

A violent downside of health care for all?

On the effect of universal health insurance on home violence in Mexico and
possible perverse impacts of well-intended policy

by

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Preface

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Abstract

The launch of the health insurance program *Seguro Popular* (SP) enabled all Mexican residents lacking employer-based coverage to enroll, for the very most part without any co-payments necessary. This paper aims to evaluate whether this well-intended policy may have perverse impacts affecting household dynamics. More specifically, I will assess whether SP had any effects on the occurrence of home violence, opposing predictions from a bargaining power approach with those of the theory of male backlash. The gradual, quasi-exogenous rollout of SP on a municipality level makes implementing a difference-in-differences (DD) estimation strategy feasible, controlling for municipality-specific and time fixed effects (FE). This is done in practice by combining data containing information about families affiliated with SP with hospital discharge data including details on victims of home violence that sought medical attention at hospitals available from 2000-2012. Main regression results indicate that SP lead to small, but significant increases in home violence in line with a male backlash approach. Short-term effects for up to two years after SP introduction imply 0.05 additional incidents of home violence per municipality on average whereas estimated long-term effects (more than three years after introduction) are double in size. The results are robust to alternative specifications which suggests the absence of confounding factors. Interestingly, impacts are driven by the rich municipalities in Mexico. To understand underlying mechanisms, a complementary analysis on marriage and divorce rates was conducted. Results reflect a heightened degree of female empowerment and independence which supports the increasing impact of SP on domestic violence within a male backlash framework. To make sure that the detected effect on the occurrence of violence is not simply due to increased usage of medical services typically occurring when health care is provided freely, survey data from before, whilst and after SP rollout is analyzed. Corresponding results point to increases in home violence in rich municipalities, which is reconfirming the validity of the main results.

For analysis, the statistical software package STATA/SE 16.0 was used.

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Chapter 1: Introduction

“One of the great mistakes is to judge policies and programs by their intentions rather than their results.”

Milton Friedman

The direct and indirect impacts of violence against women account for substantial long run economic and social costs. When violence against women occurs, it is in most cases an intimate partner that is perpetrating the crime (United Nations 2015). As domestic violence is a global issue, prevailing in both rich and poor regions, Mexico is one of the countries where women are victimized especially frequently (United Nations 2010). At the turn of the millennium, it was estimated that around 2.5 per every 100.000 women fell victim to femicide in Mexico (SEGOB, INMUJERES, and ONU Mujeres 2017). Compared to European countries this rate is high and it has been drastically increasing since, reaching more than double the 2000 value by 2017 (The World Bank 2020). In the early spring of 2020, tens of thousands of women in Mexico failed to appear to their place of work in protest against these untenable circumstances. Whilst the death toll of violence against females is rising, exceeding 10 victims daily, women demand action by the government (Averbuch 2020).

In Norway, the government addresses this matter by regularly issuing a plan of action against home violence¹ with the latest one being in force between 2014 and 2017. As such plan was lacking during the last years, a new one is in the works as of 2019 and expected to be published in 2020 (Justis- og beredskapsdepartementet 2020). The latest version of the action plan contained 45 measures (Justis- og beredskapsdepartementet 2013). These measures fall into one of the following five categories: 1) Prevention and recognition, 2) Knowledge, competence and research, 3) Assistance and treatment for victims of home violence, 4) Criminal prosecution of perpetrators, 5) Coordination and cooperation between institutions.

Besides its immediate negative impact, home violence can affect victims adversely in the long run as well. Using Dutch data, Bindler and Ketel (2019) address long run effects of violent victimization on labor market outcomes and find significant and persisting decreases in

¹ The terms *domestic violence*, *home violence* and *intimate partner violence* will be used interchangeably in this paper.

earnings and longer durations of receiving benefits. Estimated effects of home violence are even more distinct than those of other types of violent criminal victimization such as assault or sex offences. Moreover, the consequences of violence are not only devastating for the direct victims and families, but it can hinder economic development and progress for whole countries. Even if impacts may be most severe for less developed countries, high income countries pay the exorbitant price of home violence, too. Estimates by the Norwegian Ministry of Justice and Public Security (*Justis- og beredskapsdepartementet*) for 2012 indicate costs due to domestic violence in Norway between 4,5 and 6 billion NOK (Rasmussen et al. 2012). Hence, it is of utmost importance to investigate and research impacts of public policy on the incidence of intimate partner violence and how interventions may help alleviate the victim's burden.

Due to both the difficulty in terms of reliably identifying and measuring incidents of domestic violence as well as the sensitive nature of the issue, researchers face serious challenges in accessing suitable data. As this area of research has gained attention lately, economic research on domestic violence is thus still underrepresented when compared to research on other crime-related topics. Therefore, my thesis is dedicated to adding to the literature by empirically examining the possible impact of the introduction of the universal health care program *Seguro Popular* ("People's Insurance", referred to as SP hereafter) in Mexico on rates of domestic violence.

When universal health care becomes available, it can be suspected that power structures within households are impacted. In Mexico, the male tends to be the breadwinner of the family. Numbers from the OECD (2017) indicate that almost half of the households with both parents and at least one child present consist of an unemployed housewife and a full-time working man. This implies that many women were depending on their possibly violent male partner for health insurance prior to the introduction of SP. With SP in place, this constraint disappears as health insurance becomes available to everyone. The incentive to stay with a violent partner would then decrease, which in turn indicates a relatively more empowered female.

According to this reasoning, I aim to answer the following research questions which form the basis of the main analysis: Does the introduction of SP measurably impact relationship-related female decision-making, which could be reflected by changes in marriage rates, divorce rates

or the composition of the couple? In the case of such changes being detected, do they imply increased empowerment on the woman's part? Building on these issues, the primary research question of this paper will be whether relative increases in female empowerment caused by SP have the potential to induce perverse effects in the shape of increased domestic violence or if women become better off overall.

SP was gradually introduced on a state level, starting as a pilot project in five states in 2002 and reaching all municipalities by 2010. Besides a basic health package which was provided to the poorest within the means of the *Oportunidades* poverty alleviation program, health insurance had previously been employer-based and therefore only available to families with at least one formally working household member. Thus, SP offered coverage for all those citizens who were uninsured until then. Enrollment remained voluntary, and in practice free of charge for most households (Bonilla-Chacín and Aguilera 2013). By 2012, SP covered as much as 72% of the poor population in Mexico (World Bank 2015). Ten years after the last states had officially introduced SP in 2005, close to 50% of the total Mexican population was affiliated with SP (INEGI - Instituto Nacional de Estadística Geográfica e Informática 2020).

When crisis hits, the value of health care available to everyone becomes evident. During the ongoing COVID-19 pandemic, many people lose their jobs on a global scale and there is thus a high demand for publicly provided health insurance. A large concern in times like these is that the circumstances of victims exposed to violence at home deteriorate. This could be the case because the time exposed to the perpetrator most likely increases due to measures of social isolation and because of increased levels of stress and frustration among household members. Indeed, the number of distress calls to domestic violence hotlines in Mexico appears to have doubled since quarantine measures were introduced (Mellmann 2020). However, this issue is a global one, just as home violence itself, and deserves the attention of high-income countries as well. Recent data from a British domestic abuse charity finds that killings tied to domestic abuse more than doubled during the COVID-19 lockdown so far (Grierson 2020).

In Norway there also exists the concern of increases in home violence due to the COVID-19 related lockdown of the country and the associated preventive measures stipulated (Elster 2020). The number of calls to crisis hotlines actually decreased in Norway since the outbreak, which can possibly be due to the constant presence of the perpetrator at home (Myrvang

2020). Crisis centers do however expect drastic increases in reporting when restrictions are gradually relaxed, as seen in China. In the Chinese city of Jingzhou, the number of domestic violence incidents not only doubled, but it is estimated that the motive behind the abuse is in 90% of the cases related to COVID-19 (Wanqing 2020).

Returning to the case of Mexico, there exist theories that predict contradicting impacts on domestic violence levels when the female outside option is improved by policy such as SP. A bargaining power model which assumes a cooperative household solution made by utility maximizing household members forecasts a decline in violence, whereas a male backlash approach suggests an increase. The latter would hence indicate perverse effects of well-intended policy. To address this, I use publicly available administrative data from Mexican hospitals between 2000 and 2012 containing narrowly defined information on each patient admitted. These details make it possible to identify victims of domestic violence with a reasonable degree of certainty. Combining this data with information on the timing of SP rollout in each municipality allows me to implement a difference-in-differences (DD) estimating strategy in an attempt to isolate causal impacts on domestic violence. The validity of this strategy relies on assuming parallel domestic violence trends across municipalities introducing the program at different moments in time in the absence of treatment. The DD strategy is implementable due to the fact that the unregulated SP rollout generated quasi-random variation on a municipality level, on which the main analysis will be conducted.

Estimated results imply a small but significant increase in intimate partner violence due to SP. Whilst this might solely be because of an increased usage of health services, as those become free of charge with the universal health insurance in place, survey data was examined to assess the validity of the results. Respective findings are encouraging and support the previous conclusions. In addition, I conduct a complementary analysis focusing on marriage and divorce rates. I do this in order to understand whether women behave in a more empowered manner, becoming increasingly independent from a male counterpart. Overall, results imply reductions in marriage rates due to SP as well as increases in divorce rates. Minor changes in the composition of the couples point to the same conclusion. This gives support to the underlying assumption about females becoming relatively more empowered which the main analysis builds on.

The following thesis consists of seven main parts. I will start with giving an overview of the

background and introduction of SP as well as important connected topics of interest in chapter 2. Subsequently, existing literature and theories addressing the issue at hand will be elaborated upon in chapter 3 and 4 before the data and estimation strategy will be presented in detail in the next two chapters. Estimation results are presented and discussed at full length in chapter 7. Chapter 8 will be dedicated to assessing the robustness of the main findings. The paper will conclude with discussing the implications of the findings overall and for further research in chapter 9. Concluding comments can be found in the tenth and final chapter of this thesis.

Chapter 2: Background

Considering the potential link between the health insurance program SP and domestic violence, there are several topics that need to be put in perspective in order to understand the supposed underlying mechanisms and related theories. As the “treatment” in this context is tied to the introduction of health care coverage for uninsured citizens, I will begin with elaborating upon the health care system in place prior to SP as well as the specific characteristics of SP itself. This is of distinct importance as it forms the basis for recognizing how a possible change in the power structure within households could arise. In chapter 3, which addresses relevant theory, this specific issue will be discussed in more detail. In addition, the preexisting situation in Mexico concerning cultural norms, criminal activity and especially domestic violence and the female standing in society is examined. I will conclude this chapter with giving a more detailed and concrete definition of female empowerment. This helps illustrating in what way the introduction of SP could alter the relative level of female empowerment which in turn strengthens the validity of the question of how changes in female empowerment impact the incidence of domestic violence.

2.1 Health care in Mexico

Universal health insurance is called upon as a main pillar for development and progress by economists (Summers 2015). Following the example of other Latin American countries such as Brazil, Chile and Colombia, which implemented health insurance reforms already during the 1980’s and 1990’s, the Mexican government made a step towards health insurance for all during the early 2000’s. In 2003, the Mexican congress passed legislation which ensured the

public health sector increases in funding by 1% of the GDP (Frenk, Gómez-Dantés, and Knaul 2009). The System of Social Protection in Health (SSPH) was simultaneously established with SP being its main feature. Hitherto, SP had only been run in selected states as a pilot program. It offered health insurance to all those without any prior form of coverage.

Before the introduction of SP only about half of the Mexican population enjoyed the benefits of health insurance (Frenk et al. 2006). Health care coverage was generally employer-provided, not unlike employer-sponsored programs in the United States, and covered employees as well as dependents. For workers in the private sector the Mexican Social Security Institute IMSS (*Instituto Mexicano del Seguro Social*) provides coverage financed by contributions of the employer, the employee and the state up until this day. The equivalent institution for public sector employees is the Institute for Social Security and Services for State Workers ISSSTE (*Instituto de Seguridad y Servicios de los Trabajadores del Estado*). Lastly, employees working in the oil and gas industry receive health insurance by PEMEX (*Petroleos Mexicanos*). Foreseeably, IMSS covered the largest share out of the three institutions before SP was introduced (Frenk et al. 2006). Health services for the insured are provided in public hospitals (Conti and Ginja 2017). However, private insurance and services in private facilities can be purchased in addition.

Before the establishment of SP, around 50 million Mexican citizens were not covered by any of the above mentioned institutions (Frenk et al. 2006). Hence, according to the system of national health accounts during the mid-1990's, over 50% of the total national health expenses were out-of-pocket expenses (Frenk, Gómez-Dantés, and Knaul 2009). Those remaining citizens, mostly poor and living in rural communities, would have the opportunity to receive medical attention by teams of medical staff who visited especially affected areas within the means of a program named PAC (*Programa de Ampliacion de Cobertura*) (Conti and Ginja 2017). In addition, the poverty alleviation program *Oportunidades* (known under the name *Progresa* upon its establishment in 1997 and referred to as OP hereafter) provided direct cash transfers i.a. conditioned on attending health checkups for selected participants. OP is found to have had positive impacts on health outcomes among the poor. It has i.a. been shown that the birthweight of newborns among female recipients was significantly higher than for women in the control group (Barber and Gertler 2008).

SP first came into play in the year of 2002. What started as a pilot scheme in only a handful of Mexican states eventually covered almost everyone previously uninsured in 2014 with enrollment remaining voluntary (Huffman and van Gameren 2018). The rollout of SP on a state-level between 2002 and 2005 was controlled by the federal government and targeted states with certain characteristics in regard to the timing of introduction. These characteristics are related to both capacity and potential for implementation as well as the demand for coverage. Early introducers were typically states with relatively rich municipalities that could provide the needed infrastructure. The funding of SP comes in large part from the federal government, which allocates means to the states in accordance with the number of affiliates, as well as from the states themselves (Huffman and van Gameren 2018). Therefore, states have the incentive to enroll their entire eligible population to expand their budget (Frenk et al. 2006). As SP generally covers the less wealthy, families are in almost all cases excepted from co-payments (Bonilla-Chacín and Aguilera 2013). Hence, this source of funding is negligible in practice. When households, being the unit of enrolment, affiliate with SP they are allotted a health unit and GP and receive access to an extensive health package.

2.2 Culture, crime and gender roles

Mexico is a country still coined by tradition, cultural norms and religious customs. 84% of the Mexican population is affiliated with the catholic church (Willey et al. 2019). Couples marry earlier on in their lives than couples in other countries and divorce rates are astoundingly low (OECD 2019). Comparing the number of divorces relative to number of marriages per year across countries using statistics by the UN (2019), Mexico had a ratio of 0,15 (as of 2009) whereas Norway had a ratio of 0,44 (as of 2011). Pick, Contreras and Barker-Aguilar (2006) acknowledge that social practices and gender roles in Mexico place women in a position of subordination. Hierarchical structures within households are the consequence, with men exercising control and power over women. This contributes to a widespread machismo culture, where the male feels entitled to rule over his partner, predominant in many Latin-American countries (Pick, Contreras, and Barker-Aguilar 2006). Machismo culture may have been one of the reasons why it was not until 1997 that Mexican Congress passed the bill that declared domestic mental and physical abuse a criminal act (González and Maganda 2000). Divorce is frowned upon and considered a mortal sin by the catholic church. Social constraints do not grant women much independence in this respect either, which has

economic implications, too. Numbers by the OECD (2017) comparing how much household members respectively contribute to the shared income across countries exemplify this. For households with females aged 25 to 45 and at least one child present, almost 50% of Mexican households depend solely on the income of the male spouse. This share is very high compared to the other countries examined and accounts for less than 5% in Norway, where it is most common for both partners to work full time. The low female labor market participation for this subgroup of women illustrates the economic dependency on men, who tend to be the breadwinner of the family.

Crime in Mexico is often tied to organized crime or drug trafficking where violent offenses are frequent. The homicide rate is among the highest in the world and has been increasing in Mexico since 2010, contrasting trends in most other Central American countries (United Nations Office on Drugs and Crime 2019). In terms of the occurrence of violence perpetrated by an intimate partner or spouse, a 2003 survey uncovered that almost 50% of all women above 15 years of age living with a partner suffer from domestic violence of some sort (Pick, Contreras, and Barker-Aguilar 2006). Estimates from the National Survey on the Dynamics of Household Relationships (ENIDREH) in 2016 convey that 44% of women over 15 years of age experience intimate partner violence at some point in their life (INEGI - Instituto Nacional de Estadística Geográfica e Informática 2016).

2.3 Female empowerment

The expression *empowerment* is frequently used in different contexts and gladly employed to support all sorts of political or social agenda. However, a precise definition of what empowerment entails is often lacking. In order to grasp the context of the problem statement, different types of female empowerment are discussed in the following section.

i. Types of empowerment and recent interventions

Empowerment is generally defined by the Cambridge Advanced Learner's Dictionary (2020) as "*the process of gaining freedom and power to do what you want or to control what happens to you*". Mandal (2013) distinguishes between five different types of empowerment: social, educational, economic, psychological and political.

Female social empowerment addresses the common regard and standing of women in society. Social structures and norms may be considered rigid and hard to impact in the short run. Hence, social empowerment is difficult to impact directly through public policy. Economic and educational empowerment targeting females can be more directly addressed through intervention. As providing educational possibilities can act as a tool to increase the scope for economic independence in the long run, economic empowerment can be a consequence of educational empowerment (Mandal 2013). Contributing monetarily to a shared household income, employed females become more self-sufficient. At the same time, their intra-household standing can be impacted. In terms of public policy, providing educational programs, job training or micro finance can act as a tool for governments to enhance female empowerment. Yet, also policies with other primary agendas, such as the introduction of SP in Mexico, can affect the power structure and economic balance between partners. Indirectly, the woman's outside option is improved, leading to empowerment.

Political and psychological empowerment are the remaining two types of empowerment. By actively getting involved politically, women seize the power that is given to them by law. As of today, it can be observed that women are disproportionately underrepresented in the political process (UN Women 2019). Psychological empowerment may be an important aspect in respect to domestic violence, especially in Western countries. When abuse is committed by an intimate partner, severe trauma is induced and self-worth and confidence suffer. These struggles are commonly accompanied by mental health issues. Victims may feel helpless, weak and unable to escape the toxic situation they are in. The process of regaining psychological stability and a feeling of self-worth may be important in terms of empowering victims of domestic abuse.

Interventions on female empowerment are often multi-layered and address different kinds of empowerment. A recent measure includes a program in Uganda targeting young females evaluated by Bandiera et al. (2018). Within the scope of the program ELA (Empowerment and Livelihood for Adolescents), adolescent girls were offered vocational training as well as education about more general life skills including e.g. sexual and reproductive health. In addition, a mentor-system and regular meetings in groups generated a safe space to meet others and a sense of community. Findings of the evaluation of this policy imply increases in labor market activities, especially entrepreneurship, as well as reductions in early pregnancies and marriages among participants (Bandiera et al. 2018). Another intervention and

corresponding impacts are presented by Armand et al. (2018). They study the effects of allocating funds within the scope of a cash transfer program in Macedonia to the women of eligible households. In the case of the Macedonian program, it was randomly selected which household member receives the cash amount. The randomized nature of this approach makes it then feasible to identify possible differential effects of allocating the money to women instead of men. Armand et al. (2018) find that spending on food significantly increases when women were assigned cash transfers and that diets become more nutritious if initial levels of food expenditures were low.

Chapter 3: Theories

The problem that I aim to shed light on in this thesis covers the possible relationship between health insurance coverage and domestic violence. The suspected mechanisms at work establishing and impacting this connection are not straightforward. Yet, there exist two theories that can be directly applied in an economic context explaining why an impact of SP on domestic violence is possible and reasonable to expect. I will focus on and test these two theories, namely the bargaining power theory as well as the male backlash model. The two approaches acknowledge that household dynamics may change when SP is introduced, but predict contradicting effects on levels of intra-household violence. Which factors contribute to domestic violence is typically not a question answered by economists, but by sociologists and criminologists.² Two angles evolving from research in these fields that can have relevance in the context of SP will be elaborated upon towards the end of the following section.

