THREE ESSAYS ON LAW AND DEVELOPMENT IN MEXICO

A Dissertation

Presented to the Faculty of the Graduate School

of Cornell University

in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy

by

José Roberto Balmori de la Miyar

May 2014

UMI Number: 3583134

THREE ESSAYS ON LAW AND DEVELOPMENT IN MEXICO

José Roberto Balmori de la Miyar, Ph.D.

Cornell University 2014

This dissertation provides empirical evidence on the causal relationship between the rule of law and economic development in both directions, taking Mexico as a case study. The first chapter examines the effect of Oportunidades, Mexico's flagship social program, on reporting violence against women to the police. I use specialized survey data to estimate the average treatment effect of additional reports to the police for women who experienced spousal abuse prior to participating in the program. The identification strategy for this chapter consists of two instrumental variables that are based on institutional characteristics of Oportunidades. Findings indicate an increase of 30.2% in the reporting rates as a consequence of receiving Oportunidades. The causality channels include assimilation of women's rights, increasing trust in the police, and changes in the marriage market.

Large-scale military conflicts oftentimes disrupt economic development. The second chapter studies the case of the Mexican Drug War for treated states, employing synthetic control methods. To prove causality systematically, I use variation on statewide military operations conducted by the Mexican Army, and the rollout of the war. Findings indicate a decrease in GDP per capita equal to 0.5%. Determinants by which the Mexican Drug War hampered economic development include a proportional reduction in consumption per capita, and a decline in productive investment of at least 0.3%, driven by a drop of 3.2% in commercial credit granted to businesses.

The thrid chapter analyzes the effect of drug-related violence on depression among adults in Mexico during Mexican Drug War. The empirical strategy consists of first-differences in aggregate health outcomes at the municipality level before and after the beginning of the conflict. To account

for potential migration biases, I use variation on net cocaine supply from Colombia and on federal-local enforcement cooperation. Results suggest an increase of 1.0% in depression among women, for every additional one-standard deviation expansion in drug-related homicide rates. In stark contrast, Mexican men seem largely unaffected by drug-related violence.

BIOGRAPHICAL SKETCH

José Roberto Balmori de la Miyar received a LLM from the University of Hamburg (Erasmus), a MA from the Warsaw School of Economics (Erasmus), a MS from Cornell University, and a BA from the University of Texas-Pan American. Before coming to Cornell to pursue his doctoral studies, José conducted research at the Center on Budget and Policy Priorities, as an intern. José's research interests focus on the promotion of the rule of law, and on the interaction of crime, development, and gender. His work is mostly empirical, and concentrates on Latin American countries.

A mis papalitos, Ricardo y Ana Irma....and to my grannie, Lida.

ACKNOWLEDGEMENTS

I wish to express my deepest gratitude to Dr. Sharon Tennyson for her immeasurable guidance during my doctoral studies, and for acting as the chair of my committee. I will never forget the many things she did to assist me in accomplishing my academic goals. For sharing her knowledge, energy, and talent with me, and for believing in me, I will always be grateful to Dr. Tennyson.

Special thanks go to my minor committee members, Dr. Ravi Kanbur and Dr. Emily Owens, for encouraging my researching ideas, and for helping me narrow down my broad interest in law and economics. The expertise of Dr. Tennyson, Dr. Kanbur, and Dr. Owens were critical to the development of my doctoral dissertation. I could not have chosen a better group of scholars to guide and enrich my research at Cornell.

For challenging me to pursue a Ph.D., and for introducing me to the world of economic research, I wish to thank Dr. José Pagan, my undergraduate adviser. For all the academic and non-academic help, I would also like to thank Carlos Rodriguez, Neil Belcher, Kevin Sutherland, Ana Hill, Kathy Michelmore, Lauren Jones, Ross Milton, Shady Atallah, the faculty of the Department of Policy Analysis and Management, and my swimming buddies from Teagle's lane five.

Finally, for their love and support, I want to thank my dad, my mom, my American grannie, my grandad, my uncle Fernando Balmori, Carlo Balmori, Nathan Wells, Rita Wells, Joe Wells, Richy Balmori, Amie Evans, Inga Ruhm, Gil Orozco, Alex Salcedo, and my two best friends: Sergio Lois and Eduardo Tovar.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

1 THE EFFECT OF OPORTUNIDADES ON REPORTING VIOLENCE AGAINST WOMEN TO THE POLICE

New national statistics, gathered by the Mexican Women's Institute, point to a hostile environment for women living in Mexico. As of 2006, one in every four Mexican women has, at some point in her marital life, experienced physical or sexual intimate partner violence (IPV), the terminology used in the criminology literature to refer to violence against women at the household level (ENDIREH, 2006).[1] Compared with other countries, Mexico usually lags behind or at best ranks in the middle part of the world on issues related to IPV (UN, 2010).

Worse yet, *Mexican women rarely report IPV to the police, even though IPV is a crime in Mexico*. Only two in ten physically or sexually abused women ever seek help from the Mexican justice system (ENDIREH, 2006).[2] To place this under-reporting situation in perspective, Mexico performs far behind the United States, where "[a]pproximately 60% of family violence victimizations were reported to the police between 1998 and 2002" (Durose, 2005; p.6).

This could be a mere reflection of poor institutional quality in the Mexican police departments (e.g. *Ministerios Públicos*). However, roughly seven in ten women who do report IPV to the police claim having received "good attention and orientation" from them (ENDIREH, 2006). Far from being perfect, the quality of institutional services can hardly be the main reason for underutilization of the justice system by abused women in Mexico.

Furthermore, reporting IPV to the police seems to work well in terms of reducing subsequent IPV. Over 65% of women who do report IPV to the police claim that, after having used the justice system, IPV stopped or diminished (ENDIREH, 2006). Hence, reporting IPV to the police is a good strategy for abused women on average.

So if not institutional quality and subsequent outcomes, what exactly keeps abused women from reporting IPV to the police in Mexico? When asked this question, seven out of ten physically

[1]The victimization rate increases to 50% of the total married or separated women population when including all types of IPV.

[2]Empirical evidence shows that women who only experience emotional and economic IPV do not report IPV to the police (ENDIREH, 2006). See Table 1.1 below.

or sexually abused women identify social norms —shame, family rejection, children's future and personal disregard to women's rights— as the main reason for not reporting IPV to the police (ENDIREH, 2006). Likewise, two in every ten physically or sexually abused women claim fear of possible retaliation by their abusive partners (ENDIREH, 2006).

Broadly speaking, social norms substitute for the rule of law on issues related to IPV in Mexico. A deeply conservative society, in which male chauvinism prevails, pushes Mexican women to lose their basic human rights for all practical purposes. In other words, ostracism costs are too great for Mexican abused women to overcome.

The absence of the rule of law, for IPV issues or any other matter, hampers economic growth (Knack and Keefer, 1995; Barro, 2003). For instance, studies in Latin American countries (e.g. Chile and Nicaragua) estimate an economic cost of male-to-female IPV close to two percent of the national GDP (Morrison and Orlando, 1999). Therefore, establishing the rule of law and incentivizing women to preserve their basic human rights and productivity are of extreme importance, economically speaking.

This chapter examines whether social programs targeted to Mexican women —with the purpose of empowering them and improving their socio-economic status— incentivizes abused women to report IPV to the police. In particular, I examine the case of a conditional cash transfer (CTT) program in Mexico that has been imported by several countries around the world (e.g. Brazil, Peru, USA). The name of this program is *Oportunidades* (e.g. Opportunities), and it is Mexico's flagship social program.

To estimate the effects of Oportunidades on reporting IPV to the police, I use the 2006 wave of the Mexican National Survey of Relationships within the Household (ENDIREH-06). The survey contains a detailed IPV section with dates, frequency, and severity of IPV, as well as information about the *last* IPV report to the police, if any. The data also provides economic and demographic indicators, which lets me identify the population eligible for Oportunidades: the "poor". To do so, I strictly follow the poverty definition used in the program. Most important, ENDIREH-06 contains information about current recipient status for Oportunidades.

However, one complication arises from this exercise: —I expect changes in IPV as a consequence of receiving Oportunidades. My expectations are based on an existing literature that explores the effects of CCTs on IPV. For the case of Oportunidades in Mexico, Angelucci (2008) finds aggressive behavior to diminish by 37% among rural recipients receiving a small transfer, but negative side effects (30% increase in IPV) among rural beneficiaries receiving larger transfers.[3] Similarly, Bobonis, González-Brenes and Castro (2013) estimate a 40% decrease in physical IPV among rural Oportunidades beneficiaries, although a significant increase in violent threats. Last, Bobonis and Castro (2010) find no influence of Oportunidades on IPV among rural beneficiaries in the long run. Other studies in Latin America include Peru, where Perova (2010) finds a reduction in emotional and physical IPV for *"Juntos"* beneficiaries equal to 11% and 9%, respectively.

