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Can Land Policy Be Gender-Neutral?
Evaluating the Impact of Mexico's 1992 Land Titling Reform On Female Empowerment

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Abstract. Many recent land titling reforms have failed to include a gender perspective. In a context of deep-rooted gender disparities, this omission can give rise to gendered effects on land rights. Further, empirical evidence points to a link between female land ownership and empowerment: land appears to be a determining factor for women's status in the household and in the community. This paper investigates whether, by unwittingly producing differential gender effects on land ownership, gender-neutral land policy might indirectly exacerbate asymmetries in gender relations. I evaluate the impact of one such policy – Mexico's 1992 land titling reform – on female empowerment. After compiling a novel dataset, I analyze the locality-level effects of the reform on three outcomes of female empowerment: fertility, educational attainments, and femicides. I exploit spatial and temporal variation in reform rollout to identify causal effects using an event study design. My main findings are three-fold. First, fertility appears to be negatively affected through the increase in tenure security. Second, both female and male educational attainments are found to decrease in treated localities, which might indicate a reduction in female empowerment; however, this result is not robust to controlling for outmigration. Third, the femicide rate is found to increase in treated localities by 6 to 8 deaths per 100,000 women. This finding is robust to a variety of tests and suggests that female empowerment decreased as a result of the reform, consistent with the bargaining framework adopted to explain gender relations.

Keywords: Land titling, Gender, Land rights, Bargaining power, Ejido, Mexico

JEL: D13, J16, K42, O13, O17, Q15

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All errors are my own.

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Glossary

Comunidad agraria (*singular*), **comunidades agrarias** (*plural*)**:** Agrarian community resulting from the recognition and/or restitution of collectively-owned indigenous lands. Together with the ejidos, comunidades agrarias were formally established following the Mexican Revolution; although, unlike ejidos, they originated in an ancestral right to the land, anterior to the Revolution.

Dominio pleno: The form of land tenure typical of an ejido which has been transformed into private property. Dominio pleno (full domain) converts the land from ejido land to private property. The result is one (or more) ownership title belonging to an individual, governed by civil law and no longer subject to the ejido regime.

Ejidataria (*female*), **ejidatario** (*male*)**:** Member of the ejido, entitled to land within the ejido. Membership in the ejido includes usufruct rights over individual parcels. Unless explicitly stated otherwise, the term 'ejidatarios' (*plural*) refers to both male and female ejido members.

Ejido (*singular*), **ejidos** (*plural*)**:** Collective landholding unit whose members have usufruct rights rather than ownership rights to land. The ejido system is a land tenancy system unique to Mexico, first established following the Mexican Revolution, when land was expropriated from large landholders and redistributed to the peasantry. The entire process took place over the 1914-1992 period.

INEGI (Instituto Nacional de Estadística, Geografía e Informática)**:** The National Institute of Statistics and Geography in Mexico.

RAN (Registro Agrario Nacional)**:** The National Agrarian Registry of Mexico.

1 Introduction

> “Women have had limited access to land nearly everywhere throughout history. Agrarian reform and resettlement programmes have failed to solve this problem – indeed they have aggravated it by allocating land to the head of the family, who is presumed to be a man. Those responsible for the design and execution of such programmes have paid little attention to the question of who is really responsible for the household or productive unit.” (FAO, 1999)

For rural women and men, land is often the most important household asset, providing a source of livelihood and income generation, collateral for credit and a means of insurance for the future. In addition to being central to economic empowerment, land is also a social asset, with the potential to expand capabilities and strengthen participation in decision-making. Granting secure tenure rights and control over land is therefore key to boosting economic and social development. Land policy, in particular land titling, has the crucial role of regulating the security and distribution of land rights and defining the conditions of use and access to land. In recent decades, large-scale land titling programs have been widely promoted by national governments and international development agencies, representing a defining moment for rural property rights. While the economic benefits of land titling have received considerable attention, the differential gender effects of these programs are often overlooked. Policy-makers typically deem it sufficient to aim for “gender neutrality”, on the assumption that women and men will be affected equally. However, this assumption is not plausible in a context of deep-rooted gender disparities. Long-term cumulative processes of discrimination have in fact created a substantial gender gap in land rights: globally, less than 15 percent of all landholders are women, although nearly half of the agricultural labor force is female (FAO, 2018). Barriers to female landownership often had a legal nature in the past, but nowadays mostly stem from traditional customs and practices, as well as power structures within communities and households. The failure of land policy to include a gender perspective in spite of these barriers not only maintains status quo but also compounds the disparities in land ownership.

Increasing empirical evidence points to a link between female land ownership and empowerment. This empirical literature uses a bargaining framework to explain asymmetries in gender relations through the relative ownership of resources. Female empowerment is thought of in terms of capabilities, which are constituted by a woman’s agency (the ability to make choices) as well as her resources (the pre-condition to unlock agency) (Kabeer, 1999). Land ownership becomes a determining factor for women’s status in the household and in the community, as it strengthens women’s fallback position and thus their negotiating power. Therefore, if land policy unwittingly produces differential gender effects on land ownership, could it further aggravate gender-based issues – such as, for instance, violence against women?

This paper studies the impact of one such land policy – the land titling reform initiated by Mexico in 1992. This reform, which is still ongoing, was designed with the aim to grant individual land titles over communally held land known as the ejido. Ejidos are agrarian communities established through a century-long process of land redistribution, in which community members hold usufruct rights over individual agricultural plots. With the 1992 land titling reform, ejido members were given the possibility to opt for dominio pleno, which meant converting their usufruct rights in the ejido into full private property rights under civil law. As the reform was purported to be gender neutral, it did not include any gender-progressive norms, nor did it take into account existing gender disparities in land rights. In Mexico, these disparities are numerous and well-documented: women have largely been excluded from past land policy and land redistribution efforts, and despite equal statutory rights nowadays, cultural barriers still remain (Deere & León, 2001a). Through the lack of a gender perspective, some legal provisions of the reform unwittingly turned into mechanisms for women's exclusion. Specifically, the requirement for land to be titled in the name of the sole household head disproportionately favored men's land ownership. Also, as the decision-making power regarding the adoption of dominio pleno was placed in structures from which women had been historically excluded – the ejido assemblies – women were precluded from participating in the process. As a result, the reform ended up producing differential gender impacts on land ownership by disproportionately increasing men's land titles relative to women.

The hypothesis tested in this paper is that the gendered effects of Mexico's land titling reform on land ownership indirectly undermined female empowerment at the locality level. The purpose of my analysis is to investigate whether the lack of a gender perspective in land policy not only reinforces gender disparities in land ownership, but further exacerbates asymmetries in gender relations. To measure local female empowerment, I use three outcomes of bargaining power which are especially relevant for the Mexican context: fertility, educational attainments, and the incidence of femicides. For the scope of my analysis, I focus on the Bajío region, which is formed by seven states in central Mexico.

The gendered aspects of Mexico's land titling reform are extensively documented by qualitative research and small-scale case studies. However, to the best of my knowledge, there is no empirical evidence on the effects of the reform on female empowerment, largely due to the lack of easily accessible data for quantitative analysis. I address the lack of locality-level data on the adoption of dominio pleno by personally compiling a novel dataset using information extracted from the RAN archives. This dataset details treatment status and treatment year for all ejidos in the Bajío; it is then spatially matched with five different georeferenced datasets providing information on local outcomes and characteristics. The data thus assembled allows me to observe all localities located within ejidos in the Bajío, over the period 2000-2010.

My identification strategy is an event study design which exploits both the spatial and temporal variation in treatment assignment, since take-up is not universal and rollout is staggered. To minimize the threat of selection bias arising from the non-randomized nature of the treatment, I use late-treated localities as counterfactual. Namely, late-treated localities are known to have received treatment after 2010 but are not observed post-treatment due to time constraints on the data. When analyzing the impact of land titling on femicides, I restrict the analysis to urban localities, which have lower variance in the dependent variable. Due to the smaller sample size and the higher degree of comparability resulting from this selection, I retain never-treated urban localities as counterfactual for the femicides analysis. Therefore, the validity of my estimates rests on two alternative, crucial assumptions: first, that late-treated localities are a valid counterfactual for localities treated by 2010, and second, that never-treated urban localities are a valid counterfactual for urban localities treated by 2010.

The results of my empirical analysis are the following. First, I find that after adoption of dominio pleno, fertility actually decreases in localities where reliance on family networks is traditionally greater, such as those with a larger average household size. This finding suggests that the higher tenure security provided by land titling lowers the number of children desired by couples. However, it cannot prove whether the land titling reform is affecting female empowerment, since any potential repercussions of the empowerment mechanism on fertility are being more than offset by the tenure security mechanism.

Second, I show that the adoption of dominio pleno is linked to a reduction in both female and male average educational attainment. This would suggest that female bargaining power is shrinking in treated localities, under the assumption that mothers have a stronger preference for the educational investment of children. However, another mechanism might be causing the observed reduction in schooling. Namely, if land titling facilitated outmigration from treated localities, the levels of schooling might decrease due to the educational selectivity into outmigration. Since I cannot fully rule out this possibility, the finding on educational attainments is not conclusive of a decrease in female empowerment.

Last, I find that the adoption of dominio pleno gives rise to a substantial increase in the incidence of femicides in treated urban localities. I estimate an average yearly increase of 6 to 8 femicides per 100,000 women, although this effect might shrink over time. This result only holds when defining femicides as the number of total homicides with a female victim, but it proves to be otherwise robust to a variety of tests, despite the small sample size used. Also, the effect does not appear to be driven by differences in local occurrence of organized violence. Therefore, this increase in femicides suggests that female empowerment indeed decreases as a result of the reform, which is consistent with the bargaining framework adopted to explain gender relations. There are two possible reasons why a loss of female empowerment might translate into an increase in femicides. At the household level, the relative decrease in women's resources might reduce their abilities to escape violent or abusive relationships. At the community level, women's diminished economic role might reinforce the spatial division of genders, making it less socially legitimate – and thus, safe – to engage in activities outside the domestic sphere.

The contribution of my empirical analysis is therefore threefold. First, my analysis finds a plausible link between women's relative land ownership and an important outcome of female bargaining power – the incidence of femicides. This finding adds to the empirical literature on the link between female empowerment and asset ownership. The estimated effect suggests a substantial increase in the local incidence of femicides following the implementation of the land titling reform. Second, my analysis bridges the general literature evaluating land reforms with the feminist economics literature on the gender gap in land rights. I believe that my results highlight the importance of taking into account potential gendered impacts when designing as well as when evaluating land policies. Last, I add support to the extensive qualitative evidence which points to the failure of Mexico's 1992 land titling reform to include a gender perspective. My results show that the gendered effects of the reform went beyond widening the gender gap in land rights, and further exacerbated asymmetries in gender relations. The empirical evidence provided in this paper points to an increase in the incidence of femicides in localities affected by dominio pleno, which suggests that women lost bargaining power as a result of the reform.

The remainder of this paper is structured as follows. Section 2 provides context on Mexico's ejido system, the previous agrarian reforms and the land titling reform. Section 3 reviews the literature on the gendered aspects of the 1992 reform, as well as the literature on the link between female empowerment and asset ownership. Section 4 presents the data, describes the data collection process and the outcome variables chosen to measure female empowerment. Section 5 discusses the identification strategy, the expedient to identify a valid counterfactual and the identifying assumptions required for estimation. Section 6 presents the results while Section 7 discusses their limitations and validity as well as avenues for further research. Section 8 concludes.

2 Background

2.1 A brief history of the ejido

In 1910 the Mexican rural working class rose at the cry of "*Tierra y Libertad*" ("Land and Freedom"). Up until then, since colonial times, the land had been gradually concentrated in increasingly vaster latifundia. It was estimated that 87 percent of the land was held by only 0.2 percent of the landowners (Secretaría de la Reforma Agraria, 1998, p.35). This process of land concentration had resulted in the misplacement and near enslavement of the rural population, thus creating the grounds for revolutionary turmoil. Peasants fought to restore collective tenure on the land from which they had been dispossessed.

The result of the Revolution, in 1917, was to establish a constitutional right for the communities to be given access to agricultural land, and a ceiling to the extension of private property (Assies & Duhau, 2008). Through Article 27 of the Constitution, the so-called social property sector was created. It was constituted by two distinct agrarian institutions – ejidos and comunidades agrarias. While comunidades agrarias were formed through the restoration of land of which they had previously been dispossessed, ejidos were formed anew. Namely, land was granted to ejidos through a process of land distribution which followed the expropriation of the existing large landholdings (Appendini & Torres-Mazuera, 2018). This process of land redistribution took place up until 1992 – at the end of which, over half of the Mexican farmland constituted part of the social property sector, divided into some 28,000 ejidos and 2,300 comunidades (INEGI, 1990). The Revolution left also a deep cultural impact; land acquired significance beyond its mere economic value, as the ejido was by all means the conquest of the peasantry after the Revolution (Assies & Duhau, 2008).

The legal framework of the ejido was based on the concept that land should belong to the tiller. Hence, ejidatarios were only granted usufruct and residual claimant rights to their parcel of land within the ejido, and such rights were subject to conditions. Most notably, if they failed to personally cultivate their parcel for more than two consecutive years, it was reassigned to another member of the community. Also, land could not be sold, rented nor be used as a collateral. Each ejido was internally regulated and its organization was structured into three internal authorities – the *asamblea* (assembly), the *comisariado ejidal* (ejido executive board) and the *consejo de vigilancia* (supervisory committee). The most prominent of these organs was the assembly, which was constituted by all members with rights in the ejido and therefore held the decision-making power on all matters internal to the ejido.

2.2 The 1992 agrarian reform

In 1992, the Mexican government considerably reformed Article 27 of the Constitution. The main elements of the reform were the end of the land redistribution process and a series of initiatives that would fundamentally alter the reality of the ejido. First, it introduced the option of selling or renting individual parcels to other ejidatarios. Second, it launched a land certification program known as PROCEDE[1], which granted ejidatarios some advantages – such as the possibility to rent the land – without giving full ownership. Third, after obtaining land certificates through the PROCEDE program, ejidatarios were given the option to adopt dominio pleno and transform their plots into private property (Ramirez-Alvarez, 2019).

Set within a wider neoliberal shift which included the negotiation of the NAFTA, the 1992 agrarian reform sought to put an end to the social property sector by undoing its collective nature and strengthening individual property rights. Inspired in part by World Bank recommendations, the aim of the reform was to increase productivity within the agricultural sector and address tenure insecurity (Assies & Duhau, 2008). Advocates of the reform underlined its potential to incentivize investment, give access to credit, and enable an efficient distribution of land by incorporating ejido land into the market. In actual fact, these expectations were not fully met (Appendini & Torres-Mazuera, 2018). Ejidatarios mostly applied for dominio pleno in order to sell their land for urbanization, rather than to ask for credit using it as collateral (Galeana, 2004). Except for urban areas, a vibrant land market did not emerge (Assies & Duhau, 2008). In some areas, powerful actors with an interest in exploiting the land for non-agricultural purposes (e.g., tourism or mining) encroached upon the ejido, deepening tenure insecurity (Appendini & Torres-Mazuera, 2018). On the whole, dominio pleno turned out to be quite unpopular, contrary to expectations. While over 95 percent of ejido land has been certified through PROCEDE, only 5 percent has been privatized under dominio pleno (RAN, 2017b).

The procedure to obtain dominio pleno – which is still ongoing – entails the following steps. First, the necessary precondition is that all plots in the ejido have undergone certification through PROCEDE. At that point, any individual ejidatarios who aspire for full private property under civil law over their plots must present a formal request to the ejido assembly. To be successful, these requests then require a two-thirds majority vote approval by the assembly (Tribunales Agrarios, 1994). In practice, when one or more ejidatarios request and obtain dominio pleno over

[1] "*Programa de Certificación de Derechos Ejidales y Titulación de Solares*" ("The Program for Certification of Rights to Ejido Lands").

their plots, the rest of the ejido can choose to be fully privatized and thereupon dissolved, or to exist in a state of "partial dominio pleno" (partial privatization). Partial dominio pleno implies that any parcels which have been converted into private property are cancelled out of the ejido[2], which continues business as usual; however, the remaining ejidatarios can still apply for dominio pleno at a later time (Assies & Duhau, 2008). According to RAN, only a handful of ejidos have been fully privatized and disbanded, while the vast majority continue to exist in partial dominio pleno (personal communication, January 14, 2020).

3 Literature review

This paper argues that Mexico's 1992 land titling reform had gendered effects on land rights, due to the lack of a gender perspective; on that account, it investigates whether the reform more broadly worsened women's bargaining power with respect to men. Therefore, this paper draws on two strands of literature: first, the literature on the gendered aspects of the reform, second, the literature on the effects of economic resources on female empowerment.

In the next sections, I provide an overview of these two strands. In the first section, I discuss the qualitative evidence on the failure of the 1992 agrarian reform to include a gender perspective. To contextualize this failure and explain why the purported "gender neutrality" resulted effectively in "gender blindness", I outline the pre-existing gender gap in land rights and the history of women's exclusion from agrarian reforms. Then, I describe the specific legal provisions of the 1992 reform which contributed to consolidating these disparities. As these provisions disproportionately favored men, the reform produced an increase in men's land rights relatively to women.

In the second section, I construct a bargaining framework to explain the link between gender relations and the relative distribution of economic resources. Then, I present key findings from the empirical literature drawing from this bargaining framework. The focus is on the literature that studies land as a determinant of bargaining power, and on the literature that observes bargaining power outcomes through fertility, educational attainment or violence against women.

3.1 Gendered aspects of the 1992 reform

Gender-neutral policies are policies designed without a gender perspective, on the assumption that they will affect women and men equally. This assumption is rarely spot on in a context of pre-existing gender inequality. The failure to take into account gender disparities not only maintains status quo but may also aggravate gender inequality by producing differential impacts on women and men. When that is the case, gender neutrality effectively amounts to gender blindness. The point of departure for this paper is that the 1992 agrarian reform constituted a gender-blind land reform. In this section, I present the evidence behind this hypothesis by

[2] Likewise, members who opt for dominio pleno lose their voting rights in the ejido assembly and effectively exit the community.

reviewing the history of women's exclusion from agrarian reform, and the gender disparities existing prior to the reform.

Since its official start in 1917 – with the new Article 27 of the Constitution – the land redistribution effort that gradually gave shape to the ejido excluded and discriminated women (Deere, 1985). It was not until 1921 that women were deemed eligible to receive land within the ejido (Circular 48 of September 1st). Moreover, under the interpretation of Article 27 prescribed by Circular 48, women were only eligible for ejido rights on the conditions that they had dependents (such as children) and were single or widowed. Married women and single women with no dependents could therefore not qualify. Men, on the other hand, could apply for land irrespective of their family status. For all purposes, men and women were not recognized as equal in their legal capacity to obtain a land endowment (RAN, 2017a). Furthermore, the Agrarian Code of 1940 established that women with land in the ejido would lose such rights upon marriage with another ejido member (Article 139).

Rural women had to wait until 1971 to be formally recognized as equal to men in their rights to land. The agrarian reform of 1971, in fact, recognized for the first time ejidatario status equally to women and men (Deere & León, 2001a; RAN, 2017a). Potential land beneficiaries were subject to the same requirements independent of their gender[3] (Article 200); female ejido members were to have rights equal to those of male members (Article 45) and they no longer lost their land rights upon marriage (Article 78). If only from a legal point of view, women were no longer being discriminated against.

The agrarian reform of 1971 also incorporated the first effort to specifically address women and include them within the ejido, through the so called *parcela de la mujer* (woman's parcel). Namely, Articles 103 and 104 established that every ejido should reserve an area of land to be collectively exploited by women without land rights in the ejido for the purpose of agro-industrial activities (RAN, 2017a). Recommended activities included, for instance, childcare centers, group classes in sewing and cooking, and "all those installations destined specifically for the service and protection of the peasant woman" (Deere, 1985, p. 1047). This initiative has been deemed by the literature as severely ill-conceived, if not even counterproductive (Deere, 1985; Deere & León, 2001a; Valenzuela & Berlanga, 1996). Not only it failed to recognise women's roles as agricultural producers, but it also reinforced the sexual division of labor by confining women to activities that were merely an extension of their domestic roles (Deere, 1985). Moreover, very few ejidos actually complied with this provision; in some states, less than 10% of the ejidos established the *parcela* (Valenzuela & Berlanga, 1996, p. 13).

Overall, while the 1971 reform was a major accomplishment for the rights of rural women in Mexico, it was arguably inadequate to rectify the decades long discrimination. By then, it was too late for women to gain access to newly redistributed land through the reform. Most of the land redistribution had already taken place in the previous years, particularly up to 1940; with only a handful of short-lived redistribution attempts to come in the following years (Assies & Duhau,

[3] Those requirements were: "being Mexican by birth, male or female, over sixteen years of age – or of any age if with dependents" (Article 200).

2008). In fact, the impact of the 1971 reform on the share of female landowners was limited. In 1970, women accounted for barely 1.3 percent of ejidatarios (Valenzuela & Berlanga, 1996, p. 37). By the 1980s – a decade after the reform – they still averaged no more than 15 percent (Hamilton, 2002, p. 122). Besides, they were mostly widows, who had received the land not directly from the state but through inheritance from their late husbands.

