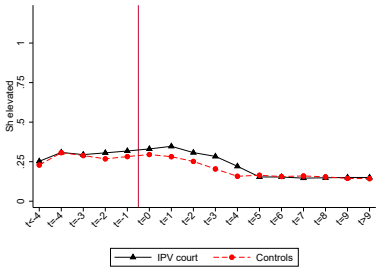
(e) Other IPV offences per 100,000 inhab



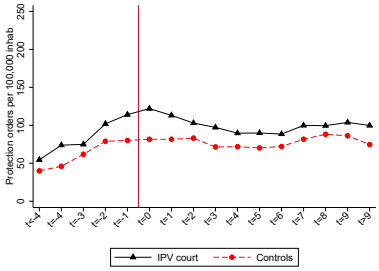
(f) Share cases dismissed



(g) Share cases that ended in conviction



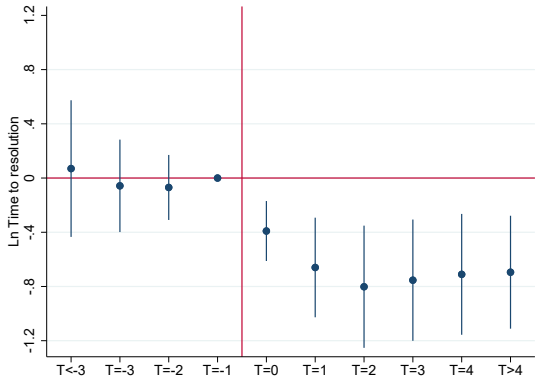
(h) Share cases that were elevated after investigation



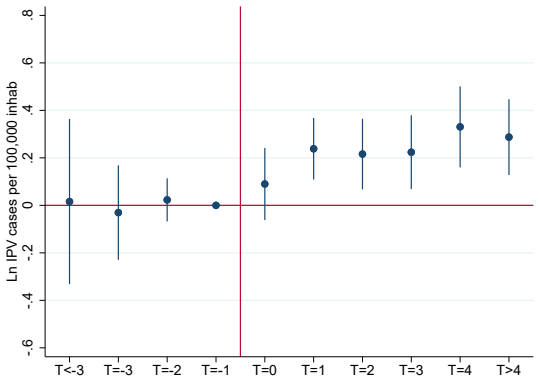
(i) Protection orders per 100,000 inhab

Note: This figure shows the evolution of the reporting of IPV offences by type of offence and of judiciary outcomes 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, IPV cases, population of the district in 2005 and the unemployment rate in the province in 2005.

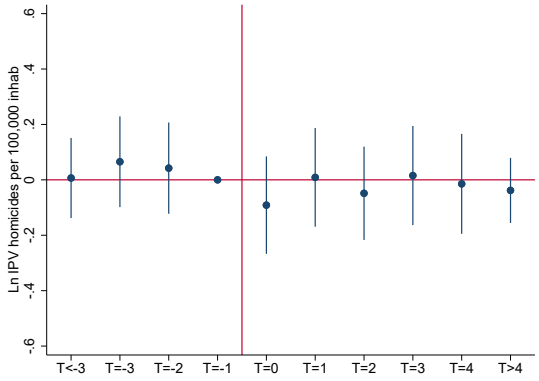
Figure 5: Leads and lags estimation of the effect of IPV courts on the reporting of IPV cases, IPV homicides and time to disposition



(a) Time to disposition



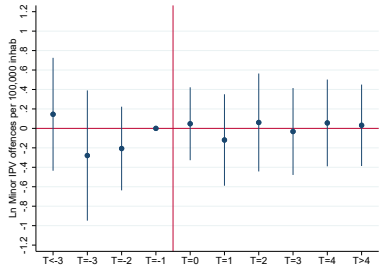
(b) Total IPV cases per 100,000 inhab



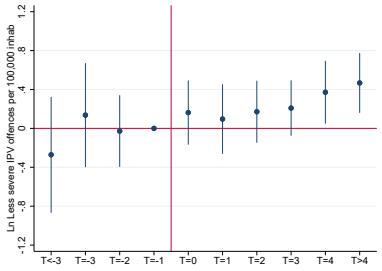
(c) IPV Homicides per 100,000 inhab

Note: This figure shows the results of a leads and lags estimation of the effect of opening an IPV court on the reporting of IPV cases, IPV homicides and time to disposition 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 court values of the outcome of interest, IPV cases, population of the district in 2005 and the unemployment rate in the province in 2005.