Whereas "extractive-private information models" match the existing evidence of changes in IPV as a consequence of CCTs in Mexico, "bargaining models" do so for CCTs in Peru. The former set of models predicts changes in the costs of violence threats for extractive purposes (Bloch and Rao, 2002; Bobonis et al., 2013).[4] Conversely, the latter theories suggest shifts in the woman's reservation utility (Tauchen, Dryden Witte and Long, 1991; Farmer and Tiefenthaler, 1996).

Based on the evidence by Angelucci (2008) and Bobonis et al. (2013), I control for potential negative changes in IPV —as in the extractive-private information models— among some beneficiaries. Therefore, I limit my sample to only those women who experienced IPV prior to receiving Oportunidades. Moreover, I exclude all women who identify Oportunidades as a cause for an increase in IPV, based on information from my data.

In all, the research design for this chapter consists in comparing treated "poor", prior-to-treatment abused women with control "poor", abused women. Nonetheless, there are further potential self-selection problems —attrition or take-up of the program— arising from this inferential

[3] Angelucci (2008) uses an evaluation survey with a sample drawn from 506 villages in 7 different states. The questions asked for her data are the following: "[w]ho is (are) the individual(s) who drinks the most in this household, irrespective of the frequency?" and "[w]hile drinking, does this person (referred to the heaviest drinker) have an aggressive behavior?" Rural recipients receiving a small transfer account for 40% of Angelucci's (2008) total sample.

[4] "Extractive-private information models" assume private information on gains to marriage for men and a preference for marriage over separation for women. Both of these assumptions are conducive to men extracting rents from women.

exercise.[5] Hence, my identification strategy suggests two instrumental variables (IV) to solve for omitted variable biases. The first of my instruments exploits drop out variation in rural and semi-rural communities caused by the type of hospital available in any given community (e.g. federal versus state-run) to solve for attrition. The second instrument uses the roll-out in the densification process for urban and semi-urban communities to account for take-up of the program in these areas.

After dealing with all possible biases, findings indicate a positive effect of Oportunidades on reporting IPV to the police. Namely, treated "poor", prior-to-treatment abused women are 4.2% more likely than the equivalent control group to report IPV to the police. In relative terms, this is an increase of 30.2% in the reports of IPV to the police. In order to explore the mechanisms of causality, I propose several channels through which treatment might work into the final outcome: i) assimilation of women's rights (25.8%); ii) an increase of trust in the police (13.8%); and iii) a new equilibrium in the marriage market in which more future dissolutions (27.7%) and fewer reconciliations (18.1%) occur among abused beneficiary women.

The chapter proceeds as follows. Section 1 describes Oportunidades, briefly. Section 2 lays the foundations for the empirical design. In section 3, I propose an identification strategy to deal with self-selection biases. Section 4 presents the results and robustness checks. Section 5 analyzes the channels through which Oportunidades affects reporting IPV to the police. Last, I conclude with immediate policy applications.

1.1 Oportunidades

1.1.1 Background

Ernesto Zedillo's administration initiated Oportunidades, formerly *Progresa,* in August 1997. The program aims at eliminating the cycle of *poverty* through health, nutrition, and education in areas where these human capital investments are lacking. In particular, Oportunidades provides the *woman* of the household a cash transfer for complying with specific requirements. This gender-

[5]ENDIREH-06 is a cross-sectional survey and is thus not able to identify every woman who received the program in the past.

targeting satisfies an additional goal of Oportunidades: women's empowerment.[6] At the same time, it reduces the likelihood of private spending as opposed to household spending because women are generally more altruistic than men in "poor" Mexican households (Benería and Roldán, 1987; SEDESOL, 2000; Wooley, 2004; Bobonis et al., 2013).

The cash transfer contains two separate monetary components —a food grant and a school scholarship.[7] Each of the monetary components has an "independent conditionality requirement: [...] The food grant, which is the same amount for each beneficiary household, is conditional on health check-ups for all family members and on attendance by the recipient at public health lectures [*pláticas*]" (Álvarez, Devoto and Winters, 2008; pp. 643). Every other month, Oportunidades staff gather health check-up and *pláticas* attendance records from healthcare providers. Failure to meet these requirements causes administrators to drop non-compliant household from the program.[8] Unlike food grant, the scholarship amount varies by school grade and gender of eligible children in the household, and is conditional on maintaining an attendance rate greater than or equal to 85%. "If attendance requirements are not met, the amount linked to that particular child is deducted from the bimonthly total payment to the family" (Álvarez et al., 2008; p. 643).[9]

According to Skoufias (2005), beneficiary households receive, on average, about 20% of their income through Oportunidades (Table A.1 provides the 2005 formula for the monetary components).[10] Moreover, since the inception of Oportunidades, its creators have been careful to keep the program apolitical. Hence, cash transfers have been handed out by public financial companies

[6] Only in cases where all adult women in the household are permanently absent can beneficiaries be male. "Male recipients represent less than 10% of the recipient population" (Álvarez et al., 2008).

[7] Oportunidades provides the cash transfer on a bimonthly basis (SEDESOL, 2000). Oportunidades also provides a health component consisting of free preventive health courses, free nutritional supplements for children under 5 and pregnant women, and free medical check-ups (Samano, 2010).

[8] If a household fails to meet health check-ups and *pláticas* conditionality requirements "for 4 consecutive months (2 bimonthly periods) or for 6 non-consecutive months out of any 12 months (3 bimonthly periods out of six)," then the system drops this household out of the program (Álvarez et al., 2008; pp. 643).

[9] "Failure to meet the conditions associated with children's schooling [...] does not result in expulsion from the program but rather in a reduced payment" (Álvarez et al., 2008; pp. 643).

[10] The cash entitlement depends on the demographic characteristics of the family. For example, in 2005, an eligible family with a boy in 3rd grade and a girl in 10th grade received a monthly cash transfer of $930 MXN, which is approximately $80 US dollars. In 2005, the maximum transfer allowed per family was $1775 MXN. Consequently, "[l]arger households potentially receive more money," but only up to the cap set by the rules of Oportunidades (Álvarez et al., 2008; pp. 650). Administrators revise the formula twice a year to determine the amount of aid with respect to changes in the Mexican CPI (inflation).

in rural areas and by the private commercial bank in urban areas (Hevia de la Jara, 2009).

1.1.2 Beneficiary Selection

The process for selecting beneficiaries consisted of three different stages. First, administrators chose a community based on population, a marginality index,[11] and access to health and educational services (SEDESOL, 2000).[12] At first, the government gave priority to the most marginal rural communities —localities with fewer than 2,500 inhabitants— and semi-rural communities —localities with a total population between 2,499 and 15,000 inhabitants, hereinafter rural areas.[13] Five years into the operation of the program, the government also incorporated semi-urban communities —localities with more than 14,999 inhabitants but fewer than 50,000 inhabitants— and urbanized communities —localities with 50,000 inhabitants or more, hereinafter urban areas.

Second, once a locality was eligible, staff members conducted a household survey to gather economic and demographic information, used to construct a discriminant score (*puntaje*). This discriminant score differentiated the "poor" from the "non-poor". For rural areas, the survey was applied to all (100%) households. Thus, the take-up rate in rural areas was around 97% (Angelucci and Attanasio, 2009). In stark contrast, households living urban areas had to sign up at registration offices (*módulos*) during a registration period. Once a household was registered, Oportunidades staff conducted a household survey. Because of this, the take-up rate in urban areas was much lower. About 40% of eligible urban households did not apply for the program (Behrman et al., 2012).

Finally, the third stage of the selection process consisted in incorporating eligible households into the program. At this stage, staff members contacted the woman of the household to give her an identification and inscription forms to take to the healthcare provider and designated school

[11] This marginality index contained the proportion of illiterate population, the proportion of adults working in the agricultural sector, and the proportion of houses without access to water, without a sewage system, without electricity, or with dirt flooring.

[12] This accessibility feature required that a chosen community had a school and clinic either in the locality or through an accessible road within a radius of 10 km for federal roads, 6 km for state roads, or 2 km for unpaved roads (SEDESOL, 1999).

[13] Administrators implemented the program in those communities with scores 4 and 5 from a scale of 1 to 5, from less marginal to more marginal.

(SEDESOL, 1999). In December 2004, Oportunidades reached its original goal of expanding the program to every state in Mexico and benefiting 5 million vulnerable Mexican families (Samano, 2010).