Granting formal gender equality in access to land had not been enough to ensure gender equity within the ejidos. However, the government and land authorities did not acknowledge the ongoing vulnerability of rural women, and the design of the 1992 reform failed to include gender-progressive norms for the implementation of the land privatization process. The gender neutrality of the reform was considered sufficient to ensure gender equality, and no specific references were made to women's land rights. Deere and León, reviewing the gendered impacts of various land titling reforms that took place throughout Latin America, argue that "the least favorable legislation for gender equity is that which purports to be gender neutral" (2001b, p. 443). Measures ought to have been taken in order to address pre-existing vulnerabilities and prevent potential discrimination. In fact, the Latin America experiences proved that, in countries where such gender considerations were included[4], the negative effects of the privatization process on rural women were mitigated. In essence, of all the neoliberal land titling reforms that took place across the Latin American continent in the 1990s, the Mexican one stood out as one of the most prejudicial to rural women, according to a vast body of literature (Deere & León, 2001a; see also Bonfil, 1996; Botey, 2000; Stephen, 1993; Zapata, 1995).

There were two main channels through which the reform contributed to consolidating previous gender disparities and widening further the gender gap in land rights. First, a requirement for the land to be titled in the name of one household member only. Second, the exclusion of women and other household members from the decision-making process regarding the future of the ejidos and of their own land.

3.1.1 The head of household rule

The most important source of discrimination was the condition that only one household member – the household head – could be titled the land. By all means, this requirement did not consider the "bundle of rights"[5] contained within the household; a household's land endowment might be made up simultaneously by the wife's, the husband's, and the jointly owned property (Deere & León, 2001b). Instead, it was assumed that the family land was solely the property of the (designated) household head. Such an oversimplification gave rise to two main problems.

The first and more glaring consequence was that women were largely excluded from being the direct beneficiaries – since, in Mexico, "social custom dictates that, if both an adult man and an adult woman reside in a household, the man is considered the head" (Deere 1985, p. 1041; see also

[4] Most notably, in the cases of Colombia, Nicaragua and Honduras (Deere and León, 2001b).
[5] According to Peterman et al. (2014), "bundles of rights" are the gradients of control that exist over a resource, such as land. These rights include the right to use the land, the right to appropriate the return from land, the right to change its form, substance and location, and the right to alienate it. Different actors often overlap in their levels of rights.

Deere et al., 2004). In fact, according to the ENIGH[6] survey, self-declared female-headed households[7] represented a mere 14% of all Mexican households in 1992 (INEGI, 1993), and the share was likely even lower among ejidatario households. Therefore, the reform had a direct impact on the gender of landowners, allowing a relative increase in male land rights.

As for the second consequence, one must consider that the "head of household rule" was justified by the expectation that the entire household would benefit from a title granted to the household head. However, as I will explain in the next section, this rested on the unlikely assumption that the household head would act as a "benevolent dictator", as per the unitary model of the household. There is ample reason to believe that this expectation was not met. As a result, not only the reform ended up concentrating land titles in the hands of men, it also affected power balances within the household; men's new asset endowment increased their bargaining power relative to women's, leading to a decline in female status. I endeavor to prove this indirect effect of the reform on female bargaining power within my quantitative analysis.

In sum, the "head of household rule" turned the *patrimonio familiar* (family patrimony) into the individual property of the male household head (Botey, 2000; Deere & León, 2001b; Esparza-Salinas et al., 1996). It has been noticed by Botey that its formulation was unexpectedly at odds with the Mexican civil code. Namely, under the default marital property regime – that of community property – half of the common property of the couple should rightfully belong to each spouse (Botey, 2000, p. 154).

It is worth noting that the both the direct and the indirect consequences described above could have in fact been avoided through the prescription of joint titling. According to Deere and León, joint titling would have reinforced the notion of the household as dual-headed, it would have protected women's property rights against divorce or being disinherited, and it would have left unchanged or perhaps improved the gender power balances within the household (2003, 2001b). Their view receives confirmation in a vast number of empirical studies which found joint titling to have a positive effect on female empowerment (e.g. Allendorf, 2007; Datta, 2006; Deere & Twyman, 2012; Field, 2003; Menon et al., 2017; Wiig, 2013). Regrettably, though, "no serious discussion arose about whether, in this defining moment for private property rights, land should be jointly titled in the name of couples" (Deere & León, 2001a, p. 302).

3.1.2 The decision-making process

Another important factor behind the gendered impacts of the reform was the way decision-making took place at the community level, since it largely excluded women. As mentioned in Section 2.2, whether to privatize or not was decided internally by each ejido. More specifically, the privatization process required a two-thirds majority vote approval by the ejido assembly (Tribunales Agrarios, 1994).

[6] *Encuesta Nacional de Ingresos y Gastos de los Hogares* (National Survey of Household Income and Expenditures).
[7] Either households where both an adult woman and man are present and the woman is declared to be the household head, or households where only the adult woman is present (e.g., a widow, a single mother).

The ejido assembly is composed by all recognized ejidatarios, but only one member per household can participate – once again, the household head – effectively excluding spouses and other family members from decision making. Historically, the household member taking part in the assembly has been the man, and the ejido assemblies (as well as the other organs of the ejido) have therefore been heavily male dominated in their composition (Bonfil, 1996). As of 1996, ninety percent of ejidos had no women in any of the ejido organs (Valenzuela & Berlanga, 1996, p. 53).

The gendered composition of the assemblies resulted in the exclusion of women from this crucial decision; most women were prevented from having any influence on the future of their communities (Deere & León, 2001a, 2003). As a result, the impact of the reform on women's agency reached beyond the confines of the household; their bargaining power was undermined at the community level as well. Just like in the household, the hierarchical nature of gender relations within the community is determined through a continuous process of contestation, whose outcome depends on relative bargaining powers (Agarwal, 1994). Therefore, it can be expected that the exclusion of women at such a crucial time caused a deterioration of gender relations also within the public arena. My quantitative analysis will be partly based on this expectation, although I will not endeavor to differentiate between changes in female empowerment happening at the household level or at the community level.

Overall, it can be concluded that the purported gender neutrality of the land reform amounted to gender blindness. The failure of the land reform to include a gender perspective reinforced previous disparities and produced differential impacts on women and men. As a result of these legal provisions, men gained disproportionately from the reform, hence the gender gap in land rights was widened further. My ultimate objective is to investigate whether the gender blindness of the reform consequently worsened women's bargaining power with respect to men. In the next section, I discuss a bargaining framework to explain the link between gender relations and economic resources (particularly, land), and review the empirical literature on this relationship.

3.2 Female bargaining power and economic resources

The previous section highlighted the direct consequences of the gender blindness of the 1992 agrarian reform. I have argued that the reform reinforced previous disparities and widened the gender gap in land rights. Next, I explore the hypothesis that the reform broadly aggravated gender relations.

There is now a vast literature documenting the positive effects that the control over economic resources – land, in particular – plays on women's empowerment, as well as on the welfare of their families. Before launching into this literature, it is valuable to introduce Kabeer's (1999) conceptualization of empowerment. Empowerment must be thought of in terms of the *ability to make choices*, corresponding to an individual's capabilities – in other words, the "potential that people have for living the lives they want". These capabilities are constituted by both resources (the pre-condition to empowerment) and agency (the process itself). Indeed, agency – intended as the "ability to define one's goals and act upon them" – is not sufficient if it is not backed by the material, human and social resources that effectively serve to unlock it (Kabeer, 1999, p. 436-438).

The attention to female land rights as a potential determinant of empowerment can be traced back most notably to Agarwal (1994). Focusing primarily on South East Asia, Agarwal argued that the gender gap in land rights was the most important factor behind gender inequities. According to Agarwal, increasing women's independent and secure land rights would lead to substantial improvements in terms of welfare, efficiency, empowerment, and equality. In similar fashion, Deere and León (2001a, 2003) examined the question of female land rights in Latin America; their extensive review of recent agrarian reforms across the continent brought the attention on their gendered effects on land rights. The Latin American experience also highlighted the role of mandatory joint titling as a way to implement more egalitarian land reforms. Finally, Englert and Daley (2008) evaluated the situation in Africa, where, although women do most of the work in agricultural smallholder production, their rights to land are still not secure and tied to their status as daughters, mothers or wives.

The common thread and starting point in this literature is the rejection of the once predominant unitary model of the household. First proposed by Becker (1965, 1974) and Samuelson (1956), the unitary model views the household as a single utility maximizing agent, whose members express agreement on all decisions, moved by the aim to maximize a common welfare index. In particular, the household head is portrayed as a "benevolent dictator" who makes decisions for the good of the entire household. Remarkably, this conceptualization fails to account for heterogenous preferences and unequal distribution of resources within the household – factors that make in fact the household a "site of conflict as well as of cooperation" (Doanh et al., 2015, p. 69).

In light of the shortcomings of the unitary model – amply refuted by empirical evidence, as well as by the experience of civil society and government[8] – Manser and Brown (1980) introduced a new conceptualization of the household with the collective bargaining models. These models, by contrast, describe a household in which decisions are reached through a process of bargaining between individuals with different preferences and different bargaining powers. Each household member has a utility function and an outside option ("threat point") which is their utility from opting out of marriage; a cooperative solution will be chosen only insofar as preferable to the outside option (Manser & Brown, 1980; McElroy & Horney, 1981). However, there are several possible cooperative outcomes, some more beneficial to one party than another. The precise outcome depends on the relative bargaining power of each household member; the member holding the most power will be able to influence the outcome in their own favor (Agarwal, 1994).

These differences in power are determined by the distribution of economic resources within the household, and it follows that gender plays a crucial role. Effectively, the bargaining power of each individual is a function of their outside options – hence, in contrast to the unitary model, the distribution of resources within the household affects the final outcomes (Agarwal, 1994; Doss, 2013). When the relative share of assets held by the woman compared to the man rises, the woman's risk from demanding greater influence in household decision-making – which might potentially lead to marriage breakdown – is lowered, in light of her improved outside option.

[8] E.g. cash transfer programs or credit programs in which the effects were found to differ according to the gender of the household participant.

Furthermore, a land title increases the woman's self-perceived economic contribution, which strengthens her influence within the household (Sen, 1990).

Therefore, this paper follows the assumption – nearly unanimously embraced in the literature – that households behave according to the collective bargaining models, as opposed to the unitary model. The primary consequence of assuming so for the purpose of this analysis is that any changes in the distribution of resources within the household will affect the power balances. A relative decrease (increase) in the woman's share of household assets will negatively (positively) affect both her fallback position and ability to contribute to the household's economy; therefore, her relative bargaining position will emerge weakened (strengthened). The implication is that there exists a link between a woman's access to and control over resources and her status within the household, as well as her overall empowerment.

It is worthwhile to narrow down the type of economic resources under study, whether it is assets, labor or nonlabor income. According to Agarwal (1994, p. 62), a rural person's resources can include the access to income earning means, communal resources, traditional external social support systems (such as remittances from relatives), and support from the State or NGOs; but among all, land ownership occupies the predominant position. There is a number of reasons that make land ownership the most conducive to improve an individual's economic position and status within the household. First, the security provided by property is more certain than that provided by employment; second, the land itself can provide an immediate outside option in case of divorce, giving the possibility to arrange a shelter; last, access to land directly enhances livelihood options (Panda & Agarwal, 2005). While the focus of this paper is on land ownership, a small part of the literature reviewed in the following paragraphs includes empirical studies assessing the impact of non-land resources.

Lastly, the literature considers female land ownership as originated either from independently held or jointly held titles. In the Mexican 1992 agrarian reform, due to the head of household rule discussed in Section 3.1.1, joint titles are not applicable. However, some of the empirical studies reviewed look at land reforms granting joint titles to couples as a source of variation in female land titles. If not quite the same, perhaps, as assigning independent land titles to women, it can be argued that joint titles are preferable to assigning land titles to the man alone. Moreover, the possibility of introducing joint titling as a mandatory provision is a worthwhile topic for future discussion in the case of Mexico. According to Deere and León (2001a), joint titling can be even preferable to independent land rights for women in the Latin American context, given the prevalence of family farming. This is supported by Deere and Twyman's (2012) finding that assigning joint titles to the couple rather than to the woman alone is more likely to enable egalitarian, joint decision-making within the household.

The following three sub-sections review some of the empirical evidence that exists on the topic. They are organized according to the type of empowerment outcome studied. Ideally, female empowerment should be measured by the ability to exercise agency; including, but not limited to, the ability to take decisions within the household or within the public arena. However, this is fundamentally unobservable. Therefore, the preferable approach is to measure instead the outcome of that decision-making, particularly through outcomes that are regarded as important policy targets, such as children's health or female educational attainment. The main challenge is that women's preferred outcomes from decision-making are rarely known; hence, this method relies on an inferential approach[9] as well as the necessary precondition that women and men have different preferences within their negotiations (Doss, 2013). In this paper, I use three such indicators of female empowerment[10] – fertility, female education and femicides. Therefore, the next three sub-sections summarize the empirical literature relating each of these three indicators to women's asset ownership. At the beginning of each section, I explain the relevance of the indicator within the Mexican context.

3.2.1 Fertility rates

Between the 1960s and the 1970s, trends in fertility rates have seen a major shift throughout Latin America. In Mexico, total fertility (measured as the number of children per woman) went from 6.7 in the 1950s to 3.6 in the early 1990s, and finally 2.4 in the mid 2000s (Juarez & Gayet, 2015). However, trends in fertility rates appear to differ substantially according to socioeconomic status, with the lowest social strata still experiencing high fertility rates (Juarez et al., 2013). Furthermore, Mexico is marked by a large unmet need of contraception, at 25% among married young women and 37% among the unmarried (Juarez et al., 2013). Teenage pregnancy rates are concerningly high at 17.2% – the highest of all OECD countries – and so is the incidence of child marriage, with 23.6% of women aged 20-49 having first married before the age of 18 (UNICEF & INSP, 2015). All these statistics are indication of a wider issue in Mexico, which is the likelihood for women to have limited agency in their own reproductive choices, particularly in economically disadvantaged contexts. This points to potential inefficiencies in household decisions concerning fertility, making it an interesting outcome to observe in relation to women's bargaining power.

Data from the Demographic and Health Survey (DHS) indicate that men tend to report larger ideal family sizes than their wives (Westoff, 2010). Therefore, increasing female empowerment has been argued as an effective tool to reduce fertility rates and bridge this gap (Sen, 2001). In this regard, the demographic transition of the last decades has been attributed by extensive empirical research to the improvements in women's education and labor force participation, but the influence of women's property rights has been scantly analyzed.

Empirical studies on the topic have shown that women's assets endowment is an important factor affecting family size. Focusing on increases in female land rights arising from a joint land titling program in Peru, Field (2003) finds that fertility reduced on average by 22% in beneficiary families, with female empowerment believed to be the main channel. In a similar vein, Pitt et al.

[9] This is based on observing how outcomes change within the household when women acquire a stronger voice – following an exogenous increase in female bargaining power. For example, if after an increase in women's land rights more of the household budget gets spent on children's schooling, then it is inferred that women prefer to spend more money on their children's schooling (Doss, 2013; Thomas, 1990).

[10] These have been selected on the basis of both data availability and relevance for the Mexican setting; however, there are other indicators that, while not used in this paper, have a prominent role in the literature. For instance, female empowerment is often measured through explicit survey questions on women's decision-making in the household. Empirical studies on the relationship between female asset ownership and decision-making in the household include e.g., Allendorf (2007), Datta (2006), Deere and Twyman (2012), Hare et al. 2007, and Menon et al. (2017).

(2006) show that increasing female bargaining power through participation in a micro credit program leads to enhanced spousal communication about family planning decisions. In Vietnam, Menon et al. (2017) find no effect of female land rights on fertility, but they impute it to the fact that female landowners tend to be older on average than landless women.

On the other hand, some studies show that land titling projects per se may reduce fertility rates, regardless of the gendered effects on intra-household bargaining power (Galiani & Schargrodsky, 2010; Ali et al., 2014). That is because with the security of a land title comes a form of insurance and a source of old-age subsistence. Traditionally, these had been provided by the offspring; with tenure security, therefore, the productive value of additional children decreases.

Overall, I expect that a loss in women's empowerment resulting from the land titling reform will lead to higher fertility rates in affected localities. However, this effect might be partially offset by the increase in household tenure security resulting from the land title, which will likely decrease fertility rates. The final effect will depend on the extent of the loss in bargaining power suffered by women.

3.2.2 Female educational attainment

The levels of education of Latin American women have risen substantially throughout the last half century. Barro and Lee (2013) estimate that the average schooling years of women aged 15 and above went from 2.38 in 1950 to 8.21 in 2010. Female schooling, however, is simultaneously an outcome and a determinant of bargaining power. More educated women have better bargaining chances, since education affects their outside options; at the same time, when women acquire a stronger voice within the household, they can use it to improve the educational prospects of their daughters or even their own. It is thus problematic to use education as an indicator of female bargaining power. Nonetheless, given the importance of female schooling as a direct policy goal, it is still worthwhile to investigate its relationship with bargaining (Doss, 2013).

Previous empirical research on the effects of female empowerment on schooling includes, for instance, Deininger et al. (2010). The authors exploit an exogenous variation in female bargaining power arising from the Hindu Succession Act amendment, in India, which improved women's land inheritance rights. They find that the resulting increase in female bargaining power had a positive effect on women's educational attainment. Suggested mechanisms behind the effect are the fact that daughters' education becomes more important, as it now affects farm income as well as off-farm income. Also, if mothers value daughters' education more than fathers, an increase in bargaining power allows them to demand higher educational attainments for their daughters. Analogous findings were reported by Rangel (2006). Following a legal change that extended alimony obligations to couples in consensual unions, thus strengthening women's position, Rangel finds that household resources were reallocated towards the schooling of older girls.

On a different note, increased control over assets by women has been linked to improvements in children's education investments, of both genders (World Bank, 2001). This suggests that women have a stronger preference for investing in their children's education, with respect to men. Therefore, the educational attainments of the offspring, regardless of gender, may decrease as women's relative share of land titles within the household diminishes. While primary attention in this paper will be dedicated to the education outcomes of females – both children and adults – it will be worthwhile to further investigate the possibility of an effect on all children. This hypothesis is receiving considerable attention by development practitioners and policymakers, and the empirical evidence is growing. For instance, in Honduras and Nicaragua, Katz and Chamorro (2003) find that an increase in female land rights led to a significative increase in household expenditure on children's education. In Ghana, Doss (2006) estimates that women's asset ownership is associated with a higher household budget share spent on education.

Overall, a reduction in women's relative share of assets is likely to negatively affect female educational attainments, due to the deteriorated power position held by women within the household. I expect the average years of schooling to shrink for females with respect to males, especially among children and youth, but eventually also among adults. However, mothers are found to have a stronger preference for the educational investment of children – of both genders – with respect to fathers. Therefore, the weakening of women's position within the household may lead to lower educational achievements for all children, with potentially no differential effect according to gender. In sum, the final effect in female schooling brought on by the reform will depend on several factors.

3.2.3 Femicides and domestic violence

Domestic violence, and in particular spousal violence[11], is a pervasive phenomenon in Mexico. Results from the 2016 wave of the ENDIREH[12] survey indicate that more than ten percent of the women surveyed had experienced non-spousal domestic violence within the previous year; this figure includes physical, sexual or emotional violence. When considering intimate partner violence, the results painted an even bleaker picture; over twenty-five percent of women had been inflicted violence within the previous year, and a staggering forty-four percent had experienced it at some point throughout the course of the relationship (INEGI, 2016). Along with domestic violence, the incidence of femicides is an unfortunately pervasive issue in Mexico and Latin America. It is estimated that around 60,000 femicides occur in the region every year, making Latin America the place in the world where women are most likely to be killed; impunity is also high, as only two percent of these femicides are prosecuted in the criminal justice system (USAID, 2019, p. 2). Femicides are largely a by-product of domestic violence, with roughly sixty percent of victims having been killed by a current or former intimate partner, or by another family member (UNODC, 2018).

Rates of domestic violence – and by extent, femicides – are considered an outcome of women's bargaining power within the household; women with fewer outside options are in fact more likely to remain in marriages or relationships where violence is present, to the point where that might turn into homicidal (Doss, 2013). At the same time, increases in women's authority (and demand

[11] For the purpose of this paper, I consider exclusively the domestic and intimate partner violence that is inflicted by male partners or relatives on female victims.
[12] *Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares* (National Survey on the Dynamics of Household Relationships).

thereof) within the household may result in heightened conflict. This theory is known within the sociological literature as the "male backlash" effect, best exemplified by Macmillan and Gartner (1999). The expectation is that, as a result of women's increased autonomy or financial independence, men feel their traditionally "dominant" gender role threatened. The issue with this theory, though, is that it fails to acknowledge the individual rationality constraint faced by women trapped in abusive relationships; owing to which, women's financial independence is an essential prerequisite to be able to leave the relationship (Aizer, 2010; Jacobs, 2002). Within a model of household bargaining that incorporates violence, the improvement in women's outside option should produce a net decrease in domestic violence, in spite of the possibility of arising familial tensions. Altogether, the empirical evidence is not unanimous on the direction of the effect of women's bargaining power on the level of domestic violence they suffer.