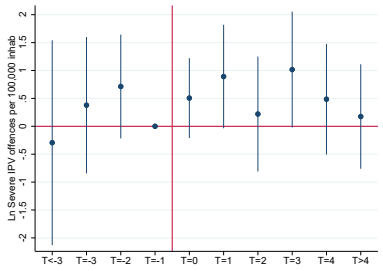
Figure 6: Leads and lags estimation of the effect of IPV courts on the reporting of IPV offences by type of offence and judiciary decisions



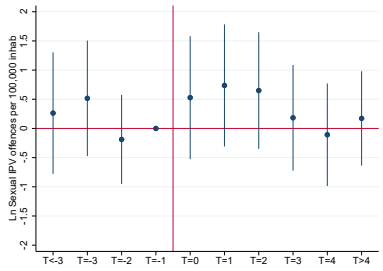
(a) Minor IPV offences per 100,000 inhab



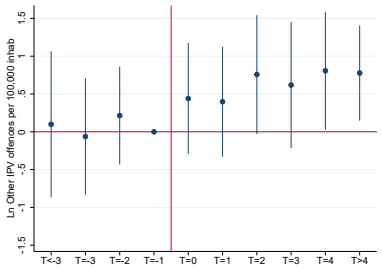
(b) Less severe IPV offences per 100,000 inhab



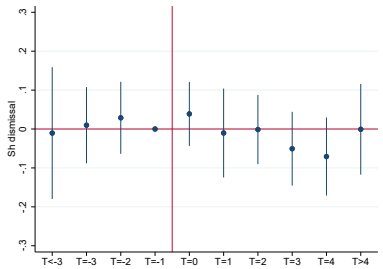
(c) Severe IPV offences per 100,000 inhab



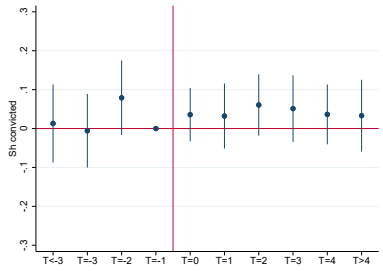
(d) Sexual IPV offences per 100,000 inhab



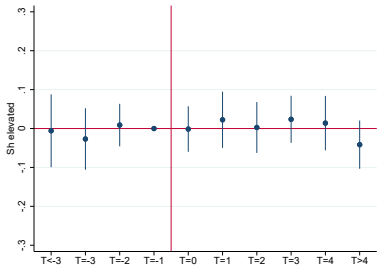
(e) Other IPV offences per 100,000 inhab



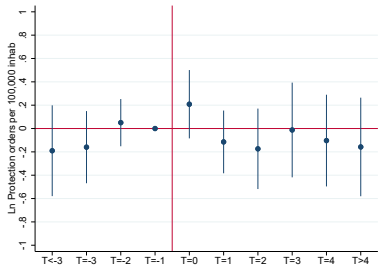
(f) Share cases dismissed



(g) Share cases that ended in conviction



(h) Share cases that were elevated after investigation



(i) Protection orders per 100,000 inhab

Note: This figure shows the results of a leads and lags estimation of the effect of opening an IPV court on the reporting of IPV offences by type of offence and on judiciary outcomes 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 court values of the outcome of interest, IPV cases, population of the district in 2005 and the unemployment rate in the province in 2005.

A Additional tables and figures

Table A1: Descriptive Statistics: Judicial districts with and without IPV courts (including judicial districts with an IPV court created in 2005)

	Judicial districts with IPV court (N=76 districts)			Judicial districts without IPV court (N=353 districts)			
	N	Mean	SD	N	Mean	SD	Diff
Total IPV cases per 100,000 inhab	1,064	458.786	306.599	4,942	256.415	159.018	202.37***
IPV homicides per 100,000 inhab	1,064	0.186	0.375	4,942	0.131	0.643	0.05**
Time to resolution (days)	1,064	64.586	47.411	4,936	118.025	102.277	-53.23***
Sh convicted	1,064	0.756	0.186	4,585	0.790	0.233	-0.03**
Sh elevated	1,064	0.234	0.144	4,935	0.232	0.168	0.00
Sh dismissal	1,064	0.362	0.157	4,935	0.363	0.194	-0.00
Protection orders per 100,000 inhab	1,064	98.346	71.000	4,942	68.845	52.430	29.50***
Minor IPV offences per 100,000 inhab	1,064	28.032	24.386	4,942	18.388	23.530	9.64***
Less severe IPV offences per 100,000 inhab	1,064	288.477	218.879	4,942	146.195	109.964	142.28***
Severe IPV offences per 100,000 inhab	1,064	15.442	35.852	4,942	5.735	16.309	9.71***
Sexual IPV offences per 100,000 inhab	1,064	3.060	5.844	4,942	1.216	5.991	1.84***
Other IPV offences per 100,000 inhab	1,064	107.429	131.473	4,942	42.011	49.516	65.42***
Unemployment rate	1,064	17.510	7.910	4,942	18.338	8.217	-0.87
Population	1,064	304,782.158	393,927.860	4,942	57,047.045	38,696.637	248,056.50***