1.2 Empirical Design

The main purpose of this chapter is to estimate the average treatment effect (ATE) of Oportunidades on reporting IPV to the police. Both, the treatment and outcome variables are binary. Therefore, the central *probit* model I estimate is the following:

$$P\left(Report\right)_i = \alpha + \beta T_i + \theta X_i + \varepsilon_i, \tag{1.1}$$

where *Report* takes the value 1 if abused woman i reports IPV to the police; T is equal to 1 if abused woman i is currently receiving Oportunidades; X represents a subset of observable controls; and ε indicates other unobservables that influence the outcome. The parameter of interest —the ATE of Oportunidades on reporting IPV to the police— is β. In theory, equation (1.1) provides the true value of β as long as T is uncorrelated with ε.

1.2.1 Data

I calculate the effect of Oportunidades on reporting IPV to the police using the 2006 wave of the Mexican National Survey of Relationships within the Household (ENDIREH-06). The Mexican Institute for Women conducted ENDIREH-06 between October and November 2006. Respondents were 113,561 women older than 15 years of age.[14]

The survey gathers economic and demographic characteristics of the chosen woman, her (ex) spouse, and other household members. ENDIREH-06 also contains a section about IPV with detailed information on dates, frequency, and severity of IPV.[15] Table A.2 reports the questions

[14] A special feature about ENDIREH-06 is that all interviewers were women who conducted the survey while the spouse, if any, was away. ENDIREH-06 surveyed only one eligible woman per household. What is more, ENDIREH-06 classified the surveyed women into three groups, i) single, ii) divorced or separated, and iii) married or cohabiting.

[15] For obvious reasons, the IPV section is only available for women who are either married, cohabiting, or separated.

included in the survey's IPV section. I assign the standard IPV classification from the sociology literature —physical, emotional, sexual, or patrimonial IPV— to each of the questions in the IPV section (Coker et al., 2000; Ellsberg et al., 2001).

Moreover, the survey indicates whether an abused woman has ever reported IPV to the police as well as the year and month of the *last* report. Specifically, the question asks: "After experiencing aggressions from your [ex] husband, have you gone to ... 1) the police, 2) the public ministry, 3) other authority, 4) none of the above?" I consider any of the first three options to be positive outcomes (e.g. *Report* takes the value 1).[16]

Most important, ENDIREH-06 provides information about *current* recipient status for Oportunidades and the number of years that the respondent has been receiving the program, if currently enrolled. Thus, participation is self-reported and not pre-determined by the survey's planners. Still, the correlation between the proportion of self-reported beneficiaries in ENDIREH-06 and the official Oportunidades data at the municipality level is 0.905, suggesting an accurate representation of the actual set of beneficiaries.

In addition to ENDIREH-06, I use the 2004 United Nations Mexican Municipalities Human Development Index (MxIDH) to control for local characteristics. The MxIDH contains two main development indexes: the Human Development Index (IDH) and the Gender Empowerment Index (IPG). Specifically, IDH gathers information on life expectancy, education, and per capita income. On the other hand, IPG summarizes three different gender-gap components: political participation empowerment, economic participation empowerment, and economic resources empowerment.[17]

Therefore, the sample excludes all single women, leaving a potential sample of 95,615 women.

[16]This is because, in the Mexican justice system, all three authorities belong to the same structure and, thus, should lead to the same outcome.

[17]MxIDH defines political participation as the proportion of women occupying a parliamentary position; economic participation as the percentage of women who are parliamentarians, appointed political bureaucrats, or executive directors in a company; and economic resources empowerment as the income earned by women in comparison to men.

1.2.2 Constructing a Control Group

To build a comparable control group, I complement the data above with external sources to produce the discriminant score (*puntaje*) used in the second stage of the beneficiary-selection process (see section 1.2). This allows me to identify eligible women who are not current recipients of Oportunidades and who could thus serve as good counter-factuals. I do so following the same poverty definition and information used by administrators of Oportunidades, based on the following variables (Hernández Franco, Corona, and Baez, 2008):

1. *House characteristics*: located in a rural community, dirt floor, drainage, availability of toilet but without running water, overcrowding index, and ownership of a gas stove, a refrigerator, a washer machine, and a motorized vehicle.

2. *Household characteristics*: Gender, age, and education of the head of the household; dependency index; access to employer's health insurance; and number of children under the age of 12.

3. *Regional characteristics*: 14 different regions based on household spending and administrative data from Oportunidades.

However, the weights assigned to each of these variables are confidential to avoid any misuses of the program. A way around this secrecy is to estimate the weights using the household surveys conducted during the selection process: ENCASEH and ENCALURB. Public access to both of these surveys, which contain all the above variables and the discriminant scores, allows me to construct the respective variables' weights. As expected, the linear model including the variables above is extremely significant (e.g. F-statistic above 100,000) and explains 99% of the variation on *puntaje* for both surveys.

Most important, ENDIREH-06 provides information about all the variables used to estimate the discriminant score.[18] Therefore, I am able to predict the *puntaje* for my sample by interacting

[18] With the exception of the ownership of a gas stove, which I proxy using access to gas or piped water in a house. The logic for the latter proxy is that houses with access to water are more likely to have gas delivered.

the information provided in ENDIREH-06 with the estimated weights obtained from ENCASEH and ENCALURB. As a caveat, *puntaje* and severity of poverty move in the same direction.

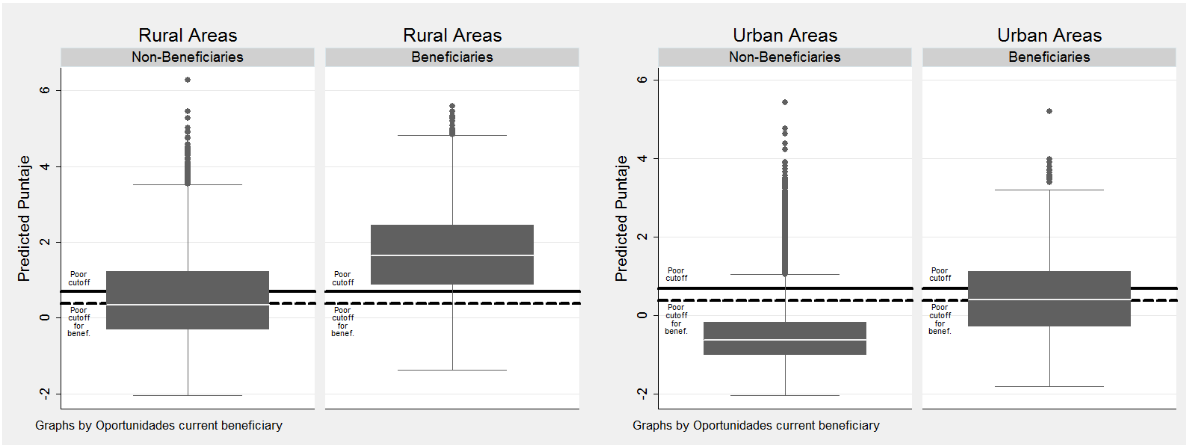

Figure 1.1: Box Plot: Discriminant Score (*Puntaje*)

Figure 1.1 presents the estimated *puntaje* for ENDIREH-06. The thicker, solid line in both panels of Figure 1.1 represents the discriminant score (0.69) between the "poor" and the "non-poor" at the time of selection. Any household at or above the threshold is "poor" and thus eligible to receive Oportunidades. Likewise, the thicker, dashed line shows the discriminant score (0.383) between the "poor" and the "non-poor" for households already in the roster, which is the threshold for transitioning into graduation from the program.

The left panel in Figure 1.1 contains the *puntaje* for women living in rural areas. Clearly, beneficiaries have a higher *puntaje* than non-beneficiaries (e.g. beneficiaries are "poorer"). Moreover, the data show very little economic mobility among rural beneficiaries. Only 11% of current rural recipient women do not meet the poverty discriminant score for beneficiaries (0.383), making them technically non-eligible for the program in the next re-evaluation. This distribution matches the findings of Campos Vazquez, Chiapa, and Santillán (2012), who report that 90% of rural families on the roster continue to be eligible for all components of Oportunidades after the first two revaluations conducted every two to three years. Conversely, about 30% of rural non-beneficiary women are eligible to enter Oportunidades (errors of exclusion), and close to 50% of rural non-beneficiary women meet the poverty discriminant score for beneficiaries (*puntaje* higher than 0.383).