3.1 Bargaining Power Model

Evidence suggests that the way households consisting of several household members reach decisions cannot be described as simply maximizing collective utility (see e.g. Bobonis 2009, Schultz 1990, Thomas 1990). The allocation of resources will rather be determined by a process of bargaining where individual preferences will be integrated depending on the intra-household decision-making power of respective household member (Majlesi 2016). The

² In addition to exposure time being a relevant predictor of domestic violence (in line with the analysis by Dugan, Nagin and Rosenfeld (1999) discussed in more detail in section 3.3) Gelles and Straus (1979) review a range of contributing factors which form the basis of selected theories of violence. Such factors include high stress or frustration levels, attending to conflict exacerbating activities as well as psychopathology and the abuse of alcohol and drugs by the perpetrator.

bargaining power model is an intra-household model based on rational, utility-maximizing household members that incorporates exactly this. I will focus on the bargaining power model as presented by Aizer (2010) taking into account violence within the household. As both the male and female part of the household are trying to enhance their own respective utility, they are assumed to settle on a cooperative solution to ensure the sustainment of the common household. It is supposed that the male derives utility from exerting violence upon the female whilst she is negatively affected by it. Moreover, as standard economic theory implies, the level of consumption of each individual influences their levels of utility. More formally, this can be expressed in the following manner

$$U_{female} (C_{female}, S)$$

$$U_{male} (C_{male}, V)$$

where U stands for utility, C is the consumption, S denotes safety and V represents violence against the female. In addition, both the male and female member of the household positively benefit from being in a relationship and creating a shared household together. This positive benefit exists both on a personal level in terms of increased economic stability and shared risk in case of economic shocks as well as a societal level, depending on how strict social rules in regards to partnership and marriage are. Incorporating this into the utility expression yields

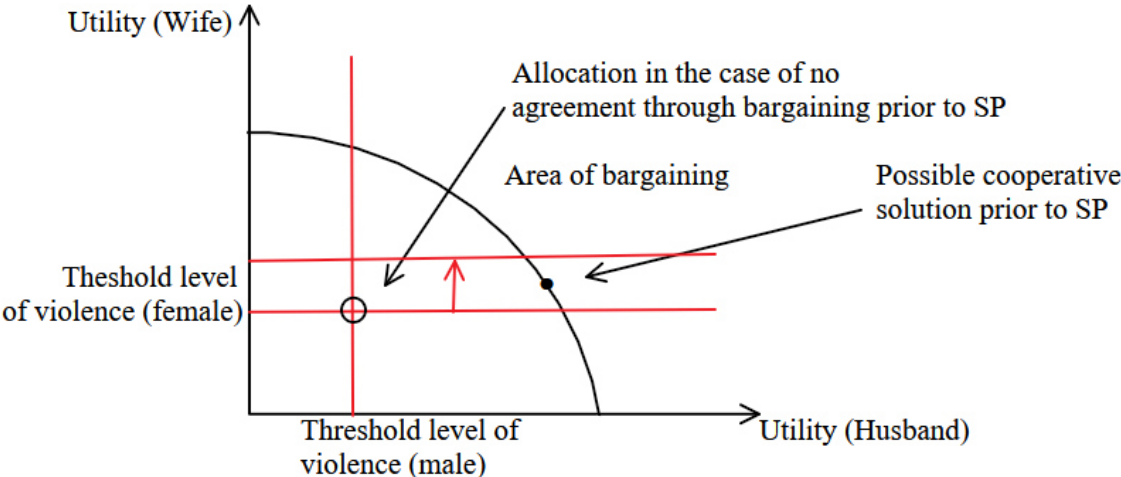
$$U_{female} (C_{female}, S, P)$$

$$U_{male} (C_{male}, V, P)$$

with P implying a partnership. In this simplified model, the man is assumed to prefer a high level of violence at home, all else being equal. Yet, a level too high would lead to the women facing more disutility from violence than utility from the partnership. Leaving the relationship and choosing her outside option would then be the rational thing to do. The male should therefore choose the level of violence in a way that does not risk the woman leaving him. The area of a cooperative solution as well as the threshold values of tolerable violence levels for both male and female are graphically emphasized in figure 1.

As the actual bargaining power of each household member can never be directly observed by the researcher, implicit exogenous changes of the intra-household power structure are usually considered in order to set changes in outcomes of interest into context (Majlesi 2016). With a more attractive outside option, women become relatively more empowered. With SP in place, women do not face the vulnerable position of not having health care coverage in case of a split from a partner previously providing employer-based health insurance.

Figure 1: Intra-household bargaining model incorporating violent abuse by the male spouse



In such a situation, the women is in a position where she can more credibly threaten leaving the relationship due to violence, already at lower levels (Aizer 2010). This mechanism is visualized in figure 1, when the red horizontal line indicating that the violent relationship and the female’s outside option are equally beneficial to her is shifted upwards (implying lower levels of violence). The critical level of violence the woman is willing to accept decreases and the cooperative outcome from bargaining should lead to a level of violence lower than before. In other words, the female fallback position in case of a split improves which means that the area of bargaining depicted in figure 1 becomes smaller and the man has to adjust the level of violence in order to sustain the relationship. Overall, the bargaining power approach to domestic violence predicts that the relative increase of power on the woman’s part leads to lower rates of intimate partner violence within households. In respect to the introduction of SP, this would imply a consequent decrease of home violence in Mexico.

3.2 Male Backlash

According to Rudman (1998), the term backlash refers to penalties for counter-stereotypical behavior. Such penalties can impair affected individuals both socially and economically. In Mexico, traditional views on gender roles are still present and widespread nowadays. Empowered and independent female behavior could hence be considered counter-stereotypical by many. When women improve their relative intra-household standing, e.g. by an increased income, new employment opportunities or an otherwise enhanced outside option, the man will consider his dominance over her in jeopardy according to the male backlash approach (Macmillan and Gartner 1999). Especially in countries shaped by conservatism and patriarchal culture, tradition and rurality, men could fear for their reputation and consider empowered women a threat to their intrinsic identity. Independent women, in the eyes of men or society as a whole, then violate a culturally imposed norm. The backlash approach consequently implies that the man will lash back at the woman and hurt her in order to restore order and keep her in place. Aizer (2010) acknowledges that this theory lacks a component accounting for a female rationality constraint as it would be rational to leave a partner if violence increases to intolerable levels. Victims of domestic violence may however feel trapped in toxic relationships, sometimes not even confiding in anyone. It appears uncertain whether behavior can be classified as rational under such conditions. Also, the threat of leaving the relationship will not be a credible one in settings where divorce is tied to social stigma and a deterioration in status (Chin 2012, Srinivasan and Bedi 2007). The introduction of SP could thus have perverse impacts, increasing intimate partner violence.

3.3 Exposure Reduction

In their analysis on the contributing factors to the significant decline in domestic violence between the 1970's and the late 1990's, criminologists Dugan, Nagin and Rosenfeld (1999) promote the theory of exposure reduction leading to decreases in domestic violence. This simple theory implies that violence will decline due to a reduction in the time spent together with the respective partner. The authors base this conclusion on that fact that domesticity declined during the same time period as violence rates fell. Participating more in the labor force, women became economically more independent. The theory of exposure reduction is often implicitly talked about when the prominent topic of domestic violence occurrence

during holiday seasons is discussed. Boutilier et al. (2017) assert that professional sporting events, summer months as well as certain holidays correlate with higher rates of domestic violence using data from Calgary, Canada. In general, it yet remains uncertain whether the mere exposure or other triggers that coincide with such events, such as increased stress levels and frustration, are the cause for this. Dahl and Card (2011) examine the latter factors by analyzing the real-time impact of football match results on home violence. They find that unexpected losses of the favored team lead to a 10% increase in reported home violence. The theory of exposure reduction will not be examined in further detail in the context of this thesis as I do not have information on the relevant variables. It is however impossible to completely negate that this approach may be relevant, also in the case of the SP introduction in Mexico. The reason for this it that SP may have changed incentives to participate in the labor market, as described in more detail in section 3.4 that follows. In addition to substituting formal for informal employment, the possibility of some citizens completely dropping out of the labor force cannot be excluded. Hence, exposure may increase due to SP which in turn may lead to higher levels of domestic violence within Mexican households.

3.4 Labor market informality

Another angle on the issue of changes in domestic violence related to SP stands in connection with incentives to participate in the formal labor force. As highlighted by Wagstaff and Manachotphong (2012), introducing universal health insurance has the potential to distort labor market decision in favor of informal employment. This is an undesirable effect in itself if informality is associated with tax evasive behavior. With respect to the incidence of domestic violence it also appears possible that the mode of employment in itself could represent a predicting factor. Since informal work arrangements can result in monetary instability and heightened stress levels, SP may reinforce already existing issues at home. Another possibility is that men that previously were formally employed find their way into self-employment more frequently when SP is introduced. Freelance work could then result in more hours spent at home and hence more exposure time. Besides the pure time spent together, the fact that work-related stress is introduced into the living place could contribute to making matters worse for abused women. Yet, as I will elaborate upon in the next chapter, research on the effects of SP on informality only find small impacts, if any. Whilst there will be no further specific focus on this approach, its potential relevance cannot be excluded.

Chapter 4: Literature Review

There exists little empirical research in the field of economics which tries to investigate mechanisms tied to domestic violence that are strictly comparable to the suspected effects at play in the context of this paper on Mexico. Reviewing relevant literature is dealt with in sequences. This way I aim to connect the relevant puzzle pieces of carried out research in order to reach reasonable assumptions about prevalent impacts of SP. Firstly, literature on the introduction of universal health insurance will be addressed. As SP ensures that everyone without previous coverage has the opportunity to enroll, it can in its essence be categorized as universal health care for the Mexican population. The research focusses mainly on health impacts, yet also illustrates and addresses possible issues that can arise whilst trying to estimate causal effects. Subsequently, I will focus more specifically on the existing research on the impacts of SP. As in the general literature on health insurance coverage, direct health effects are of specific interest when researching SP's impacts. Yet, matters related to effects on the labor market and informality may be relevant for the problem statement at hand, too. Eventually, the existing literature on domestic violence will be elaborated upon, differentiating based on the specific setting of the respective paper. I do this because the external validity of any findings rests on the comparability between households examined. As it is reasonable to assume that both culture and family dynamics differ immensely across countries, I sum up results from western countries as well as developing and threshold countries separately. Emerging patterns within these two groups will be described eventually. Another approach that could have been chosen instead would have entailed to distinguish between the estimated effects of different kinds of policies and interventions. This is due to the fact that not only the observation units of analysis may differ significantly, impacting estimated effects, but also the types of intervention.

4.1 Universal health insurance (UHI)

There exists a variety of academic papers that are dedicated to evaluating the impact of expanding health care coverage in various settings. Famous ones on the RAND (Brook et al. 1983, Aron-Dine, Einav, and Finkelstein 2013) or the Oregon Health Insurance Experiment (Finkelstein et al. 2012) are based on a RCT (randomized control trial) strategy and dedicated to analyzing effects on mostly self-assessed health outcomes as well as health service utilization. Whereas the utilization of medical services as well as total expenditures tend to

increase when UHI is introduced, direct effects on health are often negligible or hard to identify, presumably in part because data only covers limited time spans. Evidence which actually points to improvements in both physical and psychological health comes from Finkelstein et al. (2012) and Baicker et al. (2013) examining an expansion of Medicaid³.

Two Latin American countries that introduced UHI programs resembling the basic concept of SP are Colombia and Peru. One difference to SP is that both programs determine eligibility based on a threshold value of the index of wealth. Evaluating the impacts of these insurance schemes using a regression discontinuity design, which compares observations right below and above the cutoff index value, is then viable. Doing so, Miller, Pinto and Vera-Hernandez (2013) find that the Colombian program (*Régimen Subsidiado*) not only shields from financial distress and increases preventive usage of services, but leads to significant health improvements for children. Following a similar approach for the case of Peru, Bernal, Carpio and Klein (2017) conclude that the Peruvian program *Seguro Integral de Salud* increased the propensity of people receiving medicines, visiting hospitals and having medical analysis performed. They take this as evidence that the insurance contributed to a heightened sense of awareness amongst the population in regards to health issues leading to an increased willingness to pay for medical services overall (Bernal, Carpio, and Klein 2017).

A recurring concern tied to UHI is that guaranteeing health care coverage may lead to skewed incentives. On one hand there exists the danger of moral hazard described in detail by Einav and Finkelstein (2018). On the other hand, labor market decisions may be impacted when insurance previously was tied to a formal employer, especially in less developed countries with a large informal sector. Individuals may hence substitute informal work for formal employment. Testing this hypothesis for the case of Vietnam where UHI was rolled out gradually during 2001 and 2002, Wagstaff and Manachotphong (2012) find that formal-sector employment among married men declined and informal-sector work, especially amongst married women, increased due to the change in policy.

³ Medicaid offers health insurance coverage to especially marginalized US citizens including low-income adults, children, pregnant women, elderly people as well as people with disabilities. The program is managed on a state-level and financed jointly by the states and the federal government (U.S. Centers for Medicare & Medicaid Services 2020a).

4.2 Impacts of SP

Due to the manner of introduction, which conveniently allows the implementation of a quasi-experimental research design, there exists considerable literature evaluating SP. Of special interest are impacts on outcomes related to health measures. Furthermore, effects on labor market participation are considered. These may be important within the scope of this thesis's problem statement, as changes in informality could have the potential to affect family dynamics within households.

i. Health

One of the most discussed and controversial questions regarding the introduction of UHI is whether recipients experience positive effects on their health. In regards to SP, there exists an array of papers that examines this matter. Little direct impact on the health of enrolled residents is detected overall. Again, it is possible that this is at least in part due to the fact that conducted studies examine short-term impacts only. However, for especially vulnerable groups, such as infants in marginalized areas, SP may have improved outcomes.

A paper by Melissa Knox (2008) focusses on health-related and labor supply outcomes for early introducers of SP between 2002 and 2004. Corresponding findings imply an increased probability of making use of health services as well as a rise in the frequency of usage. Yet, overall health outcomes were not positively altered.

Implementing a triple-difference estimation strategy, comparing workers of the informal and the formal sector, Barros (2008) uses data from Mexican cross-section household surveys. Whereas findings imply that household health expenditures significantly decreased and enrolled households have a higher propensity to use medical services, SP did not directly impact health.

Focusing on the health of infants avoids issues tied to detecting lagged health impacts common for adults and should make the identification of direct effects of policy easier (Pfütze 2014). Gallardo-García (2006) finds positive effects of SP on the birthweight of newborns. Accounting for selection on the outcome, as live births are more reliably recorded than still births, Pfütze (2014) examines effects of SP on infant mortality during the first five years of

SP. Using census data on households from 2010, associated findings imply that infant mortality is reduced by 5 in 1000 for the overall population and by 7 in 1000 for the population eligible for the program.

Conti and Ginja (2017), analyzing mortality records as well as the data on hospitalizations also used in this paper, cover the same time frame (2000-2012) and employ a similar empirical strategy as I chose to. They examine whether the health of people from different age groups within the population is affected by SP. As they find no significant impact for young children, adults and the elderly, infant mortality within the poor municipalities of the country is reduced by 10%. This reduction implies that the lives of around 800 infants can be saved per year.

ii. Labor Markets and Informality

Another area of interest focuses on the effect of SP on labor market decisions of individuals. As social insurance programs are generally viewed as desirable, they pose the risk of giving disincentives to work in the formal sector. Concerning the introduction of SP, a number of papers find negligible to no evidence of any impact on informality (see e.g. Azuara and Marinescu 2013, Barros 2008, Campos-Vázquez and Knox 2010). Others discover an effect, such as Aterido, Hallward-Driemeier, and Pagés (2011). They establish that there exists an especially high degree of mobility between formal and informal sector in Mexico. Yet, Aterido, Hallward-Driemeier, and Pagés (2011) find that SP did not directly lead to a substitution of formal with informal employment. The inflow into formal employment is discouraged however. Short term effects seem to be most important here, as this effect is especially distinct in the years that directly follow SP implementation. According to their results, SP triggered a 20% decline in the inflow of workers into formal employment. Also, as SP is making people better off overall, fewer people are moving from unemployment to employment, leading to an overall lower labor force participation rate. The detected effects are robust and significant as well as biggest for large households with relatively uneducated household members.

Knauth et al. (2012) address a common misconception regarding the definition of informality in past research on this issue. Non-salaried workers, including freelance workers as well as

those who do not participate in the actual labor force, are commonly misconstrued as partaking in informality and hence contributing to tax dodging. As SP provides insurance for everyone without affiliation to a formal employer, a much larger group is covered than those working informally. Possible effects on informality may then be watered down and difficult to identify empirically. Knaul et al. (2012) argue that a long-term analysis of the impacts of SP on labor market outcomes is needed in order to evaluate this issue. Also, positive effects of investments into health care on productivity should not be neglected in this setting.

4.3 Domestic Violence

Economic literature on domestic violence is overall quite limited. Considering the available research requires careful consideration of respective setting when the idea of extrapolating from results is entertained. Tankard et al. (2018, 3379) acknowledge the need for more research on the effects in high-income countries as well as “*comparison across programs based on size and design*”. In the following I present the most important and relevant pieces of research in the context of this paper. First, studies from Western high-income countries are addressed. Subsequently, evidence from less developed countries is summed up.

i. Industrialized countries

The first paper to be reviewed comes from Aizer (2010) and is concerned with the relationship between the gender wage gap and domestic violence. Earlier papers with similar topics failed to establish a causal relationship between women’s income and domestic violence because of several issues. Firstly, the endogeneity of income through either omitted variable bias or reverse causality remained in most cases unaccounted for. Moreover, no paper before incorporated the fact that it is not only the actual female wage that matters, but also the relative wage compared to the male partner’s as well as the potential wage that is realistically obtainable. Aizer (2010) uses administrative hospital data on assaulted women in California that do not depend on self-assessment and therefore bypass reporting related biases. Instead of using women’s actual wages, which are endogenously determined, Aizer utilizes the fact that certain fields of work predominantly occupy males or females. Constructing local wage proxies by measuring industry particular wage changes to create a gender specific wage ratio (female relative to male), Aizer finds that an increase in the wage ratio reduces the number of women treated at hospitals for assault. As this result could reflect the predictions of the theory

of exposure reduction as presented by Dugan, Nagin and Rosenfeld (1999), Aizer provides evidence that the reduction in domestic violence in fact occurs outside of working hours. She hence argues that her results can be seen as evidence in support of a bargaining power approach.

A second relevant paper by Anderberg et al. (2016) is concerned with the impact of unemployment on domestic abuse. They incorporate a model built on the bargaining power approach. According to the model, unemployment should have differential impacts on each sex. As unemployment for men would increase the relative bargaining power of women, domestic violence should fall as a result. A partnership can in this context be seen as having a direct economic benefit. Living in a shared household can shield from the most extreme exposure to economic hardship, especially when both partners initially work. Therefore, it seems reasonable to assume that the man will be adjusting his violent behavior in order not to risk losing his partner. A rise in female unemployment should hence have the opposite effect. Anderberg et al. (2016) use disaggregated labor market data from England and Wales which they link up to survey data on domestic abuse in their analysis. Their results support the initial theory, as an increase in male unemployment with 1% leads to a reduction of violent abuse of the female partner with approximately 3%. Heightened female unemployment has the opposite effect of similar size. Gender unspecific increases in unemployment leaves violent abuse against the female within households unaffected. Anderberg et al. (2016) conclude with recommending the enhancement of women's job security, as this would represent a consequential measure against domestic violence perpetrated by a male partner.

Tur-Prats (2017) contributes to this area of research with evidence from Spain. Observing changes in female employment status, her hypothesis implies that impacts of female unemployment on intimate partner violence will depend on prevalent family structures. She refers to a stem family, where descendants remain in the familial household with their own spouse and children and contrasts it with the nuclear family structure, where children leave the household upon marriage. Assuming that women in nuclear families bear the unshared burden of domestic work, women in stem families share this burden with other household members and could hence be expected to seek employment more regularly and to be economically more independent. In line with this, Tur-Prats (2017) identifies heterogeneous effects of female employment in areas where stem and nuclear families respectively were historically predominant. For women living in regions with prevalently nuclear family

traditions, partaking in the labor market leads to increases in home violence whereas the opposite is true for women living in regions coined by stem family traditions.⁴

Addressing the potential impact of unemployment on the violent victimization among young females, Tertilt and van den Berg (2014) employ high-quality register data from Southern Sweden covering the period between 1999 and 2008. This data contains narrowly defined diagnoses of all patients seeking medical treatment which allows to identify victims of violence. As there exists evidence that unemployed individuals generally are more likely to be victim to violence (see e.g. Bowlus and Seitz 2006, FRA 2014), the authors hypothesize that the unemployed youth may be at higher risk of being victimized, too. Their argument implies that young unemployed individuals tend to socialize more with peers who are also unemployed, potentially partake in criminal activities or consume illegal substances. Increased exposure to such a “wrong crowd” may then increase the probability of being victimized. The presented results imply that this indeed seems to be the case, as otherwise comparable women who are employed are significantly less likely to become a victim of violence. Yet, as the argument behind the hypothesis already hints, the violence inflicted in such cases is not of a domestic nature, and estimated effects are largest for violence not committed by a member of a shared household.

On a more general level, Schneider, Harknett and McLanahan (2016) examine the impacts of the Great Recession in the US during the late 2000’s on violent behavior of men towards their female partners. Their approach differs from other research that incorporates observed unemployment rates, as they also consider how the pace of increasing unemployment rates impacts intimate partner violence. The results imply that a quickly deteriorating economy with rapidly rising unemployment triggers increases of abusive behavior. Controlling for household-specific experiences of unemployment and financial distress, this effect remains which points to indirect effects caused by the general uncertainty of the economic state.