Note: The table presents descriptive statistics for all the judicial districts in Spain. These also includes the judicial districts with an IPV court opened in 2005. 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 A2: Descriptive Statistics measured in 2005 before opening of IPV courts (analytical sample)

	Judicial districts with IPV court (N=60 districts)			Judicial districts without IPV court (N=353 districts)			
	N	Mean	SD	N	Mean	SD	Diff
Total IPV cases per 100,000 inhab	60	171.669	143.199	353	107.873	62.492	63.80***
IPV homicides per 100,000 inhab	60	0.078	0.220	353	0.075	0.520	0.00
Time to resolution (days)	60	80.468	54.947	350	72.872	53.934	7.60
Sh convicted	60	0.879	0.337	328	0.815	0.245	0.06
Sh elevated	60	0.280	0.151	349	0.264	0.211	0.02
Sh dismissal	60	0.327	0.213	349	0.296	0.253	0.03
Protection orders per 100,000 inhab	60	48.798	36.286	353	35.630	22.969	13.17**
Minor IPV offences per 100,000 inhab	60	11.239	9.806	353	10.908	15.239	0.33
Less severe IPV offences per 100,000 inhab	60	85.434	79.511	353	54.396	48.377	31.04***
Severe IPV offences per 100,000 inhab	60	4.735	10.036	353	2.238	8.103	2.50*
Sexual IPV offences per 100,000 inhab	60	0.687	0.910	353	0.873	3.977	-0.19
Other IPV offences per 100,000 inhab	60	64.788	56.380	353	37.251	42.931	27.54***
Unemployment rate	60	8.901	3.199	355	10.134	3.364	-1.23**
Population	60	202,499.800	73,480.051	355	56,725.654	38,873.902	145,774.15***

Note: Table includes descriptive statistics measured in 2005 excluding judicial districts with an IPV court opened in 2005. Thus, none of the districts included in the table has an IPV court in 2005 and differences between groups are therefore not affected by the creation of IPV specialised courts. 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 A3: Effect of opening an IPV court in the judicial district: Analysis conducted using Region×Year fixed effects

	(1) Time to disposition	(2) Total IPV cases per 100,000 inhab	(3) IPV homicides per 100,000 inhab	(4) Minor IPV offences per 100,000 inhab	(5) Less severe IPV offences per 100,000 inhab	(6) Severe IPV offences per 100,000 inhab	(7) Sexual IPV offences per 100,000 inhab	(8) Other IPV offences per 100,000 inhab	(9) Sh convicted	(10) Sh elevated	(11) Sh dismissal	(12) Protection orders per 100,000 inhab
IPV Court × POST	-0.708***	0.238***	-0.061	0.072	0.442***	-0.065	0.094	0.616*	-0.009	-0.030	-0.009	-0.171
	(0.219)	(0.065)	(0.052)	(0.196)	(0.156)	(0.592)	(0.299)	(0.318)	(0.082)	(0.030)	(0.045)	(0.196)
Region × Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,218	1,260	1,218	1,193	1,160	744	791	1,122	1,231	1,204	1,190	1,148
Dependent var logged/IHS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	Yes
Mean	68.45	422.6	0.17	27.23	256	12.53	2.62	88.69	0.76	0.23	0.37	92.89

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 dependent variable is the Ln of the variable when the outcome is an integer or a continuous variable (IPV cases, IPV homicides, time to disposition, minor IPV offences, less severe IPV offences, severe IPV offences, sexual IPV offences, other IPV offences and protection orders). When the dependent variable is an integer or a continuous variable but the value is 0 in many observations, we use the inverse hyperbolic sine (IHS) transformation rather than taking Ln. The IHS approximates the interpretation of the Ln and avoids the loss of observations. The dependent variable is the value of the variable when the variable is a share (share of cases that ended with a conviction, share of cases elevated after investigation to be judged in the criminal court and the share of cases dismissed). In all regressions, standard errors are clustered at the judicial district level. ***p<0.01;**p<0.05;*p<0.1