Similarly, the right panel in Figure 1.1 depicts the estimated *puntaje* distribution for women living in urban areas. The results also show a pronounced separation between the "poor" and the "non-poor". However, this panel shows a faster economic mobility among urban beneficiary women. That degree of mobility is exactly what Campos Vazquez et al. (2012) find: 28% of the urban recipients have lower *puntaje* during each of the re-certification processes and only 59% of all urban beneficiaries continue to be "poor" after the first two revaluations. This is a slightly higher proportion than that depicted in the right panel of Figure 1.1, which is approximately 52%.[19]

1.2.3 Sample

First and foremost, the sample includes only those women who are eligible or marginally ineligible for the program. Specifically, I use the transition cutoff score (0.383) to distinguish between "poor" and "non-poor" women (dashed line in Figure 1.1). Therefore, I estimate the effects of Oportunidades on reporting IPV, conditional on being "poor". This restriction reduces the sample to those who participate the most. Also, this condition minimizes the distance in the differences on observables and, hopefully, on unobservables. Even though the sample includes some marginally ineligible non-beneficiary women, this threshold also keeps more current beneficiaries in the sample than if I were to use the selection cutoff (0.69), because of economic mobility among beneficiaries.[20]

Second, I only keep those women who claim having ever experienced physical or sexual IPV. This restriction follows extensive evidence showing that only these types of IPV are ever reported. Table 1.1 presents the rates of ever experiencing IPV and the rates of reporting IPV to the police (conditional on ever experiencing IPV) by type and area, for the whole sample (first column) and for the "poor" sample, separately. The last row in Table 1.1 shows that women who only experience emotional and economic IPV do not report it to the police, irrespective of poverty

[19]The focalization on the selection process of beneficiaries results in having some errors of exclusion for rural households and some errors of inclusion for urban households. Mainly, this is because much of the poor population in Mexico lives in rural areas as opposed to urban areas.

[20]This lower threshold allows me to accommodate some measurement error in the estimated *puntaje*. In addition, I use the predicted *puntaje* together with the standard errors to further account for potential mispredictions. Standard errors are, on average, only 0.04 points.

conditions. Based on the "severity" of IPV, this is exactly what the criminology literature would predict (Black, 1976; Felson et al, 2002; Thompson and Kingree, 2006). What is more, Table 1.1 also shows higher rates of physical or sexual IPV among "poor" women, particularly for those living in urban areas. Hence, this restriction maintains a relevant sample.

Table 1.1: IPV Rates and Rates of Reporting IPV to the Police

		*Conditional on being "poor"**					
	All "poor" &	All "poor"		Rural "poor"		Urban "poor"	
	"non-poor"*	Non-B	Benef	Non-B	Benef	Non-B	Benef
Never-IPV	0.51	0.48	0.54	0.54	0.56	0.39	0.39
Ever Emotional (Low) IPV	0.4	0.42	0.36	0.37	0.35	0.52	0.51
Ever Emotional (High) IPV	0.21	0.24	0.21	0.2	0.2	0.3	0.33
Ever Economic IPV	0.31	0.33	0.27	0.28	0.25	0.42	0.43
Ever Sexual IPV	0.1	0.11	0.11	0.1	0.11	0.13	0.15
Ever Physical IPV	0.24	0.28	0.26	0.24	0.24	0.33	0.37
Ever Phys or Sex IPV	**0.26**	**0.3**	**0.28**	**0.26**	**0.27**	**0.36**	**0.4**
Conditional on Ever IPV:							
Report | Ever Phys or Sex IPV	**0.23**	**0.22**	**0.22**	**0.2**	**0.2**	**0.24**	**0.32**
Report | Never Phys and Sex IPV	0	0	0	0	0	0	0

Notes: Means are weighted by inverse survey sampling weights. * All married and separated women.

Third, following Bobonis et al. (2013), I take into consideration transformations "in marital matching and sorting patterns due to [...] expected changes in household resources and intra-household dynamics". Said transformations in the marriage market only affect younger women who were beneficiaries of Oportunidades during their childhood. Therefore, Bobonis et al. (2013) suggest restricting the sample to women more than 28 years old and who began cohabiting in 1997 or earlier.[21] What is more, to account for changes in mortality rates and further omitted variable biases, I only include women less than 60 years old.

Fourth, I restrict the outcome to reports happening after 2001, for two reasons. On the one hand, the majority of beneficiary women (81%) report that they began receiving the program in 2001 or after. On the other hand, the exact date of reports to the police becomes dubious or missing if the time elapsed is too long.

[21] Oportunidades began in 1997 and the survey's base year is 2006. Hence, these women were 18 years old in 1997.

Finally, I consider possible direct effects of Oportunidades on physical and sexual IPV. Namely, I exclude all women who begin experiencing physical or sexual IPV after receiving Oportunidades. Furthermore, I control for changes in the frequency and severity of IPV, based on information provided in ENIDREH-06, by dropping all women who claim that "IPV aggravated as a consequence of [...] receiving Oportunidades". In total, 13.2% of the remaining beneficiary women are excluded from the sample because of these two last restrictions.

A concern about these last two restrictions is possible changes in the "real" population of beneficiaries, which could lead to a different probability of "ever" experiencing physical or sexual IPV. To test this possibility, I test for changes in the population characteristics. Moreover, I predict the probability of ever experiencing physical or sexual IPV within municipalities, before (excluding women who began experiencing sexual or physical IPV after receiving Oportunidades) and after treatment (including women who began experiencing sexual or physical IPV after receiving Oportunidades). Following Bobonis et al. (2013) and Bobonis and Castro (2010), the prediction model includes demographic variables, economic variables, a dummy for mother's IPV during childhood, as well as a host of polynomials and interaction terms.[22]

Table A.3 shows the result of changes in the beneficiary population. Apart from a significant difference in years cohabiting (0.19), there is no real change in the beneficiary population. Likewise, Table A.4 present the findings of a difference-in-difference regression for the probability of experiencing physical or sexual IPV on Oportunidades. The last column in Table A.3 shows a statistically significant increase in the probability of physical or sexual IPV, on average. However, the overall magnitude is very small in absolute and relative terms. Furthermore, when I break the sample down into urban and rural areas, the probability approaches zero. Therefore, both sets of results show that the sample composition continues to represent the "true" beneficiary population.

All together, *I estimate the effects of Oportunidades on reporting IPV conditional on being a "poor" woman, physically or sexually abused, older than 28 but younger than 60 years of age, reporting IPV after 2001, and experiencing IPV with a similar intensity prior to receiving Oportu-*

[22] I predict the probability of physical or sexual IPV for the sample of "poor" women (e.g. *puntaje* higher than 0.383) older than 28 but younger than 60 years of age.

Table 1.2: Summary Statistics: Beneficiary and Non-beneficiary Women in the Sample (t-stats)

			Rural Areas			Urban Areas		
Group	Name of Variable	All	Non-Ben.	Benef.	Diff.	Non-Ben.	Benef.	Diff.
Dependent	*Report IPV to police*	*0.14*	*0.12*	*0.12*	*0*	*0.15*	*0.26*	*0.10***
Locality	Community>14999	0.37	0	0	0	1	1	0
	Mun. Empower.	0.51	0.5	0.48	-0.01	0.54	0.54	-0
	Mun. Develop.	0.74	0.73	0.7	-0.03***	0.8	0.78	-0.02***
Demogr.	Indigenous woman	0.16	0.11	0.26	0.15***	0.07	0.17	0.10***
	Indigenous men	0.16	0.11	0.25	0.15***	0.1	0.17	0.07
	Age	39.53	40.16	41.36	1.19**	37.04	37.97	0.93
	No schooling	0.2	0.23	0.23	0	0.14	0.21	0.07
	Primary incomplete	0.31	0.3	0.43	0.13***	0.19	0.25	0.06
	Primary complete	0.23	0.22	0.19	-0.03	0.28	0.26	-0.02
	Secondary	0.2	0.18	0.14	-0.04*	0.29	0.24	-0.04
	>Secondary	0.06	0.07	0.01	-0.06***	0.1	0.03	-0.07***
	Children<11y.	0.7	0.64	0.66	0.03	0.79	0.8	0
	Family size	4.6	4.33	5.3	0.97***	3.88	4.89	1.01***
	Divorced	0.11	0.11	0.06	-0.04**	0.18	0.1	-0.07***
	Free-Union	0.29	0.26	0.25	-0.01	0.35	0.37	0.03
	Years cohabiting	18.75	19	21.79	2.79***	14.97	17.77	2.81***
Economic	Predicted *puntaje*	1.47	1.43	1.91	0.48***	0.99	1.4	0.41***
	Asset index	-1.02	-0.98	-1.31	-0.33***	-0.68	-1.03	-0.35***
IPV	Family harmed	0	0	0	0	0.01	0.01	-0
	Seriously harmed	0.15	0.16	0.14	-0.02	0.17	0.15	-0.01
	Harmed	0.41	0.38	0.4	0.02	0.44	0.41	-0.03
	Hospitalized	0.2	0.22	0.19	-0.03	0.19	0.22	0.03
	IPV Frequency	0.24	0.25	0.23	-0.02	0.23	0.23	0
	Childhood IPV	0.56	0.57	0.57	0.01	0.55	0.58	0.03
	Sample Proportion	1	0.36	0.27	-	0.08	0.29	-

Notes: Means are weighted by inverse survey sampling weights. Difference in means clustered at the municipality level. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

nidades. These restrictions result in a sample size equal to 3,444 women.