⁴ Brassiolo (2016) presents additional evidence from Spain. In 2005, the costs of divorce were significantly reduced when a reform on marital dissolution was passed by the Spanish government. This means that divorce was becoming a more credible and less costly threat. Available survey data implies that the change in policy led to a large, significant decrease in intra-household conflict by 30%. These results are consistent with a bargaining power approach and in line with its predictions when the female outside option improves.

ii. *Less developed countries*

The first paper I want to summarize set in a non-Western country presents evidence from Vietnam. Bulte and Lensink (2019) examine interventions in Vietnam aimed to empower women and the consequential impacts on domestic violence. They use data from a randomized control trial and actually find that women in the treatment group are more likely to be exposed to domestic violence than women in the control group. Hence, the interventions aiming to promote gender equality seem to have perverse effects. They develop an advanced model, incorporating women's bargaining power within the household, as well as the household's income share contributed by women. The model implies that these two factors of female empowerment actually could have effects of opposed direction in regards to the prevalence of intimate partner violence. As bargaining power would decrease violence, economic independence could increase it. The authors emphasize that these results hinge on the women's outside option and hence on how socially acceptable divorce is. In rural Vietnam, divorce is for instance socially widely unaccepted. When women cannot plausibly threaten divorce, encouraging women's economic independence seems to put them especially at risk as their bargaining power does not effectively increase.

These results are similar to the findings of Heath (2014). She uses data from Bangladesh and finds a positive correlation between female employment and the occurrence of home violence. Findings yet apply only to either uneducated women or women who are rather young upon marriage. It seems thus reasonable to argue that these women cannot credibly threat leaving and have a low degree of bargaining power to begin with. In line with Bulte and Lensink (2019), economic independence may then worsen the violence suffered at home as the husband will seek to mark is dominant position.

Recent research by Bhalotra et al. (2018) addresses the same question as Anderberg et al. (2016), namely how shocks in employment status for men and women differentially affect home violence. The external circumstances differ, as data from 31 developing countries between 2005 and 2016 is utilized in this case. The results are intriguing as they almost perfectly predict effects of the same size as Anderberg et al. (2016), but of opposite direction. Here, increases in male unemployment by 1% raise the incidence of home violence by 2.75% whereas female unemployment reduces it by 2.87%. Contrary to Anderberg et al. (2016), these findings are in line with a male backlash approach.

One more paper is specifically interesting in terms of the problem statement at hand because of its setting. Angelucci (2008) analyzed the effect of cash transfers to Mexican women within the scope of the OP program on both alcohol abuse and domestic violence which are assumed to be strongly correlated. The average monthly cash transfer amounted to 218 pesos (circa 20 USD at the time), which duplicated the mean female income, since the average married woman earned in the sample only 17 pesos per month in the absence of treatment. In 1998, data on residents from 506 villages in the states of Guerrero, Hidalgo, Michoacán, Puebla, Queretaro, San Luis Potosi, and Veracruz were collected via survey. Households that were excluded from the sample were e.g. single-headed households or households where the wife was not the one responding to the survey, as the husband will not be a reliable source of information regarding domestic abuse. Angelucci (2008) finds that the cash transfers led to a 15% decrease in alcohol abuse in the full sample of relevant households. The picture becomes blurrier in the context of domestic abuse, however. As domestic violence decreases due to OP by 37% in households receiving the minimum cash amount with males who completed primary school, there is a subgroup of women who experience harmful effects. Incidents of home violence actually increase for women who are entitled to large sums and are living with uneducated males. This applies especially when there is a considerable age gap between male and female. These findings are in line with a male backlash approach. As the male perceives the increased liquidity of the female as a threat to his identity and his dominant role within the household, he will exert even more violence in order to keep the woman in place. This result is especially interesting, as it punctuates the possibility of heterogeneous effects of well-intended policy, as well as the possibility of detecting backlash effects in Mexico.

Not addressing domestic violence directly, Elsayed and Roushdy (2017) examine how relaxing human capital constraints for women in Egypt affects their future labor market outcome as well as their intra-household decision making and view on gender roles. As financial prospects and skills were improved by the intervention, the latter aspects remained unchanged. In this context, social norms may still represent a hinder to female empowerment, even though women become economically more independent. The authors thus suspect that this may change when observing long run effects.

Rather than examining impacts of economic factors or policy on home violence as the outcome, Canavire-Bacarreza and Rios-Avila (2010) investigate the impacts of violence on female labor market outcomes for different ethnic groups within the Bolivian population.

Bolivia exhibits very high rates of home violence compared to other developing countries, especially among the indigenous population. The authors hypothesize that the impact of violence will be relatively small for the indigenous, as domestic violence tends to be commonly and socially accepted within this group (Albo 1994). Female labor market outcomes may be unaffected, frankly speaking, because indigenous women are used to suffer violence. Establishing measures of physical and psychological abuse as well as a combination of both, findings point to a positive significant correlation between dropping out of work and the incidence of violence. Addressing their hypothesis tied to heterogeneous effects for indigenous and non-indigenous women they find that indigenous women in fact respond less to domestic violence than their non-indigenous counterparts. This does however not apply to women with high income and indigenous background.

iii. Patterns

The existing research on this topic might quantitatively not be sufficient in order to conclude with any distinct, certain pattern emerging. However, it seems as though the bargaining power approach, implying reduced domestic violence as a consequence of increased female empowerment, applies especially well to high-income nations such as the US, UK and Spain, as shown in the examples above. The picture becomes blurrier in developing and threshold countries. As comparatively low levels of economic development correlate with more restrictive norms, a higher number of religious citizens, as well as traditional views on women and their role in society, a relative improvement in female empowerment may have perverse effects. When threatening leaving the relationship is a non-credible threat due to social stigma etc., male backlash predictions may prove true, as seen in the case study of e.g. Vietnam mentioned above. However, it is important to note that there appear to be heterogeneous effects on different groups of women in Mexico, as presented by Angelucci (2008). The bottom line is that there may be impacts drawing in opposite directions in any country examined, but it varies which one dominates overall. It hence does not seem unreasonable to assume that there might be perverse effects on some, even in developed countries, but that these are dominated and overshadowed by positive impacts on others. The possibility that some population subgroups become worse off thus exists. This issue specifically will not be addressed in this thesis any further, but represents an interesting area for future research.

Chapter 5: Data and descriptive statistics

To examine the problem at hand, a total of five data sets was utilized. For the main analysis I employ administrative data on the respective outcomes of interest combined with the registry called *Padrón*, containing information on each household enrolled with SP. To back up the validity of the generated results on home violence, I employ survey data from three interview rounds covering the periods before, during and after the implementation of SP. Hereafter, I will describe each of the data sets used and discuss the key variables that are of importance for the further analysis. As the raw data sets needed a considerable amount of cleaning and manipulation, I will also sum up the most important steps that were undertaken in STATA to prepare the data for analysis.

5.1 Data sources

i. Padrón

The registry of families who became enrolled with SP between 2002 until 2010, also referred to as *Padrón*, encompasses information on the households affiliated with SP in each year during the rollout. This data includes e.g. a unique identifier for each state and municipality of residence, socioeconomic features of the households as well as the exact date of affiliation with SP. The lastly mentioned feature will be central in creating an indicator on when each municipality actually had SP in active use. I will get back to this in detail.

I employ a modified version of the *Padrón*⁵ which was also used by Conti and Ginja (2017) in their paper on health impacts of SP. Whereas the original registry data contains information on each affiliated family on an individual level, the data set at hand pools this information on a municipality level for families residing in the same area. This makes it possible to determine the point in time when a municipality had a specific number of households enrolled with the program. Unit-wise, time is measured in full years. When defining a threshold value of the number of families that need to be enrolled in order to consider a municipality “treated”, this

⁵ This data was provided to me by Rita Ginja (thesis supervisor), who was granted access to this data alongside her co-author Gabriella Conti for their research on the effects of SP on child mortality: *Who Benefits from Free Health Insurance: Evidence from Mexico* (2017).

data allows to stipulate the treatment status of each municipality in each given year. As proposed by Conti and Ginja (2017), I define the treatment dummy taking value 1 when ten families have signed up with SP. This value is chosen due to two reasons mainly. Firstly, a total instead of a relative measure should be picked, since it is not necessarily relevant that a specific share of the population is covered, but the focus should be on people actively adopting SP. Secondly, values smaller than ten will be more prone to measurement error, which would introduce bias to any estimates (Conti and Ginja 2017).⁶

Besides details on the timing of rollout, information on the wealth across municipalities measured at the baseline (2000) is incorporated in the data set. The parameter used in this context is the marginalization index, which is more broadly utilized by Mexican authorities to distinguish between rich and poor areas.⁷ Slightly over 50% of municipalities are considered poor according to the index. The respective variable in the data set is a dummy taking value 0 if respective municipality is considered rich and 1 if the municipality is poor. This variable will be helpful in the subsequent analysis as it allows me to contemplate whether there exist heterogeneous effects on women in municipalities which differ in terms of average affluence. Additionally, the data was supplemented with information on the population counts within municipalities.⁸ Total population counts were complemented by counts for specific age groups that I generated in STATA. This information will be useful when controlling for prior to SP existing trends tied to changes in the municipality-specific population size.

Ensuring a balanced panel, two things have to be considered. Firstly, the creation of new municipalities and accordingly the split of old ones between 2000 and 2012 has to be accounted for. Thus, I exclude municipalities that exhibit missing information on the poverty indicator measured in 2000, as those are municipalities that did not exist at that point in time. Hereby the sample is restricted to the municipalities existing at the baseline. Secondly, municipalities in the sample must have implemented SP within the year of 2010. This leaves me with a sample of $N = 2431$ municipalities.

⁶ The threshold value of ten enrolled families is also adopted in other SP-related research by e.g. Bosch and Campos-Vazquez (2014) as well as del Valle (2014).

⁷ Values on the index of marginalization from 2000 were retrieved from http://www.conapo.gob.mx/es/CONAPO/Datos_Abiertos_del_Indice_de_Marginacion.

⁸ The population data is obtained from the CONAPO (*Consejo Nacional de Población* - National Population Council). See <https://www.gob.mx/conapo>.

ii. Hospital data

The second main source of data containing information on the primary outcome of interest comes from administrative records from both private and public Mexican hospitals. These records are listing details on each patient discharged between the years 2000 and 2012.⁹

The rich hospital data set holds information on a variety of factors on the admitted individual as well as the treating entity. In regard to the subsequent analysis, the information on the admitted patient is essential. Patient-specific variables include i.a. age, gender, the place of residence, the length of stay, the kind of insurance as well as so-called ICD-10 codes. The latter is short for *International Statistical Classification of Diseases and Related Health Problems* and 10 remarks the 10th revised version of the classification by the WHO. The usage of such codes in order to classify the patient's condition as precisely as possible is common practice all over the world. To identify affected individuals for my analysis regarding domestic violence, the ICD-10 are crucial, as they narrowly define the type of medical condition. While domestic violence reporting suffers from a substantial dark figure due to underreporting, this approach ensures objectiveness. However, it has to be addressed that it only focusses on the subgroup of victims in need of medical attention at hospitals, which represents a shortcoming in terms of issues tied to selection. Using hospital data in this context will hence be selective with regard to the type and the severity of home violence inflicted (Bindler and Ketel 2019). Detected effects could hence be local in nature. Yet, it is probably one of the most reliable approaches available within this thematic field.

ICD-10 codes consist of four figures in total, starting with one letter followed by up to 3 digits. The system primarily follows an alphabetical order. Combining the respective letter with the following digits establishes groups and subgroups over all kinds of medical conditions. The third digit is generally optional and specifies conditions even more precisely in cases where this is necessary. It takes value 0 if further accuracy is redundant or impossible to specify. When conditions occur due to external causes, ICD-10 codes describing the actual trauma are complemented by an additional code specifying the nature of the source of injury. As the primary outcome of interest is the occurrence of violence against women perpetrated by an intimate partner, the relevant ICD-10 codes of interest will be those linked to *Assault*

⁹ The publicly available data was downloaded from http://www.dgis.salud.gob.mx/contenidos/basesdedatos/da_egresoshosp_gobmx.html.

under the chapter labeled *External causes of morbidity and mortality*. Mentioned chapter covers the codes starting with the letter-digit combinations V01–Y98. Focusing on the subcategory of assault, this leaves me with 25 general codes in total, namely those beginning with X85–Y09. These can again be divided into more precisely defined subcategories.

One possible way to identify those who have suffered injuries from domestic violence is to focus on a subset of ICD-10 codes (Y06/07/08/09) that either directly imply or specifically mention maltreatment, neglect and abandonment from a spouse or partner. However, as domestic violence suffers from underreporting and often fails to be detected, I decided to rather exploit the richness of the data set and identify potential victims of domestic violence employing some available alternative factors. I do this both in an attempt to cover those who were not accurately recognized as domestic violence victims as well as to increase sample size to improve the preciseness of estimates to be generated. Yet, I use the firstly mentioned approach as an alternative domestic violence specification, which I will discuss in further detail in chapter 7 where regression results are reviewed.

The main domestic violence specification focusses on females aged 20 to 40 years. This age interval was chosen because it is reasonable to assume that the probability of living together with a partner for women that age is high.¹⁰ Additionally, I limited the pool of potential victims using the ICD-10 codes on assault, which leaves me with the subgroup of women aged 20 to 40 who fell victim to assault. One additional variable that is of major interest and importance for this specification is an indicator variable which tells us about the place of incidence when an injury is recorded.¹¹ As established, it is most often the spouse or intimate

¹⁰ The registry data on marriages between 2000 and 2012 implies that more than 60% of marriages occur when the female partner is aged 20 to 40 years. Also, most women are around the age of 20 upon marriage (see figure A2 in the appendix). Marital status data by the United Nations Population Division (2020) shows that almost 50% of women aged 20-24 are married in Mexico in 2000 (baseline). This share is increasing until the age of 40, where the share peaks around 80%. Afterwards, this rate steadily declines. The described trend continues in newer data on women's marital status in Mexico measured in 2009, 2010 and 2015 respectively.

¹¹ The variable indicating the place of incidence in the case of an externally caused injury takes nine distinct values. According to the catalog containing variable descriptions for this data, values 0 and 1 mark incidents that occurred at the place of residence or at residential institutions respectively. The remaining values describe events that happened e.g. in schools, at other public facilities or on the streets. Yet, examining how the recorded frequency distribution of this variable develops over the course of the observation period for assaulted women points to inconsistent recording. Whereas the indicator only takes value 0 ("place of residence") in a total of 3 cases between 2000 and 2009, it accounts for 17-24% of female assaults in the remaining three years of the observation period. In the first nine years, there is however an accumulation of cases that supposedly occurred in residential institutions (indicator taking value 1). Observations with indicator value 1 account for almost one third of all recorded instances of assault perpetrated against females in this period. After 2009, only 2.6% of observations were listed as having occurred at residential institutions, which seems like a more realistic value. Hence, considering the patterns in the data, I conclude that the indicator taking value 1 between 2000 and 2009

partner who is perpetrating the crime when a woman falls victim to violence. Limiting the hospital admissions to women in the age group that exhibits a high likelihood of sharing a household with a partner and who simultaneously fell victim to assault at the place of residence represents a rather convincing proxy for domestic violence victims. Of course, there still exists room for error using this specification. Yet, in the case of domestic violence no specification will be completely error-free due to issues of reporting and identification. This is why I argue that this specification is the best bet given the data at hand.

Using the described main specification, I generate a dummy variable indicating a case of assumed domestic violence when taking on value 1. In order to be able to use this information in the further analysis, incident counts per municipality for each year are generated. With these counts representing the outcome of interest, the data is subsequently merged with the rollout data described in the previous section based on the unique municipality identifier.

iii. Registry data on marriages and divorces

To understand how marriage and divorce rates changed due to SP, I use registry data on both marriages and divorces in Mexico.¹² This data is employed in the context of home violence to better understand how SP impacted relationship-related decision making which reflects the level of female empowerment and independence. In turn, this may have effects on the occurrence of intra-household violence. Three main areas of interest were considered with this data. Firstly, it is of relevance to assess how the composition of newly married couples is impacted by the program rollout. Secondly and thirdly, the rates of marriage and marriage dissolution will be considered. For reasons of comparability, the same time span as available for the hospital data was analyzed, namely 2000-2012.

The data listing each registered marriage in Mexico contains information on couple characteristics in addition to the date and place where the marriage took place. Details are recorded separately for wife and husband and cover i.a. nationality, age, education, occupation and the place of residence. As I am interested in the number of marriages

most likely summarizes events taking place at home, whereas the same types of incidents are disaggregated into two categories in the following three years. For the main specification I therefore use the cases with indicator value 1 for the years 2000-2009, and those taking value 0 and 1 in the years 2010-2012 to identify episodes of violence against women happening at home.

¹² Both publicly available data sets were retrieved from <https://www.inegi.org.mx/programas/nupcialidad/>.

registered and whether this number is impacted by SP, the data is collapsed on a year-municipality level in order to generate marriage counts per municipality per year. Secondly, the quality of the match between partners can be examined. Using the mentioned characteristics that are included in the data, a range of indicator variables were established to check whether certain assumptions about possible changes in female partner choice hold true. This will be discussed in further detail when addressing estimation results in chapter 7.

The divorce data is structured in the same way as the marriage data, where one observation represents one couple that filed for divorce. In addition to information also covered in the marriage register, the divorce data contains auxiliary information on i.a. the number of children that originated from the marriage and custody arrangements, as well as the cause of termination and which party it was who filed for divorce. This data makes it then feasible to e.g. check whether there was a general increase in the number of divorces, more divorces initiated by wives or fewer/more terminations of marriage due to reasons tied to intra-household violence as a consequence of SP.

In the same manner as for the hospital data, dummy indicators for respective factors of interest were established and then collapsed to generate municipality-year counts. The same applies for the overall number of marriages and divorces. It is then possible to merge the data with the rollout data for further analysis. How this is practically done in econometric terms is described in detail in chapter 6. Respective results are presented in chapter 7.

iv. Health Survey

Besides the hospital data, one additional data set with information on the occurrence of domestic violence was employed. This is done in order to corroborate the main results of the analysis. I use data from three interview waves of the National Health and Nutrition Survey (*Encuesta Nacional de Salud y Nutrición, ENSA/ENSANUT*) which examined 45,711, 47,152 and 50,528 households living in 321, 582 and 712 municipalities in the respective years of 2000, 2006 and 2012.¹³ This means that there is available data in the period before SP (2000), during rollout (2006), as well as after the rollout when all states had officially implemented

¹³ Data sets were retrieved from <https://ensanut.insp.mx/>.

SP (2012). Analyzing this data, the sample is restricted to households surveyed at least twice to guarantee comparability between interview waves.

The surveys were conducted with detailed questionnaires on a variety of individual and household characteristics. In regards to health, both self-reported as well as objective health measures were included. For my analysis, questions implying that the interviewee suffered home violence, sexual violence, violence as a consequence of an argument or violence due to no specific motive are of interest. Besides that, information on the gender, age and marital status were used in order to identify a group comparable to the sample examined in the main analysis. In contrast to the hospital discharge data, the survey data is analyzed on an individual level in order to control for household specific characteristics.

5.2 Descriptive trends and statistics

To set the subsequent analysis into context, I present descriptive statistics and plotted trends in this section. In table 1 baseline characteristics prior to the introduction to SP are depicted. In the year 2000, the average municipality counted just short of 34,000 inhabitants with the smallest only having a population count of 85. The largest municipality, which is located in Mexico City (up until 2016 called the Federal District or *Distrito Federal* (DF)), counted substantially more than one million residents. 52.78% of the municipalities in the sample are considered poor according to the index of marginalization measured in 2000.

Observing absolute numbers for the main outcome of interest, it can be seen that there were only few hospital admissions due to domestic violence in 2000. For the vast majority of municipalities there were no women identifiably affected by domestic violence at the baseline. This is also illustrated in figure 2 which plots the trend in domestic violence counts over the 12 years of data available. Domestic violence incidents are once again defined as hospital admissions of assaulted women aged 20 to 40 years who were injured in their place of residence, as stated in the main specification. Figure 2 illustrates that there is a large increase in home violence in the hospital data between 2002 and 2003, with the first states having introduced SP in 2002. After that, the increase slows down until there is a decrease in cases between 2008 and 2009. Subsequently, numbers rise again until 2011. 2012 experiences lower rates of home violence compared to the previous year. Taking into account the overall

trend in total admissions which is increasing as the years pass (see figure 4), I suspect that there exist additional factors causing the drastic increase between 2002 and 2003. On one hand side, this increase may be caused by a heightened usage of health services due to the introduction of SP. This is reasonable to suspect as health care finally becomes accessible for many who did not have coverage during previous years. Adding figure 3 to the deliberation, which plots the total admissions due to assault, the same drastic increase between 2002 and 2003 becomes visible. This could speak for the existence of increased health care usage effects. One has yet to be careful not to put too much weight on these arguments, as illustrated trends are purely descriptive, depicting total numbers only.

As the numbers for assault and hence for domestic violence are drastically low for the first three years data is available for, one could suspect reporting issues or systematic changes in reporting around the time SP was introduced in pilot states. This could represent a potential threat to the validity of the main regression results. Such suspicion becomes even stronger when observing the total numbers of assaults reported in the hospitals between 2000 and 2002. In 2001, there were no registered admissions due to assault, which appears rather dubious. Reporting will always contain error, but if reporting and associated error is systematically correlated with the rollout of SP which started in 2002, estimation results will contain bias. That is, if underreporting is not due to permanent local conditions which are accounted for by municipality fixed effects (Conti and Ginja 2017). This issue will be discussed in more detail in chapter 8, discussing potential confounders of the analysis.