An important factor that might determine the differential effect is the very source of bargaining power; in fact, a large part of the literature reporting a detrimental effect on violence focuses on changes in bargaining power linked to women's income or employment. It is possible that labor income in many developing countries is not a stable enough source of livelihood as to provide women with a secure outside option if opting out of marriage. Property rights, and especially land ownership, may ensure greater economic security and therefore produce a cleaner ameliorating effect on domestic violence. In this regard, Panda and Agarwal (2005) argue that property rights, as opposed to employment, provide women with a form of independent economic support that allows them to escape violent marriages, and that can also deter the violence itself. That is because the security provided by property is more certain than the one provided by labor income. Most importantly, the land or house owned can give physical shelter, and as such constitute an immediate escape option for the woman; particularly in developing countries, where women may face specific social barriers in obtaining accommodation, or wages may not be sufficient to cover the costs. When looking at the effects of land rights on levels of spousal violence experienced by women in India, Panda and Agarwal find that indeed female landowners are less likely to face violence, as well as more likely to escape violent relationships. Nearly half of the landless women in the study experienced long-term physical violence, compared with 18% among the women owning land, and 7% among the women owning a house (p. 836).

Another empirical study reporting an ameliorating effect of female bargaining power is Grabe et al. (2015). Using both quantitative and qualitative methods, the authors find that when women own land, they gain power within relationships and are less likely to experience physical or psychological violence. The qualitative evidence gathered from women's testimonies highlights the causal nature of the process – respondents appeared aware of the linkage between their economic dependence and the risk to receive violence from their partner. Analogous findings are reported by other empirical studies such as Hare et al. (2007) and Grabe (2010). In China, Hare et al. report that women's landlessness – resulting from a gendered land reform – is associated with a rise in tensions within the household. In Nicaragua, Grabe finds a link between female land ownership and a reduction in domestic violence suffered.

On the other hand, there is some empirical evidence pointing to a detrimental effect of female bargaining power, owing to the above-mentioned "male backlash". This literature is for the most part focused on female employment as a determinant of intra-household bargaining power. For instance, Luke and Munshi (2011) find that, in India, a relative rise in female income brings about increased marital violence; presumably, because the shift in bargaining power challenges the norm of male decision-making. According to Schuler et al. (1996), the male backlash effect depends on the levels of the woman's earnings; beyond a certain threshold, additional earnings have the potential to reduce the risk of suffering violence.

In the context of Mexico, the evidence is mixed. The existing empirical studies largely focus on female bargaining power arising from women's increased participation in the labor market, or from other sources of cash income such as government cash transfers. Most notably, Bobonis, Gonzalez-Brenes and Castro (2013) and Bobonis, Castro and Morales (2015) look at the effects of the conditional cash transfer program known as *Progresa*[13]. As this program was specifically targeted to women, it plausibly increased women's relative economic contribution in beneficiary households. The first study finds that women in recipient households were significantly less likely to experience physical abuse than women in comparable non-recipient households, but more likely to experience emotional abuse (specifically, threats of violence). The second study, adopting a more long-term perspective, finds that, in the longer term, there is no difference in the likelihood of experiencing violence (physical or emotional) for recipient and non-recipient women. There seems to be more marital selection in couples where the woman's economic contribution is higher, and any emotional abuse suffered in the shorter term leads to marriage dissolution over time.

Finally, additional insights on the link between female empowerment and intimate partner violence in Mexico comes from Castro et al. (2008). The authors observe that changes in gender-based power do not generate unidirectional changes in the relationship, depending instead on the type of authority asserted by the woman. In some respects, men have adapted to new social norms – e.g., regarding women's ability to decide whether to work, and to decide whether to have sexual relations. Women's higher assertiveness in these areas is met with a lower risk of physical violence. At the same time, men's desire to control women manifests in areas that continue to be seen as "negotiable" within the relationship, such as in reproductive matters. Hence, women that push to be more independent with respect to these decisions face a risk of higher violence (i.e., backlash) from their partners.

Finally, I expect that a loss in women's empowerment resulting from the land titling reform will lead to higher incidence of violence against women – including femicides – in affected localities. The vast majority of the empirical literature reviewed focuses on *improvements* to women's fallback position and assesses whether these are stronger than any potential "male backlash" effects. As the Mexican 1992 agrarian reform induced a relative decrease in female asset ownership, I expect there to be a *deterioration* in women's fallback position, hence the discussion around male backlash effects is effectively ruled out. Therefore, the effect of the reform on violence should be straightforward, with no specific factors intervening to offset it or reverse it.

[13] Later renamed *Oportunidades*, then *Prospera*.

However, the evidence from studies on domestic violence in Mexico highlights the presence of changing norms within gender relations, which might make the overall effect harder to predict.

4 Data

The first important clarification to be made concerns the region interested by my analysis. The land titling reform of 1992 was a national level effort that involved all 32 Mexican states. However, my analysis will only focus on seven of these States, forming a region known as the Bajío: Aguascalientes, Guanajuato, Jalisco, Michoacán, Queretaro, San Luis Potosí and Zacatecas[14]. This region is generally characterized by a relatively strong and diversified economy with an advanced agricultural production. Figure 1 illustrates the position of the Bajío within Mexico, as well as the distribution of the ejidos (by privatization status) within the Bajío.

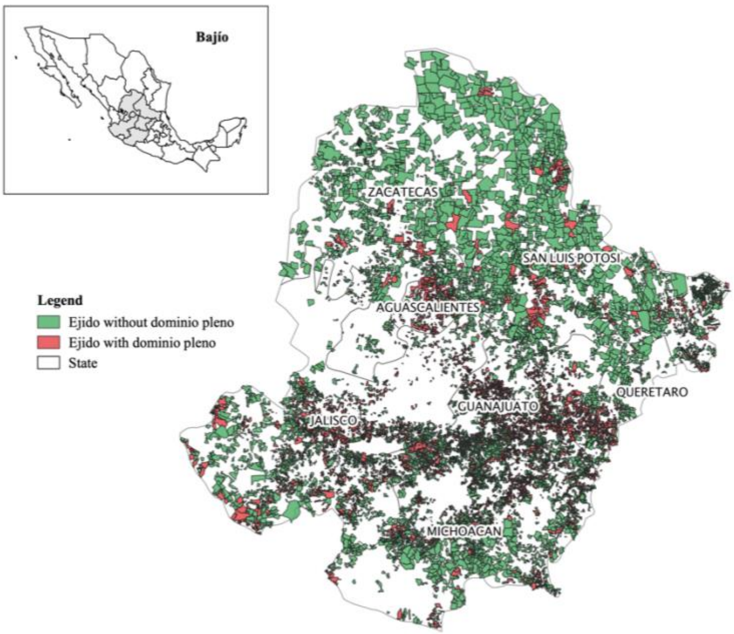

Figure 1 – Ejidos in the Bajío by privatization status

The decision to limit this study to the Bajío was motivated by the fact that part of the data had to be manually gathered, as will be explained in the next section. In Mexico there are a total of about 30,000 ejidos; by restricting the analysis to the Bajío, I am left with roughly 7,000. Furthermore, there are considerable differences between ejidos across the country, which may result in different

[14] Technically, the Bajío is only formed by parts of each of these states. For convenience, here I have defined the Bajío as the entirety of the seven states. The main reason for this simplification is that there is no unanimous consensus over which municipalities are unquestionably part of the Bajío. In fact, a simplification such as mine is not uncommon, owing to the better ease of obtaining state-level data and statistics as opposed to municipal-level. An additional reason is to be found in the challenges faced for the data collection process (which is described in the next sub-section). When compiling the ejido dataset, it was not possible to select in advance only the ejidos considered "technically" within the region. Once the ejido dataset collection was completed – listing all ejidos lying within the seven states – filtering out some of those ejidos would have appeared inefficient. My definition thus identifies a region with a larger geographical extension, and potentially more heterogenous, than the official Bajío.

responses to the agrarian reform. These differences pertain to the type of land and agricultural management practices, the history of agrarian reform in the region, and most importantly, the agricultural potential of the land (Lewis, 2002). These differences are particularly accentuated between ejidos in the North and ejidos in the South, with the South being characterized by a rugged geography, lower land quality, severe demographic pressures, and higher poverty rates (Assies & Duhau, 2008). Due to its generally favorable geographical characteristics and arguably higher value of land, the Bajío – located in the center of Mexico – is a fairly homogenous region. The advantage is that land privatization is less likely motivated by non-agricultural interests. Namely, dominio pleno is adopted either to convert agricultural land into residential (in urban localities), or to convert collectively-owned agricultural land into privately held agricultural land. Therefore, limiting the analysis to the impact of the reform in a homogeneous region such as the Bajío allows me to identify a more uniform response and more clear-cut effects.

4.1 Data collection

The data used for this analysis was compiled using a total of five georeferenced datasets and two additional sources. First, I used GIS digital maps from SEDATU to define the boundaries of each ejido in the Bajío region. Since data on the privatization status and year of privatization by ejido was not readily available, I personally compiled it through access to two archives of the RAN; the query system "*Padrón e Historial de Nucleos Agrarios*"[15] (PHINA) and the online archive "*Sistema Integral de Modernización Catastral y Registral*"[16] (SIMCR). The creation of this dataset consisted in first searching each ejido on the PHINA to find out whether it had adopted dominio pleno; if it had, I then proceeded to access the history of assembly decisions pertaining to that ejido on the SIMCR. The date of the earliest assembly decision in which dominio pleno had been approved (and requested from the RAN) was thus considered as the year of treatment. The end result is a dataset containing each ejido of the Bajío region, its boundaries and extension, its treatment status and, if treated, the year of treatment. This is an original dataset, which spans the entire time period from the start of the reform to present day, thus 1992 to 2019.

The second dataset used is the Population and Housing Census conducted by INEGI, in its 2000, 2005 and 2010 waves. The unit of observation is the locality, for which the coordinates of the centroid are provided. From the INEGI Census I obtained the vast majority of covariates used in the analysis, as well as the outcome variable of fertility. This is the dataset with the most limited temporal extension, and as such, it sets the maximum period covered by my analysis. However, I interpolated data for intercensal years using a linear interpolation technique[17], so that the final time unit is year.

Third, I used the annual register of general deaths compiled by the INEGI to obtain the outcome variable of femicides. Since precise geographical references are only available for this dataset starting from 2002, the time span for the femicides analysis is further restricted to 2002-2010.

[15] "Register and History of Agrarian Nuclei"
[16] "Comprehensive System for the Modernization of the Cadaster and Register"
[17] With the exception of the fertility outcome variable, which is thus only observed in 2000, 2005 and 2010.

From the register of general deaths, I selected the homicides in which the victim was female, then aggregated the homicides by locality and year. For the alternative definition of femicides according to Data Cívica's criteria, I additionally imposed those criteria (which will be listed more in detail further on) when selecting the homicides from the register.

Fourth, I used the 2000-2017 Local Burden of Diseases Educational Attainment dataset[18] provided by the Institute for Health Metrics and Evaluation (IHME) to obtain the outcome variables of educational attainment by gender. This is a raster dataset that contains estimates for mean years of education attained by adults aged 15-49, disaggregated by gender, at the 5x5 km-level.

Lastly, information on the local occurrence of organized violence was sourced from the 1989-2018 UCDP Georeferenced Event Dataset compiled by the Uppsala Conflict Data Program (UCDP) (Sundberg & Melander, 2013). This dataset lists all organized violent events, defined as "incidents where armed force was used by an organized actor against another organized actor, or against civilians, resulting in at least one direct death at a specific location and a specific date" (Högbladh, 2019, p. 4). The variable sourced from this dataset – a binary variable indicating if any organized violence events took place in a locality in a given year – is used only for the femicides analysis.

All these datasets were joined using a spatial matching procedure[19] that took into account the different geographical definition available for ejidos (identified as the entire polygon) and for localities (identified by the coordinates of the centroid). Hence, a locality is matched to an ejido if its centroid is located within or on the ejido boundaries, as is shown in Figure A1. More than one locality may be matched to the same ejido. In the case of the IHME dataset, each locality is assigned to the 5x5 km cell in which its centroid was contained. The data thus obtained is at the locality level; from now on, I will refer to localities as being treated or not depending on the treatment status of the ejido which they are matched to. In the geographical scope of Mexico, localities are the third level of sub-national division recognized by the INEGI[20].

The final data to be used in the analysis is a balanced panel dataset that covers the years 2000-2010 (or 2002-2010 for the femicides analysis). There is temporal and spatial variation in treatment status among localities. Localities treated earlier than 2000 were dropped. Localities treated later than 2010 were kept and repurposed as an "ideal" control group. Table 1 shows the timing of treatment rollout for all localities in the Mexican Bajío. The localities that were dropped constitute 16 percent of all treated localities, while the late-treated localities that are to be used as control represent 28 percent. The list of all variables contained in my final data is illustrated in Table 2.

[18] The complete dataset name is Low- and Middle-Income Country Educational Attainment Geospatial Estimates 2000-2017 (IHME, 2019).
[19] See Appendix and Figures A1-A3 for further details.
[20] According to the definition of INEGI, the presence of one building suffices to constitute a locality, as long as the place is recognized by a name given by some legal provision or custom.

Table 1 – Treatment rollout in the Mexican Bajío

	Privatized until 2000	Privatized in 2001–2005	Privatized in 2006–2010	Privatized after 2010
No. ejidos	229	270	335	287
No. localities	313	469	631	543
% localities	16%	24%	32%	28%

Notes: The earliest privatization year was 1994. The latest privatization year was 2019.

4.2 Choice of outcome variables

The need to find georeferenced, time-varying data posed some constraints to the choice of outcome variables that could fit the scope of this analysis. All three of the outcome variables selected, indeed, serve as proxies for my target outcome variables. As described in Section 3.2, I would ideally want to measure the impact of the reform on the fertility rate (births per woman), on girls' school enrolment and on domestic or intimate partner violence. Instead, I use the child-woman ratio, the average educational attainment of women aged 15 to 49, and the incidence of femicides per 100,000 women. In the following paragraphs I define such outcome variables and discuss their relevance and suitability for this analysis.

My measure of fertility – the child-woman ratio (CWR) – is defined by the number of children under age 5 per 1,000 women in reproductive age (15 to 49 years old)[21]. In absence of more direct measures, the CWR can be used as a crude estimate of the fertility rate, since its relationship with fertility levels has been observed to be nearly linear for given levels of mortality (Rele, 1967). However, the CWR would be a rather inadequate measure in areas where infant mortality is very high, as it would systematically underestimate the actual fertility rate. According to Wang et al. (2020), Mexico's under-5 mortality rate (defined as number of deaths per 1,000 live births) has been on a decreasing trend throughout the period covered by my analysis; in 2019, it stood at 14.4, which is substantially lower than both the global average (37.1) and the Latin American average (19.0). Therefore, the child-woman ratio is a reasonable proxy for the fertility rate in this setting, as long as it can be assumed that child mortality is not directly affected by the reform.

To measure female educational attainment, I use the mean years of schooling attained by women aged 15-49. Likewise, to compare the effects of the reform between men and women, I use the mean years of schooling attained by men aged 15-49. The problem with these variables is that the age bracket (15 to 49 years old) is rather wide and comprises multiple generations. The younger generations such as primary school age children, that would be most interesting to observe, are missing entirely. Considering that the mean educational attainment in the Bajío localities for both

[21] Normally, it is a figure below one thousand: considerably below one thousand for low fertility countries and just under one thousand for high fertility countries. From Table 3, it can be observed that it averages just below five hundred for ejido-matched localities in the Mexican Bajío.

men and women is roughly seven years (see Table 3), it seems unlikely for these variables to show substantial effects in the short and medium term. Any underlying reform effects will almost exclusively operate through the teenage-age men and women that are comprised in this age bracket. Furthermore, as I do not have information on the mean age of population in the localities, it is imperative to assume that the age structure does not experience differential trends between treated and untreated localities. If treated localities should somehow acquire an older age structure as a consequence of the reform, for example, I might see a reduction in adult schooling attainments that is not necessarily the result of a decrease in educational investments, but merely the reflection of a generational difference in schooling attainments. Also, the wide age bracket makes this variable very sensitive to differences in local migration patterns. Since there are reasons to suspect that migration trends might differ between treated and untreated localities (de Janvry et al., 2015), I will address this concern more in depth in the results section.

The third outcome variable is the incidence of femicides, quantified as the number of femicides per 100,000 women (femicide rate). The choice of including this variable stems from the intention to observe the effects of the reform on the prevalence of violence against women. Of all the acts of violence against women, femicide – the murder of a woman – is the most extreme. While the understanding of the phenomenon is limited, it is known that a large proportion of femicides are of women in violent relationships, committed by current or former partners (Campbell et al., 2007; UNODC, 2018). Within this framework, a femicide represents the fatal consequence of an abusive relationship that the woman lacked the means to escape. As such, the phenomenon of femicide, like domestic violence, is likely affected by changes in women's household bargaining power. When drawing the distinction between femicides and male homicides, the World Health Organization highlights that the former "often involve ongoing abuse in the home, threats or intimidation, sexual violence or situations where women have less power or fewer resources than their partner" (2012, p. 1). However, femicides can also have a non-intimate nature, such as when perpetrated by strangers – often in conjunction with sexual assault. In Mexico, the vast majority of femicides that took place in the public space had a non-intimate nature (USAID, 2016).

The phenomenon of femicides is especially relevant in the setting of Mexico, where increasing efforts to address the issue have been deployed in recent years, despite a continuing system of impunity (Data Cívica, 2019). Mexico's Criminal Code loosely considers femicide as the homicide of a woman committed as a direct result of the gender of the victim, although the exact definition differs in each of its 32 states (USAID, 2016, 2019). Indeed, the heterogeneous criminalization of femicide has created obstacles to its effective prosecution, as it often requires the fulfilment of specific criteria that are fundamentally hard to prove or quantify (USAID, 2016). In practice, it can be hard to identify with certainty which homicides constitute femicides.

For this reason, I will define the outcome variable of femicides in two alternative ways. First, I consider femicides as the total of homicides in which the victim is female; this is the definition used by INEGI and the Instituto Nacional de las Mujeres (INMUJERES), among others. Second, I consider as femicides only those homicides that meet any of these criteria: (i) the homicide took place inside the house, (ii) the victim had experienced (and reported) domestic violence, prior to the homicide, (iii) the cause of death was declared to be sexual assault. These criteria have been laid out by Data Cívica (2018), a Mexican civil society organization that is pioneer in the tracking of this phenomenon, and they are easily assessed using the information recorded in the annual death register. However, while the first approach risks overstating the true incidence of femicides, the second approach risks understating it[22]. Therefore, I will perform the analysis on both variables, and compare the results.

The advantage of looking at violent deaths is that they are easier to observe than a variable such as domestic violence, which is routinely underreported (INEGI, 2020). There can be misreporting in this variable too – for example if homicides are covered up as accidental deaths – but the extent of this issue is less concerning. Also, in the case of total homicides, there is less likely to be a correlation between underreporting and local gender norms. However, I will need to use caution when using the Data Cívica definition; local authorities have been known to show reluctance at classifying homicides as femicides, and as such, may not record the full information needed to classify a homicide as femicide (Riquer-Fernández & Castro, 2012; USAID, 2016).

Nonetheless, working with violent deaths has a major drawback, which is the fact that they are, fortunately, rare events. An increase in gender-based violence resulting from the reform would be less easily observable through femicides, which are understandably less elastic. Furthermore, most localities do not have a single femicide (regardless of the definition) in most years. As cautioned by Dower and Pfutze (2020) – who study the incidence of violent deaths across Mexican municipalities – smaller localities have extremely high variance in this variable; a single femicide can increase it from zero to a very high number, once population size is accounted for. This could introduce substantial noise in the estimates. Therefore, I adopt an approach similar to theirs, and select only localities with higher population size for the femicides analysis. In order to establish a threshold, I follow INEGI's definition, which classifies localities as "urban" if their population surpasses 2,500 inhabitants; since population size can fluctuate across time, I impose this threshold on the 2002-2010 average population size. Hence, the final sample to be used for the femicides analysis consists of only urban localities, which tend to have more homogeneous population size and characteristics. In fact, an additional benefit of doing so is that I am rid of many pre-existing heterogeneities: the result is a sample of localities that are much more comparable with each other. Evidence of this is shown in Section 5.3.

[22] For example, femicides are increasingly taking place outside the home (USAID, 2019); there exist substantial challenges and obstacles to reporting instances of domestic violence (INEGI, 2020); sexual assault, while often present in a femicide, is not always the direct cause of death (Data Cívica, 2019); and many femicides, particularly in Mexico, are perpetrated by strangers (non-intimate) (WHO, 2012).