Table A4: 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 per 100,000 inhab	(3) IPV homicides per 100,000 inhab	(4) Minor IPV offences per 100,000 inhab	(5) Less severe IPV offences per 100,000 inhab	(6) Severe IPV offences per 100,000 inhab	(7) Sexual IPV offences per 100,000 inhab	(8) Other IPV offences per 100,000 inhab	(9) Sh convicted	(10) Sh elevated	(11) Sh dismissal	(12) Protection orders per 100,000 inhab
IPV Court × POST	-0.664***	0.235***	-0.056	0.096	0.366***	0.133	0.101	0.634**	0.010	-0.009	-0.022	-0.078
	(0.172)	(0.064)	(0.040)	(0.169)	(0.129)	(0.471)	(0.300)	(0.252)	(0.048)	(0.023)	(0.040)	(0.148)
Unemployment	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,218	1,260	1,218	1,193	1,160	744	791	1,122	1,231	1,204	1,190	1,148
Dependent var logged/IHS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	Yes
Mean	68.45	422.6	0.17	27.23	256	12.53	2.62	88.69	0.76	0.23	0.37	92.89

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 includes region-year FE, judicial district FE and cohort FE. The dependent variable is the Ln of the variable when the outcome is an integer or a continuous variable (IPV cases, IPV homicides, time to disposition, minor IPV offences, less severe IPV offences, severe IPV offences, sexual IPV offences, other IPV offences and protection orders). When the dependent variable is an integer or a continuous variable but the value is 0 in many observations, we use the inverse hyperbolic sine (IHS) transformation rather than taking Ln. The IHS approximates the interpretation of the Ln and avoids the loss of observations. The dependent variable is the value of the variable when the variable is a share (share of cases that ended with a conviction, share of cases elevated after investigation to be judged in the criminal court and the share of cases dismissed). In all regressions, standard errors are clustered at the judicial district level. ***p<0.01;**p<0.05;*p<0.1

Table A5: Effect of opening an IPV court in the judicial district: Analysis conducted using the synthetic difference-in-differences estimator

	(1) Time to disposition	(2) Total IPV cases per 100,000 inhab	(3) IPV homicides per 100,000 inhab	(4) Minor IPV offences per 100,000 inhab	(5) Less severe IPV offences per 100,000 inhab	(6) Severe IPV offences per 100,000 inhab	(7) Sexual IPV offences per 100,000 inhab	(8) Other IPV offences per 100,000 inhab	(9) Sh convicted	(10) Sh elevated	(11) Sh dismissal	(12) Protection orders per 100,000 inhab
ATT	-65.836***	98.537***	0.006	-0.160	53.405**	5.261*	0.844	23.354	-0.008	0.004	-0.038*	-22.968***
	(8.239)	(25.718)	(0.044)	(3.131)	(25.898)	(2.992)	(0.978)	(16.848)	(0.025)	(0.017)	(0.021)	(8.669)
Effect as % of dep var	-63	17	3	-1	32	68	30	23	-1	1	-10	-25
Mean	104.80	593.01	0.20	21.06	165.68	7.73	2.77	100.02	0.76	0.30	0.38	93.54

Note: ATT stands for average treatment on the synthetic difference-in-differences estimator developed in Arkhangelsky et al. (2021). The synthetic controls are constructed using the pre-IPV court levels of the outcome variable, total number of IPV cases, population of the judicial district in 2005 and unemployment rate in the province. Following standard practice in the use of synthetic controls, the dependent variables are all in levels rather than in logs. The effect as a percentage of the dependent variable for the treatment municipalities before the opening of the IPV courts are also provided to facilitate interpretation of the coefficients. ***p<0.01;**p<0.05;*p<0.1

B Matching based on Stepwise selected covariates

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, number of IPV cases, the population of the district and unemployment rate in the province, we match on a set of variables selected using Stepwise regression methods to optimize the prediction of treatment status. The pool of potential matching variables includes the pre-treatment values of the number of IPV cases, time to disposition, minor IPV offences, less severe IPV offences, severe IPV offences, sexual IPV offences, other IPV offences, share of cases that ended with conviction, share of cases dismissed, share of cases elevated, protection orders, 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). In this analysis, the estimation of the effect of the IPV courts on the different outcomes is conducted using the same sample of treatment and matched cotrol districts.

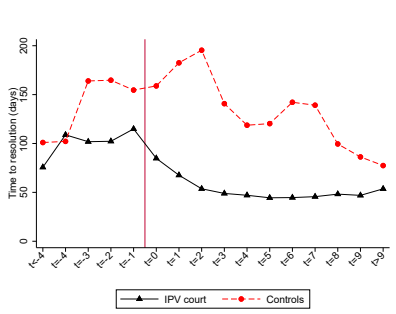
The results of this analysis are reported in Table B1 and Figure B1. While the estimates should be taken with caution due to some concerns with the fulfilment of the parallel trends condition for some outcomes, the results of this robustness check are overall consistent in terms of the direction of the effect and statistical significance with those reported in the main analysis.