Examining marriage and divorce rates as complementary outcomes, it can be seen that marriage rates are substantially higher than divorce rates in 2000. The municipality with the highest per capita marriage rate that year even exhibited ca. 0.5 marriages per resident. The mean female age upon marriage measured on the municipality level was 25 years. The corresponding age in respect to divorce was 35 years.

Table 1: Descriptive baseline statistics (2000)

	Municipality Mean (2000)	Standard Deviation	Min.	Max.	N (Mun.)
Population	33888.7100	92765.7000	85	1338580	2431
Domestic violence incidents	0.0026	0.0589	0	2	2431
Age upon incident	28.9848	3.2247	23	40	2431
Number of divorces	21.3571	92.8118	0	1712	2431
Number of marriages	289.0782	879.3965	0	14707	2431
Marriages per capita	0.0064	0.0042	0	0.4922	2431
Divorces per capita	0.0004	0.0006	0	0.0095	2431
Female age upon marriage	25.1097	1.6852	12	98	2431
Female age upon divorce	35.1796	1.4689	12	98	2431
Share of poor Municipalities (2000)	0.5278	0.4993			2431

Figure 2: Hospital admissions due to domestic violence

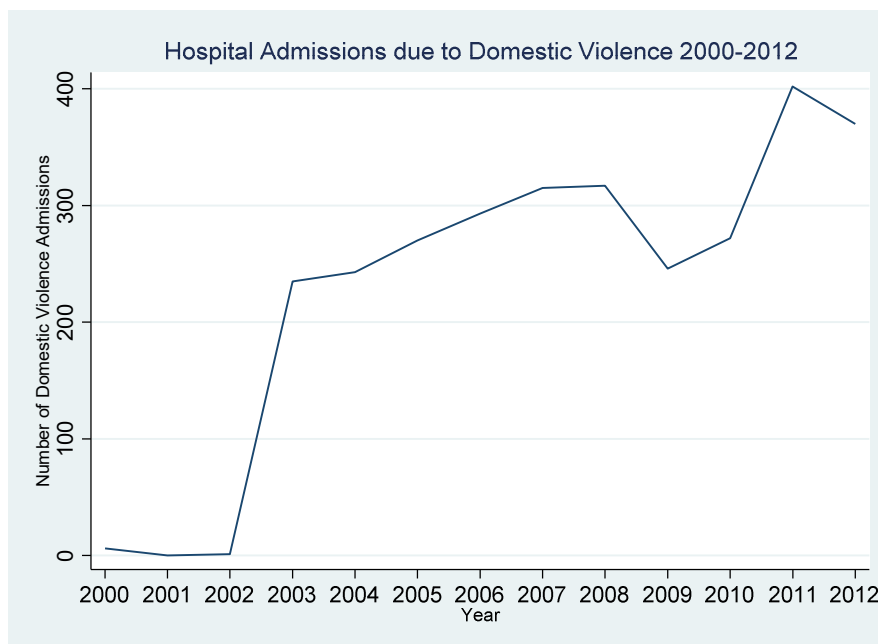


Figure 3: Hospital admissions due to assault

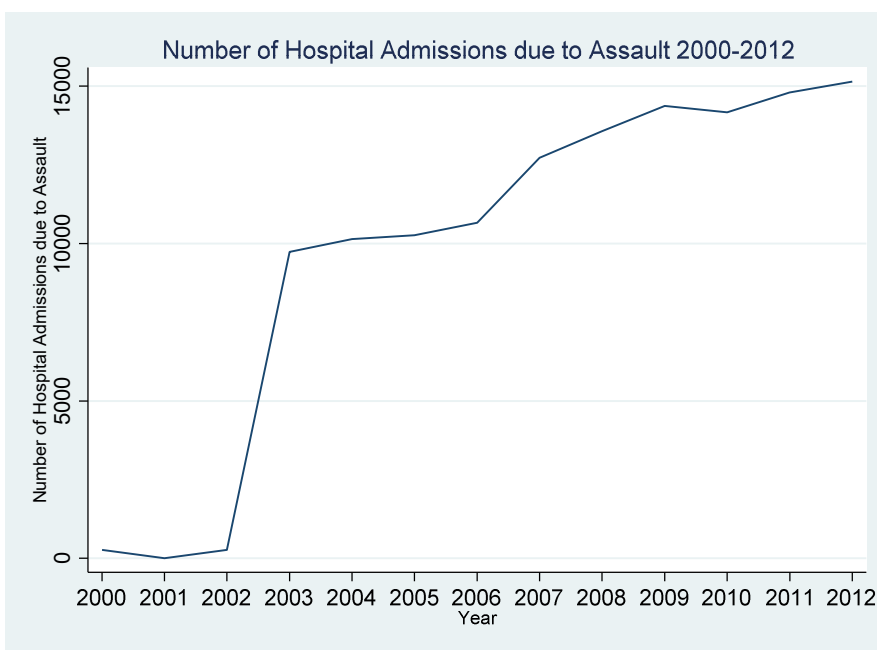
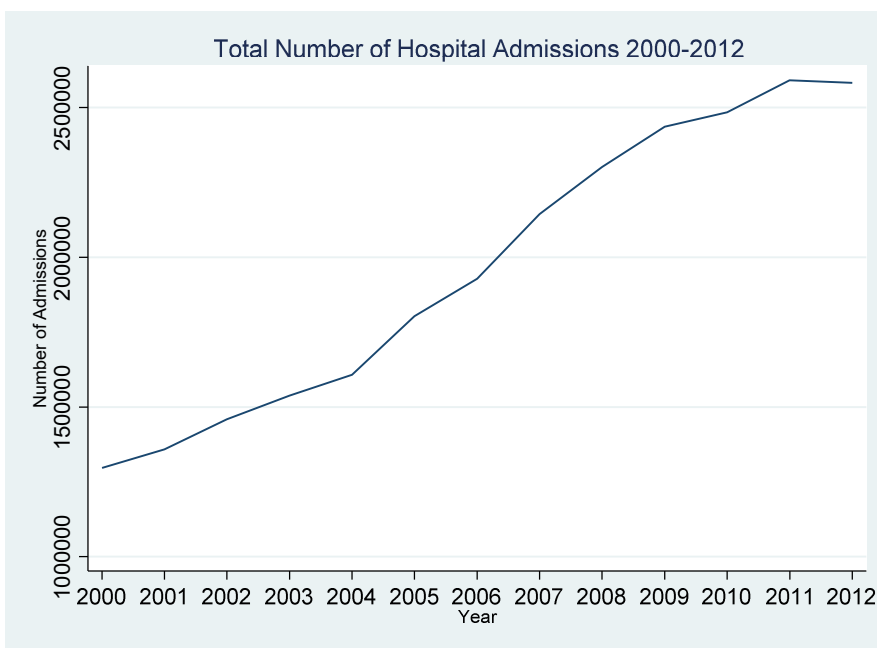


Figure 4: Hospital admissions (full sample)



Chapter 6: Estimation strategy

SP was launched gradually on a state-level. Starting in 2002 as a pilot project, states with certain characteristics were intentionally prioritized and became early introducers. Yet, the uncontrolled rollout of SP on a municipality level creates quasi-random variation in the data and leaves us with conditions suitable for implementing a quasi-natural experiment strategy. The panel data at hand allows to control for time-constant unobserved heterogeneity across municipalities as well as time effects common to all municipalities. In the following, I will elaborate upon the most commonly used estimation methods for panel data, the choice of strategy for the issue at hand, the identifying assumption that this strategy rests upon, as well as how I implement the described procedure and tackle arising challenges in practice.

6.1 Regression analysis for panel data

i. Ordinary Least Square (OLS) Regression

The OLS estimator is commonly used as a central guide value in most empirical research. Integrating relevant factors in order to explain values of a dependent outcome variable of interest, the OLS estimator fits a linear function of the chosen covariates to the observations that minimizes the squared residuals. The respective regression equation will then be

$$y_i = \alpha + \boldsymbol{\beta}'\mathbf{x}_i + \varepsilon_i$$

where y_i describes the dependent variable of interest. α denotes the constant term whereas \mathbf{x}_i is a vector over respective covariates accompanied by $\boldsymbol{\beta}'$, which represents the transcribed vector of coefficient estimates for each covariate. The residual error term is included as ε_i . Associated minimization problem is then defined as

$$S(\boldsymbol{\beta}) = \sum_{i=1}^N (y_i - (\alpha + \boldsymbol{\beta}'\mathbf{x}_i))^2$$

with $S(\boldsymbol{\beta})$ being the sum of the squared residual.

In order to be able to uncover causal effects using OLS estimation, a set of assumptions has to be fulfilled. These are commonly referred to as the Gauss-Markov assumptions. One of the assumptions indicates that the estimated error term has to be uncorrelated with the included regressors, which is of specific relevance when using panel data.

When applying the OLS estimator, panel data will be pooled (Wooldridge 2013). This means that data from different individuals, states, or as in this case municipalities will be treated as if they origin from one homogenous population. Results will then only be reliable if there is no remaining unobserved heterogeneity that can create omitted variable bias. In other words, one has to include all relevant covariates that may be correlated with outcome and error term to avoid skewed estimates. In most cases this is a rather unrealistic assumption to make. Panel data estimation methods solve this issue to a certain extend. What exactly these more advanced methods entail will be discussed in the next sections.

ii. *Fixed Effects (FE) Models*

As long as the Gauss-Markov assumptions are fulfilled, employing the OLS estimator presents the most efficient way of using data (Verbeek 2012). If there however is reason to believe that there exists unobserved heterogeneity across observation units, omitted variable bias will corrupt the validity of estimation results. The use of a fixed effects (FE) model can, at least to a certain extent, alleviate that problem. When panel data is available, using the FE estimator, which is also commonly referred to as the within-estimator, filters out time-invariant individual specific (or group specific) heterogeneity from the equation. As this heterogeneity is assumed to be fixed over time, *demeaning* (subtracting the mean) debilitates biases which occur due to such unobserved factors. With the general model as follows

$$y_{it} = \alpha_i + \delta_t + \boldsymbol{\rho}'\mathbf{x}_{it} + \varepsilon_{it}$$

including time-constant individual characteristics denoted as α_i , individual unspecific time trends δ_t and \mathbf{x}_{it} being a set of regressors that vary both over time and across individuals, demeaning yields the following expression

$$y_{it} - \bar{y}_i = \delta_t - \bar{\delta} + \boldsymbol{\rho}'(\mathbf{x}_{it} - \bar{\mathbf{x}}_i) + (\varepsilon_{it} - \bar{\varepsilon}_i)$$

where effects due to α_i are absorbed. A factor that becomes relevant in this context is efficiency, as some of the variation in the data is effectively removed when using the fixed-effect estimator. If impacts tied to individual characteristics that are constant over time are of interest to the researcher, this can create issues as it is not possible to ascribe time-invariant variables any precise, unique coefficient estimate.

iii. Random Effect (RE) Models

An alternative model which similarly to the presented FE model makes use of variation over time in panel data is the random effects (RE) model. The RE model builds on one main assumption which allows modeling unobserved individual time-invariant heterogeneity as part of the error term. Taking the same general regression equation structure as shown in the section on the FE model as a starting point, this assumption entails an uncorrelated α_i and \mathbf{x}_{it} .

$$cov(\alpha_i, \mathbf{x}_{it}) = 0$$

If this assumption holds and α_i can be modelled as part of the stochastic error term, using the RE estimator will yield unbiased and consistent estimates (Wooldridge 2013). Furthermore, the RE estimator will use data more efficiently than the FE estimator. However, the underlying assumption is very strong and in practice most often unlikely to hold. This is why the FE model is more regularly used in today's empirical research.

iv. Comment on the model of choice

There are several factors that influence the choice of panel data estimation strategy, some already swiftly touched upon in the previous sections. Considering the data at hand, it becomes obvious that the simplest approach, namely a pooled OLS estimation, will not yield reliable results. Pooling observations from all Mexican municipalities will let area-related heterogeneity go unaccounted for. Mexico is a very diverse country and municipalities differ in many respects that most likely have an impact on both the outcome of interest, domestic violence, as well as the timing of SP introduction. OLS estimates would hence be skewed due to omitted variable bias.

The remaining two estimation methods, FE and RE, should manage to absorb the effects of pre-existing heterogeneity across municipalities. Considering which of the two models to choose, it would be faulty to try to establish whether the actual effects are of a fixed or random nature (Wooldridge 2013). What really matters is to consider whether the underlying RE assumption of an uncorrelated α_i and \mathbf{x}_{it} holds. In the Mexican case, the independent variable of interest is the treatment indicator of SP introduction which can be viewed as a part of the vector \mathbf{x}_{it} . Conti and Ginja (2017) examine the determinants of the timing of SP rollout on a municipality level in more detail. They find that half of the variation in rollout timing can be explained by time-constant state characteristics. Moreover, SP was introduced earlier in municipalities with e.g. a relatively high population count, a low marginalization index, and relatively many hospital and health center facilities. These differences need addressing in order to avoid biased estimates, but should be accounted for when using FE or RE models. Yet, it becomes apparent that time-invariant municipality characteristics are in fact correlated with the rollout of SP, which is why a FE model was chosen instead of a RE model in order to analyze the effects on domestic violence.

6.2 Difference-in-differences estimation strategy

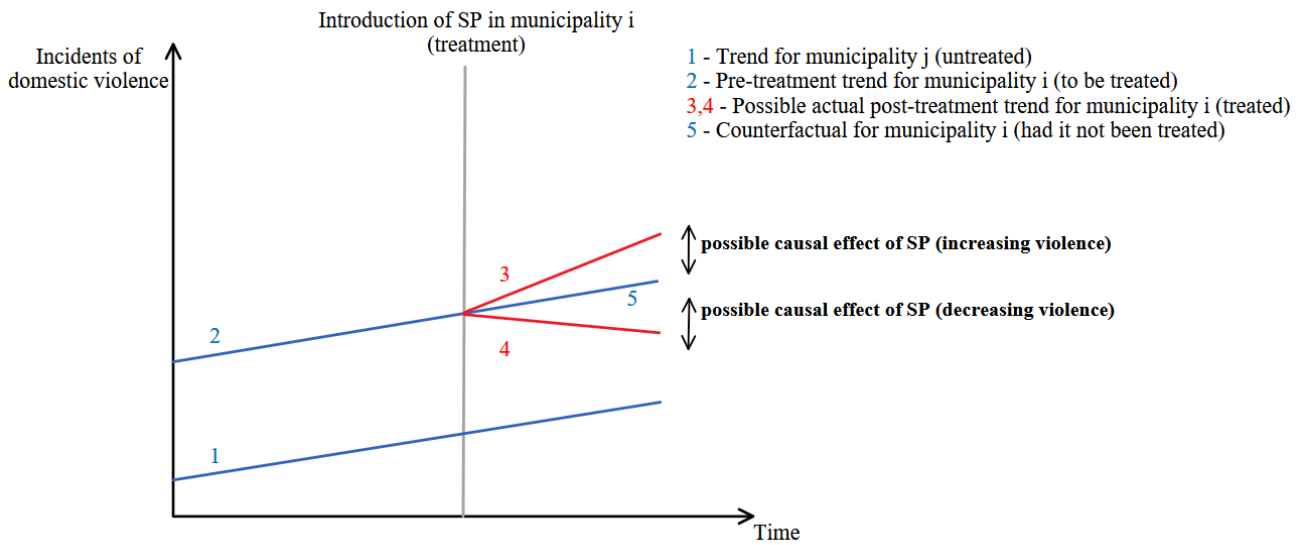
With real world data from uncontrolled environments, there are many confounding factors making it difficult to identify causal effects without making further assumptions. In such cases, researchers employ quasi-experimental estimation strategies. With the Mexican data at hand, the quasi-experimental estimation strategy of choice is the difference-in-differences (DD) design which is a special form of FE estimation. Generally speaking, DD exploits the differential outcomes in treatment and control group over time, controlling for both time-invariant individual characteristics as well as overall time trends, in order to detect the causal impact of policy changes (Blundell and Dias 2009). In the Mexican setting, this implies that changes in the occurrence of domestic violence across municipalities implementing SP at different points in time between 2002 and 2010 are compared. In general, observations from at least two such points in time are required to generate DD-estimates, hence panel data is a prerequisite. The deviation from the expected counterfactual outcome of the treatment group can in such scenario be interpreted as the causal effect of treatment. This is visualized in figure 5 below. The general identifying assumption is the so-called parallel trend assumption, that supposes same trends in municipalities in the absence of treatment.

The assumption allows us to write the following term

$$E(y_{0mst}|a_{ms}, SP_{mst}, t) = E(y_{0mst}|a_{ms}, t)$$

which includes the expected outcome in the absence of treatment ($E(y_{0mst})$), time-constant municipality characteristics with s indicating the respective state (a_{ms}), a general time trend (t) and a treatment indicator (SP_{mst}). Treatment is hence as good as randomly assigned conditioned on a_{ms} and t . Additionally, the absence of group-specific contemporaneous shocks, policy changes or changes in group composition that could impact the outcome is crucial in order to interpret estimates as causal.

Figure 5: Difference-in-differences estimation



Setting the DD approach into context with a simple example, I consider the two Mexican municipalities Puebla and Tijuana. These two municipalities adopted SP in different years. I assume Tijuana to be an early adopter of SP, having implemented it in year 1 whereas Puebla did not introduce it until later on. Hypothetically, data is available from just around year 1 and year two, after introduction. To find the causal impact of SP, municipality fixed-effects that are constant in time as well as time trends that are alike across municipalities have to be filtered out. This would result in a simple regression equation of the following form

$$Y_{mst} = \delta_{ms} + \pi_t + \beta SP_{mst} + \varepsilon_{mst} \quad (\text{with } E(\varepsilon_{mst}|m, s, t) = 0)$$

with δ_{ms} being municipality fixed effects, π_t depict time fixed effects and β is the coefficient of interest displaying the impact of SP on the outcome. ε_{mst} represents an idiosyncratic error term. In our very simple example with Tijuana and Puebla, β can be identified by taking first differences of the outcome between the two time periods for both municipalities. Constant municipality characteristics are thereby eliminated.

$$\begin{aligned} E(Y_{mst}|m = Tijuana, s = Baja California, t = 2) \\ - E(Y_{mst}|m = Tijuana, s = Baja California, t = 1) &= \pi_2 - \pi_1 + \beta \\ E(Y_{mst}|m = Puebla, s = Puebla, t = 2) \\ - E(Y_{mst}|m = Puebla, s = Puebla, t = 1) &= \pi_2 - \pi_1 \end{aligned}$$

By calculating the difference between the respective first differences time fixed effects common to regions cancel out and we are left with β representing the estimate of SP's causal effect.

$$\begin{aligned} E(Y_{mst}|m = Tijuana, s = Baja California, t = 2) \\ - E(Y_{mst}|m = Tijuana, s = Baja California, t = 1) \\ - E(Y_{mst}|m = Puebla, s = Puebla, t = 2) \\ + E(Y_{mst}|m = Puebla, s = Puebla, t = 1) &= \beta \end{aligned}$$

6.3 Time-to-event analysis

Since the data not only covers two but all Mexican municipalities as well as nine (2002-2010) instead of two possible implementation years, the DD estimation procedure will be slightly more complex. This implies that I will not only compare differences in outcome between two municipalities over time, but between numerous municipalities that implemented SP at different points in time during the rollout phase. In order to achieve a flexible model that suggestively indicates whether the identifying assumption of the estimation strategy holds, a time-to-event model is run. The simple treatment indicator in the regression equation is then

replaced by a term indicating any effects prior to treatment, as well as a term measuring effects after the implementation of the program.

As one year has to be set as a base category and is consequently omitted, I follow the approach employed by e.g. Conti and Ginja (2017) and Bailey and Goodman (2015) and use $t = -1$ as the control year. A resembling strategy is also used by Almond, Hoynes and Schanzenbach (2011). The flexible pre-treatment term will then cover years $t \leq -2$. The most flexible model will hence be the following

$$Y_{mst} = \sum_{k=-K}^{-2} \beta_k^{before} SP_{mst} + \sum_{k=0}^L \beta_k^{after} SP_{mst} + \delta_{ms} + \pi_t + \varepsilon_{mst}$$

where SP_{mst} is a dummy variable taking value 1 if municipality m in state s had implemented SP in year t , and 0 otherwise. K and L respectively represent the available number of years before and after implementation in the data whereas k indicates the number of years between implementation and t . If the parallel trend assumption holds and all unobserved heterogeneity across municipalities is accounted for by δ_{ms} , β_k^{before} should not be significantly distinguishable from zero. β_k^{after} , covering the effects of SP on domestic violence in the years following its implementation on a municipality level, will ultimately be the coefficient of interest.