Table 2 – List of variables

Variable	Source	Description	Units
Outcome Variables			
Fertility (CWR)	INEGI C. 2000-2010	Number of children aged 0-4 per 1,000 women in reproductive age (15-49 years old)	Children per 1,000 women
Femicides (homicides)	INEGI M. 2002-2010	Incidence of femicides, defined as all homicides of women taking place within a 5 km radius from the locality centroid	Deaths per 100,000 women
Femicides (Data Cívica)	INEGI M. 2002-2010	Incidence of femicides, defined according to Data Cívica's criteria and taking place within a 5 km radius from the locality centroid	Deaths per 100,000 women
Female educ. attainment	IHME 2000-2010	Average years of schooling for women aged 15-49	No. of years
Controls			
Dominio Pleno (Treated)	RAN PHINA 1992-2019	Binary variable taking the value of one for a locality situated within an ejido that has adopted dominio pleno	0 or 1
Treatment Year	RAN SIMC 1992-2019	Year in which dominio pleno was first approved by the assembly	Year
Population	INEGI C. 2000-2010	Total population, from 0 to 130 years old	No. of individuals
Average household size	INEGI C. 2000-2010	Average number of occupants per inhabited private dwelling (i.e. does not include shelters)	No. of individuals
Average years of schooling	INEGI C. 2000-2010	Average years of schooling in the 15+ years old population	No. of years
Literacy Rate	INEGI C. 2000-2010	Share of population aged 15+ that is literate (i.e. can read and write)	Percentage
% Population speaking indigenous languages	INEGI C. 2000-2010	Share of population aged 5+ that speaks an indigenous language	Percentage
% Population recently immigrated	INEGI C. 2000-2010	Share of population aged 5+ that resided in another state five years earlier	Percentage
% Population with access to healthcare	INEGI C. 2000-2010	Share of population entitled to healthcare service in a public or private institution	Percentage
% Dwellings that are privately-owned	INEGI C. 2000-2010	Share of inhabited dwellings that are private (neither shelters nor collective dwellings)	Percentage
% Dwellings with access to electricity	INEGI C. 2000-2010	Share of inhabited private dwellings with access to electricity	Percentage
% Dwellings with hard flooring	INEGI C. 2000-2010	Share of inhabited private dwellings with floor made of material other than earth	Percentage
% Households owning a television	INEGI C. 2000-2010	Share of inhabited private dwellings with a television	Percentage
% Households with female head	INEGI C. 2000-2010	Share of households with a female head of household	Percentage
Organized violence	UCDP 2000-2010	Binary variable taking the value of one if any events of organized violence took place within a 20 km radius from the locality centroid	0 or 1

Notes: INEGI C. refers to the Population Census, INEGI M. refers to the annual death register. Variables sourced from the Population Census are observed every five years, all other variables are observed yearly.

4.3 Data limitations

An important data limitation pertains to the variable of interest: the treatment indicator variable, which identifies whether the ejido associated to the locality has adopted dominio pleno or not. The issue is that the transition to dominio pleno does not necessarily apply to the entirety of the ejido. The way in which ejidos generally adopt dominio pleno is that any ejido members who wish to gain full ownership rights over their parcel present individual requests to the ejido assembly. As soon as the first such requests are approved – with a full majority vote of the assembly – the ejido is said to have adopted dominio pleno. While it is unlikely for the entire procedure to be performed for the mere benefit of a single member or for a negligible amount of land, technically the ejido is only *partially* privatized as a result of the assembly decision.

Therefore, there are two potential implications that may compromise the intended use of my data. The first one is that all ejidos that adopted dominio pleno at some point in time are considered alike, without taking into account that some might have privatized a substantial proportion of their land, while some only little. The second implication is that treatment rollout might have in fact been staggered even *within* an ejido – meaning that the degree of privatization in an ejido might have increased gradually over the years through subsequent request approvals. Both these implications are hard to evaluate without a substantial additional work of data collection. Also, the novelty of the data used in this paper does not allow to borrow "good practices" from the relevant literature. The only study to the best of my knowledge using comparable data on dominio pleno does not consider the possibility of partial dominio pleno to be an issue for identification (Ramirez-Alvarez, 2019). The likely consequence of these potential implications is akin to a measurement error in the independent variable. As long as there are no strategic intentions behind the mismatching of the treatment indicator with the actual extent of treatment, the only risk is to obtain oversized standard errors. This could bias the estimates towards zero and make it difficult to detect any underlying effects of the reform.

Additionally, I am concerned with several issues that stem from the lack of available georeferenced data that can fit the scope of my analysis. First of all, local income is an important confounding factor, as it might have been affected positively or negatively by the reform, and on turn might have affected outcomes such as fertility or education; but I do not have data on it. However, I am able to proxy for it using a vector of socioeconomic variables that will be listed in Section 5.1. Secondly, I have linearly interpolated the control variables sourced from the INEGI Population Census, to obtain values in intercensal years. The reason for doing so is to match the granularity of the outcome variables and retain year as the time unit. Linear interpolation has the advantage of being a fairly simply technique, but it can produce biased results; ideally, a more complex method such as multiple imputation would have been a better alternative. Due to the linear interpolation, if there is a large non-linear change in the covariates that is correlated with the treatment variable, the results could be biased. Given that treatment participation was determined at least in part based on pre-treatment characteristics, this possibility is minimized. Linear interpolation of the covariates is also applied in previous studies using data from the INEGI

Census (e.g. Barham, 2011; Barham & Rowberry, 2013; Jiang & O'Neill, 2018; Lindstrom & Saucedo, 2007).

4.4 Summary statistics

Table 3 reports the summary statistics for the outcome and control variables in each of the three datasets used for my analysis.

Table 3 – Summary statistics

	Fertility dataset		Educ. attainment dataset		Femicides dataset	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Dominio pleno	0.22	0.42	0.22	0.42	0.20	0.40
Year of treatment	2009	4.77	2009	4.77	2010	4.16
Fertility (CWR)	489.97	250.58	–	–	–	–
Inhabitants	363.24	758.99	360.66	754.50	354.92	761.96
Avg. household size	4.62	0.95	4.61	0.89	4.53	0.85
% Pop. recently immigrated	0.01	0.03	0.01	0.03	0.01	0.02
% Households with female head	0.18	0.12	0.18	0.11	0.18	0.11
Avg. years of schooling	4.76	1.29	4.76	1.21	4.86	1.17
Literacy rate	0.81	0.11	0.81	0.11	0.82	0.11
% Pop. speaking indigenous lang.	0.04	0.18	0.04	0.17	0.04	0.18
% Pop. with access to healthcare	0.37	0.33	0.35	0.28	0.39	0.27
% Households owning a television	0.74	0.27	0.74	0.26	0.76	0.25
% Dwellings with hard flooring	0.73	0.28	0.72	0.26	0.73	0.25
% Dwellings with access to electricity	0.83	0.30	0.84	0.28	0.85	0.26
% Dwellings that are privately-owned	1.00	0.02	1.00	0.01	1.00	0.01
Avg. schooling (males aged 15-49)	6.88	0.67	6.95	0.62	–	–
Avg. schooling (females aged 15-49)	6.88	0.71	6.94	0.66	–	–
Femicides (homicides) /100,000 women	–	–	–	–	103.03	1491.94
Femicides (Data Cívica) /100,000 women	–	–	–	–	42.95	781.39
Organized violence	–	–	–	–	0.01	0.09
Observations	22068		80916		64287	
No. localities	7356		7356		7143	
No. ejidos	4068		4068		3955	
Time periods	3		11		9	

Notes: The fertility outcome variable was not interpolated in intercensal years. Hence, the fertility dataset only features three time periods – 2000, 2005, and 2010. The educational attainment dataset features eleven time periods, spanning the years 2000-2010. The femicides dataset spans the years 2002-2010, for a total of nine time periods.

The datasets span different time periods; in the fertility dataset, the time unit is a five-year interval, while in the other two datasets the time unit is a year. In all datasets, about 20% of localities have been treated at any time – between 2001 and 2019 in the first two datasets, between 2003 and 2019 in the last dataset. As a matter of fact, the average treatment year is between 2009 and 2010. As mentioned, localities treated after 2010 (which is the maximum time limit for all datasets) will be repurposed as an ideal control group.

5 Identification strategy

A valid evaluation of the impact of the land reform should compare outcomes in treated localities to what those outcomes would have been had the same localities not received treatment. Constructing this unobserved counterfactual is the basic dilemma of impact evaluation. Merely comparing changes in outcomes between localities that were privatized (i.e., treated) and localities that were not, even controlling for their characteristics, may give a biased estimate of program impact. Specifically, I am concerned by the possibility that unobserved characteristics correlated with the outcome of interest might have affected the probability of undergoing privatization. In this case, since take-up is voluntary, the most likely source of selection bias is the self-selection into privatization by the localities.

In order to identify the desired measure of treatment effect (the Average Treatment effect on the Treated), it is necessary to ensure that outcome is independent of treatment status. To that scope, I use an event study design to construct a model that exploits both the variation in treatment timing and the comparison across treated and non-treated localities.

In this section, I introduce my identification strategy by describing the event study framework utilized and comparing it to a classical difference-in-differences design. Next, I explain my expedient to identify a more valid counterfactual. Lastly, I discuss the identifying assumptions required for estimation.

5.1 Event study framework

In the empirical literature, an event study design is characterized as a staggered adoption design where units are treated at different times, and never treated units may or may not be present (Sun & Abraham, 2020). It is a variant of the typical difference-in-differences design, in which units are generally either treated simultaneously or never treated.

Let Y_{iet} be the outcome of interest for locality i in ejido e in year *t*. All localities matched to a given ejido e are treated (or not treated) simultaneously, since treatment is assigned at the ejido level. I define event time (e_{et}) as the time at which ejido e – with all matched localities – first becomes treated; note that treatment is non-reversible. Then, $D_{et}^{k} = I[t = e_{et} + k]$ so that D_{et}^{k} is a dummy indicating that the ejido was privatized *k* periods ago (where *k* might be negative). These are the leads and lags of the treatment. Throughout I assume that the observations $\{Y_{iet}, D_{et}^{k}\}_{t=0}^{T}$ are independent and identically distributed (i.i.d.). The event study framework at the base of my various empirical specifications is thus:

$$Y_{iet} = FE_i + FE_t + \sum_{k=\underline{C}}^{\overline{C}} \beta_k D_{et}^{k} + \gamma X_{iet} + \varepsilon_{iet} \tag{1}$$

Where the event study coefficients β_k provide estimates of treatment leads and lags, after controlling for locality-specific and year-specific effects as well as locality-specific time-varying characteristics. The earliest lead and the latest lag are represented, respectively, by $\underline{C} < 0$ and $\overline{C} > 0$. Since my data is a balanced panel with heterogenous event dates, including all available leads and lags would lead to an unbalanced sample in event time. Therefore, my approach is to bin up the endpoints, so that $D_{et}^{\overline{C}} = I[t \geq e_{et} + \overline{C}]$ and $D_{et}^{\underline{C}} = I[t \leq e_{et} + \underline{C}]$. In the educational attainment specification, $\underline{C}$ is -4 years and $\overline{C}$ is 4 years. In the femicides specification, given the substantially smaller number of observations, I prefer a specification in which $\underline{C}$ is -3 years and $\overline{C}$ is 3 years. In the fertility specification, since there are only three time periods, there are effectively only two observable lags and one lead, and it is thus not necessary to bin up the endpoints.

To estimate the model in Equation 1, one of the event coefficients must be normalized to zero. Following the standard approach in the literature, I normalize the first lead (β_{-1}) to zero, as it makes it easier to interpret coefficients and to observe the immediate impact of privatization (Sun & Abraham, 2020). Accordingly, the event study coefficients measure the difference in outcome between treated and non-treated localities in each event year relative to the year before treatment.

All my empirical specifications include a range of time-varying, locality-level characteristics (X_{iet}). The choice of these variables is based on evidence from the literature and the author's interviews with RAN authorities about the possible determinants of the adoption of dominio pleno. Most notably, dominio pleno is common among ejidos that are favorably located (nearby a city, nearby a coastal or a touristic area), as well as among ejidos that are wealthier and better connected to the infrastructure. The ejido's location is accounted for by locality-specific effects (FE_i). In order to proxy for local income (which is not directly available), I control for the share of households that own a television, the share of dwellings that are privately owned and the share of dwellings that have a hard flooring. I also account for the local access to public goods and services, by controlling for the share of households with access to electricity and the share of population with access to healthcare. Furthermore, to capture local socioeconomic characteristics, I control for the local literacy rate and the share of population that speaks any indigenous languages. Lastly, the demographic controls comprise the average household size (number of members per household), the share of households that are female-headed, the locality's population size, and the share of population that has immigrated from elsewhere within the previous 5 years.

Finally, in all specifications, standard errors are clustered by ejido, since treatment is assigned at the ejido level.

5.1.1 Comparison with two-way fixed effects difference-in-differences

The event study is essentially a difference-in-differences framework which allows for dynamic treatment effects, is more flexible to variation in treatment timing, and allows to check for the existence of pre-trends. For these reasons, it is my preferred identification strategy. Alternatively, I could have selected a two-way fixed effects difference-in-differences design, which yields average treatment effects across all groups and times when treatment timing varies (Sun & Abraham, 2020). However, the use of this single-coefficient, two-way fixed effects specification may not be appropriate if treatment effects are time-varying or dynamic (Goodman-Bacon, 2018).

The treatment effect is captured solely by the coefficient on the post-treatment indicator, here referred to as $Treated \times After$, which turns on for ejido-matched localities only once, and if, the ejido becomes treated. In this context, the two-way fixed effects difference-in-differences specification takes the following form:

$$Y_{iet} = FE_i + FE_t + \beta_1 (Treated \times After)_{et} + \gamma X_{iet} + \varepsilon_{iet} \qquad (2)$$

Estimates from this difference-in-differences specification will be shown next to my main event study estimates, to aid the interpretations of the results. If treatment effects are not in fact time-varying, the average treatment effect estimated with the difference-in-differences specification should not differ substantially from the average of the post-event coefficients.

5.2 Choice of counterfactual

The event study design exploits both variation in treatment timing and variation in treatment status. This implies that, for every time period before and after treatment, the coefficient measures the difference in outcome between treated and non-treated localities. Non-treated localities are comprised both of localities that are yet to be treated (but will be treated within the period observed) and of localities that will never be treated. The main threat to identification comes from this latter comparison, since never-treated localities might be intrinsically different from treated localities if there is self-selection into treatment.

However, a peculiarity of my data – owing to the heterogeneity of the data sources – can be exploited in order to form more comparable treatment groups. My expedient is similar in spirit to the approach of Greenstone et al. (2010). Namely, one of my primary data sources (the INEGI Census) only spans the period 2000-2010, therefore constraining my entire analysis to the same time limit. On the other hand, data on treatment status is available up to 2019, as it was collected by the author in 2020. Essentially, this allows me to know that some localities *will* be treated later on, even though time constraints on the data prevent me from observing them post-treatment. These localities, where privatization will be undertaken beyond the period observed ("late-treated") can therefore be used as a valid counterfactual for those that undertake it between 2001-2010 ("treated"). As long as treatment timing is independent of differences in outcome trends (i.e., not endogenously determined), late-treated localities are arguably not intrinsically different from the localities treated by 2010.

Hence, my identification strategy exploits variation in treatment timing and variation arising from the comparison between treated and late-treated localities (that are effectively *not* treated within the period observed). As for the never treated localities, they are excluded from the sample.

There is an important exception to this. Namely, in the femicides analysis, never treated localities form my preferred counterfactual, while specifications using late-treated localities are shown on the side. The reason is that, as anticipated in Section 4.2, the sample used for the femicides analysis includes only urban localities. This makes it necessary – given the much-reduced sample size – and adequate – given the higher degree of comparability thus obtained – to consider never treated localities as a counterfactual for the femicides analysis.

To conclude, the comparability between treated and non-treated localities relies on two alternative assumptions. The first assumption is that localities where privatization will be undertaken beyond the period observed ("late-treated") are a valid counterfactual for those that undertake it between 2001-2010. The second assumption is that never treated urban localities are a valid counterfactual for urban localities that receive treatment. In the next section, I perform balance checks to elaborate on the credibility of these assumptions.

5.3 Balance checks

The balance checks consist in verifying that, prior to treatment, treated and non-treated localities did not significantly differ in levels of observable characteristics. This is especially important in cases, such as this, where treatment is not randomly assigned.

I start by running the balance checks for the fertility and educational attainment datasets; since these two datasets are actually equal to each other in 2000, the checks need not be performed distinctly. In Table 4, I compare all treated localities (i.e., between 2000-2019) with never-treated localities. Out of fifteen variables, eleven present a statistically significant difference between treated and non-treated, confirming that never-treated localities are not a good counterfactual for the treated. Therefore, I proceed to run the balance checks using the "ideal" counterfactual delineated in the previous section: the late-treated localities. Never-treated localities are dropped from the data. Table 5 presents the comparison between treated localities (i.e., treated by 2010) and the late-treated. It is immediately evident that late-treated localities constitute a better counterfactual for the treated. Out of fifteen variables, only four are significantly different between the two groups: namely, the share of population with access to healthcare, the share of dwellings with hard flooring and the educational attainment variables. These remaining differences should not be a major concern, provided that they are fixed in levels (rather than in trends) and not correlated with treatment timing.

Next, I perform balance checks for the femicides dataset (where the starting year is 2002). The sample for the femicides analysis is restricted to urban localities only – that is, localities with an average population of over 2,500 inhabitants. Once non-urban localities are dropped, there are just under 80 localities in the sample. Therefore, given the small sample size, it is preferable to use never-treated localities as control group. Furthermore, it appears that, across urban localities, comparability is high regardless of treatment status. Table 6 compares treated and never-treated urban localities. Only two variables out of fifteen are significantly different between the two groups. Therefore, even for the femicides dataset – where never treated localities form the control group – the identifying assumption that will be discussed in Section 5.4 appears reasonable. Lastly, the contrast between Table 6 and Table 3 adds support to my decision of restricting the analysis to urban localities. In Table 3, the mean of femicides (homicides) on the whole sample for years 2000-2010 was as high as 103. In Table 6, on the subset of urban localities, the mean goes dramatically down[23]. This confirms that removing non-urban localities helps to reduce the

[23] In Table 6, the mean is computed only for the year 2002. However, if it were to be computed for the whole time period 2002-2010, it would still be very low at 3.23.

variance in the outcome variable and prevent the presence of considerable noise in the estimates (Dower & Pfutze, 2020).

Table 4 – Balance checks. Data from 2000

	Treated	Control (never treated)	Difference (p-value)
Fertility (CWR)	538.74	574.82	0.00***
Avg. schooling (males aged 15-49)	6.71	6.30	0.00***
Avg. schooling (females aged 15-49)	6.52	6.25	0.00***
Inhabitants	467.90	326.98	0.00***
Avg. household size	5.06	5.04	0.53
% Pop. recently immigrated	0.02	0.02	0.14
% Households with female head	0.16	0.16	0.43
Avg. years of schooling (age 15+)	4.54	4.01	0.00***
Literacy rate	0.82	0.79	0.00***
% Pop. speaking indigenous lang.	0.03	0.05	0.00**
% Pop. with access to healthcare	0.18	0.14	0.00***
% Households owning a television	0.76	0.63	0.00***
% Dwellings with hard flooring	0.72	0.60	0.00***
% Dwellings with access to electricity	0.83	0.73	0.00***
% Dwellings that are privately-owned	0.99	0.99	0.93
Observations	1643	5713	7356
% of sample	22%	78%	

Notes: The control group are never-treated localities. Valid for both fertility and educational attainment datasets.

Table 5 – Balance checks. Data from 2000

	Treated (by 2010)	Control (treated after 2010)	Difference (p-value)
Fertility (CWR)	540.23	535.73	0.73
Avg. schooling (males aged 15-49)	6.77	6.60	0.00***
Avg. schooling (females aged 15-49)	6.56	6.44	0.00***
Inhabitants	486.97	429.26	0.22
Avg. household size	5.06	5.05	0.94
% Pop. recently immigrated	0.02	0.02	0.15
% Households with female head	0.16	0.16	0.54
Avg. years of schooling	4.58	4.45	0.04*
Literacy rate	0.82	0.82	0.85
% Pop. speaking indigenous lang.	0.03	0.03	0.75
% Pop. with access to healthcare	0.19	0.16	0.00**
% Households owning a television	0.76	0.75	0.38
% Dwellings with hard flooring	0.74	0.68	0.00***
% Dwellings with access to electricity	0.83	0.83	0.95
% Dwellings that are privately-owned	0.99	0.99	0.27
Observations	1100	543	1643
% of sample	67%	33%	

Notes: The control group are late-treated localities. Valid for both fertility and educational attainment datasets.

Table 6 – Balance checks (femicides dataset). Data from 2002

	Treated (by 2010)	Control (never treated)	Difference (p-value)
Femicides (homicides) per 100,000 women	2.90	6.91	0.54
Femicides (Data Cívica) per 100,000 women	2.90	5.83	0.65
Organized violence	0.00	0.00	.
Inhabitants	4519.05	4498.93	0.98
Avg. household size	5.01	4.72	0.03*
% Pop. recently immigrated	0.02	0.02	0.93
% Households with female head	0.19	0.20	0.19
Avg. years of schooling	5.74	5.59	0.51
Literacy rate	0.86	0.87	0.54
% Pop. speaking indigenous lang.	0.02	0.02	0.87
% Pop. with access to healthcare	0.34	0.25	0.05*
% Households owning a television	0.89	0.89	0.79
% Dwellings with hard flooring	0.87	0.86	0.70
% Dwellings with access to electricity	0.96	0.96	0.40
% Dwellings that are privately-owned	1.00	1.00	0.58
Observations	28	50	78
% of sample	36%	64%	

Notes: Sample is restricted to urban localities (population > 2,500 inhabitants).