Table B1: Effect of opening an IPV court in the judicial district (selection of matched control districts based on variables selected using Stepwise regression methods)

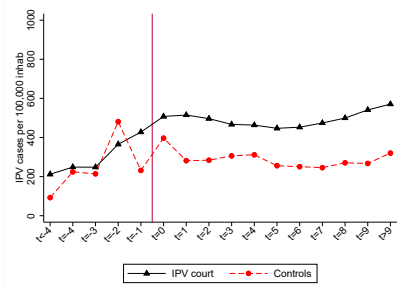
	(1) Time to disposition	(2) Total IPV cases per 100,000 inhab	(3) IPV homicides per 100,000 inhab	(4) Minor IPV offences per 100,000 inhab	(5) Less severe IPV offences per 100,000 inhab	(6) Severe IPV offences per 100,000 inhab	(7) Sexual IPV offences per 100,000 inhab	(8) Other IPV offences per 100,000 inhab	(9) Sh convicted	(10) Sh elevated	(11) Sh dismissal	(12) Protection orders per 100,000 inhab
Estimation method (A): TWFE												
IPV Court × POST	-0.710***	0.167**	-0.002	0.475	0.651***	0.001	0.241	0.723	-0.105	0.038	-0.055	-0.477
	(0.146)	(0.083)	(0.036)	(0.567)	(0.235)	(0.461)	(0.309)	(0.685)	(0.104)	(0.039)	(0.075)	(0.386)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Judicial district FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	952	952	952	952	952	952	952	952	949	952	952	952
Estim with dep. var logged/IHS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	Yes
Mean	73.47	437	0.172	26.60	264.6	13.37	3.007	95.78	0.766	0.237	0.358	95.75
Estimation method (B): Callaway and Sant'Anna												
ATT	-0.710***	0.131*	-0.023	0.206	-0.256	0.042	0.349	0.907*	-0.024	0.047	-0.057	-0.278
	(0.141)	(0.074)	(0.053)	(0.293)	(0.452)	(0.402)	(0.302)	(0.471)	(0.062)	(0.044)	(0.062)	(0.276)
Dependent var logged/IHS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	Yes

Note: ATT stands for average treatment on the treated difference-in-difefrences 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 dependent variable is the Ln of the variable when the outcome is an integer or a continuous variable (IPV cases, IPV homicides, time to disposition, minor IPV offences, less severe IPV offences, severe IPV offences, sexual IPV offences, other IPV offences and protection orders). When the dependent variable is an integer or a continuous variable but the value is 0 in many observations, we use the inverse hyperbolic sine (IHS) transformation rather than taking Ln. The IHS approximates the interpretation of the Ln and avoids the loss of observations. The dependent variable is the value of the variable when the variable is a share (share of cases that ended with a conviction, share of cases elevated after investigation to be judged in the criminal court and the share of cases dismissed). In all of the regressions, standard errors are clustered at the judicial district level.***p<0.01;**p<0.05;*p<0.1

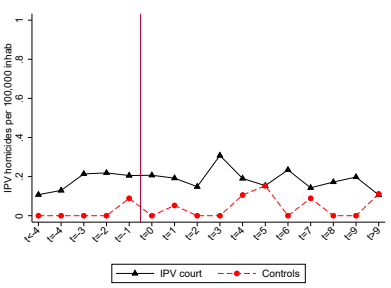
Figure B1: Evolution of the reporting of IPV offences by type of offence and judiciary decisions over time: treatment and matched control districts based on covariates selected using Stepwise techniques



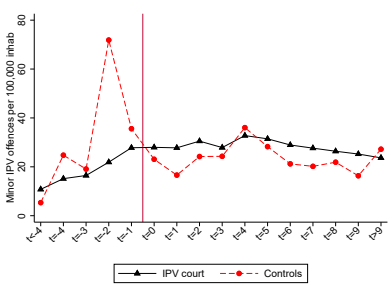
(a) Time to disposition



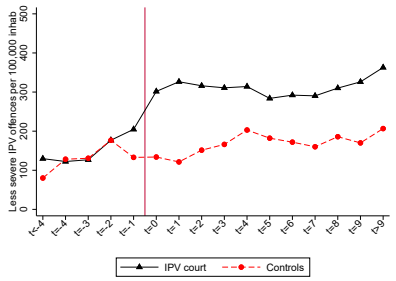
(b) Total IPV cases per 100,000 inhab



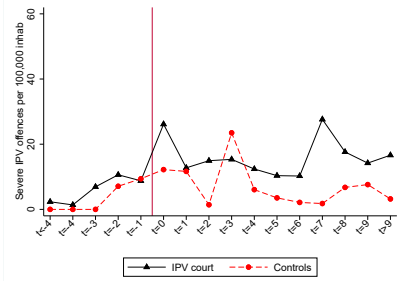
(c) IPV Homicides per 100,000 inhab



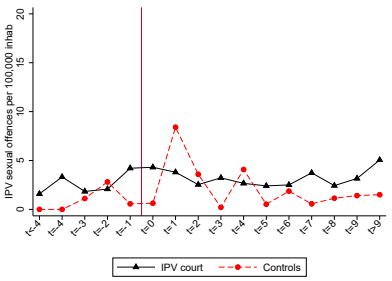
(d) Minor IPV offences per 100,000 inhab