Table 1.2 reports the means of the covariates for the selected sample, broken down by area (urban and rural) and treatment group. The vast majority of beneficiaries live in rural areas (83%),[23] even though a good proportion of abused, "poor" women live in urban areas (37%). According to Table 1.2, a simple difference in the outcome means between beneficiaries and non-beneficiaries

[23]This is the same proportion than as in Oportunidades administrative records.

of Oportunidades is likely to be biased. Thus, I control for difference in observables across groups. Last, these *summary statistics, along with difference in the selection process of Oportunidades, suggest providing estimations by community size (e.g. rural versus urban areas) for greater accuracy.*

1.3 Identification Strategy

Even after controlling for differences in observables, there is still a high chance of omitted variable bias (OVB) due to self-selection into Oportunidades. Namely, there are two main cases in which the assignment of treatment might be co-determined with the outcome of interest: attrition and take-up of the program. The former bias happens more frequently in rural areas, where Oportunidades has been operating the longest. The latter bias, in contrast, only occurs in urban areas, where women had to register for an eligibility evaluation (see Section 1.2).

In the case of attrition, women may self-select out of the program, voluntarily, by not complying with the conditionality requirements (undergoing health check-ups, filling out compliance forms, picking up cash or a check). What is more, other involuntary mechanisms like transition into graduation of the program, mistakes in filling compliance forms from the health personnel, or failure to deliver beneficiaries' identification forms can affect the permanence of a household on the roster (Álvarez et al., 2008; p. 644).[24] However, here, I am particularly concerned about voluntary drop-out situations in which abused women, who are undergoing through the process of reporting IPV to the police, are not able to comply with the conditionality requirements because of overwhelming health and legal problems.

According to a survival analysis conducted by Álvarez et al. (2008), about 3% of rural households drop out of Oportunidades every year, voluntarily (excluding graduation attrition).[25] González-

[24]Dropout rates peak at certain periods. This spikes are associated with administrative problems such as operational difficulties in launching Oportunidades at the beginning of 1998, changes in the operational guidelines in 1999, and the introduction of the "just-in-time" system in 2001 (Álvarez et al., 2008).

[25]Wealthier household among the "poor", living in marginal communities, were more likely to drop out of the Oportunidades. However, in less marginal communities, the poorest households were also as likely to drop out of the program as the wealthier among the "poor". Similarly, male recipients, older recipients, less educated beneficiaries, more-dependent families, indigenous Mexicans, and single-headed households had higher attrition rates (Álvarez et

Flores, Heracleous and Winters, (2012) expand this survival analysis to household living in urban areas, where voluntary drop-out rates are 5.6% per year (excluding graduation attrition).[26] Nonetheless, Oportunidades has been operating in rural areas much longer than it has in urban areas. Hence, provided that I control for graduation attrition in my sample selection, total drop-out rates are likely to be lower in urban areas than in rural areas.

Just as attrition does, take-up of the program in urban areas might bias the ATE estimations in equation (1.1). For instance, eligible women who are more likely to seek alternatives outsides marriage or to gain bargaining power within the same relationship are also the ones signing-up for the program in urban areas. Put differently, treated urban women would use the justice system at a higher rate, no matter what.

If take-up biases turn out to be true, then the estimations are biased upwards: $Cov(T_i, \varepsilon_i) > 0$. On the other hand, if the drop-out scenario persists, then the estimations are downward biased: $Cov(T_i, \varepsilon_i) < 0$. These self-selection biases, however, are not mutually exclusive. Hence, both kinds of biases can co-exist, at least for urban beneficiaries.

In the presence of OVB, the assumption of exogeneity on the assignment of treatment for equation (1.1) no longer holds. Therefore, I recur to two exogenous variables —the ratio of *IMSS-Oportunidades* hospitals to total Oportunidades healthcare providers, and the roll-out in the densification process of Oportunidades— to estimate the assignment of treatment (T):

$$P(T_i) = \zeta + \gamma Z_m + \phi X_i + \nu_i, \tag{1.2}$$

where Z is the set of excluded instrumental variables (IVs) at the municipality level (m), X is the same matrix of individual observable controls than as in equation (1.1), and ν are all individual

al., 2008).

[26] Just as in less marginal rural areas, wealthier households, older beneficiaries, male recipients, more-dependent families, and single-headed households were more likely to leave Oportunidades for behavioral reasons in urban areas. However, "unlike in rural areas, indigenous households [were] much less likely to drop out for behavioral [...] or administrative [...] reasons. [Perhaps, this is because] [...] [t]he indigenous population living in the urban areas may have fewer issues with understanding program materials in Spanish (96% of indigenous recipients in the sample are bilingual) compared to the rural indigenous population, [...] [and because] Oportunidades made a greater effort to reach out to this population" (González-Flores et al. 2012).

unobservables that affect treatment. The exclusion restriction is that $Cov(Z_m, \varepsilon_i) = 0$, where ε_i is the error term in equation (1.1). Put differently, the exclusion restriction asserts that both IVs do not affect the outcome of interest other than through the assignment of treatment. I use a bivariate probit model for my main two-stage estimations because the outcome and treatment variables are both binary. In addition, I apply a linear two-stage least squares for comparability purposes.

1.3.1 Ratio of IMSS-Oportunidades hospitals to total Healthcare Providers

The two institutions in charge of providing the healthcare components of Oportunidades and filling out the conditionality compliance forms are *IMSS-Oportunidades* (IMSS) and *Secretaría de Salud* (SSA). The federal government manages the former, whereas the states' governments administer the latter. According to Álvarez et al., (2008; p. 651), rural beneficiary households "using IMSS as a healthcare provider are much less likely to drop out than those using SSA." Specifically, in a period of 6 years, 25% of rural recipients using SSA are expected to drop out from the program, compared with 10% for IMSS's rural recipients users (Álvarez et al., 2008; p. 654).

Álvarez et al. (2008, p. 654) attribute this gap in drop-out rates to institutional quality differences between healthcare providers:

> *SSA staff are often recent graduates from medical schools, who are deployed to these health posts for durations of less than a year. This may lead to increased mistakes in monitoring conditions and in reporting failure to meet conditions. It may also be that IMSS staff get to know recipients better and are thus more likely to follow through to ensure that recipients meet conditions. [...] [Moreover,] there is a geographic overlap in the coverage of the providers in that they serve different communities within the same state, [meaning that this variable does not capture regional effects].*[27]

I gather healthcare infrastructure data from the Mexican National Institute of Statistic and Geography (INEGI) in order to build the first IV: the ratio of IMSS-Oportunidades hospitals to total

[27] In 1984, when the Mexican public health sector was decentralized (co-run with the State governments), some IMSS-Oportunidades hospitals —933 units in 14 states— became SSA hospitals (Merino, 2003). This political decentralization process —an exogenous policy decision— is the main explanation for why IMSS-Oportunidades operates in certain municipalities as opposed to and not others. Therefore, local infrastructure cannot be the main reason for "institutional" attrition in Oportunidades but rather the human resources in charge of running these hospitals. What is more, the vast majority of SSA and IMSS-Oportunidades hospitals were already standing prior to the implementation of Oportunidades. IMSS-Oportunidades hospitals had a different name, IMSS-Coplamar, at the time.