5.4 Identifying assumptions

In the context of impact evaluation where treatment is non-randomized, such as this one, obtaining credible estimates of causal effects requires dispelling any concerns about possible selection bias. In this setting, where treatment participation is voluntary, there is a concern that ejidos (and thus, localities) might self-select into treatment according to criteria correlated with the outcomes. Therefore, the identification strategy has been chosen as the one that can best address this threat to the validity of the estimates. Since the identification strategy differences within localities, selection on levels is not a concern. That is, if localities had self-selected into treatment due to different outcome levels, this would not bias the estimates. However, the identification strategy does not dispel all concerns of selection bias, most prominently the bias arising from selection on trends. If localities had self-selected into treatment due to different growth trajectories in the outcome variable, the estimates would be biased.

For this reason, the validity of my methods rests on fulfilling a set of assumptions, which will be discussed in this section. To keep notation more concise, I suppress the ejido index e in the notation (treatment is as if at locality level).

The first key identifying assumption is the assumption of parallel trends. It can be formalized as follows (Sun & Abraham, 2020; see also Callaway & Sant'Anna, 2020):

$$\textit{For all } s \neq t, \textit{ the } E[Y_{it}^{\infty} - Y_{is}^{\infty} \mid G_i = g] \textit{ is the same for all } g \in supp(G_i)$$

Where Y_{it}^{∞} is the potential outcome at time t if locality i never receives the treatment, and $G_i = g$ indicates that locality i first receives treatment in time g. Essentially, it is stating that, in the absence of treatment, differently treated localities would have had the same trends in outcome (throughout the period observed). By itself, this assumption imposes parallel trends between all treated localities regardless of differences in treatment timing, so that parallel trends must hold between treated and not-yet-treated localities. It requires for the timing of treatment to be statistically independent of the potential outcome distributions.

Furthermore, this assumption can be adapted to both the case in which I compare treated localities with late-treated, and to the case in which I compare treated localities with never-treated (i.e., in the femicides analysis). In the first case, I define late-treated localities as $G_i = h$ where $h > t$ for all $t \in \{0, \dots, T\}$. For all $h \in supp(G_i)$, parallel trends must also hold between treated localities and late-treated localities; trends in outcome would have been the same between treated and late-treated, in the absence of treatment. In the second case, I define never-treated localities as $G_i = \infty$, and impose that $\infty \in supp(G_i)$; then, the assumption states that parallel trends must further hold between treated and never-treated localities. The latter is a stronger assumption to claim, therefore, it is only applied to the context of the femicides analysis; as mentioned, the femicides analysis is run using urban localities, which have proved to be comparable in terms of observable characteristics.

While made more credible by my choices of counterfactual, the parallel trends assumption is strong and easily violated. Nonetheless, the event study design allows me to partially test it. Namely, if trends were not parallel prior to treatment, I would be warned by the significance of the pre-event coefficients in the event study estimation. This gets rid of part of the concern, although there is still the possibility that something else changed at the same time as treatment and led to a systematic difference in trends – which is untestable. One robustness check will thus consist in dropping the late-treated or never-treated localities and repeating the estimation on treated localities only. The validity of that estimation still requires parallel trends to hold within treated localities, but not between treated and late-treated or treated and never-treated.

The second identifying assumption that must hold for the validity of the estimation strategy is the assumption that there is no anticipation effect of treatment. It can be formalized as follows (Sun & Abraham, 2020):

$$For\ t < g,\ the\ E\left[Y_{it}^{g} - Y_{it}^{\infty} \mid G_i = g\right] = 0\ for\ all\ g \in supp(G_i)$$

Where, in addition to the notation previously specified, Y_{it}^{g} is the potential outcome at time *t* if locality *i* is first treated in *g*. Essentially, there must be no treatment effect in pre-treatment periods. In all periods prior to treatment, outcomes in treated localities must be *as if* they were not to be treated. This assumption must also hold for late-treated localities ($G_i = h$). Callaway and Sant'Anna (2020) propose a more relaxed variant of this assumption, in which a limited anticipation effect is permitted, if the precise periods with anticipation behaviour are known and well understood. Given the nature of the treatment, I do not believe that a limited anticipation effect would be plausible. This is because the year of treatment is directly the year in which the assembly decision took place. Due to the mechanism described (dominio pleno is proposed by any ejido members) it is unlikely that the proposal and eventual approval could have been largely anticipated within the locality or the ejido.

The third assumption required for causal inference within this framework is the stable unit treatment value assumption (Rubin, 1980). It establishes that the potential outcome of any locality depends only on its treatment status (assigned at the ejido level) and is not affected by the assignment of treatment to other localities; namely, there is no "interference" effect. Also, for every locality *i*, at a specific time *t*, only one of the potential outcomes can be observed; that is, treatment must be "stable". While the second part of the assumption is unlikely to be broken in this context, the first part can be cause for concern.

In particular, as can be seen in Figure A1, ejidos are densely distributed across the region studied. Hence, there are several instances in which treated and non-treated ejidos lie directly next to each other. Gender norms are not strictly localized, so it is not impossible for the effects of treatment in an ejido (and in its set of matched localities) to partly spill over to adjacent – non-treated – localities. In point of fact, my research question posits that ejido privatization, per se, affects all ejido-matched localities at large – even if the women directly affected (i.e., who suffered a relative decrease to their relative share of household assets) are only a small share. Therefore, I expect spillover effects to happen *within* the same cluster, that is, among the localities (and households) that pertain to a given ejido. However, spillover effects must not occur *among* clusters, i.e., among different ejidos or among localities matched to different ejidos. I expect this to be tenable for the following reasons. As mentioned, the number of directly affected women in treated localities is variable and possibly small. Yet, in non-treated localities – no matter how adjacent – this number is strictly zero; the direct effect on land rights, namely, is not prone to spillovers. In absence of treatment, there are thus no direct impacts to the bargaining power balances of local households. Some degree of interference on gender norms might still occur through, for example, social interactions between localities. However, such an effect would be weak at best, and certainly not comparable to the full effect of treatment. Moreover, I note that, for the femicides analysis, the assumption of no interference effect is nearly certain to hold. Figure A4 clearly shows that urban localities (and the ejidos they are matched to) are distributed thinly across the region, with a tendency to be far apart from each other.

Lastly, the unbiasedness and consistency of my estimates is subject to the fulfilment of the standard Ordinary Least Squares (OLS) assumptions required for estimating a classical linear model with panel data.

The most crucial of these assumptions is the zero conditional mean assumption. This is written as follows (Stock & Watson, 2015):

$$E(\varepsilon_{it}|\boldsymbol{X}_i, FE_i) = 0 \textit{ for all } i = 1, \dots, n \text{ and } t = 1, \dots, T$$

Namely, the idiosyncratic error term must have conditional mean zero, given all T values of $\boldsymbol{X}$ for locality i, where $\boldsymbol{X}$ is the vector of independent variables. Fulfilling this assumption is the prerequisite in order to draw causal inference on the basis of these models.

On the other hand, the assumption of homoskedasticity is certainly going to be violated in my models, since standard errors are correlated between localities in the same treatment cluster (i.e., ejido) as well as between observations of the same locality. However, I adjust for this by clustering standard errors at the ejido level.

6 Results

The results on the effect of privatization on fertility are presented in Section 6.1. In Section 6.2, I set out to estimate the effect of privatization on educational attainments, separately for females and for males. Finally, in Section 6.3, I present the results on the effect of privatization on femicides. Table 7 briefly recaps the main empirical specifications to be used according to the outcome variable.

Table 7 – Recap of empirical specifications by outcome variable

	Fertility	Educational attainment	Femicides
Time period covered	2000–2010	2000–2010	2002–2010
Time unit	5 years	1 year	1 year
Units	All ejido-matched localities	All ejido-matched localities	Ejido-matched localities with pop. > 2,500
Main counterfactual	Late-treated	Late-treated	Never treated

6.1 Impact of privatization on fertility

To estimate the effect of privatization on fertility (intended as the child-woman ratio) I use the following specification, modelled after the event study framework of Equation 1:

$$CWR_{iet} = FE_i + FE_t + \sum_{k=-2}^{1} \beta_{5k} D_{et}^{5k} + \gamma X_{iet} + \varepsilon_{iet} \tag{3}$$

where *i* denotes the locality, *e* the ejido and *t* the year. The dependent variable is the child-woman ratio (CWR) for locality i in ejido e at time t, and ε_{iet} is the idiosyncratic error term. The vector of locality-level time-varying covariates X_{iet} includes all the local income and socioeconomic proxies listed in Section 5.1.

The data is a balanced panel dataset, spanning 2000-2010, in which localities are observed every five years. Linear interpolation is unsuitable for the dependent variable (sourced from the INEGI Census), as it would make the time effects in intercensal years essentially meaningless. Hence, there are only three time periods – 2000, 2005, 2010. To adapt to this format, event time (e_{et}) is defined as the nearest subsequent census year after ejido *e* (with all matched localities) is treated.

Since the time unit is an interval of five years, the dummies for the leads and lags become defined as D_{et}^{5k}, indicating that an ejido was privatized $5 \cdot k$ periods ago. For example, the first lag turns on five years after event time; and the first lead (which is normalized to zero, as per standard) turns on five years prior to event time. Thus, the event-study coefficients measure the difference in the CWR between treated and non-treated localities in each five-year interval relative to the five-year interval before treatment. Non-treated localities are comprised both of the counterfactual (late-treated localities) and of the treated localities that have not yet been treated (but will be treated by 2010).

Table 8 (column 1) reports the results from estimating the event study specification proposed in Equation 3. I find no statistically significant effect of privatization on the child-woman ratio. The event study coefficients and 95% confidence intervals estimated in column 1 are plotted in Figure 2. The figure confirms that, in each year relative to the year of treatment, the difference in the CWR between treated and non-treated is not significantly different from zero.

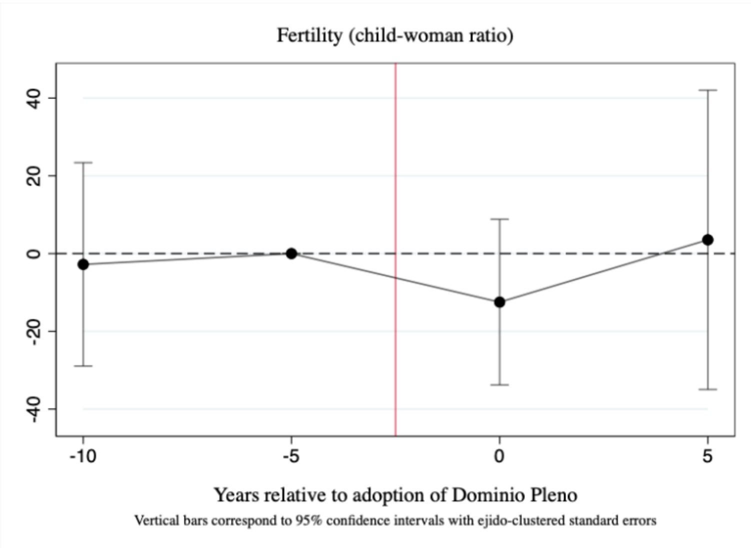

Figure 2 – Event study coefficients and confidence intervals from column 1 of Table 8.

I proceed to compare the event study estimates with those obtained from a difference-in-differences specification, since the more parsimonious model might be more likely to deliver statistical precision. Column 2 (Table 8) shows the results from estimating the difference-in-differences specification proposed in Equation 2. Once again, I find no effect of privatization on the CWR.

Next, I want to investigate if the absence of effects might be driven by heterogeneities in local characteristics that are correlated with fertility. Galiani and Schargrodsky (2010) find that land titling can contribute to *reducing* fertility when offspring and family networks are the only available insurance mechanisms. This might especially be the case in localities where household size is on average larger and family networks are thus more relied upon. Therefore, I investigate the possibility of heterogeneous treatment effects by average household size. For simplicity, these heterogeneous effects are incorporated in the difference-in-differences specification. Table A1 and

Figure A5 (Appendix) show that results are equivalent when including the heterogeneous effects in the event study specification, and there is no evidence of pre-trends.

I proceed to run a specification with heterogeneous effects by average household size, interacting the treatment indicator with household size and keeping the main effects of household size:

$$CWR_{iet} = FE_i + FE_t + \beta_1(Treated \times After)_{et} + \beta_2 Avg.hh.size_{iet} \\ + \beta_3(Treated \times After \times Avg.hh.size)_{iet} + \gamma X_{iet} + \varepsilon_{iet} \quad (4)$$

Column 3 (Table 8) shows the results from estimating Equation 4. Once heterogenous effects are allowed for, I find a strongly significant effect of privatization on the CWR, at 0.1% level. The CWR appears to decrease over time in treated localities, and particularly so in localities where household size is larger. To interpret these results, I calculate the average household size in treated localities over the entire period to be approximately five members. For a locality with an average household size, ceteris paribus privatization reduces the CWR by roughly 45 children per 1,000 women. For a locality where household size is one standard deviation above the mean (5.5 members) the effect of privatization is even more drastic, reducing the CWR by roughly 75 children per 1,000 women, ceteris paribus. On the other hand, for a locality where household size is one standard deviation below the mean (4 members) ceteris paribus privatization increases the CWR by roughly 15 children per 1,000 women.

The initial expectation was for privatization to increase fertility rates (and thus, the child-woman ratio) as a result of the hypothesized decrease in women's household bargaining power. However, the effect of the reform on fertility appears to be strongly driven by local socioeconomic characteristics, such as the average household size. Fertility is found to decrease in localities with average and higher-than-average household size, whereas it moderately increases in better-off localities that have smaller households. In the discussion of the relevant literature which helped to form my initial expectation (Section 3.2.1), I also mentioned previous findings pointing to a fertility-reducing effect of land-titling reforms (Ali et al., 2014; Galiani & Schargrodsky, 2010). My results seem to align with these studies. Namely, my findings are consistent with the hypothesis that land titling provides a form of insurance previously supplied by the offspring, therefore reducing the productive value of additional children.

On the other hand, my findings cannot prove whether the land titling reform resulted in a decrease in female empowerment. Since I am not able to decompose the estimates further, I cannot discern the mechanisms at play behind them. Land titling might impact fertility entirely through the tenure security channel, hence leading to the negative estimate – as land titling increases tenure security, which decreases the productive value of offspring and hence the child-woman ratio. However, land titling might affect fertility not only through the tenure security channel, but also through the female bargaining power channel; that is, land titling lowers female bargaining power, which leads to an increase in the child-woman ratio, since women are less able to bargain for the desired (i.e., lower) number of children. In this scenario, the negative sign of the estimated coefficients would imply that the tenure security mechanism more than offsets the female bargaining power mechanism. Overall, while I cannot prove that privatization is affecting fertility through the female bargaining power channel, I cannot entirely rule it out either.

Table 8 – Dependent variable: Fertility (child-woman ratio)

	Event study	Difference-in-differences	
	(1)	(2)	(3)
Time since event:			
$k = -10$	-2.778		
	(13.34)		
$k = -5$	0		
$k = 0$	-12.48		
	(10.87)		
$k = 5$	3.535		
	(19.63)		
Treated × After		-12.88	255.5***
		(10.79)	(55.82)
Treated × After × Avg. household size			-60.11***
			(12.05)
Avg. household size			30.29**
			(9.554)
Locality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Control group	Treated after 2010	Treated after 2010	Treated after 2010
F-stat	14.42	16.26	16.97
P-value	0.00000841	0.00000236	8.47e-10
R^2	0.0942	0.0938	0.105
Mean dep. var.	480.2	480.2	480.2
Localities	1642	1642	1642
Observations	4925	4925	4925

Notes: Standard errors clustered by ejido, in parentheses. The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).
+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

I perform a series of tests to confirm the robustness of the results. First, as anticipated in Section 5.4, the standard robustness check is to repeat the estimation on treated localities only (i.e., treated by 2010). My main estimates rest on the assumption that late-treated localities are a valid counterfactual for localities treated by 2010. However, localities might have selected into earlier treatment assignment due to pre-existing trends in female empowerment. For instance, if localities where female empowerment was on a downward trend were more likely to receive treatment early on, then localities treated after 2010 would not be an appropriate counterfactual. Therefore, this robustness check allows me to repeat the estimation relaxing this assumption. All variation now comes exclusively from the differential timing of treatment among the localities treated by 2010. The estimates thus obtained are presented in Table 9. The results are indeed robust to excluding the counterfactual (i.e., late-treated localities). Estimates from the difference-in-differences specification with heterogenous effects (column 3) are still strongly significant at 0.1% and large in magnitude. Moreover, when looking only at treated localities, a modest negative effect of privatization emerges even in the event study specification (column 1).

The remaining robustness checks are presented in the Appendix. In Table A2, I investigate whether heterogeneous treatment effects hold when considering other local socioeconomic characteristics, such as the average schooling attainment. Although the average schooling variable is in fact endogenous, it confirms the presence of heterogeneous effects, with fertility decreasing in the more disadvantaged localities (with average or lower levels of schooling). In Table A3, I repeat the estimations of columns 2-3 (Table 8), first dropping localities treated in 2006-2010, then localities treated in 2001-2005. The estimates are found to be robust to removing subsets of localities according to treatment timing, implying that results are not driven by a particular subset of treated localities. This finding further confirms my assumption that treatment timing is statistically independent of the potential outcome distributions, as stated in Section 5.4.

Table 9 – Dependent variable: Fertility (child-woman ratio). Robustness: Only treated

	Event study	Difference-in-differences	
	(1)	(2)	(3)
Time since event:			
$k = -10$	20.30		
	(17.54)		
$k = -5$			
$k = 0$	-35.91*		
	(15.66)		
$k = 5$	-42.09		
	(31.34)		
Treated × After		-22.70^{+}	275.3***
		(13.74)	(61.20)
Treated × After × Avg. household size			-64.30***
			(12.81)
Avg. household size			39.43**
			(12.30)
Locality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Control group	None	None	None
F-stat	10.07	10.61	11.82
P-value	5.19e-22	0.000341	4.65e-08
R^2	0.0933	0.0930	0.110
Mean dep. var.	480.0	480.0	480.0
Localities	1099	1099	1099
Observations	3297	3297	3297

Notes: Standard errors clustered by ejido, in parentheses. The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring),

local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).
+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

6.2 Impact of privatization on educational attainment by gender

To estimate the effect of privatization on educational attainment by gender I use the event study framework proposed in Equation 1. The main dependent variable is the average years of schooling attained by females aged 15 to 49 for locality i in ejido e at time t. Additionally, to capture the differential effect by gender on the schooling received post-privatization, I run the same specification on the average years of schooling attained by males aged 15-49. The vector of locality-level time-varying covariates X_{iet} includes all the local income and socioeconomic proxies listed in Section 5.1.

The data is a balanced panel dataset, spanning 2000-2010, in which localities are observed yearly. I use the number of event time dummies indicated in Section 5.1, which means that I bin up the highest endpoint at +4, and the lowest endpoint at -4. Each of the event-study coefficients measures the difference in educational attainment between treated and non-treated localities in each year relative to the year before treatment. Non-treated localities include both the counterfactual (late-treated localities) and treated localities that have not yet been treated (but will be treated by 2010).

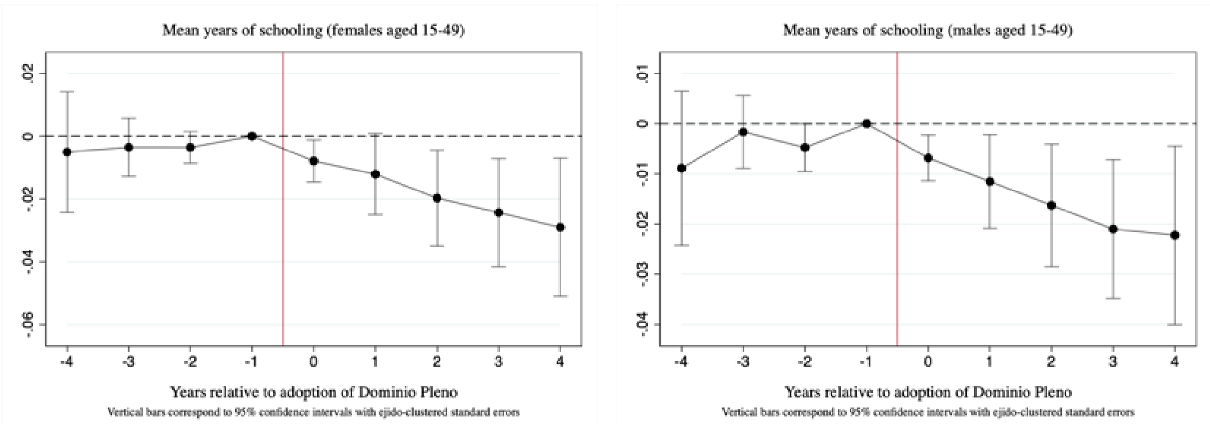

Figure 3 – Event study coefficients and confidence intervals from columns 1 and 3 of Table 10

Table 10 (column 1 and column 3) presents the results from estimating the event study specification proposed in Equation 1, respectively on female and male schooling. Additionally, the event study estimates are compared with those obtained from a difference-in-differences specification. Column 2 and 4 (Table 10) show the results from estimating the difference-in-differences specification proposed in Equation 2, respectively on female and male schooling.

The results suggest that female educational attainment decreases in treated localities with respect to late-treated. Statistical significance ranges between 5% and 1%. However, a similar effect is obtained on male educational attainment, indicating that there is no differential impact by gender[24].