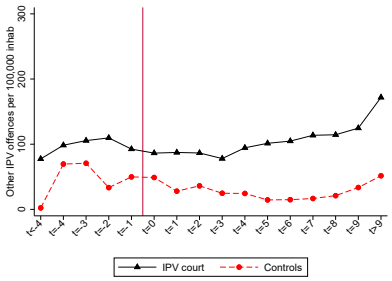
(e) Less severe IPV offences per 100,000 inhab



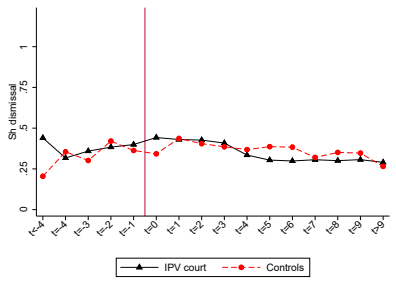
(f) Severe IPV offences per 100,000 inhab



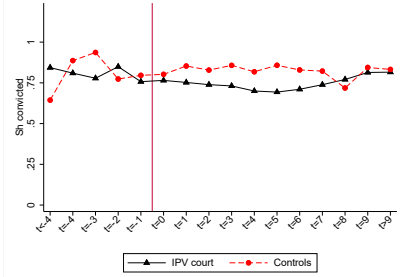
(g) Sexual IPV offences per 100,000 inhab



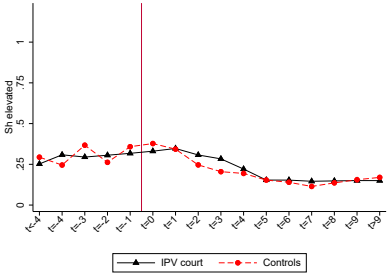
(h) Other IPV offences per 100,000 inhab



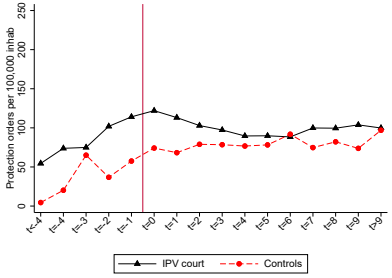
(i) Share cases dismissed



(j) Share cases that ended in conviction



(k) Share cases that were elevated after investigation



(l) Protection orders per 100,000 inhab

Note: This figure shows the evolution of the reporting of IPV offences by type of offence and of judiciary outcomes 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, IPV cases, population of the district in 2005 and the unemployment rate in the province in 2005.

C Effect of specialised IPV courts on the incidence of IPV and on other outcomes

One potential concern with the results reported in the main analysis is that men could react to the opening of IPV courts by increasing the incidence of IPV. While this hypothesis seems unlikely in our setting, previous studies have documented that interventions that increase women's bargaining power can trigger the incidence of IPV in countries such as Cambodia or Turkey (Erten and Keskin, 2021a, 2018, 2021b). If this is the case, the increase in the number of reported cases of IPV might be the result of a higher level of IPV and not of a higher share of crimes reported. On the other hand, it is also possible that if IPV courts decrease the incidence of severe IPV offences, the lack of effect on the reported number of these offences spotted in the previous analysis might be the result of opposite effects on the incidence of these offences (negative) and on the probability of reporting these offences (positive).

In this appendix we first investigate the effect of specialised IPV courts on the incidence of IPV using 6 rounds of the gender-based violence survey conducted by the Spanish government in 1999, 2002, 2006, 2011, 2015 and 2019. In every round, the survey collects individual level information from a nationally representative sample of women on the incidence of 4 types of IPV: psychological, physical, sexual and economic. A crucial aspect of this repeated cross-section survey is that the smallest geographical identifier of the women in the survey is the province. Because each province include several judicial districts, the variable measuring exposure to IPV court is defined at the province level. Specifically, we estimate the following regression:

$$Y_{ipt} = \beta Sh\ pop\ with\ IPV\ court_{pt} + Year_t + Province_p + u_{ipt} \tag{C.1}$$

where Y_{ipt} measures whether woman i, from province p, interviewed in year t, is suffering each type of IPV violence. *Sh pop with IPV court* is the treatment variable and measures the share of the population living in province p in year t that live in a judicial district with an IPV court. The regression also includes a vector of year of the survey and province fixed effects. u is the error term. The regression is estimated with and without unemployment in the province as control variable. The parameter of interest in the regression is β, which measures the effect of changing

from 0 to 1 the share of individuals in the province exposed to an IPV court on the incidence of IPV.