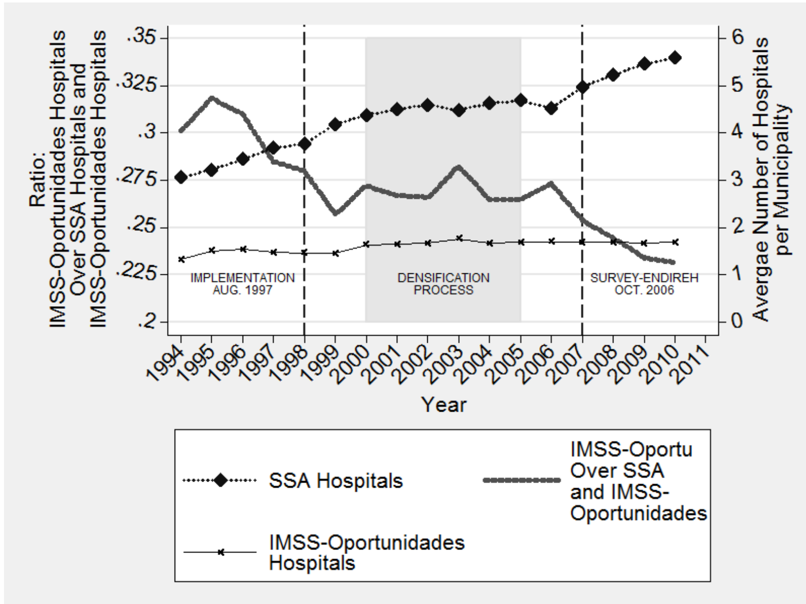

Figure 1.2: Dynamic Analysis of Oportunidades Health Providers

Oportunidades healthcare providers (sum of IMSS-Oportunidades plus SSA hospitals). The logic behind this IV goes as follows: *As the probability of having IMSS as a healthcare provider increases, so does the likelihood of remaining in Oportunidades*.

Whether this IV may be excluded from the second stage is an untestable question. Nevertheless, there are very few channels, alternative to the assignment of treatment, through which the presence of IMSS hospitals as opposed to SSA hospitals may influence the reporting of IPV. The most obvious one is preferences for hospitalization after an IPV attack. Hence, I include a dummy for individual hospitalizations while controlling for health hazards and frequency of IPV.[28] According the results below, hospitalization is a significant predictor of reporting IPV to the police.

Fortunately for my identification, the ratio of IMSS hospitals to total Oportunidades healthcare providers remained stable between 1998 and 2006, the years in which a recipient in my sample could have been treated. The dynamic stability of my IV reinforces its exogeneity validity because abrupt variations could affect the outcomes through channels other than the assignment of

[28]Thus, the exclusion restriction for this first IV is the following: Conditional on individual's hospitalization, the ratio of IMSS-Oportunidades hospitals to total Oportunidades health providers has no effect on reporting IPV to the police other than through the assignment of treatment.

treatment.[29] Figure 1.2 depicts the ratio of IMSS-Oportunidades hospitals to total Oportunidades healthcare providers (solid line), and the average number of IMSS-Oportunidades and SSA hospitals per municipality (lines with figures).[30]

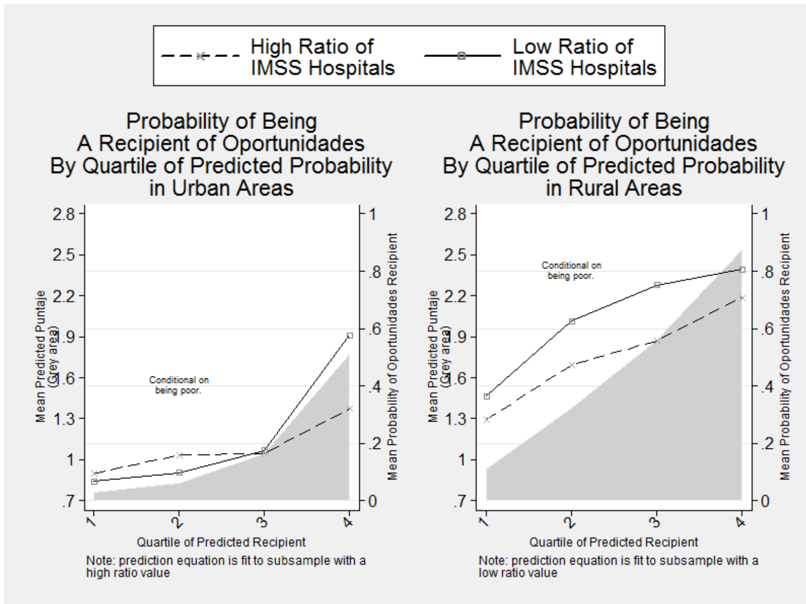

Figure 1.3: Compliance: Ratio of IMSS-Oportunidades over Oportunidades Health Providers

Last, according to administrative data, healthcare provider drop-out variation does not exist for urban households (González-Flores et al., 2012). This could be because staff in urban SSA hospitals are more experienced due to higher labor competition for medical positions (González-Flores et al., 2012). To illustrate this fact, Figure 1.3 presents the results of an exercise for the assignment of treatment similar to the one conducted by Card (1993). This exercise divides discretely the sample by the variation in the excluded variable (e.g. high versus low ratio using the median value).[31]

Evidently, Figure 1.3 shows that the IV works rather well for household living in rural areas

[29]Therefore, under this IV, the risk in the assignment of treatment is the same for all years, for most municipalities, regardless of when the woman begins receiving Oportunidades.

[30]The ratio remains around 2.5 SSA hospitals for every IMSS-Oportunidades hospital (left axis). The average number of IMSS and SSA hospitals per municipality is 1.75 and 4.5 (right axis), respectively.

[31]Card's (1993) visual exercise consists in predicting the probability of being a beneficiary of Oportunidades using the predicted *puntaje* estimated above (left y-axis) and all other observables. To test whether the IV is correlated with a higher probability of being a recipient of Oportunidades, I fit the prediction using a sub-sample with the upper-half of the IMSS-provider ratio value. Finally, this visual exercise divides the predicted treatment probability into quartiles.

(right panel). Even though the poorest urban recipients also seem to comply with the IV (left panel), the results are weak because there are not many households in the (pooled) fourth quartile in urban areas. Consequently, this IV does not find a solution for drop-out or take-up biases in urban areas.

1.3.2 Roll-out in the Densification Process of Oportunidades

In order to solve for self-selection biases in urban areas, I recur to a second IV: the roll-out in the densification process of Oportunidades. This densification process was carried out in seven years (1998-2004), meaning that the program was densified in different places at different times. For this IV, I use Oportunidades's administrative data containing the roster of beneficiaries at the locality level (smaller geo-political unit than municipality).[32] In particular, I consider a municipality to be densified once its roster reaches 25% of total beneficiaries in 2005.

The intuition for this second IV suggests that *as more urban women enroll in Oportunidades, then other urban women with similar economic characteristics are more likely to register in the future due to flow of information.* Moreover, urban households mistakenly excluded have higher chances of enrolling during re-certification campaigns conducted every two years. Finally, the roll-out in the densification process of Oportunidades is unlikely to have a direct effect on reporting IPV to the police. However, provided that administrators gave priority to the most marginal municipalities (see section 1.2), I control for human and gender development of a municipality.[33] These development indexes turn out to be significant predictors of reporting IPV to the police and, thus, necessary for a valid exclusion restriction.

[32] I use the variation at the municipality level rather than at the locality level because the smallest geographical area publicly available for ENDIREH-06 is municipalities.

[33] All together, the exclusion restriction for this IV claims that, after controlling for municipality-specific development factors, the roll-out for the densification process of Oportunidades does not affect the reporting of IPV to the police other than through treatment.

1.4 Results

1.4.1 Effect of Oportunidades on Reporting IPV to the Police

Table 1.3 contains the estimated effect of Oportunidades on reporting IPV to the police for the pooled sample (rural and urban women). Columns 1 to 3 present the results of running a maximum-likelihood probit regression for equation (1.1).[34] The first column is the most parsimonious specification and includes only municipality and demographic controls. Municipality controls are two development indexes: IDH (human development) and IPG (women's empowerment development). Demographic controls contain indicator variables for woman's age, woman's ethnicity (indigenous), woman's education, presence of children under the age of 12, family size, years in union, and current marital status (e.g. married, cohabiting, or divorced). Furthermore, I include a dummy for rural areas to control for differences across beneficiary populations. For this specification, I find a statistically significant effect of Oportunidades on reporting IPV to the police equal to 3.8%, at the 95% level of confidence. In relative terms, this accounts for a non-trivial increase of 27.3%.

Next, column 2 reports estimations with IPV controls. These are dummies for IPV frequency, hospitalization following from an IPV attack, and physical harm. As the criminology literature predicts, the previous variables are all crucial determinants of reporting IPV to the police (Black, 1976; Felson et al., 2002; Thompson and Kingree, 2006). After incorporating IPV controls, the ATE increases to 4.2% (30.2%). This effect is also statistically distinguishable from zero.

Column 3 includes the predicted *puntaje* and an asset index in the set of covariates.[35] However, the inclusion of these controls does not change the ATE because the sample selection reduces much of the distance in the difference between treatment groups. Moreover, if treatment is endogenous, then both of these variables are also endogenous due to economic mobility from receiving Oportunidades.