The effects are picked up by both the event study and the difference-in-differences specifications. In addition, the event study allows me to identify the presence of a slight dynamic effect, with the impact of privatization on schooling growing over time. For instance, in the very year of treatment, localities experience a reduction in average female schooling by 0.008 years with respect to non-treated localities, ceteris paribus. Four years or more after treatment, average female schooling is reducing by 0.029 years, ceteris paribus. Similar trends can be observed for average male schooling. It is reasonable for effects to be more pronounced in the medium-long term, since the younger age groups, which are more likely to be affected, can only be observed in the dependent variable from the age of 15 onward.

The figures are not very large in magnitude, but this might be due to the wide age bracket analyzed; likely, the effects only operate through the teenage-age men and women that are comprised in this age bracket. Also, any reduction in average educational attainments is all the more interesting when recalling that, in the balance checks (Table 5), educational attainment as of 2000 was found to be higher in treated localities, for both men and women.

For the most part, the estimates from the event study specifications rule out the existence of pre-trends; however, it should be pointed out that one of the pre-event study coefficients (β_{-2}) is slightly significantly different from zero (at 10%) in the regression on male educational attainment. As long as it is an isolated occurrence, it should not be cause for concern; I will therefore look out for any evidence of pre-trends in the following specifications.

Overall, the results point to a negative effect of privatization on educational attainment, regardless of gender. This is not entirely in line with my expectation of a gendered impact on schooling, but it is consistent with the hypothesis, discussed in Section 3.2.2, that educational investment decreases for all children when women lose bargaining power within the household. In this sense, my results align with e.g., the findings of Katz and Chamorro (2003), Doss (2006) and Martinez (2013). Also, since land privatization should result in more efficient, less labor-intensive agricultural production (De Janvry et al., 2015), I exclude the possibility that children are receiving less formal education due to higher demand for low-skilled agricultural labor[25]. Therefore, assuming that mothers have a stronger preference for the educational investment of children – of both genders – with respect to fathers, my estimates indicate that, indeed, the land reform negatively affected women's bargaining power.

[24] While it is not shown here, I also run the same specification on the gender gap in schooling (i.e., the difference between male and female schooling). I found no statistically significant effect of privatization on the gender gap in schooling, confirming the absence of a differential impact by gender.

[25] The credibility of this hypothesis is reinforced by the fact that, prior to the reform, labor was misallocated between agricultural and non-agricultural sector; in the early 1990s, agriculture accounted for only 4 percent of GDP while 34 percent of the population lived in rural areas (De Janvry et al., 2015; Warman, 2001).

Table 10 – Dependent variable: Mean years of educational attainment

	Females (aged 15-49)		Males (aged 15-49)	
	(1)	(2)	(3)	(4)
Time since event:				
$k \leq -4$	-0.00506		-0.00888	
	(0.00981)		(0.00784)	
$k = -3$	-0.00355		-0.00167	
	(0.00472)		(0.00369)	
$k = -2$	-0.00357		-0.00477*	
	(0.00258)		(0.00243)	
$k = -1$	0		0	
$k = 0$	-0.00790*		-0.00685**	
	(0.00342)		(0.00231)	
$k = 1$	-0.0121^{+}		-0.0115*	
	(0.00658)		(0.00474)	
$k = 2$	-0.0197*		-0.0163**	
	(0.00776)		(0.00621)	
$k = 3$	-0.0243**		-0.0210**	
	(0.00877)		(0.00705)	
$k \geq 4$	-0.0290**		-0.0223*	
	(0.0112)		(0.00907)	
Treated × After		-0.0113*		-0.00886*
		(0.00518)		(0.00416)
Locality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control group	Treated after 2010	Treated after 2010	Treated after 2010	Treated after 2010
F-stat	3570.3	4336.7	2839.0	3689.0
P-value	0.0000458	0.0000305	5.59e-08	0.000000159
R^2	0.965	0.965	0.960	0.960
Mean dep. var.	7.124	7.124	7.248	7.248
Localities	1643	1643	1643	1643
Observations	18073	18073	18073	18073

Notes: Standard errors clustered by ejido, in parentheses. The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).
+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

I proceed to run a series of tests to confirm the robustness of the results. First, I perform the standard robustness check described in Section 5.4, which consists in repeating the estimation with treated localities only (i.e., treated by 2010). If localities selected into "late" treatment due to pre-existing upward trends in female empowerment, treatment effect would appear overestimated; repeating the estimation without the counterfactual would then allow me to get closer to the true treatment effect. Nonetheless, it is important to mention that, when performing this robustness check, I am using a much smaller subset of localities, which results in a loss of power. Therefore, significance is likely to drop – although this may not be necessarily attributable to a reduction in selection bias.

The estimates thus obtained are presented in Table 11 (see also Figure A6). The results are mostly robust to excluding the counterfactual (i.e., late-treated localities). Significance and magnitude are more than retained; they actually increase quite sizably with respect to the main estimates. Once again, I detect a modest significance in two pre-event study coefficients (respectively for male and female schooling). When comparing pre-event study coefficients between Table 10 and Table 11, though, I find that they are not consistent with each other; while Table 10 shows evidence of a slight, negative pre-trend, pre-trends in Table 11 would appear to be positive. This convinces me that the pre-event effects found are not reason enough to suspect a violation of the parallel trends assumption.

Next, I perform a series of robustness checks to address the concerns that were raised in relation to this dependent variable in Section 4.2. Namely, the dependent variable is sensitive to differences in the age composition of the population as well as to differences in patterns of outmigration. Therefore, the validity of the estimates rests on the assumption that the age structure does not experience differential trends between treated and untreated localities. While it is not possible to entirely relax that assumption – given that I have no information on e.g., average age in the locality – it can somewhat be tested for using the child-woman ratio. The child-woman ratio has not been included as a control in the main specification since it constitutes a classic example of "bad control", which is a variable that may itself be an outcome (Angrist & Pischke, 2009). However, it is interesting to see if, and how, estimates change when controlling for fertility. According to the previous estimates (Section 6.1), fertility appears to be somewhat decreasing in treated localities. If this decrease were large enough to affect the whole age composition, it could be responsible for the negative effect that I have observed on schooling. Therefore, I re-estimate the event study specifications of columns 1 and 3 (Table 10), but additionally controlling for the child-woman ratio. This allows me to gauge if differential age structures play any relevant role in the effects obtained on schooling. The estimates thus obtained are shown in Table A4 in the Appendix. The results are in fact robust to partialing out differences in fertility trends across localities. Therefore, the negative effect of privatization on educational attainments is presumably not driven by a difference in the age composition.

In Section 4.2 I also mentioned that the wide age bracket makes the outcome variable sensitive to differences in local migration patterns. De Janvry et al. (2015), studying the impact of PROCEDE (the certification program required prior to "dominio pleno" adoption), find that undergoing the program increases the likelihood of migration, at household and ejido level. Previously, use-based property rights required ejidatarios to work their land continuously, or else lose such rights; PROCEDE eliminated this requirement, thus reducing the opportunity cost of emigration. While the effect of PROCEDE on migration was the most decisive, dominio pleno might produce a further migration-inducing effect, through the possibility of selling one's land and using the proceeds to emigrate.

Therefore, the concern of a confounding effect from migration must be addressed for the estimates to be credible. If dominio pleno led to different migration trends, such as by facilitating outmigration from treated localities, the levels of education observed in treated localities may appear lower not due to a contraction in schooling, but due to an educational selectivity in migration (if emigrants tend to have more years of schooling).

Therefore, I set out to control for differences in outmigration patterns between localities. This is notoriously a difficult task due to the lack of data, which is certainly lacking in this setting as well. I have knowledge of local immigration (indicated by the share of population that immigrated from elsewhere within the previous five years) but not of emigration. Thus, I resume to two different expedients. First, I consider only a subset of localities which do not experience a population decline between 2000 and 2010; this is to exclude any localities that suffered substantial negative net migration. Additionally, I impose on this subset of localities that the proportion of population who are recent immigrants is not higher than the Bajío average; hence, I further exclude any localities that experienced higher immigration than average. Overall, these steps allow me to exclude any localities where migration patterns are unusual. I re-estimate the event study specifications of columns 1 and 3 (Table 10) on this new subset of localities and report the results in Table A5 (columns 1-2). Excluding these localities results in the new sample being slightly less than half the sample used in Table 10.

My second expedient to control for differences in outmigration patterns is to exclude any states which were known to have net outmigration between 2000-2010. I obtain aggregate information on migration patterns by state from INEGI (2000, 2010). Among all seven Bajío states, three states experienced net outmigration: Michoacán, San Luis Potosí, and Zacatecas. I proceed to exclude all localities situated in these states and repeat the estimation on the smaller subset thus obtained. The new estimates are reported in Table A5 (columns 3-4). Excluding these three states results in the new sample being approximately 60 percent of the sample used in Table 10.

Overall, the results in Table A5 indicate that the estimates are not entirely robust to controlling for outmigration with these two expedients. Namely, with the first expedient, estimates remain mostly significant and unchanged in magnitude; but with the second expedient, significance is lost entirely. These findings are concerning, because they indicate that the effect of treatment is largely limited to only three of the seven states. Whether this is due to an underlying difference in migration trends or to an issue of power, the credibility of my estimates of causal effects is diminished. However, given the encouraging results of columns 1-2, the estimates do not appear to be entirely driven by differences in outmigration patterns. Therefore, while it cannot be ruled out that the decrease in educational attainment is caused by migration dynamics, it seems plausible that at least some of it is caused through the female empowerment mechanism. With some caution, I can view these results as a suggestion that the land reform negatively affected female bargaining power.

Table 11 – Dependent variable: Mean years of educational attainment. Robustness: Only treated

	Females (aged 15-49)		Males (aged 15-49)	
	(1)	(2)	(3)	(4)
Time since event:				
$k \leq -4$	0.0150*		0.00978	
	(0.00722)		(0.00610)	
$k = -3$	0.00548		0.00692^{+}	
	(0.00469)		(0.00387)	
$k = -2$	0.000970		-0.000439	
	(0.00319)		(0.00262)	
$k = -1$	0		0	
$k = 0$	-0.0122*		-0.0108***	
	(0.00497)		(0.00305)	
$k = 1$	-0.0200*		-0.0188**	
	(0.00990)		(0.00654)	
$k = 2$	-0.0320*		-0.0276**	
	(0.0129)		(0.00888)	
$k = 3$	-0.0409**		-0.0361***	
	(0.0157)		(0.0107)	
$k \geq 4$	-0.0587*		-0.0498***	
	(0.0230)		(0.0148)	
Treated × After		-0.0142*		-0.0134**
		(0.00717)		(0.00484)
Locality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control group	None	None	None	None
F-stat	2711.8	3233.9	2034.7	2640.4
P-value	0.0000115	0.0000709	8.81e-09	1.06e-08
R^2	0.966	0.966	0.959	0.959
Mean dep. var.	7.161	7.161	7.302	7.302
Localities	1100	1100	1100	1100
Observations	12100	12100	12100	12100

Notes: Standard errors clustered by ejido, in parentheses. The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6.3 Impact of privatization on femicides

Lastly, I estimate the effect of privatization on the incidence of femicides, using the event study framework proposed in Equation 1. The dependent variable is the number of femicides per 100,000 women taking place in locality i in ejido e at time t. The vector of locality-level time-varying covariates X_{iet} includes all the local income and socioeconomic proxies listed in Section 5.1, plus a binary variable indicating whether any organized violence events occurred within a 20 km radius from the locality.

As discussed in Section 4.2, I consider two alternative approaches to define femicides. My preferred definition of femicides is the total number of female homicides. While total homicides might be an overestimate of the true incidence of the phenomenon, Data Cívica's definition is prone to understate the true number of femicides and to be affected by misreporting. The main estimations are therefore performed using total female homicides as the dependent variable. However, I also repeat them using the definition of femicides provided by Data Cívica.

The femicides analysis is run on the subset of ejido-matched, urban localities. In Table 6, treated and never-treated urban localities appeared to be comparable, with very similar levels of observable characteristics as of 2002. The parallel trends assumption seems thus likely to hold between treated and never-treated urban localities. Given the drastically reduced sample size, using late-treated localities as a counterfactual would not be advisable. For these reasons, it is necessary, and possibly adequate, to use never-treated localities as the preferred control group. However, I additionally show results obtained using late-treated localities as a control group.

The data is a balanced panel dataset, spanning 2002-2010, in which localities are observed yearly. Each of the event-study coefficients measures the difference in number of femicides between treated and non-treated localities in each year relative to the year before treatment. Non-treated localities include both the counterfactual (never-treated localities) and treated localities that have not yet been treated (but will be treated by 2010). I use the number of event time dummies indicated in Section 5.1, which means that I bin up the highest endpoint at +3, and the lowest endpoint at -3. Hence, there is one less lead, and one less lag, compared to the specification used for the schooling analysis. As explained in Section 5.1, this choice is motivated by the much smaller sample used here, which makes it preferrable to use a more parsimonious specification. Nonetheless, for completeness, I also repeat the main estimation using the larger number of event time dummies, binning up the endpoints at -4 and +4; I show that results are mostly equivalent (Table A6 in the Appendix).

For all results reported, I compare the event study estimates with those obtained from a difference-in-differences specification such as the one proposed in Equation 2. Since difference-in-differences is a more parsimonious model, it may be more likely to deliver statistical precision; given the small sample size, it is valuable to estimate such a specification next to the event study.

Table 12 (columns 1-2) presents the results from estimating the event study specification proposed in Equation 1. The effect is positive and significant at 5% level when using the control group of never-treated localities, signaling an increase in femicides both 1 year and 3+ years after treatment (column 1). The estimation done with the narrower control group of late-treated localities also picks up modest effects for the same lags, although they are only marginally significant at 10% (column 2); while this is not my preferred specification, it serves as reassurance that the effect observed in column 1 is not entirely driven by the variation among treated and never-treated localities. Another encouraging sign is that the difference-in-differences specification produces a positive, significant estimate in both cases, regardless of the counterfactual used (columns 3-4). Lastly, there is no evidence of pre-trends.

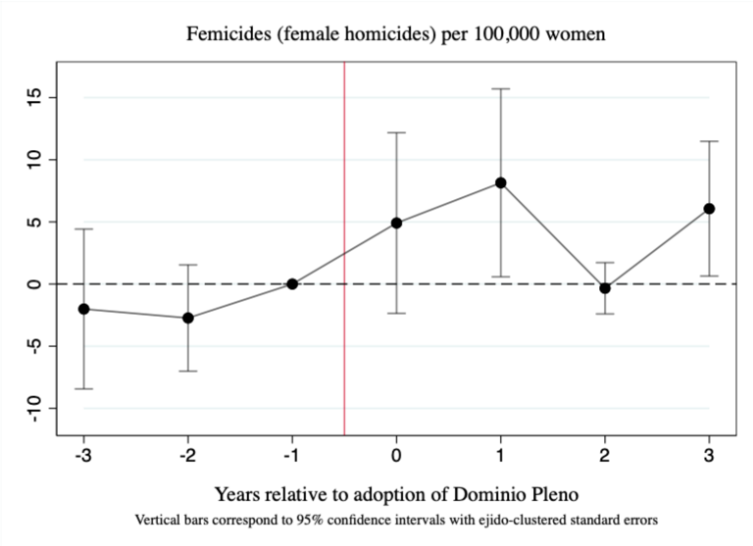

Figure 4 – Event study coefficients and confidence intervals from column 1 of Table 12

The results presented in Table 12 (column 1) indicate a sizable effect of privatization on the incidence of femicides. One year after treatment, about 8 more femicides (per 100,000 women) occur in treated localities with respect to non-treated localities, ceteris paribus. The effect appears to weaken slightly over time: 3+ years after treatment, this increase is of about 6 femicides per 100,000 women. Both figures, though, represent a very large increment, when considering that the average incidence of femicides for urban localities over 2000-2010 is approximately 3 per 100,000 women (see mean of the dependent variable in Table 12).

Table 12 – Dependent variable: Femicides (all homicides) per 100,000 women

	Event study		Difference-in-differences	
	(1)	(2)	(3)	(4)
Time since event:				
$k \leq -3$	-2.009	-0.303		
	(3.280)	(3.082)		
$k = -2$	-2.732	-2.950		
	(2.182)	(2.327)		
$k = -1$	0	0		
$k = 0$	4.908	3.859		
	(3.708)	(4.066)		
$k = 1$	8.144^{*}	7.028^{+}		
	(3.856)	(3.698)		
$k = 2$	-0.340	-1.153		
	(1.052)	(1.256)		
$k \geq 3$	6.062^{*}	4.574^{+}		
	(2.766)	(2.623)		
Treated × After			6.411^{*}	4.942^{*}
			(2.521)	(2.219)
Locality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control group	Never treated	Treated after 2010	Never treated	Treated after 2010
F-stat	1.444	3.697	1.039	1.298
P-value	0.0443	0.102	0.195	0.230
R^2	0.0552	0.0903	0.0483	0.0739
Mean dep. var.	3.227	3.598	3.227	3.598
Localities	78	38	78	38
Observations	702	342	702	342

Notes: Standard errors clustered by ejido, in parentheses. Sample is restricted to urban localities only (average population > 2,500). The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants), and whether any organized violence events occurred within a 20 km radius.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Next, the event study and difference-in-differences specifications are re-estimated using the alternative dependent variable – number of femicides fulfilling Data Cívica's criteria. The results are reported in Table 13. Both the event study (columns 1-2) and the difference-in-differences (columns 3-4) estimations do not pick up an effect this time, regardless of the control group used. Therefore, it would appear that femicides as defined by Data Civica are not affected by the reform; that is, those in which sexual assault was declared to be the cause of death, those in which the victim had formerly reported domestic violence, and those which took place inside the house.

It is important to mention, though, that the femicides meeting these criteria are much fewer than total homicides with a female victim, as can be appraised when comparing the mean of the dependent variable in Table 13 (1 femicide per 100,000 women) to the one in Table 12 (3 femicides per 100,000 women). Oftentimes, this is because the information needed to assess the criteria is missing or misreported, but it can also be that these criteria no longer reflect the changed nature of this phenomenon (see footnote 22, Section 4.2). The rarity of their occurrence, at any rate, means that the variable takes predominantly the value of zero. This can introduce a lot of noise in the estimations and prevent from reliably capturing the underlying effect.

The implication is that the findings are not robust to using a narrower definition of femicides. Therefore, it is important to underline that the validity of the estimates is limited to the causal effect of privatization on the *total of female homicides.* That being said, I turn to test the robustness of the main estimates when some of the assumptions change.

First, I perform the standard robustness check described in Section 5.4, which consists in repeating the estimation with treated urban localities only (i.e., treated by 2010). This is to verify that results hold when neither never-treated nor late-treated urban localities are a valid counterfactual. However, selecting only treated urban localities results in shrinking the already small sample size even further; from 702 observations (78 distinct localities) to 252 observations (28 distinct localities). The loss of power that will follow is likely to cause a sizable drop in significance, regardless of whether there exists an underlying effect. Table 14 shows the estimates so obtained. As expected, significance is lost entirely when shrinking the sample to treated urban localities only. This result indicates that most of the power in the estimation comes from the contrast with the counterfactual, rather than from the differential timing of treatment among the treated.

Next, I hypothesize that there might be heterogeneities in the treatment effect according to the local incidence of organized violence. Since the preferred dependent variable includes all homicides where the victim is a woman, treatment effects could appear stronger in localities where homicidal violence is generally higher due to organized violence. This is particularly important due to the setting and time frame observed; starting from 2007, drug-related violence has increased dramatically in Mexico, claiming more than 60,000 lives (Dell, 2015). Until now, I controlled for a binary variable indicating if any deaths from organized violence took place within a 20 km radius from the locality in a given year. To better investigate the presence of heterogeneities in levels of organized violence, I interact this organized violence indicator with the treatment indicator. For simplicity, these heterogeneous effects are included in the difference-in-differences specification. I proceed to estimate the following model:

$$Femicides_{iet} = FE_i + FE_t + \beta_1(Treated \times After)_{et} + \beta_2 Org.violence_{iet} + \beta_3(Treated \times After \times Org.violence)_{iet} + \gamma X_{iet} + \varepsilon_{iet} \quad (5)$$

The results are reported in Table A7 in the Appendix. As it turns out, there is no evidence of heterogeneous treatment effects by levels of organized violence; the significance and magnitude of the treatment effect remains unvaried. Unexpectedly, organized violence even appears to be modestly negatively linked to the incidence of femicides. This might suggest that femicides are more prone to happen in otherwise non-violent localities. This is consistent with my hypothesis that female homicides are related to gender power dynamics and not merely to higher incidence of violence in the locality. Furthermore, this finding adds support to my choice of using total female homicides as the preferred definition of femicides; as it implies that femicides, when defined as all female homicides, are not likely to be overinflated by differences in generalized, organized violence.