To present results more concisely, I follow Conti and Ginja (2017) as well as Bailey and Goodman (2015) and estimate a compact time-to-event specification identifying short and long run effects of SP. This is simply done by defining appropriate dummy variables covering the desired time spans. Focusing on up to two years before introduction, short (0-2 years after launch) and long run (3+ years after launch) effects, the following equation is estimated

$$Y_{mst} = \beta_1 SP_{mst} [t - T_{sm} \leq -2] + \beta_2 SP_{mst} [0 \leq t - T_{sm} \leq 2] \\ + \beta_3 SP_{mst} [t - T_{sm} \geq 3] + \delta_{ms} + \pi_t + \varepsilon_{mst}$$

where T_{sm} indicates the year of implementation within each respective municipality. To ensure that there are no confounding factors that bias the coefficient estimates of interest, I control for an economy-wide trend in the outcome variable, supplementing the year FE. In

addition to this, I account for trends in the municipality-specific population size for the respective age brackets of interest, as differential population sizes across municipalities have to potential to skew estimates. This leaves us with the final regression model

$$Y_{mst} = \beta_1 SP_{mst} [t - T_{sm} \leq -2] + \beta_2 SP_{mst} [0 \leq t - T_{sm} \leq 2] + \beta_3 SP_{mst} [t - T_{sm} \geq 3] + \delta_{ms} + \pi_t + trend + poptrend_{ms} + \varepsilon_{mst}$$

where *trend* is the global time-trend and *poptrend_{ms}* depicts the trend in the age-specific population size for each municipality. The presented model assumes that everyone eligible is enrolled with SP as soon as municipalities reach treated status. This is in practice not the case, as affiliation does not happen automatically and at once, but rather gradually over time. Hence, estimated coefficients reflect intention-to-treat effects (ITT) (Conti and Ginja 2018). Such estimates will be lower-bound. In order to obtain the average treatment effect on the treated (ATT), the ITT estimate would have to be divided by the share of eligible residents actually affiliated with SP at the point in time in question. In essence, this represents the application of an instrument variable estimator. Summing up, it is reasonable to expect potentially uncovered effects to be somewhat small in size, especially in the short run when a smaller share of the eligible population is covered by SP.

When supplementary data sets on i.a. marriages and divorces are used, I essentially employ the exact same estimation strategy, only with a varying outcome of interest.

6.4 Standard error estimation

Adding to the mentioned challenges, issues related to the estimation of standard errors may emerge. Problems could for instance arise due to heteroskedasticity, which implies that the size of the error varies given different values of the regressor. The following OLS assumption implying a constant variance σ^2 of the error term e_i given \mathbf{x}_i is then violated.

$$var(e_i | \mathbf{x}_i) = \sigma^2$$

As heteroskedasticity rarely causes large issues, a more serious problem is related to potentially correlated observations when using panel data (Angrist and Pischke 2009,

Bertrand, Duflo, and Mullainathan 2004). Firstly, serial correlation between outcomes of the same observation units at different points in time can cause incorrectly estimated standard errors. Secondly, observations frequently come from individuals belonging to a particular group or entity. In the Mexican case I consider municipalities as such subordinate groups. When treatment is deployed on a group level, it is unlikely that the estimated error terms of individuals from said groups remain uncorrelated. This issue of correlation is commonly referred to as the Moulton problem. Moulton (1986) acknowledges that estimated standard errors may be severely underestimated if intra-group outcomes are correlated, which may lead to the faulty identification of significant coefficient estimates. This reasoning builds on assuming that the variance of the outcome is not only determined by an individual, but also by a group-specific component (Angrist and Pischke 2009).

$$e_{ig} = v_g + n_{ig}$$

Here, i denotes the individual and g the respective group. The intra-group correlation which pinpoints the relative influence of being member of a specific group is then given by the following expression.

$$\rho = \frac{\sigma_v^2}{\sigma_v^2 + \sigma_n^2}$$

With a fixed number of group members that is equal across all groups, the Moulton problem can be formally illustrated using the following expression

$$\frac{var_{true}(\beta)}{var_{conventional}(\beta)} = 1 + (n - 1)\rho$$

where $var_{true}(\beta)$ represents the size of the actual, true variance and $var_{conventional}(\beta)$ the variance that is estimated with conventional methods. Standard methods fail to account for issues tied to within-group correlation. n describes the number of group members. The square-root of this expression is referred to as the Moulton factor (Angrist and Pischke 2009). The larger this ratio, the more severe the underestimation of the actual variance using conventional estimation methods. This issue will be aggravated the bigger the group size n and the larger the group-specific variance relative to the total variance (high group correlation

coefficient ρ). In the case of $n = 1$ or $\rho = 0$ conventional methods yield reliable results. If this is not the case, there exist several methods to tackle problems tied to correlated outcomes. Such solutions include the estimation on a group level, a parametric approach using the calculated Moulton factor as well as clustering standard errors on the unit of treatment. There is however no quick fix resolving the issue on a general basis. Clustering standard errors on the level of the treated entity is a very common and the most widely used approach. However, its validity hinges on the availability of a sufficient amount of clusters (Angrist and Pischke 2009). This is not an issue of concern in the case of the Mexican data since there are a total 2431 municipalities in the sample. Hence, I cluster standard errors at this level and account for issues arising due to both heteroskedasticity and correlated observations.

Chapter 7: Results

In this chapter the main findings and regression estimates from the data analysis will be presented. Different outcomes of interest were examined, the main one being the absolute count of domestic violence incidences in each respective municipality in every year of the observation period between 2000 and 2012.

Firstly, the main regression estimates on home violence will be presented. In order to be able to appropriately interpret these results I will subsequently elaborate upon changes in the observed household dynamics using registry data on marriages and divorces. With an improved female outside option as a consequence of SP, one might expect changes when examining female relationship-related decision-making. Testing several hypotheses implying increases in female empowerment due to SP, I find some evidence that this is the case even though many estimates remain insignificant. Thus, the complementary analysis implies that women seem to have become more independent from their male counterparts.

7.1 Domestic violence

i. Main results

This subchapter covers the main results of this paper addressing the impacts of SP on domestic violence. Using the main specification, I examine females aged 20 to 40 years who

have been victim to an assault in their place of residence. To reassure that the identifying assumption of the DD strategy holds, which is the common trend assumption across municipalities in the absence treatment, a time-to-event model was run. The full version included data on up to 7 years prior to as well as after SP and is graphically depicted in figure 6. After controlling for municipality and year FE as well as described trends pre-SP, there are no significant impacts on domestic violence levels before entering SP. The plotted regression line lies flat in the years prior to implementation, with a 95% confidence interval including zero at all times. Figure 6 thus supports the validity of the common trend assumption. Therefore, the time-to-event model substantiates the reliability of the estimation approach, even though with some noise. With the year before SP introduction ($t = -1$) set as a baseline, there is a significant impact of SP on domestic violence observable during the years that follow.

Figure 6: Impact on SP on domestic violence (time-to-event)

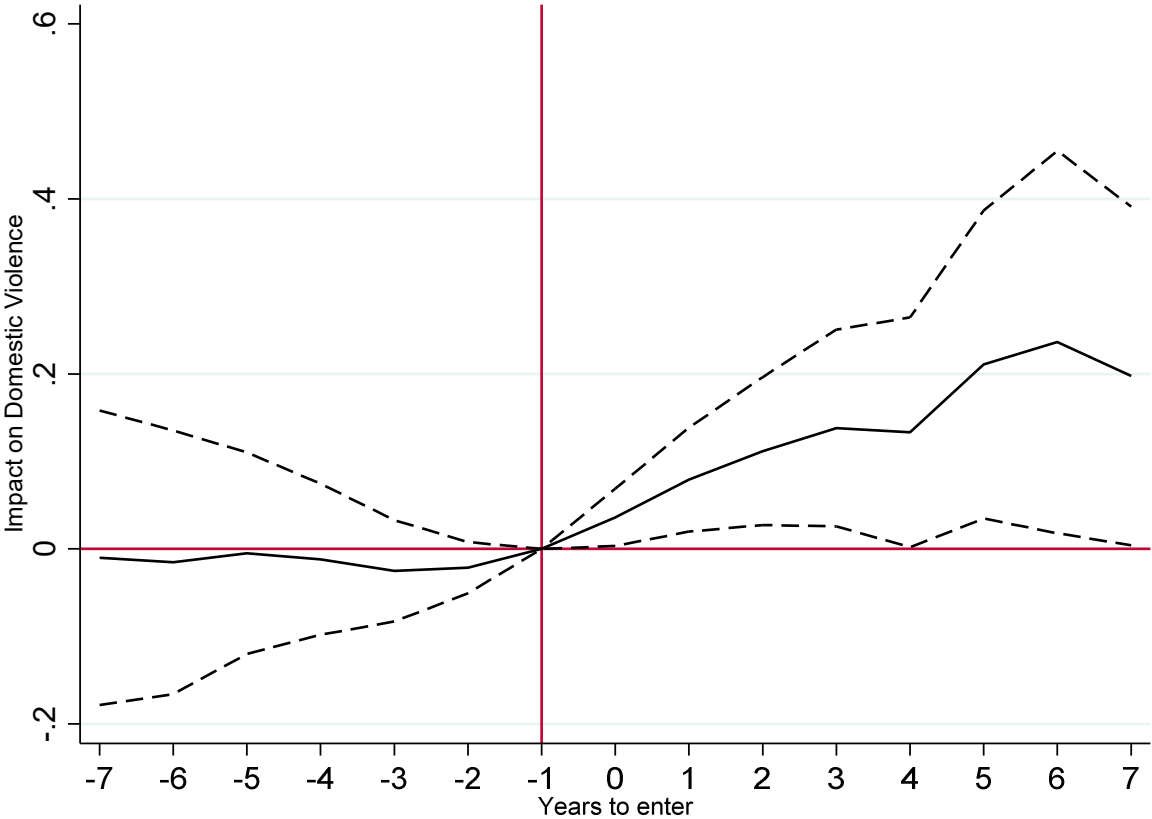


Table 2: Effects of SP on domestic violence (simple DD)

	(1) DV	(2) ln (DV)	(3) DV	(4) ln (DV)
Introduction of SP	0.0281* (0.0134)	0.0102* (0.00505)	0.0432** (0.0149)	0.0161** (0.00503)
Year & Mun. FE	X	X	X	X
Trend Controls	-	-	X	X
Observations (Municipalities)	30709 (2431)	30709 (2431)	30709 (2431)	30709 (2431)

Note: This table displays least square estimates of the impact of SP on home violence. The dependent variable in column (1) and (3) is the total count of domestic violence incidents per municipality per year (2000-2012). Respective regressand for columns (2) and (4) is the identical count measured in logs. Presented estimates follow a DD estimation strategy with all specifications including year and municipality FE. Column (3) and (4) additionally include controls for both an economy-wide trend in the outcome and trends in the municipality-specific population size for residents aged 20-40 years. Standard errors (in parentheses) are clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Simple DD estimates of the impact of SP on domestic violence levels depicted in regression table 2 indicate small, but significant increases. Results are presented both measured in levels and in the natural logarithmic form. As the existence of a common trend in levels rules out parallel trends in logarithmic form, I will focus on the absolute number of home violence incidents as the regressor of interest for the further analysis (Angrist and Pischke 2009). The depicted results in table 2 are in line with a male backlash model that predicts increasing violence levels. According to the model, male aggressive behavior is triggered in an attempt to uphold the male's dominant position within the household as the female faces a more attractive outside option.

The results listed in table 3 depict a compact version of the time-to-event specification, making it feasible to examine the impacts of SP in the shorter and longer run. These results present the principal results of this paper. Three time periods measured relative to the year prior to SP implementation are examined separately. Firstly, the time span pre-SP, covering two and more years before rollout, should ideally not generate any significant impacts. Secondly, one interval capturing the short-term impacts is included (0-2 years after SP) as well as an interval accounting for the impacts in the longer run (3+ years after introduction). Supporting the general implications of the simple DD results, estimates imply increases in violence due to SP using this model specification, too. Long run effects tend to be larger than

short run effects which is intuitive considering that estimates represent ITT effects. As the number of SP affiliates becomes larger over time, treatment intensity increases gradually. This may partially explain the size gap between short and long run effects. When estimates were to be adjusted for the take-up rate among residents, establishing ATT effects, this gap might hence diminish to some extent.

Uncovered effects on domestic violence are driven by rich municipalities.¹⁴ Especially in the short run, effects are solely due to changes in violence in relatively affluent areas. Poor municipalities in Mexico are typically much smaller and more rural than their rich counterparts.

Table 3: Effects of SP on domestic violence (compact time-to-event specification)

	(1) DV All Municipalities	(2) DV Poor Municipalities	(3) DV Rich Municipalities
Up to 2 years before (inclusive) SP (β_1)	0.00190 (0.0134)	-0.0124 (0.00691)	0.00639 (0.0283)
0 to 2 years after SP (β_2)	0.0511*** (0.0136)	0.0153 (0.00876)	0.0797** (0.0270)
3 or more years after SP (β_3)	0.0967*** (0.0258)	0.0362* (0.0154)	0.146* (0.0586)
Year & Mun. FE	X	X	X
Trend Controls	X	X	X
p-value H0: $\beta_2 = \beta_3 = 0$	0.0005	0.0628	0.0128
Observations (Municipalities)	30709 (2431)	15987 (1283)	14722 (1148)

Note: This table displays least square estimates of the short and long run impact of SP on home violence employing a compact time-to-event specification. Effects are estimated for all municipalities in column (1) as well as separately for the poor and rich municipalities in columns (2) and (3) respectively. The outcome is the total count of domestic violence incidents per municipality per year (2000-2012). The presented estimates result from a DD estimation strategy with all specifications including year and municipality FE, controls for both an economy-wide trend and trends in the municipality-specific population size for residents aged 20-40 years to ensure that the common trend assumption is met. Standard errors (in parentheses) are clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

¹⁴ This observation presents an interesting topic for further research. Assuming for instance that informality matters in the context of home violence as briefly discussed in chapter 3, it could be examined whether a noticeably high number of men became self-employed in Mexico's rich municipalities. This would imply that they exited the formal labor market, possibly resulting in more time spent at home.

The average population measured at the baseline (2000) in rich Mexican municipalities is almost five times larger than the mean population in poor municipalities. It appears reasonable to assume rurality to be somewhat correlated with stricter traditions and norms as well as less progressive views related to gender roles than in more urban areas. Violence, too, tends to be especially prevalent in rural areas (World Health Organization 2005). As Canavire-Bacarreza and Rios-Avila (2010) show for the case of indigenous women in Bolivia, a high prevalence of home violence can lead to its occurrence being socially accepted. Women may then accept violence as a given and incorporate its incidence in their views on gender roles. They may then become less responsive to empowering policies.

Beath et al. (2013) argues that measures to empower women usually fail to have desired effect on a social level in highly conservative areas, as no impacts of economically empowering policy can be found on female intra-household decision-making or views on gender roles. These implications from a study set in rural Afghanistan are in line with the findings of Elsayed and Roushdy (2017) discussed in the literature review (chapter 4). Hence, it can be suspected that this reasoning might apply to rural Mexican municipalities as women do not seem to change their behavior when they relatively gain economic independence as a result of SP. I elaborate upon the latter fact in more detail presenting the complementary results in the subsequent sections of this chapter.

The more empowered women become, the more violence will be exerted upon them if the male backlash approach holds true, which results indicate so far. The fact that poor municipalities experience an increase in violence in the long run resonates with the argument made by Elsayed and Roushdy (2017) about the stickiness of gender views from a female perspective. It may take longer to break up stereotypes connected to the prevalent machismo culture in poor and more rural areas than in the rich ones, which may eventually lead to more independent, empowered female behavior. It is also imaginable that the indirect impact of female economic independence on social empowerment established by e.g. Majlesi (2016) needs to be more substantial in order to create a significant effect on female decision-making in a setting such as the one in poor municipalities. Elsayed and Roushdy (2017) as well acknowledge this by addressing that the view on the female gender role is hardly altered by empowering measures within conservative societies.

Another factor to consider is the role of informal employment in poor municipalities. When examining poor municipalities, an initial high degree of informality has to be considered (Aterido, Hallward-Driemeier, and Pagés 2011). Data presented by Dougherty and Escobar (2013) show that states with especially high rates of informality as of 2005 are typically those consisting mainly ($\geq 50\%$) of poor municipalities according to the marginalization index measured in 2000. For poor municipalities, given initial high levels of informality, this reinforces findings of previous research which imply mostly negligible impacts of SP on labor market decisions. On the other hand, this points to households being less frequently covered by health insurance through formal employment prior to SP. Hence, the incentive to stay with a violent husband due to the fact that he provided health care for the whole family would not have been existent for many. SP presented an improvement for the household as a whole, but did not distort the power structure or the relative merit of the female outside option.

In order to evaluate the accuracy of the specified domestic violence indicator and hence the validity of presented findings, I ran the same estimation using the alternative, more narrowly defined domestic violence specification described in chapter 5. Naturally, the total count of violence incidents drops when this specification is used. According to this specification in 2000, the municipality mean of home violence incidents is 0.0096. This value is lower than the corresponding value presented in table 1. Respective regression tables (A1 and A2) can be found in the appendix. The results are overall encouraging and indicate increases of violence due to SP as well. The largest effects are once more detected in the rich municipalities of the sample. Yet, the results of the alternative specification are less reliable due to the fact that there is always one out of three pre-SP coefficients getting a significant coefficient estimate assigned, both when controlling for trends and when desisting from this control. Apart from this issue, the results can act as a reference point in terms of the direction of the true effect.

7.2 Household composition

i. Marriages and divorces

The motivation behind examining marriage and divorce rates in the context of home violence is to verify that the introduction of SP actually led to female empowerment. With SP in place women do not have to rely on either being married to a husband working in the formal sector

or being formally employed themselves in order to gain access to health insurance. Marriage could hence be discouraged by the introduction of SP. This is especially important in the case of Mexico where women, particularly those with low education, tend to be secondary earners at most (Juarez 2008). In practice, this often translates into informal work or unemployment. The same argument applies to possible changes in divorce rates due to SP. With an increasing degree of female independence divorce rates may be expected to increase overall.

In table 4, regression results for the short and long run impacts of SP on the numbers of divorces are presented. The dependent variable is measured in absolute values instead of logarithms as there were small municipalities in the sample with zero divorces in some years during the observation period (2000-2012). I present a compact time-to-event specification, controlling for an economy-wide trend as well as trends in the municipality-specific population size. The age-specific subgroup of interest in connection to the population size is defined as including all residents aged 15-64 years because it is in this age interval the lion's share of both marriages and divorces occur (see figures A1 and A2). Controlling for trends ensures that β_1 is not distinguishable from zero, which in turn indicates that there were no impacts of SP prior to introduction. This adds credibility to the parallel trend assumption being the identification assumption behind the employed estimation strategy.

Whereas column (1) shows the impact on divorces overall, the possibility of heterogeneous effects across rich and poor municipalities is accounted for by presenting estimates for both of these groups separately. The overall estimates imply an increased number of divorces in the long run due to the introduction of SP. Per municipality on average, there are around five additional divorces as a consequence of SP three to maximal seven years after rollout. Plotting the density of the female age distribution upon divorces (figures A1 in the appendix) relative to the timing of introduction of SP illustrates this as the distribution becomes wider spread. These effects in respect to divorces are driven exclusively by the rich municipalities in the sample which corresponds to the principal findings on home violence. In the long run, an average increase of around ten divorces per rich municipality is estimated for the given time frame. As the rich municipalities denote significant increases, also in the short run, there is no increase identifiable for the subsample of poor municipalities. In fact, there even seems to exist a slight, yet significant, decrease in the number of divorces three and more years after SP launch.

Table 4: Effects of SP on divorce rates (compact time-to-event specification)

	(1) Divorces All Municipalities	(2) Divorces Poor Municipalities	(3) Divorces Rich Municipalities
Up to 2 years before (inclusive) SP (β_1)	-0.851 (0.762)	0.101 (0.0986)	-1.810 (1.482)
0 to 2 years after SP (β_2)	1.397 (0.726)	-0.0422 (0.0756)	2.980* (1.362)
3 or more years after SP (β_3)	4.648** (1.584)	-0.324* (0.143)	9.997*** (2.919)
Year & Mun. FE	X	X	X
Trend Controls	X	X	X
p-value H0: $\beta_2 = \beta_3 = 0$	0.0052	0.0179	0.0008
Observations (Municipalities)	31603 (2431)	16679 (1283)	14924 (1148)

Note: This table displays least square estimates of the short and long run impact of SP on divorces employing a compact time-to-event specification. Effects are estimated for all municipalities in column (1) as well as separately for the poor and rich municipalities in columns (2) and (3) respectively. The outcome is the total count of registered divorces per municipality per year (2000-2012). The presented estimates result from a DD estimation strategy with all specifications including year and municipality FE, controls for both an economy-wide trend and trends in the municipality-specific population size for residents aged 15-64 years to ensure that the common trend assumption is met. Standard errors (in parentheses) are clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

A corresponding pattern in terms of effect size can be observed when examining marriage rates. Respective regression results are illustrated in table 5. Coefficient estimates remain insignificant when studying effects for poor and rich municipalities separately. However, the marriage rate decreases significantly both in the short and in the long run when observations from rich and poor areas are pooled. Again, the mere magnitude of the effect is driven by rich municipalities. These results add credibility to the hypothesis that SP had empowering effects on females which led to more independent decision-making behavior. Also, in terms of effect size and heterogeneity analysis, estimated effects support the main assumptions on empowerment underlying the findings on violence.