The results of this analysis are reported in Table C1. The estimates show no association of a higher exposure to IPV courts on the incidence of any type of IPV except for economic IPV. For this outcome, the association is positive and statistically significant at 10% when results are estimated including unemployment as a control variable. In line with the results observed for IPV homicides in section V, the estimates obtained in this analysis suggest small or null effects of IPV courts on the incidence of IPV. Overall, the results reassure the confidence that the positive effects of IPV courts on the reported number of IPV cases are driven by an increase in the reporting of IPV and not by an increase in the incidence of this type of violence.

Using province level data, we also examine in this section the effect of specialised IPV courts on other outcomes such as number of suicides, homicides, hospitalizations or calls to 016, which is a free phone number set by the Spanish Ministry for Equality to provide information about gender-based violence. Unfortunately, information at the judicial district or municipality level for these outcomes is not available. Thus we build a province level database with yearly information on total number of suicides, number of females committing suicides, number of homicides, number of homicides to women, number of hospitalizations, number of hospitalizations of women, and number of calls to the IPV helpline. Using this dataset, we estimate the following regression including yearly information from the 52 Spanish provinces[32] for the period 2005-2018:[33]

$$Y_{pt} = \beta Sh\ pop\ with\ IPV\ court_{pt} + Year_t + Province_p + u_{pt} \tag{C.2}$$

where *Province* and *Year* are a vector of province and year fixed-effects. u is the error term and *Sh pop with IPV court* is the treatment variable. The latter varies between 0 and 1 and indicates the share of population in the province p that in year t live in a judicial district with an IPV court. In other words, the variable measures the share of the population in the province exposed to an IPV court. The parameter of primary interest is β, which measures how the outcome of interest Y changes when the share of the population in a province exposed to an IPV court changes from 0% to 100%.

[32]This includes the 50 Spanish provinces plus the autonomous cities of Ceuta and Melilla.
[33]The database only includes information on calls to the gender violence information number from 2008.

The results of this analysis are reported in Table C2. We do not see any significant association between a higher exposure to IPV courts and the total number of hospitalizations or hospitalizations of women. This result is consistent with the hypothesis that the opening of these courts do not reduce the incidence of severe IPV crimes. Similarly, we do not find any statistically significant association between the opening of IPV courts and the number of calls to the IPV helpline. Because calls to the IPV helpline do not generate an IPV case in the court, we interpret these results as evidence that IPV courts only affect the reporting of IPV to those institutions (police and court) for which the opening of an IPV court decreases the *cost* of reporting. The results also suggest no effects of the opening of IPV courts on suicides, on suicide of women, on the number of total homicides, or on the total number of women murdered. The latter result rules out the hypothesis that the absence of effects of the courts on IPV homicides are driven by a miscategorisation of IPV homicides.[34]

[34]IPV homicides are defined as those homicides committed by partners or former partners. Missing IPV homicides in our data would be those committed by the partner or former partner but where the police or the court does not know the latter fact.

Table C1: Effect of share of population in the province covered by an IVP court on the incidence of different types of IPV violence:

	(1) Psychological violence	(2) Psychological violence	(3) Physical violence	(4) Physical violence	(5) Sexual violence	(6) Sexual violence	(7) Economic violence	(8) Economic violence
Estimation method (A): TWFE								
Perc. pop with IPV court	0.014	0.017	-0.002	-0.002	0.008	0.008	0.007	0.008*
	(0.016)	(0.016)	(0.005)	(0.005)	(0.008)	(0.008)	(0.004)	(0.005)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES	YES	YES	YES
Province unemployment control	NO	YES	NO	YES	NO	YES	NO	YES
Observations	84,454	84,4545	84,454	84,454	84,454	84,454	84,454	84,454
Mean	0.111	0.111	0.0215	0.0215	0.0472	0.0472	0.0134	0.0134

Note: The analysis is conducted at the individual level. The analysis uses all available rounds from the Spanish gender-based violence survey rounds 1999, 2002, 2006, 2011, 2015 and 2019. The treatment variable is defined at the province level because the survey does not provide geographical identifier below the province. The treatment variable *Perc. pop with IPV court* is a continuous variable that ranges between 0 and 1 and measures the share of the population in the province that live in a judicial district with an IPV court. The results are reported using standard two-way fixed-effects. The dependent variable is whether the woman interviewed has suffered in the last 12 months this type of IPV violence.***p<0.01;**p<0.05;*p<0.1.