As a last check, I run a sensitivity analysis on the threshold defined for femicides to be assigned to a given locality. When listing the variables (Table 2) I have described the femicides variables as "femicides taking place within a 5 km radius from the locality centroid". Therefore, a locality is assigned all femicides taking place in its immediate vicinity, even if, technically, they were registered or processed in another locality. The choice to set a 5 km radius is prompted by a series of reasons. First, localities can be quite small and often directly adjacent to each other. Second, I do not know the exact death location, but only the coordinates of the centroid of the locality in which they took place. However, they could have taken place at the boundaries of said locality, effectively involving, to some extent, the adjacent localities. These considerations are all the more strengthened by the fact that I am looking at urban localities. Nonetheless, since 5 km is an arbitrary level, it is appropriate to conduct a sensitivity analysis using a 20% smaller radius (4 km), and a 20% larger one (6 km). Results from this analysis are reported in Tables A8 and A9 in the Appendix. Understandably, significance drops when using the smaller radius, although results still show a marginal significance at 10%; the 4 km radius makes localities much less likely to be assigned femicides, as can be appraised from the drop in the mean of the dependent variable. On the other hand, when using the 6 km radius, significance and magnitude increase with respect to the original estimates – as does the mean of the dependent variable. Overall, I can conclude that the results are reasonably robust to the change in radius.

In sum, the results from the femicides analysis are the most striking among all presented so far. They are suggestive of a considerable effect of privatization on the incidence of femicides, which are believed to rise on average by at least 5 deaths per 100,000 women, following treatment. Due to the large magnitude – and implications – of these estimates, however, they should be taken with caution. For the most part, these estimates hold against a series of robustness checks. They are not robust to dropping the control group, suggesting that most of the power comes from the contrast with the counterfactual. Nor are they robust to changing the definition of femicides, implying that the findings are only applicable to the incidence of total female homicides – rather than to a narrower definition of femicides. However, one might argue that the loss in efficiency from applying either change is immense; the mean of femicides under the definition of Data Cívica is roughly 1 per 100,000, substantially lower than the already meagre mean of female homicides; and the sample is restricted to merely 28 localities when dropping the control group. Lastly, the estimates are robust to using a larger number of event study dummies, to changing the radius linking femicides and localities, and to allowing for heterogenous treatment effects by levels of organized violence.

Table 13 – Dependent variable: Femicides (Data Cívica def.) per 100,000 women

	Event study		Difference-in-differences	
	(1)	(2)	(3)	(4)
Time since event:				
$k \leq -3$	-0.964	0.288		
	(3.053)	(2.744)		
$k = -2$	-1.859	-2.016		
	(1.820)	(1.816)		
$k = -1$	0	0		
$k = 0$	-0.688	-1.477		
	(1.721)	(1.555)		
$k = 1$	1.489	0.926		
	(3.163)	(3.037)		
$k = 2$	-2.032	-1.488		
	(1.681)	(1.654)		
$k \geq 3$	1.041	1.476		
	(2.074)	(1.921)		
Treated × After			0.795	-0.0663
			(1.813)	(1.240)
Locality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control group	Never treated	Treated after 2010	Never treated	Treated after 2010
F-stat	0.618	1.087	0.790	1.601
P-value	0.823	0.754	0.789	0.549
R^2	0.0551	0.0629	0.0523	0.0443
Mean dep. var.	1.527	1.236	1.527	1.236
Localities	78	38	78	38
Observations	702	342	702	342

Notes: Standard errors clustered by ejido, in parentheses. Sample is restricted to urban localities only (average population > 2,500). The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants), and whether any organized violence events occurred within a 20 km radius.

+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 14 – Dependent variable: Femicides (all homicides) per 100,000 women.
Robustness: Only treated

	Event study	Difference-in-differences
	(1)	(2)
Time since event:		
$k \leq -3$	1.912	
	(5.155)	
$k = -2$	-2.026	
	(2.853)	
$k = -1$	0	
$k = 0$	2.615	
	(4.521)	
$k = 1$	4.809	
	(4.483)	
$k = 2$	-4.565	
	(4.221)	
$k \geq 3$	-1.824	
	(6.623)	
Treated × After		3.689
		(3.247)
Locality FE	Yes	Yes
Year FE	Yes	Yes
Covariates	Yes	Yes
Control group	None	None
F-stat	115.1	4.222
P-value	0.168	0.177
R^2	0.106	0.0864
Mean dep. var.	4.131	4.131
Localities	28	28
Observations	252	252

Notes: Standard errors clustered by ejido, in parentheses. Sample is restricted to urban localities only (average population > 2,500). The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants), and whether any organized violence events occurred within a 20 km radius.
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

7 Discussion

Before drawing the conclusions and policy implications of this analysis, I discuss some additional points that are crucial to interpret the results and identify my contribution to the literature. I start by addressing the limitations of this study and follow with a discussion of its internal and external validity. Lastly, I suggest further extensions for future research in this topic.

7.1 Limitations

In this sub-section, I discuss two main limitations of this study. First, the main result of this study concerns the increase in the local incidence of femicides (defined as the total of female homicides) following the adoption of dominio pleno. I have estimated an average yearly increase of 6 to 8 femicides per 100,000 women – an effect which appears to decrease over time. In recent years, though, the homicide rate in Mexico has intensified, both with regards to female and to male victims. Since 2015, every year the national female homicide rate has hiked up from the previous year. In 2017, 5.2 women (per 100,000) were murdered, compared to 4.2 in 2010 (Data Cívica, 2019). It would be of utmost interest to evaluate how the impact of the reform on femicides has evolved within this wider intensification in the phenomenon. However, the temporal limitation of the data does not allow me to observe these recent years.

Second, while the effect of the reform on female empowerment essentially stems from intra-household dynamics, the data only allows me to observe outcomes at locality level. In this sense, my approach differs from the one typically employed in the empirical literature on the link between female bargaining power and asset ownership. The standard approach has the advantage of identifying more clear-cut effects on bargaining outcomes at the household level, although it largely suffers from a problem of reverse causation; since, unless the relative variation in land rights is induced exogenously, it could be simultaneously determined by pre-existing bargaining power balances. Locality-level outcomes and changes over time are less likely to suffer from the bias of individual-level outcome measures. Therefore, one could say that the use of locality-level data is at the same time a strength and a limitation. On the one hand, it minimizes the threat of endogeneity. On the other hand, it does not allow to distinguish the change in household dynamics from its ensuing repercussions on society – that is, the extent to which the deterioration of gender relations within the household translates into their deterioration within the public arena. Also, as discussed in Section 5.4, not all households within a treated locality are actually treated. Thus, the identification of an effect must rely on the presence of spillover effects within treatment clusters.

7.2 Validity

The internal validity of my estimates requires that the identifying assumptions discussed in Section 5.4 hold. That is, trends must be parallel between treated and non-treated localities (whether those are non-yet-treated, late-treated, or never-treated). Also, there must be no anticipation effect of treatment. Lastly, there must be no spillover effects among treatment clusters. The robustness checks performed throughout Section 6 largely support the validity of these identifying assumptions.

The remaining issue to be addressed is the potential measurement error in the independent variable which was discussed in Section 4.3. Namely, the standardized treatment indicator used does not distinguish the precise extent of ejido land to which dominio pleno is applied. However, within a locality level perspective, the actual extent of dominio pleno land within the ejido does not fully matter. The turning point for the locality consists in the assembly approval of a dominio pleno transition per se. The specific amount of land may depend on a number of factors (e.g., the amount of land allocated for common uses or other prespecified purposes within the ejido). Moreover, the measurement error that might stem from the simplification of using a binary treatment variable is likely counterbalanced by the variability in the relative extent of the ejido itself across localities. In some cases, the area and inhabitants of a locality coincide exactly with the matched ejido. But in other cases, the locality is either smaller or larger than the ejido, in terms of extension but mostly of inhabitants. There is thus an additional, random source of variation in the intensity with which a given locality receives the impact of the treatment which is assigned to the respective ejido. I must assume that the overall potential measurement error in the independent variable is not systematic. In other words, the approximation in the treatment indicator must not be correlated with the true extent of treatment at locality level. The potential violation of this assumption – which would bias the estimates – is the main threat to internal validity in this setting.

I proceed to consider the external validity of my estimates. The data used spans the entire population of ejido-matched localities in the states of the Bajío. As long as the criteria for internal validity are fulfilled, the estimates identify the causal effect of land titling on female empowerment outcomes within the Bajío region. However, these results may not be easily generalized to different populations or settings, such as the ejido-matched localities in the rest of Mexico.

There are several reasons why my estimates may not be applicable to the entire country. First, local gender norms differ in other areas of Mexico, which is a country characterized by high cultural diversity. Second, the agricultural potential of land is higher in the Bajío, while e.g., in the South, land quality is inferior. The literature emphasizes how in numerous ejidos – particularly in the South – agricultural output is insufficient to sustain livelihoods (e.g., Assies & Duhau, 2008; Deininger & Bresciani, 2001; Lewis, 2002). As a result, outside the Bajío, land privatization might in fact be motivated by mostly non-agricultural opportunities, such as tourism development. Land privatization might then bring about strong economic changes, and with it, adjustments to the local labor market or intensification in migration patterns. The presence of these additional factors might somewhat offset, or reinforce, the effect of the reform on female empowerment. In spite of these differences, I believe that the *direction* of the effect estimated for the Bajío may be generalizable to the whole country. This is especially plausible for the estimated causal effect on femicides, although the intensity of this impact could vary.

Additionally, I consider the external validity of the estimates in relation to land reforms in other countries. Numerous other countries have undergone land titling reforms characterized by similar gender blindness. My estimates cannot be generalized to other such land reforms. The gendered effects of the Mexican reform arise from the specific discriminatory mechanisms implicit in its terms, as well as a prior history of women's exclusion from 20th century land policy. But to a certain extent, the findings from the Mexican experience can be informative for other countries with comparable land titling reforms, especially in Latin America. In broad terms, they caution about the likely detrimental effects of introducing purportedly "gender neutral" land reforms in settings with pre-existing gender-based discrimination.

7.3 Further research

The limitations and concerns outlined above suggest possible extensions to explore this research question beyond the confines of this paper. Primarily, these suggestions are directed towards expanding the data collection process. As the main source of data for this paper – the ejido dataset – had to be personally compiled, the level of detail and geographical scope was constrained. In absence of these constraints, it would be valuable to extend the data collection to the entirety of Mexico. Moreover, the simplification of using a binary treatment variable when treatment extent varies poses concerns for the internal validity of the estimates. A more extensive data collection process could keep track of the precise extent of treatment, taking also into account differences in locality size in relation to the ejido.

The second suggestion regards the framework of the research question. An alternative approach could explore whether the property rights regime may be unfavorable by itself, that is, whether women thrive less when land is privately owned as opposed to collectively owned, regardless of the gender bias of the privatization process itself. Support for this hypothesis comes from anthropological and sociological literature with a mostly historical perspective aimed at retracing the origins of gender inequality. For example, Whyte (1978), Goody (1973), Beauvoir (1949) and Engels (1884) identify the historical advent of private property as the main downfall for women's status, presumably due to the increased importance for men to ensure children's legitimacy and therefore women's fidelity. Additionally, ample evidence from the literature suggests the applicability of this hypothesis to increasing levels of private property, especially in Mexico. Women's restricted mobility, lower productivity and higher domestic responsibilities prevent them from thriving in a private land market context as well as men (Djurfeldt, 2020). Many of the cultural and structural factors that exclude women from state agrarian reform programs are likely to bias their participation in the land market, too (Deere & León, 2003). Women have limited opportunities to buy land and are at times openly discriminated against by local authorities (Peterman, 2011; Daley & Englert, 2010). In Mexico, those few women who receive land titles through the reform often have their titles contested by opposing male rights, or ignored by the community (Brunt, 1992; Hamilton, 2002). In case of inheritance disputes, the ejido assembly tends to favor the land rights of sons over those of widows (Brunt, 1992).

8 Conclusions and policy implications

The majority of the existing literature on the gendered aspects of Mexico's 1992 land titling reform takes the form of qualitative research and small-scale case studies. The contribution of this paper is to provide empirical evidence on the hypothesis that the gender blindness of the 1992 agrarian reform aggravated pre-existing gender relations.

After compiling a novel dataset, I analyzed the effects of the land reform on three outcomes of female bargaining power: the child-woman ratio, educational attainments by gender, and the incidence of femicides. I exploited spatial and temporal variation in the reform rollout to identify causal effects using an event study design and a difference-in-differences design. Furthermore, I employed a series of expedients to increase the credibility of the identification strategy. In the fertility and educational attainment analyses, I used late-treated localities as counterfactual. In the femicides analysis, I restricted the sample to urban localities in order to augment comparability and decrease noise in the estimates. In all the analyses, I repeated the estimations using (early-) treated localities only – in order to exploit the sole variation in treatment timing – and performed a number of ad hoc robustness checks.

The results show that, following the adoption of dominio pleno, fertility tends to decrease in socioeconomically disadvantaged localities, such as those with a larger average household size; on the other hand, it modestly increases in localities which have smaller households. This finding suggests that the higher tenure security provided by land titling decreases the number of children desired by couples. However, I am unable to discern whether the reform is affecting female bargaining power, since any potential repercussions of the empowerment mechanism on fertility are being more than offset by the tenure security mechanism.

Further, the adoption of dominio pleno is linked to a decrease in both female and male average educational attainment. The proposed mechanism is that female bargaining power shrinks in treated localities, and women's preference for higher investments in children's education is therefore not fulfilled. However, another mechanism might be causing the observed decrease in schooling. Namely, if land titling facilitated migration choices, individuals with more schooling might select into emigration. Since I cannot fully rule out this possibility, the finding on educational attainments is not conclusive of a decrease in female bargaining power.

Lastly, the adoption of dominio pleno gives rise to a substantial increase in the incidence of femicides in affected urban localities. I have estimated an average yearly increase of 6 to 8 femicides per 100,000 women, although this effect might shrink over time. This result only holds when defining femicides as the number of total female homicides, but it proves to be otherwise robust to a variety of tests, despite the small sample size used. Also, the effect on femicides does not appear to be driven by differences in local occurrence of organized violence. Therefore, this increase in femicides suggests that women have lost bargaining power as a result of the reform, which is consistent with the bargaining framework adopted to explain gender relations. At the household level, the relative decrease in women's resources reduces their abilities to escape violent or abusive relationships. At the community level, women's diminished economic role reinforces the spatial division of genders, making it less socially legitimate – and thus, safe – to engage in activities outside the domestic sphere.

Important policy implications can be derived from this finding for the context of Mexico, where the issue of violence against women has increasingly entered the government agenda. The ownership of land is unquestionably central to the livelihood of women, yet it is also a social asset that is crucial for female empowerment and protection against violence. The combination of deep-rooted gender disparities and a history of discriminatory agrarian reforms has resulted in a persistent gender gap in land rights. My finding highlights the need to design land policies with a gender perspective in order to help tackle the root causes of gender inequality. As the process of land titling is still ongoing, there is scope for adjusting current and future land policies to correct for these gender disparities. In Section 3.2 I have briefly mentioned the role of mandatory joint titling to couples as a crucial measure to promote gender equality in land rights. Joint titling has been found to avoid the gendered repercussions of land titling and even have positive effects on female empowerment. A potential policy recommendation is therefore to introduce joint titling as a mandatory provision for acquisition of dominio pleno.

Land policy cannot be gender neutral when society itself is not. Furthermore, failing to consider gender disparities often hinders the potential of policies to meet their primary targets. In this case, the expected improvements to the productivity of the agricultural sector have not materialized. Therefore, incorporating a gender perspective in land policy may additionally help to positively reshape the future of the ejido sector.

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A Appendix

A.1 On the spatial matching procedure

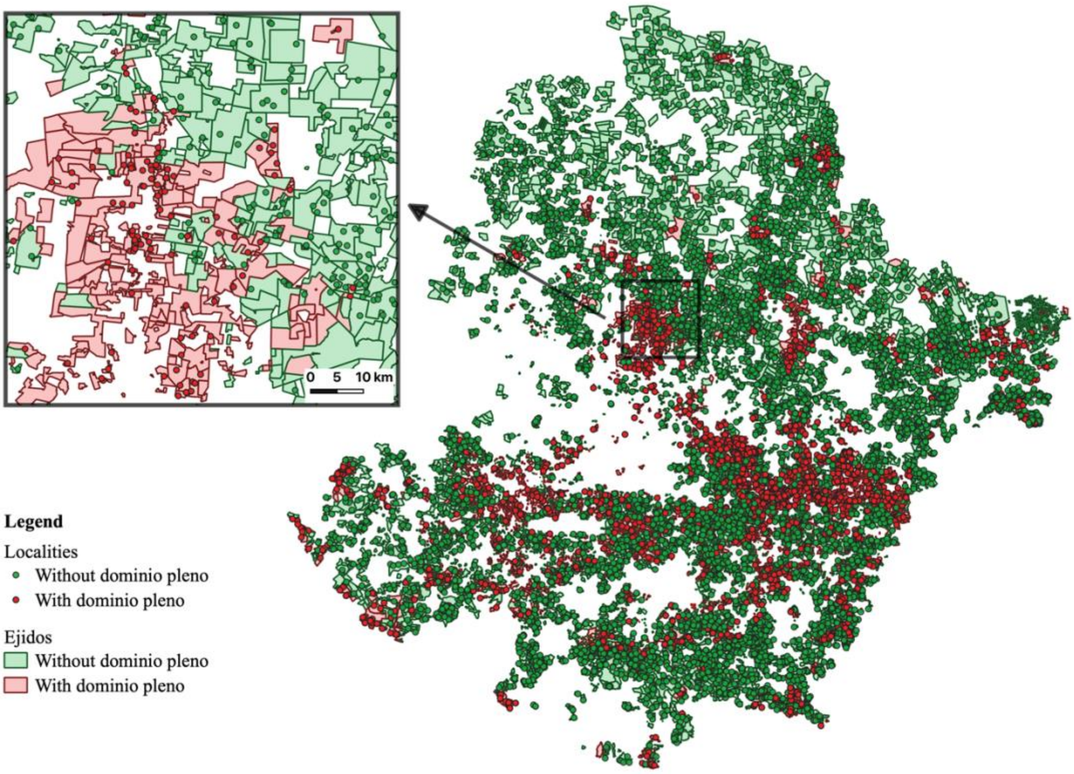

Figure A1 – Spatial matching

I use the Geographic Information System (GIS) application QGIS to join the six differently georeferenced datasets to be used for my analysis. The most critical step of this spatial matching procedure concerns joining my ejido level dataset with the two locality level datasets (the population census and the annual death register). While the precise geographical extension is known for the ejidos, only the coordinates of the centroid are observed for the localities. Therefore, a locality was matched to an ejido if its centroid was either situated on the ejido boundaries, or entirely contained within them. My approach follows that adopted by two previous studies in which the authors similarly combined ejido level data with locality level data (de Janvry et al., 2015; Ramirez-Alvarez, 2019). In Figure A1, I show the localities in my sample (the entire Bajío region) after having been matched to the ejido dataset; both ejidos and localities are colored according to their current privatization status. The inset to the left shows a magnified portion of the map to better highlight the different geographical definition of localities (represented by the point coordinates of their centroids) and ejidos (represented as the polygon identified by their boundaries). A potential concern is that a locality might extend variably further than its centroid. However, as I show in the Figure A2, the vast majority of localities have a very modest population size and are thus deemed unlikely to occupy an area much larger than the ejido they were matched with.

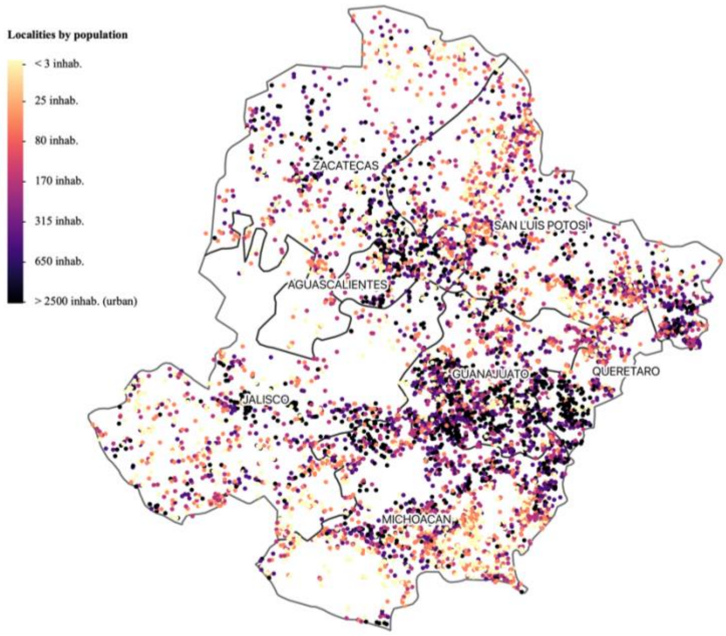

Figure A2 – Ejido-matched localities by population

Furthermore, my spatial matching procedure technically allows for more than one locality to be matched to the same ejido. In practice, however, most ejidos in my dataset did not match with more than one locality. For example, in the data used for the educational attainment analysis, there are 7,356 unique localities assigned to 4,068 distinct ejidos. This seems to indicate that ejidos in the Bajío tend to have a modest geographical extension.

On another note, the dataset on educational attainments from the IHME comes in raster format, meaning that it displays information as continuous, with each pixel representing a 5x5 km area. In this case, localities were directly matched to the pixel in which their centroid was contained. That is, the centroids were used as sampling points in which the raster values were extracted. Figure A3 offers a glimpse of the raster data type, displaying educational attainments for females aged 15-49 in the Bajío as of 2001.