Table 5: Effects of SP on marriage rates (compact time-to-event specification)

	(1) Marriages All Municipalities	(2) Marriages Poor Municipalities	(3) Marriages Rich Municipalities
Up to 2 years before (inclusive) SP (β_1)	2.050 (2.703)	0.488 (0.990)	-2.811 (5.795)
0 to 2 years after SP (β_2)	-7.560* (3.735)	-0.336 (0.776)	-7.133 (6.760)
3 or more years after SP (β_3)	-13.24* (6.235)	-0.440 (1.574)	-14.72 (11.25)
Year & Mun FE	X	X	X
Prior Trend Control	X	X	X
p-value H0: $\beta_2 = \beta_3 = 0$	0.1035	0.9097	0.4015
Observations (Municipalities)	31603 (2431)	16679 (1283)	14924 (1148)

Note: This table displays least square estimates of the short and long run impact of SP on marriages employing a compact time-to-event specification. Effects are estimated for all municipalities in column (1) as well as separately for the poor and rich municipalities in columns (2) and (3) respectively. The outcome is the total count of registered marriages per municipality per year (2000-2012). The presented estimates result from a DD estimation strategy with all specifications including year and municipality FE, controls for both an economy-wide trend and trends in the municipality-specific population size for residents aged 15-64 years to ensure that the common trend assumption is met. Standard errors (in parentheses) are clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

ii. Couple characteristics

Not only quantitative measures of marriages and dissolutions thereof can be impacted by SP. Qualitatively, the composition of couples may change when female incentives are altered. Using the register data on marriages once more, I consider potential impacts of the introduction of SP on four different measures. Firstly, I examined whether the share of women marrying older men changes. Marrying older men has historically been the norm and reflected traditional gender roles and the corresponding distribution of power within the household (Wheeler and Gunter 1987). For young women this may point to the existence of incentives to marry a provider rather than a romantic partner. Decreases in the number of couples which exhibit this characteristic would thus imply qualitatively more equal couples, possibly also in other respects than age. Compact time-to-event estimates including trend controls presented in column (1) of table 6 show no significant impact on this measure

however.¹⁵ Secondly, the impact on the share of unemployed women who marry a formally employed man are evaluated. As there is a direct monetary incentive for unemployed and hence uninsured women to get married to a man who can act as a provider of health insurance prior to SP, it is reasonable to suspect that this share declines with SP in place. The respective findings in column (2) show that there indeed is an immediate, significant decline in this parameter. Next in column (3), to address the idea of men acting as a provider, I analyze the age gap (measured in years) between partners conditioned on the female partner being younger than the male.

Table 6: Effects of SP on household composition (compact time-to-event specification)

	(1) "Marrying up" in age	(2) Unemployed Female	(3) Age Gap	(4) Education Gap
Up to 2 years before (inclusive) SP (β_1)	0.000592 (0.00305)	0.000412 (0.00305)	-0.0147 (0.0376)	0.00109 (0.00288)
0 to 2 years after SP (β_2)	0.000263 (0.00281)	-0.00897** (0.00319)	0.0495 (0.0350)	0.00116 (0.00265)
3 or more years after SP (β_3)	-0.000528 (0.00418)	-0.00481 (0.00453)	0.0716 (0.0618)	0.00203 (0.00397)
Year & Mun. FE	X	X	X	X
Trend Controls	X	X	X	X
p-value H0: $\beta_2 = \beta_3 = 0$	0.9551	0.0031	0.3662	0.3662
Observations (Municipalities)	31309 (2431)	31309 (2431)	31309 (2431)	31309 (2431)

Note: This table displays least square estimates of the short and long run impact of SP on the composition of newly married couples employing a compact time-to-event specification. Four outcomes of interest are considered: (1) the percentage of women marrying older men, (2) the percentage of unemployed women marrying employed men, (3) the age gap between partners measured in full years, given the female is younger than the male and (4) the education gap, defined as a dummy indicator taking value 1 when the man is the more educated one in the relationship, between male and female. Presented estimates follow a DD estimation strategy with all specifications including year and municipality FE as well as controls for both an economy-wide trend in the outcome and trends in the municipality-specific population size for residents aged 15-64 years to ensure that the common trend assumption is met. Standard errors (in parentheses) are clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

¹⁵ Because coefficient estimates are easy to interpret and since I am primarily interested in marginal effects, not predictions, I employ a linear probability model instead of a nonlinear model for binary outcome variables such as a logistic or probit regression.

Complementing results from column (1), the size of the age gap remains unimpacted by SP when considering all municipalities in the sample. Lastly, it is assessed whether SP had any significant impact on the education gap between husband and wife. I model this factor as a dummy variable taking value 1 if the male is better educated than the female and 0 otherwise. In this case, too, I fail to uncover any significant impacts.

As for the outcomes on marriage and divorce rates, the possibility of differential impacts in poor and rich municipalities is incorporated in the analysis. Tables A3 and A4 in the appendix present corresponding findings estimated separately for the rich and the poor. Just like in the preceding part of the analysis, any significant effects are driven by rich municipalities. Whilst there are no impacts on the quality of the match within poor municipalities, the effects estimated for rich municipalities are more distinct than in the overall estimation. Rich municipalities denote a decline in the share of unemployed women marrying a working man due to SP by over 1 percent point. This significant effect represents a direct impact, observable during the short run after SP was implemented. In addition, one rather surprising result can be found in column (3) of table A4. It shows that the age gap significantly increased both in the short and the long run in rich municipalities. This result is rather counterintuitive and does not fit underlying hypotheses of increasingly empowered female behavior as a consequence of SP. However, the estimated effect is small in magnitude, considering that values are measured in full years. Also, the share of women who marry up in age is not impacted (see coefficient estimated in column (1) of table A4). I am hence inclined to not focus on this finding in more detail.

iii. Divorce initiation and violence as a reason for divorce

Not only the initial composition of the couple upon marriage could be impacted by SP. The reason behind filing for divorce may change and the person initiating the divorce may more frequently be the wife when women become relatively more empowered. Using the registry of divorces between 2000 and 2012, I examine whether the share of divorces set in motion by the female partner increases when SP is launched. This hypothesis reflects the improved outside option women face due to SP. Also, since the tolerable amount of violence decreases in a bargaining power framework when women become more empowered (as shown in figure 1), one could expect to find that more divorces were filed due to reasons of home violence. As

same sex marriage became effectively legal in some parts of the country during the time period examined, only heterosexual couples were included in the sample.

Running the same kind of regression on the two described outcomes as in the previous sections fails to uncover any significant effects. This is of course disappointing in a sense that the contemplated hypotheses remain unconfirmed. However, such insignificant findings were not improbable to begin with. In respect to divorce initiation, a substantial amount of error in the data should be expected since it is not necessarily the spouse who wanted the split that filed for divorce. Also, as divorce is connected to social stigma, there might exist a substantial lag to any SP related impact that the time frame observed may not cover to its full extent. In regards to divorces where intra-household violence was stated as the official reason for the separation, it is highly likely that there exists a substantial dark figure tied to underreporting. The female might not want to risk potential repercussions from a violent ex-husband when stating violence as a decisive factor on the divorce papers. Hence, not discovering any attention-grabbing results does not come as a surprise in this case.

7.3 Self-reported domestic violence

One palpable concern assessing the validity of the findings on domestic violence is whether the discovered effects are solely due to an increased usage of health care connected to SP. It is self-explanatory that people will use health services more extensively when they become more accessible as well as free of charge. One alternative to pinpoint if this confounds the estimation is to examine a suitable control group whose condition is impossibly impacted by SP. Increases in hospital admissions for such a control group would point to effects due to a more frequent health care usage and render previous estimates on domestic violence unreliable. If no impacts are found however, this would strengthen the validity of the primary results of this paper. Yet, considering all the potential ways in which SP could impact people's incentives, this approach is not as straightforward as it may appear at first glance. Taking victims of vehicle accidents as an example, it may seem reasonable to assume these kinds of incidences to be random in nature, all else equal, and uncorrelated with SP. Yet, drivers may become more complacent and behave generally more recklessly when they have insurance in place. This phenomenon is commonly referred to as moral hazard and Einav and Finkelstein (2018) present a more comprehensive account of its role in connection with health

insurance. Hence, assuming that trends concerning the number of vehicle accident victims admitted to hospitals are not being impacted by SP could easily turn out erroneous. The control group-based approach using the available hospital data can therefore not be deemed reliable.

Instead of focusing on a control group, alternative data from a health survey was employed. Using the available information from the extensive survey data which covers three interview rounds from the years 2000, 2006 and 2012, a pooled OLS regression was used to check whether there was an impact of SP on reported incidents of domestic violence within the first year of implementation.¹⁶ More precisely, the outcome of interest is defined as a dummy variable indicating that an interviewee has reported experiences of home violence during the last twelve months. The regressors of interests are firstly an indicator taking value 1 if SP was introduced one year prior to the interview and zero otherwise, as well as an interaction variable, specifying the differential impact of equivalent treatment specifically within rich municipalities. The differential impact for the rich is of interest as the main analysis indicates effects to be driven by exactly these areas. This approach is chosen instead of estimating the model separately for the poor and rich to ensure sufficient sample size. I also included a range of control variables such as i.a. indicators on age and education levels of household members as well as municipality-specific population and labor force related measures. Respective regression results are illustrated in table 7. To guarantee comparability in respect to the main home violence results, I defined the group of domestic violence victims as married women aged 20 to 40 whose answers in the questionnaire implied the incidence of home violence.

Whereas there is no significant impact of SP on home violence for the poor municipalities, there exists a significant differential effect for women residing in rich municipalities. The estimate is in line with previous findings using the hospital data which is promising as it adds to the validity of the primary results of this paper. Uncovered effect in the survey data is yet small. I argue that this is firstly because estimated effects in the main analysis are small and secondly because survey data often suffers from selective reporting. Underreporting may also be the reason for not detecting any significant impacts of SP for the poor. Also, it has to be acknowledged that the significant coefficient estimate for the rich municipalities represents a differential, not a total effect. Overall, these results are still useful and encouraging as they

¹⁶ I assume the heterogeneity of households in the sample to be accounted for by included control variables. In this case, the simple OLS estimator is best linear unbiased estimator (BLUE) and uses data efficiently.

point to relative increases of violence after SP implementation within rich municipalities. This hints that previously estimated effects on home violence are valid, at least in terms of the direction of uncovered effects.

Table 7: Effects of SP on domestic violence reported in survey data

	Violence (Ages 20-40) ¹⁷
Impact of SP in poor municipalities (β) (launch one year prior to interview)	-0.003 (0.004)
Differential impact of SP in rich municipalities (β^{rich}) (launch one year prior to interview)	0.006* (0.003)
Mean Violence Poor (2000)	0.00901 (0.110)
Mean Violence Rich (2000)	0.0122 (0.0945)
p-value H0: $\beta + \beta^{rich} = 0$	0.260
Observations	24,775
R^2	0.004

Note: This table displays ordinary least square estimates of the impact of SP on self-reported home violence. Depicted effects represent impacts for poor municipalities (β) as well as the differential impact within rich municipalities (β^{rich}). The dependent variable is a dummy variable indicating an episode of home violence during the past 12 months when taking value 1. Control variables include measures on poverty, population size, labor market indicators, the supply of health care as well as the education level and age of relevant individuals within households. Mean values refer to the indicator for the occurrence of violence (either value 0 or 1) measured at the baseline (2000). Standard errors (in parentheses) are clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Chapter 8: Sensitivity analysis

8.1 Validity of the Empirical Strategy

i. Common Trend Assumption

The significance and value of the results presented hinge on the validity of the empirical strategy used. To evaluate this as carefully as possible, several issues have to be considered. Firstly, the parallel trend assumption, as one identifying assumption of the DD approach, has

¹⁷ Similar results are found when focusing on women aged 25-40. Associated effects seem therefore not to be driven solely by very young individuals.

to hold in order to be certain that estimates depict more than mere correlations. This implies that there should not exist any time-variant unobserved factors that are correlated with both the timing of SP rollout and the outcome, creating deviating trends. Such differing trends would put the validity of any estimation results in jeopardy. It is however not possible to test this assumption directly, as counterfactual outcomes always remain unobservable to the researcher. Something that can be done in order to add credibility to the assumption is to provide suggestive evidence of its validity by showing parallel trends between groups prior to treatment. As for the main outcome of home violence, I presented a visual indication of this in figure 6. Also, the compact time-to-event specification used to generate the main results implies no overall impacts of treatment for the period prior to the actual introduction of SP when using the described set of controls. Municipalities may thus experience different levels of violence accounted for by the municipality-specific FE, but follow overall similar trends over time. This supports the common trend assumption for the main results of this paper. Additionally, the robustness of this conclusion was assessed by re-scaling the outcome variable prior to SP implementation, subtracting any fixed effects, and running a regression on the treatment indicator. No correlation was found, which, again, is reassuring.

For the complementary results on marriage and divorce rates I show an alternative graphical illustration of common municipality trends in the outcome prior to SP. In figure 7 and 8, I respectively plot the average number of divorces and marriages per municipality for all municipalities implementing SP in the same year over time. Generally speaking, there are only slight overall trends observable, both for divorces and marriages, with plotlines lying relatively flat. In the case of divorces, a slight upward trend, if anything, in the average count is detectible for most municipalities. As municipalities introducing SP relatively late (2008-2010) are few in number and tend to be small, it is not surprising that there is no clear trend observable. In regards to the rates of marriage, there is also a quite constant, potentially slightly decreasing trend.

As the overall trend seems to be similar across municipalities that introduced the program at different points in time, there is one rather distinct outlier when observing the average number of divorces. The very first municipalities to implement SP during its pilot phase in 2002 seem to experience a steeper incline than later introducers. One factor that may matter in this case, is that only two years of data are available for these observation units prior to treatment.

Hence, it is generally hard to extrapolate from this trend, both in terms of refuting or confirming the common trend assumption.

Figure 7: Divorce trends prior to SP

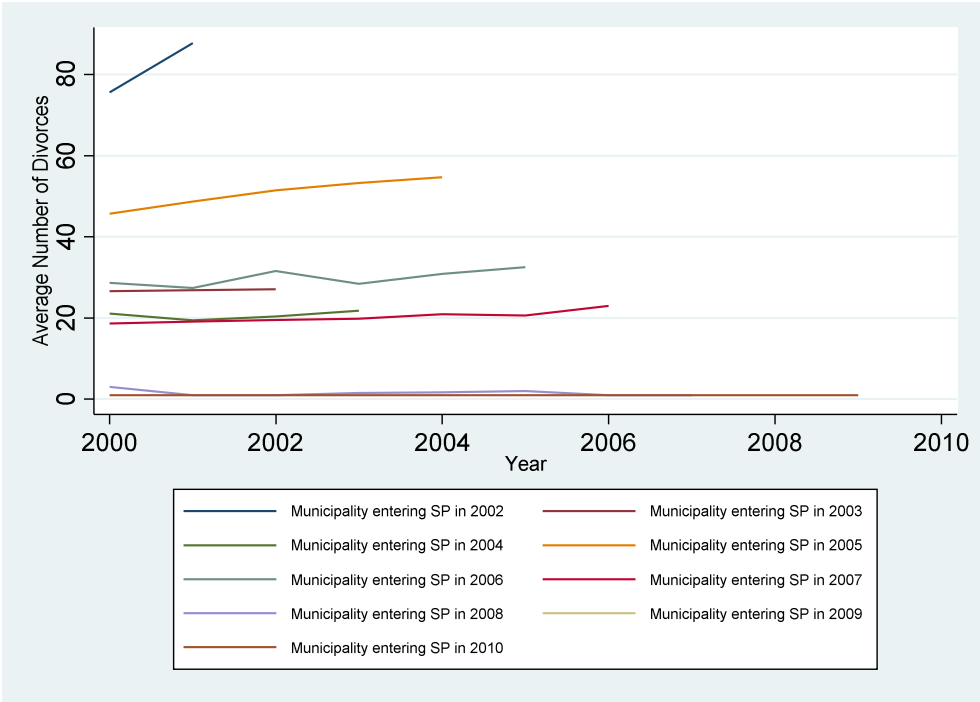
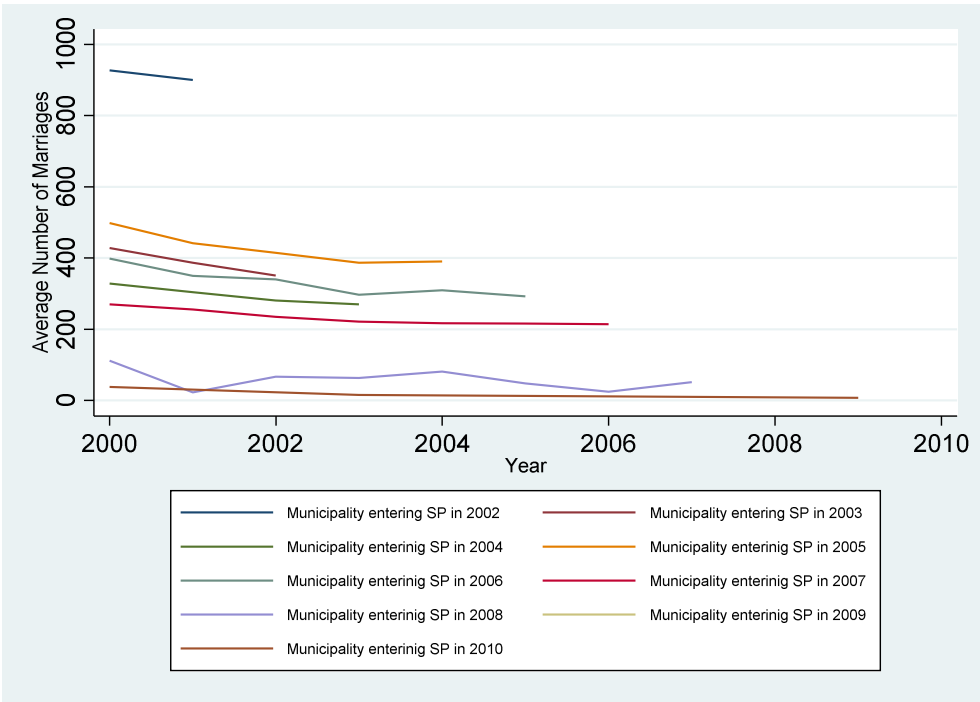
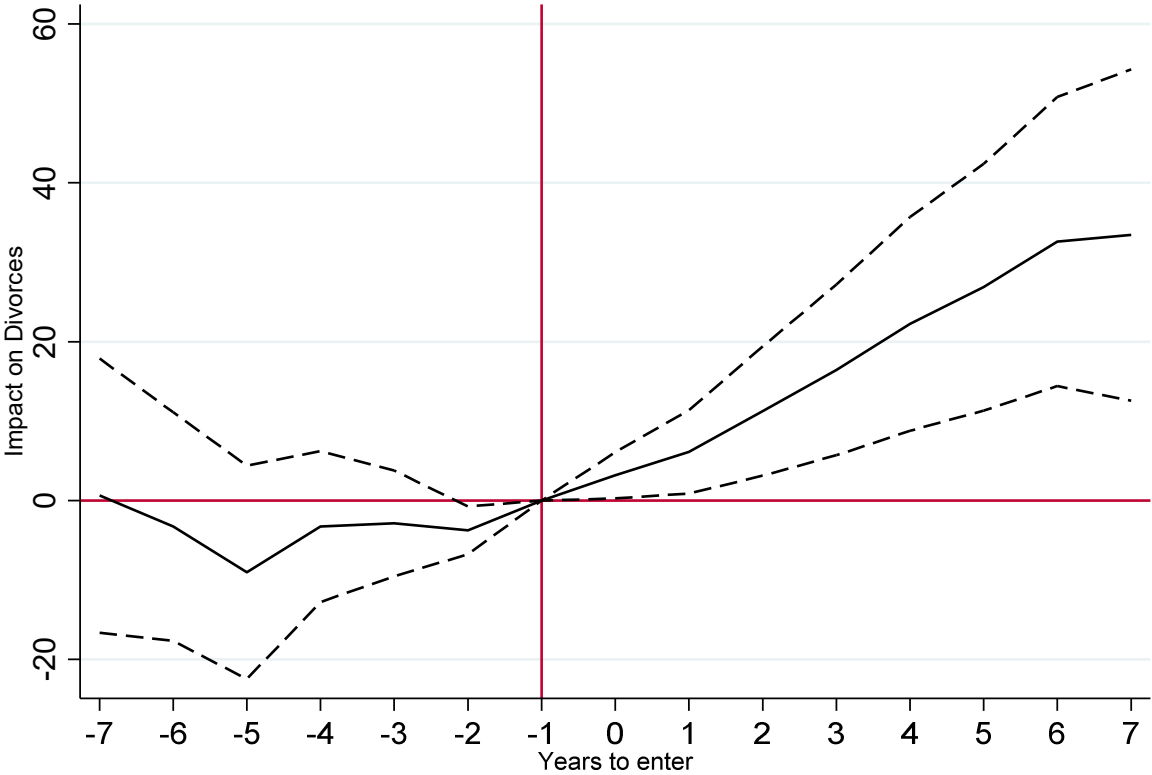


Figure 8: Marriage trends prior to SP



Yet, as the SP rollout on state level was consciously and deliberately decided upon using a set of criteria such as the available hospital infrastructure, potential SP-demand, the financial ability to execute the program efficiently etc., the first municipalities to introduce SP may be systematically different from the remaining municipalities. These areas may thus follow a differential trend in the outcome of interest. One approach to assess if this is the case may be to drop early introducers from the sample and see how estimates change. Yet, this would lead to excluding many rich municipalities which were typically among the early introducing municipalities. As shown, the rich municipalities were presumably the drivers behind uncovered effects. Considering this fact, it seems reasonable to incorporate the heterogeneity across municipalities into the assessment of the common trend assumption. I focus on the results for the rich municipalities in more detail, as it is here, I find the most distinct and significant effects. In figure 9 the flexible time-to-event specification covering all available years of data is plotted specifically for rich municipalities with the impact on the divorce count measured on the Y-axis.