In order to solve for OVB, the rest of the columns in Table 1.3 incorporate a first stage using the ratio of IMSS-Oportunidades hospitals to total Oportunidades healthcare providers and the

[34] OLS estimations draw very similar conclusions. These estimations are only available upon request.

[35] To build the asset index, I apply a factor analysis following Sahn and Stiefel (2003).

Table 1.3: Effect of Oportunidades on Reporting IPV to the Police: Pooled Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.038**	0.042**	0.042**	0.034*	0.164	0.042***	0.246*	0.044***	0.235*
	(0.019)	(0.019)	(0.019)	(0.018)	(0.136)	(0.014)	(0.135)	(0.015)	(0.136)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	3444	3444	3444	3444	3444	3444	3444	3444	3444
Municipalities	776	776	776	776	776	776	776	776	776
GOF p-stat				0.000		0.000		0.000	
Overidentif.					0.296		0.405		0.398
Endogeneity					0.403		0.147		0.174
F-stat IV					10.08		9.85		9.75
Redundacy					0.016		0.017		0.014

Marginal effects
Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variables are the rollout in the densification process and the ratio of IMSS-Oportunidades over health providers. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Redundancy is an LM test for an invalid IV. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

roll-out in the densification process of Oportunidades as IVs. I present bivariate probit (biprobit) regressions —in columns 4, 6, and 8— and linear IV (2SLS) regressions —in columns 5, 7, and 9— for equations (1.1) and (1.2). Despite sharing the same vector of excluded of variables, there are important differences in the estimation process between the 2SLS model and the biprobit model. On the one hand, the former dismisses the dependent variables as probability functions in both stages. On the other hand, the latter maintains equations (1.1) and (1.2) as likelihood functions. Most important, *the 2SLS estimator is unbiased in providing the local average treatment effect (LATE), but inefficient if sample sizes are below 5,000 or treatment probabilities are low (Chirubis, Das and Lokshin, 2012).*[36] *In contrast, the biprobit estimator provides much greater precision for the ATE, but at the expenses of having to assume standard bivariate normal error terms (Chirubis et al. 2012).*

[36]LATE is "the Wald estimand [which] can be interpreted as the effect of [the IV] [...] on those whose treatment status can be changed by the instrument" (Angrist and Pischke, 2009; ch.4). When using multiple-IVs, LATE is only "a linear combination of the underlying Wald estimators. In other words, it is a linear combination of the instrument-specific LATEs using the instruments one at a time" (Angrist and Pischke, 2009; ch.4).

Findings in Table 1.3 indicate that biprobit estimations are less than a fifth of the 2SLS. Even though the 2SLS reports the LATE as opposed to the ATE, these differences in findings come from inefficiencies in the linear estimator rather than from differences between the LATE and the ATE (Chirubis, 2012). Therefore, provided that my sample size is below 5,000, it is imperative to use a biprobit approach for efficient estimations.

The inefficiency of the 2SLS notwithstanding, I exploit some linear tests to validate my identification strategy. In particular, I am interested in over-identification tests and in F-statistic tests for excluded variables.[37] According to the Hansen J, all specifications fail to reject the null hypothesis of valid IVs. Moreover, the linear tests suggest non-weak IVs because F-statistics are around 10, which is the conventional rule-of-thumb for non-weak IVs. In addition, I test for redundancy between instruments, showing improvement in the efficiency of the estimations. Last, both IVs affect assignment of treatment in the predicted direction (see Table A.5 for first-stage estimations).

Column 4 in Table 1.3 shows the results of using a biprobit model with the most parsimonious specifications. After accounting for OVB, the ATE shrinks 0.4% from the "biased" probit estimator to 3.4% (24.4%). However, as explained in the identification strategy, the exclusion restriction may be violated because this specification does not incorporate individual hospitalizations (part of IPV control group).

When including IPV controls —my preferred specification— as in columns 6, the ATE remains unchanged at 4.2% (30.2%). Similarly, the ATE varies little when controlling for *puntaje* and assets (column 8). The lack of changes between the probit and the biprobit results imply either zero OVB or compensation of drop-out biases by take-up biases. In order to make an accurate assessment, I conduct the same analysis for rural and urban areas, separately.

Table 1.4 presents the same previous models for the rural sample, exclusively. *Prima facie*, the results are much lower than for the pooled sample and are statistically indistinguishable from zero. The most parsimonious specification, in column 1, shows an effect close to 0.7%, in absolute terms, or 5.0%, in relative terms. Once I include IPV controls (column 2), the ATE for rural abused

[37] The over-identification tests report the p-value of the Hansen-J statistic because I use robust standard errors, clustered by municipalities.

women increases to 1.2% (8.6%), although it remains relatively low. When controlling for *puntaje* and assets (column 3), then the estimated ATE is 1.4% (10.1%).

Table 1.4: Effect of Oportunidades on Reporting IPV to the Police: Rural Areas Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.007	0.012	0.014	0.041	-0.039	0.058*	0.079	0.052	0.063
	(0.020)	(0.020)	(0.019)	(0.034)	(0.224)	(0.030)	(0.230)	(0.040)	(0.223)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	1920	1920	1920	1920	1920	1920	1920	1920	1920
Municipalities	627	627	627	627	627	627	627	627	627
GOF p-stat				0.000		0.000		0.000	
Endogeneity					0.840		0.766		0.824
F-stat IV					7.17		7.01		7.83

Marginal effects

Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variable is the ratio of IMSS over health providers. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Nevertheless, drop-out biases may be attenuating (downward bias) the ATE of Oportunidades. Column 4 shows that, once OVB is accounted for by using the ratio of IMSS-Oportunidades hospitals to total healthcare providers as an IV, the ATE increases to a magnitude close to the pooled sample: 4.1% (29.4%). In fact, when I control for OVB and IPV factors (column 6) —my favorite specification— the ATE for rural women is higher than for the combined sample, at 5.8% (41.7%). Most important, this latter finding is statistically significant. Not surprisingly, the linear IV estimations remain inefficient because the sample size is reduced to almost half.

Table 1.5 shows the estimations for the effect of Oportunidades on reporting IPV to the police, for the urban sample. In stark contrast to the rural sample, the *prima facie* ATE for urban women is much higher than for the pooled sample. For instance, the shortest specification, in column 1, estimates an effect equal to 12.5% (89.9%). After including further controls (columns 2 and 3), the ATE decreases to 11.9% (85.6%) or 11.2 (80.6%), depending on the set of additional covariates.

However, as the identification strategy explains, the ATE for the urban sample is biased due

Table 1.5: Effect of Oportunidades on Reporting IPV to the Police: Urban Areas Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.125***	0.119***	0.112***	0.040***	0.613	0.044*	0.825	0.049**	0.849
	(0.040)	(0.036)	(0.037)	(0.013)	(0.516)	(0.023)	(0.537)	(0.023)	(0.574)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	1524	1524	1524	1524	1524	1524	1524	1524	1524
Municipalities	283	283	283	283	283	283	283	283	283
GOF p-stat				0.000		0.000		0.000	
Endogeneity					0.268		0.092		0.095
F-stat IV					3.37		3.40		3.28

Marginal effects
Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variable is the rollout in the densification process. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

to take-up of the program (upward bias). After introducing a first stage with the roll-out in the densification process of Oportunidades as an IV, the ATE becomes almost identical to the pooled "unbiased" effect, at 4.0% (28.8%). Similarly, my preferred specification, which controls for IPV characteristics and solves for OVB, yields an ATE of 4.4% (31.6%). In all, findings for rural and urban areas samples are consistent with the self-selection bias theories and are very close to the overall pooled sample after controlling for OVB.[38]

1.4.2 Robustness Tests

To make ensure that these results are not driven by chance, I conduct several robustness tests. The first robustness test checks for a confounding effect between states' specialized IPV laws and Oportunidades. Specifically, the Mexican Congress ratified the Belém do Pará International Convention in 1998, right at the time when Oportunidades began operating. Subsequently, the majority of the Mexican States adopted the convention through specialized IPV laws at different

[38]Findings for all the different sample (Tables 1.3, 1.4 and 1.5) seem to be consistent with the criminology literature insofar as the coefficients for IPV frequency, hospitalization following from an IPV attack, severity of harm caused to the woman, and presence of children in the household are all statistically different than zero (Black, 1976; Felson et al, 2002; Thompson and Kingree, 2006). Coefficient estimations for all these covariates are available upon request.

times during the following 12 years. Table A.6 presents the roll-out of these specialized IPV laws. If women respond to new specialized IPV laws and if there is geographical overlap between treatment and the enactment of IPV laws, then the estimated ATE for Oportunidades could be biased. Therefore, this robustness test controls for the enactment of states' specialized IPV laws, measured in years from the survey's base date (e.g. October, 2006).