Table C2: Effect of share of population in the province covered by an IVP court on different outcomes:

	(1) Homicides per 100,000 inhab	(2) Women homicides per 100,000 inhab	(3) Suicides per 100,000 inhab	(4) Women suicides per 100,000 inhab	(5) Hospitalizations per 100,000 inhab	(6) Women hospitalizations per 100,000 inhab	(7) IPV Helpline calls per 100,000 inhab
Estimation method (A): TWFE							
Perc. pop with IPV court	0.115	-0.008	0.083	-0.008	0.032	0.040	0.082
	(0.117)	(0.096)	(0.077)	(0.126)	(0.045)	(0.044)	(0.164)
Year FE	YES	YES	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES	YES	YES
Unemployment control	YES	YES	YES	YES	YES	YES	YES
Observations	700	700	700	700	700	700	572
Estim with dep. var logged/IHS	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	1.052	0.377	13.60	3.054	15902	8297	206

Note: The analysis is conducted at the province level. The treatment variable *Perc. pop with IPV court* is a continuous variable that ranges between 0 and 1 and measures the share of the population in the province that live in a judicial district with a specialised IPV court. The results are reported using standard two-way fixed-effects. The dependent variable is the Ln of the variable. When the value of the dependent variable is 0 in some observations (suicides and homicides), we use the inverse hyperbolic sine (IHS) transformation rather than taking Ln. The IHS approximates the interpretation of the Ln and avoids the loss of observations. In all regressions, standard errors are clustered at the judicial district level. ***p<0.01;**p<0.05;*p<0.1

D Effect of the opening of an additional ordinary investigation court in the judicial district

In this appendix, we explore the effects of the opening of an additional ordinary investigation court in the judicial district. In districts without specialised IPV courts, the opening of a new ordinary investigation court are likely to reduce the length of the judiciary procedures for IPV and other criminal offences since cases are split across all the ordinary investigation courts in the district. However, these courts are not specialised neither endowed with additional resources to make the process for IPV victims less burdensome. Through isolating the celerity mechanism, estimating the effect of the opening of an additional ordinary investigation court in districts without an IPV court would be a useful exercise to understand the extend to which the effect of IPV courts spotted in Section V are driven by the improvement in celerity.

As a natural experiment, we exploit the opening of 7 ordinary investigation courts in judicial districts without an specialised IPV court in 2006, 2009 and 2011. Because the number of treatment observations is very low, we estimate the effects of the opening of an additional ordinary investigation court in the district using the synthetic difference-in-differences strategy developed in Arkhangelsky et al. (2021). We do not use the matching approach implemented in the main analysis of the paper because the small number of treatment units per cohort, which ranges between 1 and 3, makes standard matching approaches invalid for causal estimation. Finally, districts with specialised IPV courts are removed from the analysis since ordinary investigation courts in these districts do not investigate or judge IPV cases.

The results of this analysis are reported in Table D1. Given the small number of treatment units, the results and confidence intervals should be interpreted with caution. While non-statistically significant, the estimates suggest that the opening of a new ordinary investigation court reduced time to disposition in IPV cases in the district. Consistently, the coefficient measuring the effect of these openings on the reported number of IPV cases in the district indicates a positive and meaningful effect, although not statistically significant at conventional confidence levels. While they need to be interpreted with caution, the results of this analysis are consistent with the hypothesis that improvement in celerity of the resolution is an important mechanism to explain the beneficial effects of the opening of IPV specialised courts on the reporting of IPV.

Table D1: Effect of opening an additional ordinary investigation court in the judicial district:

	(1) Time to disposition	(2) Total IPV cases per 100,000 inhab	(3) IPV homicides per 100,000 inhab
ATT	-22.693	83.807	0.089
	(44.502)	(109.470)	(0.312)
Effect as % of dep var	-42	22	NA
Mean	53.80	383.13	0.00

Note: The analysis is conducted using the synthetic difference-in-differences estimator. ATT stands for average treatment on the treated synthetic difference-in-differences estimator developed in Arkhangelsky et al. (2021). The synthetic controls are constructed using the pre-new court levels of the outcome variable, total number of IPV cases, unemployment and population of the judicial district in 2005. Following standard practice in the use of synthetic controls, the dependent variables are all in levels rather than in logs. The effect as a percentage of the dependent variable for the treatment municipalities before the opening of the IPV courts are also provided to facilitate interpretation of the coefficients. The 7 new ordinary investigation courts that we investigate in this analysis were opened in the judicial districts of Arrecife, Guadalajara, Leon, Salamanca, San Cristobal de la Laguna, Telde and Torremolinos.***p<0.01;**p<0.05;*p<0.1