Figure 9: Impact on SP on divorces within rich municipalities (time-to-event)



The plot shows largely no impacts in the time before rollout. The 95% confidence interval contains the value zero in all prior years, except when $t = -2$. Impacts here are however very small and the confidence interval is close to including zero. There are significant, and in the long run also quite large impact of SP on the rates of divorce within rich municipalities, as already discussed presenting regression results. Summing up, the visual evidence as well as the respective time-to-event coefficients fail to refute the parallel trend assumption in respect to the complementary outcomes when the heterogeneity across states is accounted for. This adds credibility to the presented estimation results.

ii. Possible confounders

Any data-based analysis that aims to establish causal effects between some factors of interest will meet challenges. Even randomized control trials, often coined the gold standard approach to empirical questions, can suffer from a variety of biases. Using quasi-experimental estimation designs, where identifying assumptions have to be credibly substantiated, such sources are multiple. When a DD estimation strategy is employed, the parallel trend assumption is not the only crucial underlying assumption that has to hold in order to generate credible results. In the following sections, some so far unmentioned possible confounders which present potential threats to the validity of the main analysis will be touched upon. Some of the listed factors of concern appear more serious than others. In any case, it reminds the reader that any empirical findings have to be considered with a grain of salt.

a. Quality of reporting

The DD estimation strategy builds on the *ceteris paribus* assumption when treatment occurs, as well as during the observed time following. Estimates can only be interpreted as depicting causal impacts when no other kind of policy or shock happens simultaneously, impacting the outcome. In the case of the Mexican data, this implies that the reporting of the outcome has to be consistent and should hence be of the same quality during the whole observation period.

Considering the sources of data for this paper, it is the hospital data that will likely be prone to error. Making mistakes lies in human nature and it is reasonable to assume that ICD-10 codes will in some cases be either misspecified or missing. If error occurs more or less randomly,

misreporting poses no major problem to the analysis per se. If, however, the standards and the quality of reporting admissions due to assault over time is systematically correlated with the timing of SP rollout, the validity of generated estimates may be violated (Conti and Ginja 2017). This is, if fluctuations in reporting quality cannot be accounted for by incorporated fixed effects or trend-related control variables.

Figures 2 and 3 in chapter 5 visualize that the number of recorded incidents related to assault and domestic violence appear so have changed dramatically between 2002 and 2003. In the case where this is due to changes in reporting practice or standards, estimating causal effects of SP may be problematic. The reason behind this is that the first states introduced SP in 2002 and changes in reporting may coincide with treatment for early introducers. Since there exists the same observable increase in both assault and home violence cases, I argue that a possible reporting change is tied to the IDC-10 codes relating to assault and not the other variables used to establish the domestic violence indicator. Considering figure 4, which is depicting the trend in total admission, there appears to be an increasing time trend. This alone seems however not to account for the drastic jump in figure 2 and 3. Again, the examined issue tied to potential increases in the usage of health services overall might play a part in this. Yet, this issue will not be further considered here, as the complimentary analysis using the survey data debilitated this argument to the doable extent. Also, there is only one such explicit leap even though one might suspect several if increased usage is the issue, as the rollout was gradual.¹⁸

If the reporting of assault cases changed systematically in or around 2003, estimated treatment effects for early introducers might contain bias. To suggestively check whether the sharp increase in the number of assault and domestic violence cases in 2003 is accounted for by fixed effects and trend controls, I drop early introducers (2002 and 2003) from the sample and check whether there are any significant impacts of treatment measurable prior to the actual introduction of SP for the remaining municipalities. This approach of course depends on the drastic increase in incidents occurring not only in the municipalities introducing SP in 2002

¹⁸ Such imaginable increases after 2003 could of course be less apparent. Since states introducing SP early were typically more populous, a relatively high total number of residents became eligible to SP when it was introduced as a pilot in 2002. Therefore, the increase in the total number of assault and domestic violence cases between 2002 and 2003 may turn out especially distinct. However, figures 2 and 3 do not even indicate minor corresponding leaps during the remaining time of rollout which could not plausibly be accounted for by the overall increasing time trend in total admissions.

and 2003, but also in the remaining sample, which is the case.¹⁹ The estimated impact size can be expected to change as a large share of the rich municipalities driving the initial effects are excluded. For the trimmed sample, excluding pilot states, there are no significant impacts prior to SP detectable. I examine this separately for rich and poor municipalities. Hence, I conclude that factors leading to the jump in assault-related admissions between 2002 and 2003 (such as e.g. changes in reporting practice) are accounted for by controls incorporated in the model used and pose no major threat to the validity of presented estimation results.

b. Abortion reform

Factors that often cause major issues when implementing a DD estimation strategy are critical events that happen at the same time as the policy to be evaluated taking effect. The *ceteris paribus* assumption is in such case not credible. This poses a repeating problem in empirical research. Especially regarding the data set at hand, where a long time span covering many years is observed, there are a multiple other factors besides SP that may have impacted home violence systematically. One prominent policy change coming into effect in 2007 in Mexico City may represent such a confounding factor to the analysis. Coinciding with SP rollout for some municipalities, a major reform was passed in Mexico City (still referred to as the Federal District of Mexico at that time) legalizing abortions. This pro-choice policy was groundbreaking, as abortion generally used to be illegal and in the whole country prior to 2007. Whereas there were only 62 legally executed abortions during the six years prior to the reform in Mexico City, there were over 90,000 between 2007 and 2012 (Clarke and Mühlrad 2018). Women residing in other states may also legally interrupt their unwanted pregnancies in Mexico City against a fee based on their socioeconomic background. Travelling may however present an obstacle for many of those residing far away from the capital.

As many self-induced and secret abortions have to be anticipated prior to 2007, the then introduced policy gives women the legal right to decide over their own body and future for the first time. This reform thus clearly represents an improvement in regards to female empowerment and may trigger similar intra-household responses as SP. Women become more empowered because they no longer have to risk their own health by inducing abortions

¹⁹ This fact, too, invalidates the assumption of increased health service usage being the main reason behind the increase in assault-related admissions between 2002 and 2003.

illegally without medical assistance. Also, they do no longer risk substantial penalties when deciding to terminate a pregnancy. This in turn implies an improved outside option when in doubt about whether to commit to a possibly violent man and carry his baby. Yet, not all regions of Mexico are as progressive as Mexico City and as a response to the abortion reform a range of states introduced counter-legislations increasing sanctions on illegal abortions (Clarke and Mühlrad 2018). These legislation changes came promptly during the years 2008 and 2009 in 18 of the 31 remaining states besides Mexico City. As these years too fall into the observation period of the data analyzed, both the progressive reform in Mexico City as well as the regressive policies in other states may impact this paper's main results. It remains impossible to clearly distinguish between impacts driven by SP and the abortion-related reforms. In the next section of this chapter I will yet try to suggestively assess the robustness of the main results by excluding Mexico City and neighboring states (as it is women residing in those states who are most likely to consider travelling to Mexico City in order to get an abortion) as well as regressive states respectively.

c. Selective Migration

Another threat to the validity of DD estimates arises when observed groups, control and treatment group, change composition over time. If groups do not remain constant it is once more the *ceteris paribus* assumption that may be violated. In the case of the Mexican hospital data the outcome is measured on a municipality level. It is hence the municipalities that are defined as treated or untreated. Naturally, people will move to other municipalities or other states even due to various reasons, considering the long time span the data is covering. If these relocations are not linked to the availability of SP this does not present a threat to the estimation validity. Yet, if uninsured residents selectively migrate to municipalities which already introduced SP in order to obtain coverage, generated estimates will contain bias as effects may be due to migration rather actual treatment. Whereas trends may have been common across municipalities to begin with, selective migration is non-random and correlated with factors impacting trends in home violence, marriages and divorces. Hence, changes in group composition potentially violate the common trend assumption.

Conti and Ginja (2017) examine this possible confounder using survey data from the 2010 CENSUS. They find no evidence that SP lead to selective cross-municipality migration.

d. Quality of health care

Up until this point, health care services provided by SP have been assumed to be of identical quality as employer-based insurance provided by IMSS, ISSSTE and PEMEX. In other words, SP should present a perfect substitute to the other types of available health insurance. Yet, this assumption may not hold water in practice. Since the public budget is tight in a country like Mexico and due to the high number of potential SP recipients, substantial discrepancies in the quality of treatment can be expected (Malkin 2011). Especially in poorer municipalities exhibiting a less developed health-related infrastructure the delivered quality of services might still be low. José Angel Córdova, serving as Mexico's Secretariat of Health between 2006 and 2012 accordingly admitted that “*there is still first-, second- and third-class medicine*” as late as in 2011 (Malkin 2011). An inferior quality of health services provided by SP could be the reason for discovered effects on the examined outcomes being small in scope, as treatment is less effective than anticipated. This argument will, as discussed, mostly apply to poor municipalities with an especially high share of eligible residents in regards to home violence being the outcome of interest. Recapping uncovered effects, it seems hence possible that rich municipalities were the driver behind uncovered effects partly because SP did not represent a sufficient substitute to employer-based insurance in poor areas. The female outside option will consequently not become desirable enough to trigger changes in decision-making behavior within the household. When treatment occurs with varying intensity, estimates are not directly comparable. It seems therefore reasonable to put more weight on the results for the subsample of rich municipalities which had the capacity to provide health services of a sufficient quality. It has to be noted that not only the actual quality of health services matters in this context, but also the perceived quality as addressed by Finkelstein et al. (2012) evaluating the impacts of the Oregon Health Insurance Experiment.

8.2 Specification Checks

In the following section I present regression tables in which the robustness of the main specification is assessed in several ways. This is done in order to affirm the validity of the effects uncovered in the context of home violence, marriage and divorce.

Firstly, table 8 presents sensitivity analysis results for the main outcome of home violence.

Column 1 depicts results of a simple, compact time-to-event specification only accounting for municipality and year fixed effects. The second column captures the main results discussed in chapter 7. Here, an economy-wide, global time trend as well as a municipality-specific control for trends in the age-specific population size are incorporated into the model. These controls are accounted for in the remainder of the presented alternative specifications in order to assure that the common trend assumption is not violated. Column 3 and 4 address the possibility of the 2007 abortion reform in Mexico City confounding the main results. To examine this issue, I firstly exclude Mexico City as well as the two neighboring states México and Morelos from the sample. The significant effects of SP on home violence in the short and long run remain and estimated coefficients for the full and the trimmed sample are not significantly distinguishable. Next, states that passed abortion-regressive legislature were dropped from the sample in column 4, which reduces the size of the sample from 2341 to 684 municipalities. In this case, the impacts of SP almost double in size and even exceed the estimates from the model run exclusively for rich municipalities. As this may point to the abortion-related reforms having simultaneous impacts which I am not able to precisely disentangle from any effects driven by SP, it also points to the existence of male backlash mechanisms. Since abortion-regressive states effectively diminished female independence, finding the largest impacts in municipalities where no such policy was passed points to increased violence as a reaction to women being more empowered.

In column 5, dummy variables indicating the respective number of years since the introduction of the poverty alleviation program OP were included in the estimation, following the example of Conti and Ginja (2017). This is done in order to ensure that the uncovered effects are actually driven by SP and not the forerunner program. Using this alternative specification leaves estimated impacts largely unchanged however.

Columns 6 to 8 respectively include linear trends for municipality-specific characteristics in regards to labor market, health care and socioeconomic indicators measured at the baseline (2000/2001), again following the approach of Conti and Ginja (2017). Column 9 includes all of the above. The results are reassuring in a sense that the estimated coefficients remain widely unimpacted by the inclusion of the additional controls.

In table 9, the sensitivity of the estimated effects on divorce is tested using similar alternative specifications as in table 8. The results follow the same pattern as the ones on home violence.

It can however be seen in column (1) that not including trend controls leads to significant coefficient estimates pre-SP which implies that the common trend assumption is violated. When including variables controlling for trends in the remainder of the regressions run, these concerns recede. Whereas estimates in columns (3) and (5) to (9) strongly resemble the one in the main specification, impacts increase significantly in size when dropping observations from abortion-regressive states. Divorces in states with no or abortion progressive reforms (Mexico City) increase in the short run on average by 5 in total numbers compared to 1.4 (insignificant) for the whole sample. In the long run, 15 additional divorces are filed due to SP for the trimmed sample versus approximately 5 when all states are included. Again, this points to the underlying assumption of increased female empowerment due to SP being reasonable and that backlash effects are a potential mechanism at work.

The same conclusion applies for the marriage-related results shown in table 10. Yet, as the estimated effects increase in scope in column (4) when abortion-regressive states are excluded, suggesting 15 marriages less short-term and 34 marriages less long-term due to SP in a municipality on average, estimates are imprecise and remain insignificant on the 5% level. This can in part be due to the strong reduction in sample size.

Table 8: Sensitivity (Violence)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Up to 2 years before (inclusive) SP (β_1)	0.0202 (0.0123)	0.00190 (0.0134)	-0.00874 (0.0154)	-0.00321 (0.0342)	-0.000370 (0.0139)	0.00195 (0.0132)	0.00106 (0.0130)	-0.00286 (0.0152)	0.00109 (0.0138)
0 to 2 years after SP (β_2)	0.0436*** (0.0127)	0.0511*** (0.0136)	0.0560*** (0.0151)	0.117*** (0.0317)	0.0525*** (0.0139)	0.0508*** (0.0135)	0.0518*** (0.0135)	0.0529*** (0.0139)	0.0514*** (0.0136)
3 or more years after SP (β_3)	0.0893*** (0.0246)	0.0967*** (0.0258)	0.106*** (0.0287)	0.195** (0.0668)	0.0971*** (0.0265)	0.0962*** (0.0256)	0.0979*** (0.0261)	0.0981*** (0.0263)	0.0970*** (0.0259)
p-value $H_0: \beta_2 = \beta_3 = 0$	0.0010	0.0005	0.0007	0.0010	0.0006	0.0005	0.0004	0.0005	0.0005
Observations (Municipalities)	30709 (2431)	30709 (2431)	28486 (2260)	8864 (684)	30709 (2431)	30709 (2431)	30709 (2431)	30709 (2431)	30709 (2431)
Year & municipality FE	X	X	X	X	X	X	X	X	X
Global time trend		X	X	X	X	X	X	X	X
Municipality population size trend		X	X	X	X	X	X	X	X
Excluding DF and neighboring states									
Excluding abortion regressive states									
OP dummies				X	X				
Trends in municipality labor market indicators						X			X
Trends in municipality health care indicators							X		X
Trends in municipality socioeconomic indicators								X	X

Notes: This table displays least square estimates of the impact of SP. The dependent variable is the total count of domestic violence incidents measured in absolute numbers in each municipality per year. Columns (2) through (9) include controls for an economy-wide time trend, as well as a municipality specific population size trend for the respective female age group examined (20-39 years). Column (3) excludes Mexico DF (now Mexico City) and neighboring states due to a progressive abortion law change in 2007. Column (4) excludes the states that introduced regressive laws as a response in the years following. Columns (5) to (8) include controls for baseline municipality-specific linear trends measured prior to SP rollout. Baseline characteristics include labor market indicators (share of uninsured, share of people working in primary, secondary, tertiary sector) from 2000; health care indicators (number of hospitals, number of health care centers, number of doctors and nurses in hospitals and health care centers respectively, all measures per 100,000 uninsured individual) from 2001; socioeconomic indicators (marginalization index from 2000, for which the trend enters both linearly and squared). Standard errors, clustered on a municipality level, in parentheses.

(* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

Table 9: Sensitivity (Divorce)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Up to 2 years before (inclusive) SP (β_1)	3.573*** (0.960)	-0.851 (0.762)	-1.313 (0.813)	-1.310 (1.982)	-0.772 (0.763)	-1.154 (0.738)	-0.928 (0.760)	-1.255 (0.744)	-0.928 (0.760)
0 to 2 years after SP (β_2)	-0.283 (0.619)	1.397 (0.726)	1.718* (0.712)	5.087* (2.376)	1.546* (0.786)	1.542* (0.711)	1.469* (0.711)	1.573* (0.708)	1.469* (0.711)
3 or more years after SP (β_3)	3.184* (1.333)	4.648** (1.584)	4.698** (1.606)	14.65** (5.280)	4.912** (1.741)	4.598** (1.564)	4.573** (1.572)	4.601** (1.561)	4.573** (1.572)
p-value $H_0: \beta_2 = \beta_3 = 0$	0.0000	0.0052	0.0133	0.0139	0.0067	0.0084	0.0079	0.0089	0.0058
Observations (Municipalities)	31603 (2431)	31603 (2431)	29380 (2260)	8892 (684)	31603 (2431)	31603 (2431)	31603 (2431)	31603 (2431)	31603 (2431)
Year & municipality FE	X	X	X	X	X	X	X	X	X
Global time trend		X	X	X	X	X	X	X	X
Municipality population size trend		X	X	X	X	X	X	X	X
Excluding DF and neighboring states									
Excluding abortion regressive states			X	X					
OP dummies				X					
Trends in municipality labor market indicators					X	X			X
Trends in municipality health care indicators							X		X
Trends in municipality socioeconomic indicators								X	X

Notes: This table displays least square estimates of the impact of SP. The dependent variable is the total count of registered divorces measured in absolute numbers in each municipality per year. Columns (2) through (9) include controls for an economy-wide time trend, as well as a municipality specific population size trend for the respective female age group examined (15-64 years, which is assumed to capture the vast majority of divorcing women). Column (3) excludes Mexico DF (now Mexico City) and neighboring states due to a progressive abortion law change in 2007. Column (4) excludes the states that introduced regressive laws as a response in the years following. Columns (5) to (8) include controls for baseline municipality-specific linear trends measured prior to SP rollout. Baseline characteristics include labor market indicators (share of uninsured, share of people working in primary, secondary, tertiary sector) from 2000; health care indicators (number of hospitals, number of health care centers, number of doctors and nurses in hospitals and health care centers respectively, all measures per 100,000 uninsured individual) from 2001; socioeconomic indicators (marginalization index from 2000, for which the trend enters both linearly and squared). Standard errors, clustered on a municipality level, in parentheses. (* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

Table 10: Sensitivity (Marriage)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Up to 2 years before (inclusive) SP (β_1)	-11.26* (4.423)	2.050 (2.703)	4.330 (2.598)	-1.718 (7.490)	2.599 (2.718)	0.630 (2.912)	1.820 (2.763)	1.810 (2.863)	1.761 (2.804)
0 to 2 years after SP (β_2)	-2.477 (2.873)	-7.560* (3.735)	-7.986* (4.021)	-15.21 (10.31)	-8.339* (3.864)	-7.279* (3.683)	-7.341* (3.705)	-7.521* (3.707)	-7.529* (3.717)
3 or more years after SP (β_3)	-8.788 (4.862)	-13.24* (6.235)	-11.85 (6.704)	-33.79 (18.05)	-14.75* (6.481)	-13.52* (6.249)	-12.18* (6.173)	-13.43* (6.237)	-13.45* (6.267)
p-value $H_0: \beta_2 = \beta_3 = 0$	0.0888	0.1035	0.1312	0.1487	0.0744	0.0965	0.1303	0.0978	0.0992
Observations (Municipalities)	31603 (2431)	31603 (2431)	29380 (2260)	8892 (684)	31603 (2431)	31603 (2431)	31603 (2431)	31603 (2431)	31603 (2431)
Year & municipality FE	X	X	X	X	X	X	X	X	X
Global time trend		X	X	X	X	X	X	X	X
Municipality population size trend		X	X	X	X	X	X	X	X
Excluding DF and neighboring states									
Excluding abortion regressive states									
OP dummies				X	X				
Trends in municipality labor market indicators						X			X
Trends in municipality health care indicators							X		X
Trends in municipality socioeconomic indicators								X	X

Notes: This table displays least square estimates of the impact of SP. The dependent variable is the total count of registered marriages measured in absolute numbers in each municipality per year. Columns (2) through (9) include controls for an economy-wide time trend, as well as a municipality specific population size trend for the respective female age group examined (15-64 years, which is assumed to capture the vast majority of marrying women). Column (3) excludes Mexico DF (now Mexico City) and neighboring states due to a progressive abortion law change in 2007. Column (4) excludes the states that introduced regressive laws as a response in the years following. Columns (5) to (8) include controls for baseline municipality-specific linear trends measured prior to SP rollout. Baseline characteristics include labor market indicators (share of uninsured, share of people working in primary, secondary, tertiary sector) from 2000; health care indicators (number of hospitals, number of health care centers, number of doctors and nurses in hospitals and health care centers respectively, all measures per 100,000 uninsured individual) from 2001; socioeconomic indicators (marginalization index, for which the trend enters both linearly and squared) from 2000. Standard errors, clustered on a municipality level, in parentheses. (* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

Chapter 9: Discussion

The small, yet significant and explicitly increasing effects of SP on home violence have implications on several levels. What is the take away from these results in terms of alternative settings? How should the existence of perverse, unintended policy effects influence the design of future policy aiming to empower women or tackle home violence? And do these findings represent a critique to universal health insurance on a more general level? These are some of the questions I will address in this chapter. However, this is done knowing that the presented findings add to a limited literature and that effects may be local. In order to be able to make statements in regard to the mechanisms behind the occurrence of domestic violence which are valid on a more general level, more comprehensive research is needed.