Lastly, the locality level data thus created was matched with the UCDP dataset, which contains all organized violence events by year together with the coordinates of the location where they took place. First, I selected the organized events that took place, between 2002 and 2010, in any of the seven states of the Bajío. I then assumed that organized violence events have repercussions that propagate further than the circumscribed location in which they occurred. Hence, I created buffers of 20 km radius around the location of each event; the choice of this exact radius was essentially arbitrary. All localities whose centroid was contained within this radius were assumed to be affected in some way by the violent event. I then created an indicator variable that turned on if any events had occurred in a given locality for a given year. This variable was included only in the femicides analysis, as a covariate.

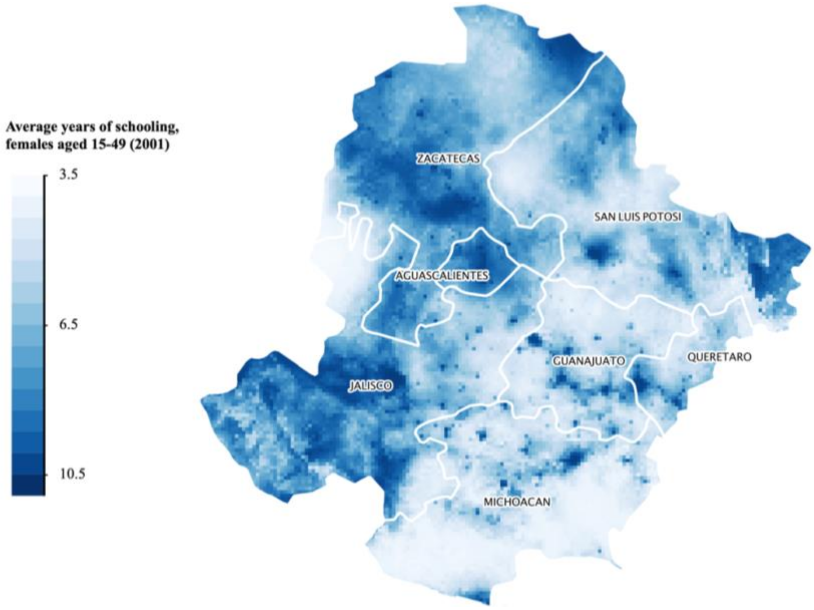

Figure A3 – Raster data on schooling. Source: IHME (2019), author's rendering

Plausibility of the stable unit treatment value assumption in the femicides analysis:

This map shows all urban localities in the femicides dataset (treated, late-treated, and never-treated), with the ejidos to which they are matched. It can be observed that all ejidos in this dataset are far apart from each other and only rarely there are adjacent ejidos with different treatment status.

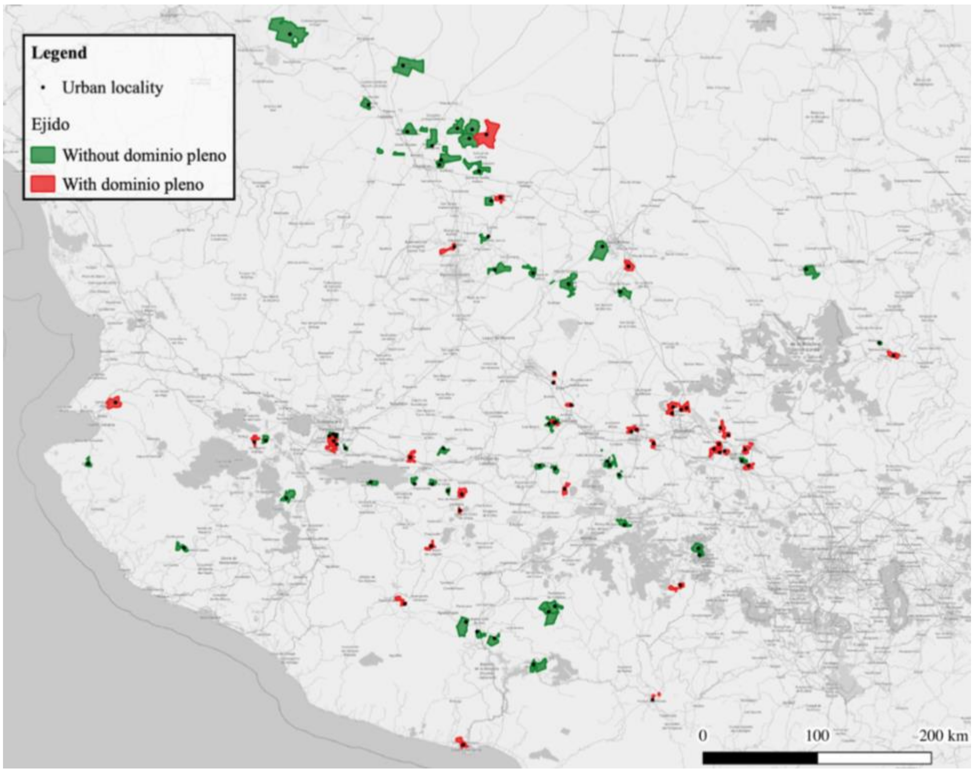

Figure A4 – Ejido-matched urban localities

A.2 Additional output

Table A1 – Dependent variable: Fertility (child-woman ratio). Event study with heterogeneous effects

	(1)
Time since event:	
$k = -10$	-60.94
	(66.64)
$k = -5$	0
$k = 0$	191.5***
	(57.19)
$k = 5$	437.2***
	(117.1)
Time since event (interactions):	
$k = -10$ × Avg. household size	12.89
	(13.00)
$k = -5$ × Avg. household size	
$k = 0$ × Avg. household size	-45.73***
	(12.45)
$k = 5$ × Avg. household size	-100.4***
	(25.44)
Avg. household size	27.33*
	(10.69)
Locality FE	Yes
Year FE	Yes
Covariates	Yes
Control group	Treated after 2010
F-stat	14.19
P-value	5.14e-09
R^2	0.108
Mean dep. var.	480.2
Localities	1642
Observations	4925

Notes: Standard errors clustered by ejido, in parentheses. This is a variant of Table 8 in which I interact the event study coefficients with average household size. Results are roughly equivalent to the estimation of heterogeneous effects with difference-in-differences, shown in column 3 (Table 8).
The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).
+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A2 – Dependent variable Fertility (child-woman ratio). Heterogeneous effects by average schooling years

	(1)
Treated × After	-179.0***
	(51.59)
Treated × After × Avg. schooling	29.55***
	(8.564)
Avg. schooling	-33.05**
	(10.90)
Locality FE	Yes
Year FE	Yes
Covariates	Yes
Control group	Treated after 2010
F-stat	15.72
P-value	0.000000195
R^2	0.101
Mean dep. var.	480.2
Localities	1642
Observations	4925

Notes: Standard errors clustered by ejido, in parentheses. In this table I investigate the presence of heterogeneous effects by levels of schooling (in the adult population aged 15+). From the empirical literature on the determinants of fertility, I expect privatization to impact fertility more strongly in localities with fewer average years of schooling. However, since the schooling variable is endogenous, results must be viewed with caution. The estimates confirm those of Table 8 (column 3) in the sense that the effects depend on socioeconomic conditions; localities with average and lower-than-average schooling experience a decline in fertility after the reform, while fertility moderately increases in better-off localities with more schooling.

The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3 – Dependent variable: Fertility (child-woman ratio). Robustness: Selecting by treatment year

	Without localities treated 2006-2010		Without localities treated 2001-2005	
	(2a)	(3a)	(2b)	(3b)
Treated × After	-1.539	190.4*	-12.25	317.0***
	(17.75)	(83.29)	(14.36)	(74.07)
Treated × After × Avg. household size		-42.57*		-76.96***
		(17.27)		(16.99)
Avg. household size		20.53+		26.52*
		(11.24)		(11.42)
Locality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control group	Treated after 2010	Treated after 2010	Treated after 2010	Treated after 2010
F-stat	10.95	10.66	13.31	14.15
P-value	0.000332	0.000145	0.0000223	1.39e-09
R^2	0.0920	0.0969	0.107	0.120
Mean dep. var.	478.9	478.9	481.3	481.3
Localities	1011	1011	1174	1174
Observations	3032	3032	3521	3521

Notes: Standard errors clustered by ejido, in parentheses. This table repeats the estimations of columns 2-3 of Table 8, first dropping localities treated in 2006-2010 (columns 2a-3a), then dropping localities treated in 2001-2005 (columns 2b-3b). Results are equivalent to the original.

The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4 – Dependent variable: Mean years of educational attainment.
Robustness: Controlling for CWR

	Females (aged 15-49)	Males (aged 15-49)
	(1)	(2)
Time since event:		
$k \leq -4$	-0.00501	-0.00878
	(0.00980)	(0.00783)
$k = -3$	-0.00357	-0.00164
	(0.00471)	(0.00368)
$k = -2$	-0.00355	-0.00474^{+}
	(0.00258)	(0.00243)
$k = -1$	0	0
$k = 0$	-0.00795*	-0.00690**
	(0.00342)	(0.00232)
$k = 1$	-0.0122^{+}	-0.0116*
	(0.00658)	(0.00474)
$k = 2$	-0.0198*	-0.0164**
	(0.00776)	(0.00622)
$k = 3$	-0.0245**	-0.0212**
	(0.00878)	(0.00706)
$k \geq 4$	-0.0292**	-0.0224*
	(0.0112)	(0.00907)
Fertility (CWR)	-0.0000112	-0.00000577
	(0.00000805)	(0.00000774)
Locality FE	Yes	Yes
Year FE	Yes	Yes
Covariates	Yes	Yes
Control group	Treated after 2010	Treated after 2010
F-stat	3494.5	2741.9
P-value	0.0000370	8.59e-08
R^2	0.965	0.960
Mean dep. var.	7.124	7.248
Localities	1642	1642
Observations	18057	18057

Notes: Standard errors clustered by ejido, in parentheses. This is a variant of Table 10 in which I additionally control for fertility (child-woman ratio). Results are roughly equivalent to the event study estimates shown in columns 1 and 3 (Table 10). The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5 – Dependent variable: Mean years of educational attainment. Robustness: Immigration

	(Population decline and immigration)		Michoacán, San Luis Potosí, Zacatecas	
	Females (age 15-49)	Males (age 15-49)	Females (age 15-49)	Males (age 15-49)
	(1a)	(2a)	(1b)	(2b)
Time since event:				
$k \leq -4$	-0.00659	-0.00570	-0.00652	-0.00732
	(0.00746)	(0.00696)	(0.00518)	(0.00478)
$k = -3$	-0.00274	0.000909	-0.00385	-0.00191
	(0.00488)	(0.00432)	(0.00344)	(0.00306)
$k = -2$	-0.00472	-0.00382	-0.00377	-0.00437^{+}
	(0.00328)	(0.00311)	(0.00254)	(0.00248)
$k = -1$	0	0	0	0
$k = 0$	-0.00414	-0.00305	-0.000468	-0.00140
	(0.00282)	(0.00214)	(0.00242)	(0.00202)
$k = 1$	-0.0114*	-0.00810*	-0.000975	-0.00196
	(0.00446)	(0.00407)	(0.00345)	(0.00304)
$k = 2$	-0.0181**	-0.0140*	-0.00515	-0.00425
	(0.00601)	(0.00594)	(0.00483)	(0.00530)
$k = 3$	-0.0170*	-0.0160*	-0.00512	-0.00750
	(0.00715)	(0.00697)	(0.00578)	(0.00606)
$k \geq 4$	-0.0195*	-0.0150^{+}	-0.00315	-0.00289
	(0.00911)	(0.00872)	(0.00706)	(0.00752)
Locality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control group	Treated after 2010	Treated after 2010	Treated after 2010	Treated after 2010
F-stat	2758.3	1811.5	4718.8	3247.0
P-value	0.0000116	0.00000247	0.266	0.00509
R^2	0.970	0.960	0.978	0.969
Mean dep. var.	7.131	7.292	7.196	7.366
Localities	1011	1011	1032	1032
Observations	8489	8489	11352	11352

Notes: Standard errors clustered by ejido, in parentheses. This table shows a robustness check in which I attempt to remove differences in migration patterns. Namely, in the first two columns (1a-2a) I re-estimate the event study specifications on localities that did not experience a population decline between 2000-2010, and that did not experience more immigration than the Bajío average. In the last two columns (1b-2b) I re-estimate the event study specifications excluding three states that are known to have had net outmigration in 2000-2010. Results are compared with columns 1 and 3 of Table 10. While columns 1a-2a are roughly equivalent to the original, columns 1b-2b are not.

The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants).

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A6 – Dependent variable: Femicides (all homicides) per 100,000 women.
Event study with standard bins

	(1)	(2)
Time since event:		
$k \leq -4$	-3.796	-1.804
	(2.976)	(2.298)
$k = -3$	0.0250	0.542
	(4.691)	(4.760)
$k = -2$	-2.849	-3.108
	(2.191)	(2.307)
$k = -1$	0	0
$k = 0$	5.004	4.029
	(3.732)	(4.114)
$k = 1$	8.324^{*}	7.341^{+}
	(3.873)	(3.670)
$k = 2$	-0.0579	-0.645
	(1.101)	(1.285)
$k = 3$	4.696	4.269
	(3.183)	(3.191)
$k \geq 4$	7.647^{+}	6.214
	(4.187)	(4.095)
Locality FE	Yes	Yes
Year FE	Yes	Yes
Covariates	Yes	Yes
Control group	Never treated	Treated after 2010
F-stat	1.362	7.740
P-value	0.0700	0.0699
R^2	0.0571	0.0920
Mean dep. var.	3.227	3.598
Localities	78	38
Observations	702	342

Notes: Standard errors clustered by ejido, in parentheses. This is a variant of Table 12 in which a larger number of event study dummies is used. Results are mostly equivalent to the original, although the coefficient on the upper endpoint loses some of its significance. Sample is restricted to urban localities only (average population > 2,500). The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants), and whether any organized violence events occurred within a 20 km radius.

+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A7 – Dependent variable: Femicides (all homicides) per 100,000 women. Robustness: Organized violence

	(1)	(2)
Treated × After	6.584*	5.000*
	(2.627)	(2.278)
Organized violence	-4.800+	-8.719**
	(2.444)	(2.866)
Treated × After × Organized violence	-5.488	-1.600
	(9.209)	(9.169)
Locality FE	Yes	Yes
Year FE	Yes	Yes
Covariates	Yes	Yes
Control group	Never treated	Treated after 2010
F-stat	1.472	2.104
P-value	0.0999	0.0279
R^2	0.0487	0.0740
Mean dep. var.	3.227	3.598
Localities	78	38
Observations	702	342

Notes: Standard errors clustered by ejido, in parentheses. This table repeats the estimations of columns 3-4 of Table 12, allowing treatment to have heterogenous effects by levels of organized violence. Results show no evidence of heterogeneous treatment effects by levels of organized violence, and confirm the significance and magnitude of the main estimates. Sample is restricted to urban localities only (average population > 2,500). The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants), and whether any organized violence events occurred within a 20 km radius.

+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A8 – Dependent variable: Femicides (all homicides) per 100,000 women.
Sensitivity: Radius of 4 km

	Event study		Difference-in-differences	
	(1)	(2)	(3)	(4)
Time since event:				
$k \leq -3$	0.0391	1.968		
	(2.583)	(2.067)		
$k = -2$	-0.510	-0.527		
	(0.678)	(0.595)		
$k = -1$	0	0		
$k = 0$	5.196^{+}	4.295		
	(2.919)	(3.036)		
$k = 1$	4.433	3.270		
	(2.956)	(2.837)		
$k = 2$	-0.692	-1.580		
	(0.872)	(1.007)		
$k \geq 3$	3.862^{+}	1.915		
	(2.214)	(2.083)		
Treated × After			3.830^{+}	2.717
			(2.211)	(1.729)
Locality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control group	Never treated	Treated after 2010	Never treated	Treated after 2010
F-stat	1.234	1.459	1.225	1.563
P-value	0.429	0.610	0.613	0.358
R^2	0.0615	0.0795	0.0556	0.0604
Mean dep. var.	1.571	1.782	1.571	1.782
Localities	78	38	78	38
Observations	702	342	702	342

Notes: Standard errors clustered by ejido, in parentheses. In this table I perform a sensitivity analysis on the radius used to attribute femicides to given localities. In the main specification (Table 12), femicides that occur within a 5 km radius from the locality centroid are attributed to the locality. Here I use instead a 4 km radius, which makes localities less likely to be assigned femicides (as can be seen from the drop in the mean of the dependent variable). However, the results still show a weak significance at 10% with the control group of never-treated.

Sample is restricted to urban localities only (average population > 2,500). The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants), and whether any organized violence events occurred within a 20 km radius.

+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A9 – Dependent variable: Femicides (all homicides) per 100,000 women.
Sensitivity: Radius of 6 km

	Event study		Difference-in-differences	
	(1)	(2)	(3)	(4)
Time since event:				
$k \leq -3$	-3.471	-3.991		
	(4.134)	(5.159)		
$k = -2$	-3.769	-4.872		
	(4.355)	(4.657)		
$k = -1$	0	0		
$k = 0$	-0.405	-0.0364		
	(5.653)	(6.249)		
$k = 1$	14.85^{*}	16.12^{*}		
	(6.584)	(7.187)		
$k = 2$	11.37	13.04		
	(8.305)	(7.783)		
$k \geq 3$	9.165^{**}	10.91^{*}		
	(3.459)	(5.114)		
Treated × After			9.576^{**}	9.799^{*}
			(3.539)	(4.315)
Locality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control group	Never treated	Treated after 2010	Never treated	Treated after 2010
F-stat	0.927	47.40	1.003	13.01
P-value	0.363	7.37e-10	0.247	0.0000252
R^2	0.0610	0.131	0.0479	0.113
Mean dep. var.	6.746	10.62	6.746	10.62
Localities	78	38	78	38
Observations	702	342	702	342

Notes: Standard errors clustered by ejido, in parentheses. In this table I perform a sensitivity analysis on the radius used to attribute femicides to given localities. In the main specification (Table 12), femicides that occur within a 5 km radius from the locality centroid are attributed to the locality. Here I use instead a 6 km radius, which makes localities more likely to be assigned femicides (as can be seen from the rise in the mean of the dependent variable). The results are strongly robust to this change in radius, with magnitude and significance increasing with respect to the original estimates.

Sample is restricted to urban localities only (average population > 2,500). The covariates include local income proxies (share of households owning a television, share of dwellings that are privately owned, share of dwellings with hard flooring), local access to public goods and services (share of households with access to electricity, share of population with access to healthcare), local socioeconomic characteristics (literacy rate, share of indigenous languages speakers), demographic controls (average household size, share of female-headed households, population size, share of population that are recent immigrants), and whether any organized violence events occurred within a 20 km radius.

+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

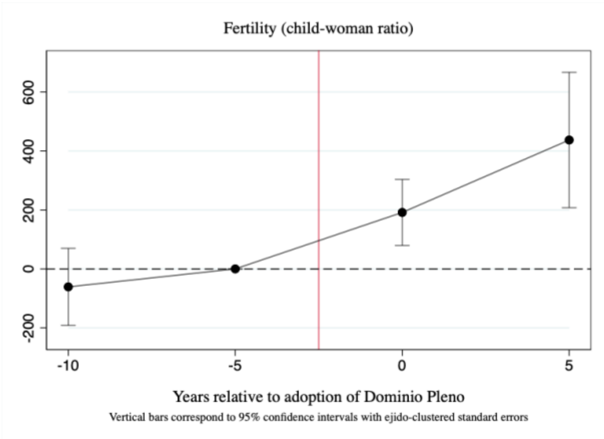

Figure A5 – Event study coefficients and confidence intervals from column 1 of Table A1

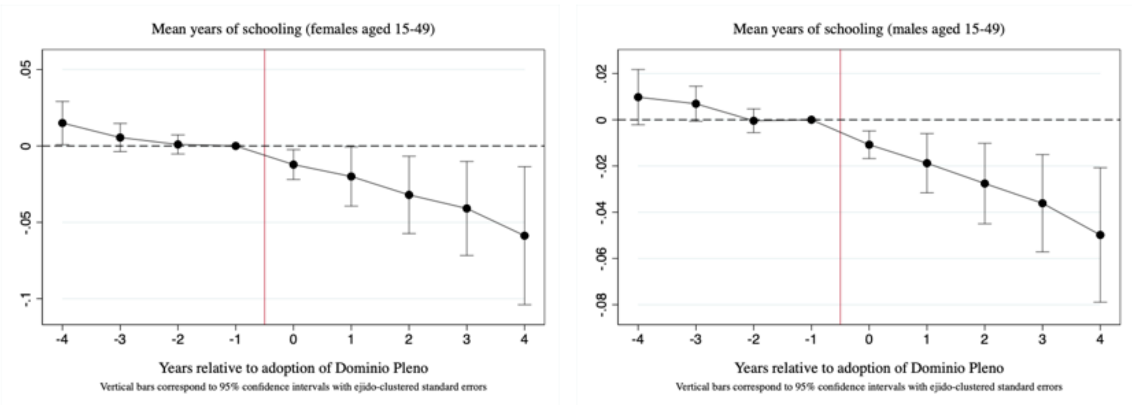

Figure A6 – Event study coefficients and confidence intervals from columns 1 and 3 of Table 11