9.1 Implications

i. Relevance for other countries

The main findings of this paper imply the existence of unwitting effects of well-intended policy. Results point to male backlash effects in Mexico, implying increased male aggressive behavior when females become relatively more empowered, in line with the findings of Angelucci (2008). However, respective results differ in terms of where within the population these effects occur. Whereas Angelucci (2008) discovers backlash effects amongst the poorest in her sample, my findings imply backlash effects in rich Mexican municipalities. One aspect to consider here is that the respective treatment groups differed to begin with. Whilst the analysis by Angelucci (2008) considered those eligible to the poverty alleviation program OP, the sample at hand contains both a larger and more heterogeneous pool of individuals, namely those eligible to SP.

This paper adds to the existing literature insofar as that it indicates the importance of considering perverse backlash effects of empowering policies not only in similar Latin American countries but potentially also in more affluent settings. As effects are most distinct in rich Mexican municipalities, these impacts may translate into high-income settings. In terms of GDP per capita for instance, some of the rich Mexican states with a high share of

rich municipalities (according to the marginalization index measured in 2000) are fairly comparable to wealthy OECD countries. The state with the by far highest average GDP per capita (calculated as the population weighted mean of the GDP per capita of the respective municipalities within the state) is Mexico City with around 18,600 adjusted USD in 2000. In the second place comes Campeche with round 13,200 adjusted USD per person. Both states cover a relatively low number of municipalities in total. Countries with a comparable GDP per capita in 2000 include the Czech Republic or Portugal (OECD 2020). The very richest municipality located in Mexico City even reaches a GDP per capita which is in the range of countries such as Norway or the United States (35,594 USD compared to 36,950 USD in Norway and 36,305 USD in the US). This illustrates that presented findings may have relevance for high income countries as well. Existing research with data from high-income settings generally lacks an element addressing potential heterogeneous effects among different groups within the population (e.g. poor vs. rich, highly educated vs. poorly educated etc.). Of course, monetary factors are not sufficient in order to argue for external validity. It has to be acknowledged that cultural factors elaborated upon in chapter 2 may play a major role in the existence of uncovered effects. However, mentioned arguments call for more comprehensive and detailed research especially set in high-income countries in order to understand underlying mechanisms at play better.

ii. Relevance in terms of health insurance

This paper focusses on a set of indirect impacts of the health care program SP in Mexico. Uncovered effects are small but may be important to consider on a broader level. However, the main takeaway of these findings should be tied to intra-household mechanisms at work when violent behavior occurs rather than a critique of expanding the public health sector per se. As universal health insurance is for the most part perceived as a desirable good within society, it is important to understand that well-intended policies can trigger perverse effects. To e.g. exclusively consider potential health benefits due to universal health insurance misses other effects such policy may trigger as not everything else remains equal in real world scenarios such as the introduction of SP.

Emerging economies focus increasingly on expanding the public health sector, following the call from economists from all over the world (Summers 2015). In its efforts to improve

affordable high-quality access to care for everyone, China quadrupled health care funding between 2008 and 2017 (The Lancet 2019). Many Latin American countries more similar to Mexico followed a similar strategy. Implementing a health care reform (*Seguro Integral de Salud*, hereafter SIS) that has many similarities to SP in 2001, Peru achieved coverage for the most vulnerable groups within its society. In 2011, SIS protected 45% of the Peruvian population against health-related adverse shocks, making it the main provider of health insurance within the country (Bernal, Carpio, and Klein 2017).

One of the most prominent reforms in health care leads us to a special case amongst the Western countries, namely the US. Generally in the US, there exists a fair share of people, often conservatives, criticizing universally designed health care plans (Thaler and Sunstein 2019). The main argument in these cases is in accordance with the principle of free markets and indicates that people should be able to choose freely according to their preferences to reach efficient allocations of resources. This implies that each individual is acting as a *homo economicus*²⁰. The market for health insurance is yet imperfect due to e.g. asymmetric information and adverse selection and it has been empirically shown for the case of Medicare²¹ that the majority of people actually make poor decisions choosing their respective individual health plan themselves. McFadden (2006) provides evidence that almost two thirds of Medicare users did not manage to minimize the expected present value of their out-of-pocket expenses. Also, it has been shown that publicly provided health care (in this case Medicaid) has the potential to improve objective health outcomes such as child mortality (Goodman-Bacon 2018). Nonetheless, the fear of being paternalized by the government and the skepticism of publicly provided health care persists in the US-American society.

The Patient Protection and Affordable Care Act (PPACA, also referred to as “Obamacare”) was initiated by the Obama administration and signed in 2010, aiming to extend coverage to uninsured US citizens with both private and public insurance (National Conference of State Legislatures 2011). Even though the PPACA requires employers to provide insurance to

²⁰ The expression *homo economicus* refers to a fully rational agent who acts in order to maximize utility and is capable of making weighed decisions.

²¹ Medicare is the federal health insurance program first introduced in 1965 in the US, offering coverage for seniors aged 65 years or older as well as younger people with certain disabilities or permanent kidney failure. The scheme consists of several parts addressing hospital and medical insurance as well as prescription drug coverage (U.S. Centers for Medicare & Medicaid Services 2020b). Thaler and Sunstein (2019) discuss Medicare’s Part D (prescription drug coverage scheme) which was enacted in 2003 under the Bush administration. Part D offers a range of different packages to the eligible to choose from, which leaves them with the freedom of choice but also introduces challenges in terms of information and guidance.

employees and individuals to obtain insurance themselves, both with some exceptions, there still exists a significant amount of uninsured US citizens. After a large decrease in the number of uninsured following PPACA, over 30 million people remained without coverage in 2018, which accounts for a little less than one tenth of the total population (Statista 2020). This comparatively high share can at least in part be traced back to described patterns of libertarianism within society. Again, the underlying free market positive argument hinges on perfectly competitive markets. Since this assumption is violated in the case of health insurance provision, unregulated markets may lead to an undersupply, making government intervention justifiable. The case of the US is an interesting one however, as the health care system is dysfunctional and still unaffordable for many. An interesting example for this is the Surgery Center of Oklahoma, a health facility located in Oklahoma City that is cash-based, skipping insurance companies as the middle man between patient and doctor. The Surgery Center was able to cut costs for surgical procedures tremendously (Roberts 2019). As the PPACA is under constant scrutiny, in large part by Republicans, some experts expect the free market health care system to gain popularity nationwide within the US (Sweetland Edwards 2017). Even though the US are a very special case in respect to health insurance, this exemplifies that there exist different approaches to health care provision and room for discussion. Yet, as a free market health care approach may work very well for many, it poses the threat of making the poorest worse off and increasing marginalization. As adverse shocks can be linked to increases in home violence (see e.g. Schneider, Harknett, and McLanahan 2016), UHI ensures that especially disadvantaged people have a system to fall back when an event such as the current crisis surrounding the COVID-19 virus occurs. Therefore, it may be reasonable to assume that the small increasing ITT effects discovered do not weigh heavy in comparison.

iii. Relevance in terms of policies targeting domestic violence

The findings of this paper imply perverse effects on home violence as a consequence of policy which indirectly empowers women. Given a setting with similar external circumstances as in the Mexican sample, such possible effects have to be taken into consideration. As direct measures that aim to tackle home violence mostly exhibit empowering components, policy may not only fail to have the intended effects, but make some victims worse off. Alternative strategies that avoid such risks might be increasing penalties for perpetrators as well as

finding ways to make uncovering domestic violence or reporting of incidents easier.

In order to be able to design optimal policy, the determinants of such backlash effects have to be canvassed in more detail. This craves more comprehensive research in this field.

9.2 Recent developments

As the main results of this paper address a pressing issue and contribute to both existing SP-related literature and economic domestic violence research, the health insurance program itself was not long-lived. Tough cost cutting measures by the in 2018 elected president Andrés Manuel López Obrador (often referred to as AMLO by the media) hit the public sector, including hospitals and health centers (Sheridan 2019). Stating that Mexicans “*can’t have a rich government in a poor country*” AMLO is said to have also cut his own salary by 60% (Busby 2018, Sheridan 2019). Yet, it seems to be exactly these austerity measures that hurt the poor the worst in some respects. The director of the public children’s hospital in Mexico City stated in the spring of 2019 that he was faced with having to let go staff and that the lack of financial means led to the postponing of operations and tests for sick patients, calling the current circumstances inhumane (Sheridan 2019). Already before AMLO’s presidency, some uttered criticism about the implementation of SP. Even though universal health care coverage was promised, the tight budget constraints considering the high number of insured allegedly lead to substantial discrepancies in the quality of treatment (Malkin 2011).

In December 2018, AMLO announced that the failing of SP was evident, “*que ni es seguro ni es popular*” (as it was neither insurance nor for the people) (El Financiero 2018). During the summer of 2019, AMLO’s plans of a new, integrated federal system began to take shape. In January of 2020, the Institute of Health for Welfare (*Instituto de Salud para el Bienestar* – INSABI) was established, gradually replacing SP. Whilst covering everyone not insured via an employer, in contrast to SP, INSABI does not require enrolment or any form of co-payment and promised to offer free access to all medicines (The Yucatan Times 2019). As this sounds auspicious, criticism was voiced by i.a. the former secretary of Health of Mexico and president of the University of Miami, Julio Frenk. Frenk forecasts that the proposed system will not live up to its promise as the available budget is too small, the design is hasty, not well thought-through and the unilateral structure of the system is outdated (Moreno 2020).

Especially the current COVID-19 crisis will put INSABI to a test almost instantly after its introduction.

Chapter 10: Conclusion

The purpose of this paper is to identify possible impacts of the health insurance program *Seguro Popular* (SP) on the occurrence of domestic violence in Mexico. Employing a DD estimation strategy which exploits the quasi-exogenous variation of the SP rollout on a municipality level, I estimate impacts on home violence for both the shorter and longer run. My findings imply small, yet significant ITT effects that point to increases of violence due to the health insurance program. While treatment triggers heterogeneous effects among the poor and rich municipalities, SP is estimated to increase the number of home violence incidents by 0.05 up to two years and by 0.1 after three or more years after introduction per municipality overall. In line with previous evidence from developing and threshold countries this suggests perverse effects of well-intended policy and corresponds with the theory of male backlash. It has to be acknowledged that uncovered impacts are small in size, even when considering the fact that they represent ITT effects. One should thus apply caution when qualitatively interpreting these results, as solely focusing on statistical significance, ignoring effect size and other factors of relevance, will not produce scientifically valuable conclusions (McCloskey and Ziliak 2010).

Addressing the robustness and validity of my findings, I presented evidence suggesting that the common trend assumption cannot be rejected. Also, estimated coefficients are robust to a range of alternative specification. Yet, as I examine a relatively long time-span with potentially many unobserved confounders one has to remain cautious interpreting estimates as causal impacts. Incorporating policy changes in 2007-2009 related to pregnancy interruption illustrates that impacts of different policies implemented simultaneously cannot be disentangled. This is a reoccurring challenge using DD estimation strategies as other factors that impact the outcome, besides the actual treatment, are not necessarily constant. Also, the validity of the presented results hinges on the correct identification of domestic violence victims in the hospital data. Hospital admitted home violence victims represent a subgroup and results may not reflect impacts for the whole population of abused women but local effects due to selection.

The fact that all coefficient estimates point to increases in home violence adds credibility in regards to the direction of the effect of SP. A complementary analysis illustrating how women became more empowered and independent when SP was introduced using divorce and marriage rates underpin the underlying mechanism of SP impacting household dynamics.

The probable existence of undesirable effects of measures empowering women could have implications for the optimal design of policy targeting home violence. To be able to specify this in more detail, the determinants of backlash effects have to be more closely examined. As male backlash impacts have empirically mostly been found in relatively poor, socially conservative countries in the past, I find effects to be driven by rich municipalities. This could point to a certain relevance of the presented findings for higher-income countries.

Nonetheless, more comprehensive research is needed in this respect. Especially for Western countries, research does usually fail to consider potential heterogeneous impacts across the population. To be able to address the growing and pressing issue of home violence against women the cultural and social determinants of male backlash behavior have to become one focus of the debate.

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Appendix

Table A1: Effects of SP on alternatively identified domestic violence incidents (compact time-to-event specification excluding trend controls)

	(1) DV All Municipalities	(2) DV Poor Municipalities	(3) DV Rich Municipalities
Up to 2 years before (inclusive) SP (β_1)	0.0575* (0.0240)	-0.0120 (0.0111)	0.0762 (0.0483)
0 to 2 years after SP (β_2)	0.0832*** (0.0228)	0.0276* (0.0108)	0.114** (0.0423)
3 or more years after SP (β_3)	0.211*** (0.0501)	0.0821*** (0.0213)	0.273** (0.0950)
Year & Mun. FE	X	X	X
p-value H0: $\beta_2 = \beta_3 = 0$	0.0001	0.0006	0.0151
Observations (Municipalities)	30709 (2431)	15987 (1283)	14722 (1148)

Note: This table displays least square estimates of the short and long run impact of SP on the alternative specification of home violence employing a compact time-to-event specification. Effects are estimated for all municipalities in column (1) as well as separately for the poor and rich municipalities in columns (2) and (3) respectively. The dependent variable is the total count of domestic violence incidents per municipality per year (2000-2012). Presented estimates follow a DD estimation strategy with all specifications including year and municipality FE. Standard errors (in parentheses) are clustered at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A2: Effects of SP on alternatively identified domestic violence incidents (compact time-to-event specification including trend controls)

	(1) DV All Municipalities	(2) DV Poor Municipalities	(3) DV Rich Municipalities
Up to 2 years before (inclusive) SP (β_1)	-0.000290 (0.0206)	-0.0299** (0.0108)	0.0162 (0.0416)
0 to 2 years after SP (β_2)	0.107*** (0.0238)	0.0324** (0.0110)	0.141** (0.0442)
3 or more years after SP (β_3)	0.235*** (0.0522)	0.0737*** (0.0211)	0.307** (0.0992)
Year & Mun. FE	X	X	X
Trend Controls	X	X	X
p-value H0: $\beta_2 = \beta_3 = 0$	0.0000	0.0013	0.0053
Observations (Municipalities)	30709 (2431)	15987 (1283)	14722 (1148)

Note: This table displays least square estimates of the short and long run impact of SP on the alternative specification of home violence employing a compact time-to-event specification. Effects are estimated for all municipalities in column (1) as well as separately for the poor and rich municipalities in columns (2) and (3) respectively. The dependent variable is the total count of domestic violence incidents per municipality per year (2000-2012). Presented estimates follow a DD estimation strategy with all specifications including year and municipality FE as well as controls for both an economy-wide trend in the outcome and trends in the municipality-specific population size for residents aged 20-40 years to ensure that the common trend assumption is met. Standard errors (in parentheses) are clustered at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A3: Effects of SP on household composition in poor municipalities (compact time-to-event specification)

	(1) “Marrying up” in age	(2) Unemployed Female	(3) Age Gap	(4) Education Gap
Up to 2 years before (incl.) SP (β_1)	-0.00275 (0.00484)	-0.00704 (0.00444)	0.0436 (0.0585)	0.00109 (0.00464)
0 to 2 years after SP (β_2)	-0.000836 (0.00459)	-0.00508 (0.00447)	0.0225 (0.0577)	-0.00124 (0.00435)
3 or more years after SP (β_3)	0.000940 (0.00686)	-0.00431 (0.00669)	-0.0411 (0.0935)	-0.00262 (0.00650)
Year & Mun. FE	X	X	X	X
Trend Controls	X	X	X	X
p-value H0: $\beta_2 = \beta_3 = 0$	0.9110	0.4860	0.4446	0.9211
Observations (Municipalities)	16464 (1283)	16464 (1283)	16464 (1283)	16464 (1283)

Note: This table displays least square estimates of the short and long run impact of SP on the composition of newly married couples in poor municipalities employing a compact time-to-event specification. Four outcomes of interest are considered: (1) the percentage of women marrying older men, (2) the percentage of unemployed women married employed men, (3) the age gap between partners measured in full years, given the female is younger than the male and (4) the education gap, defined as a dummy indicator taking value 1 when the man is the more educated one in the relationship, between male and female. Presented estimates follow a DD estimation strategy with all specifications including year and municipality FE as well as controls for both an economy-wide trend in the outcome and trends in the municipality-specific population size for residents aged 15-64 years to ensure that the common trend assumption is met. Standard errors (in parentheses) are clustered at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A4: Effects of SP on household composition in rich municipalities (compact time-to-event specification)

	(1) “Marrying up” in age	(2) Unemployed Female	(3) Age Gap	(4) Education Gap
Up to 2 years before (incl.) SP (β_1)	0.00218 (0.00366)	0.00726 (0.00408)	-0.100* (0.0496)	0.00240 (0.00330)
0 to 2 years after SP (β_2)	0.00198 (0.00325)	-0.0111* (0.00467)	0.102* (0.0432)	0.00103 (0.00304)
3 or more years after SP (β_3)	-0.00136 (0.00471)	-0.00287 (0.00651)	0.245** (0.0894)	0.00356 (0.00467)
Year & Mun. FE	X	X	X	X
Trend Controls	X	X	X	X
p-value H0: $\beta_2 = \beta_3 = 0$	0.4394	0.0026	0.0235	0.6985
Observations (Municipalities)	14845 (1148)	14845 (1148)	14845 (1148)	14845 (1148)

Note: This table displays least square estimates of the short and long run impact of SP on the composition of newly married couples in rich municipalities employing a compact time-to-event specification. Four outcomes of interest are considered: (1) the percentage of women marrying older men, (2) the percentage of unemployed women married employed men, (3) the age gap between partners measured in full years, given the female is younger than the male and (4) the education gap, defined as a dummy indicator taking value 1 when the man is the more educated one in the relationship, between male and female. Presented estimates follow a DD estimation strategy with all specifications including year and municipality FE as well as controls for both an economy-wide trend in the outcome and trends in the municipality-specific population size for residents aged 15-64 years to ensure that the common trend assumption is met. Standard errors (in parentheses) are clustered at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure A1: Female age distribution density upon divorce

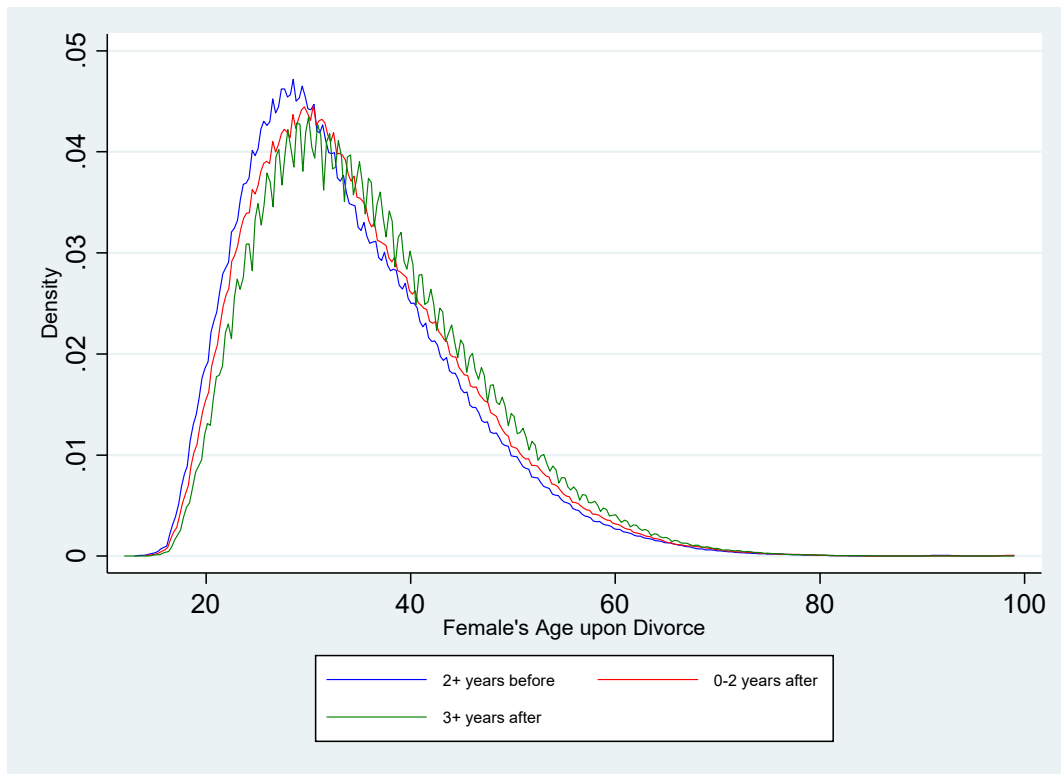


Figure A2: Female age distribution density upon marriage