Tables A.7, A.8, and A.9, in Appendix A, show the results of this robustness test for the pooled, rural, and urban samples, respectively. The results indicate a very small, positive effect of specialized IPV laws on reporting IPV to the police. Namely, the effect of IPV specialized laws is 0.15% additional reports for every additional year since enactment. Moreover, the ATE of Oportunidades on reporting IPV to the police remains practically unchanged for the probit models and biprobit models (e.g. an average drop of 0.1%). Therefore, if there is any confounding effect at all, it is very small.

Second, I test for the sensitivity of the ATE to a variety of specifications. For instance, I incorporate the following variables into the set of demographic controls: a dummy indicator for dirt floor, toilet in the household, drainage, crowding index, husband's ethnicity, higher-order polynomials for women's woman's age and family size, and interaction terms between woman's education and higher-order polynomials. In addition, I include more IPV controls such as harm to other members of the family, a dummy indicator for parent's IPV during childhood, higher-order polynomials for the enactment of specialized IPV laws, and police investigators in a municipality.

Appendix A also contains the results for this robustness test. The maximum-likelihood probit models for all samples (column 1 to 3 in Tables A.10, A.11, and A.12) show an improvement on the ATE equal to 0.2%, against the direction of the bias. However, the findings in the biprobit models barely change, if at all. Therefore, these modest improvements do not justify a "kitchen sink" regression.

The third robustness test excludes observations that do not meet the selection poverty cutoff (0.384-0.69). This test checks whether the results are all driven by the section of technically ineligible beneficiaries. In total, this new poverty threshold drops 793 observations, which accounts for one-quarter of the sample. Both beneficiaries and non-beneficiaries are excluded from the sample with this alternative poverty cutoff.

Tables A.13, A.14, and A.15, in Appendix A, shows the results for this third robustness test. For the rural sample (Table A.14), the ATE drops 1.4% on average. This could indicate a higher attrition rate for "poorer" abused rural women, who do report IPV to the police. Yet, the biprobit model also shows a similar drop in the ATE. Therefore, a good portion of the treatment effect in rural areas is driven by the "quasi-poor," although not all of it. Conversely, the urban sample findings remain rather close to the original estimations.

Last, I validate the results using conditional fixed-effects. Unfortunately, this inferential method requires variation in the dependent variable within a municipality. Thus, the sample size shrinks to almost half, taking away much of the power for estimation.

Table A.16 shows the results for the conditional fixed-effects. Although the ATE estimations differ from the biprobit model, all conditional fixed-effects models move against the bias just as the self-selection theory would predict. The improvement factor is about 1.6% on average. However, most of the coefficients for the conditional fixed-effects models are not significantly different from zero. Hence, the importance of maintaining a bigger sample size for more accurate causality estimations.

1.5 Institutional Channels

Thus far, estimations suggest a robust, positive effect of Oportunidades on reporting IPV to the police. However, results remain silent about the channels through which Oportunidades has an impact on the final outcome. In what follows, I propose two groups of causality channels: i) formal institutions and ii) informal institutions.

1.5.1 Formal Institutions

Oportunidades might have an effect on the operational effectiveness of formal institutions that lead to higher reporting rates. These institutions are rules agreed upon in advance by the "whole" society and expressed in the law (North, 1991). I use two proxies to represent the operational effectiveness of formal institutions: assimilation of women's rights, and trusts in the police. Both variables gather formal institutions because the Mexican Constitution and Procedural Organic Law spell out the existence of women's rights and procedural rules (e.g. duties of the police, due process, etc.) Moreover, in theory, both formal institutions have a positive impact on the final outcome.

ENDIREH-06 provides information for both proxies of formal institutions. For assimilation of women's rights, the survey asks each woman whether "she feels rightful, personally" . Similarly, the survey identifies the reasons for not reporting IPV to the police. "Distrust in the police" is among these reasons. Hence, I am able to measure trust in the police (inverse), conditional on not reporting IPV to the police.[39]

Table 1.6 presents the results for the effect of Oportunidades on assimilation of women's rights and trust in the police. Having established the existence of self-selection biases in the effect of Oportunidades on reporting IPV to the police, I expect an analogous bias for channels affecting the final outcome. Therefore, I report both probit and biprobit estimations of my preferred specification (municipality, demographic and IPV controls).

The top panel in Table 1.6 contains the results for the effect of Oportunidades on assimilation of women's rights by area. Columns 1 and 2 show this effect for the pooled sample. The estimated absolute (relative) effect of Oportunidades on assimilation of women's rights for the probit and biprobit models is 2.3% (2.4%) and 22.2% (23.6%), respectively. This effect is similar among rural beneficiaries (columns 3 and 4). However, for urban women, the treatment effect on assimilation of women's rights is about half of that. What is more, the self-selection theory is consistent for rural beneficiaries but inconsistent for urban beneficiaries; even though the coefficients for the urban sample are not statistically distinguishable from zero. All together, Oportunidades has a positive effect on assimilation of women's rights.

[39]For this proxy, however, I assume trust in the police among non-reporting abused women to be similar or inferior to that among those women who do report IPV to the police, based on rationality. To be clear, reporting IPV to the police would be irrational if there is null expected return.

Table 1.6: Formal Institutional Channels

Panel A. Effect of Oportunidades on Assimilation of Women's Rights†

	(1) Probit	(2) Biprobit	(3) Probit	(4) Biprobit	(5) Probit	(6) Biprobit
Oportunidades	0.023**	0.222***	0.023	0.263***	0.014	0.115
	(0.010)	(0.055)	(0.014)	(0.078)	(0.015)	(0.089)
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	Yes	Yes	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	No	No	No	No
Sample	All	All	Rural	Rural	Urban	Urban
Observations	3442	3442	1919	1919	1523	1523
Municipalities	775	775	627	627	282	282

Marginal effects

Panel B. Effects of Oportunidades on Trust in the Police, Conditional on not Reporting IPV††

	(1) Probit	(2) Biprobit	(3) Probit	(4) Biprobit	(5) Probit	(6) Biprobit
Oportunidades	0.015	0.108**	0.034*	0.213***	-0.045	0.080***
	(0.017)	(0.045)	(0.019)	(0.065)	(0.031)	(0.026)
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	Yes	Yes	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	No	No	No	No
Sample	All	All	Rural	Rural	Urban	Urban
Observations	2907	2907	1673	1673	1234	1234
Municipalities	740	740	600	600	256	256

Marginal effects

Notes: All regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variables are the rollout of the densification process (C2 and C6) and the ratio of IMSS-Oportunidades over health providers (C2 and C4).
*, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.
+ Mean (SD) of Assimilation of Women‘s Rights: 0.94 (0.23).
++ Mean (SD) of Trust in the Police, Conditional on not Reporting IPV: 0.90 (0.30).

The bottom panel in Table 1.6 reports the effects of Oportunidades on trust in the police by area. Just as with assimilation of women's rights, Oportunidades also has a positive effect on trust in the police. The unbiased effect for the pooled sample (column 2) is 10.8% in absolute terms or 12.1% in relative terms. This effect is similar in magnitude for the urban sample. In contrast, the unbiased absolute (relative) effect of Oportunidades on trust in the police for rural beneficiaries is twice as much at 21.3% (23.6%). Most important, all coefficient are statistically significant from zero across samples.

These findings are consistent in sign with the causality theory. Moreover, results relate to a body of literature looking at the positive effects of CCTs on social trust (Attanasio, Pellerano and Polanía Reyes, 2009), political empowerment (Amarante and Vigorito, 2009), and government support (Manacorda, Miguel and Vigorito 2011) in Latin American countries (e.g. Colombia and Uruguay).

1.5.2 Informal Institutions

Table 1.7: Informal Institutional Channels

Panel A. Effects of Oportunidades on Willingness to Separate, Conditional on Currently Married or Cohabiting.†

	(1) Probit	(2) Biprobit	(3) Probit	(4) Biprobit	(5) Probit	(6) Biprobit
Oportunidades	0.051**	-0.013	0.021	0.016	0.109**	0.060***
	(0.024)	(0.055)	(0.023)	(0.089)	(0.048)	(0.018)
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	Yes	Yes	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	No	No	No	No
Sample	All	All	Rural	Rural	Urban	Urban
Observations	2977	2977	1736	1736	1241	1241
Municipalities	745	745	600	600	265	265

Marginal effects

Panel B. Effects of Oportunidades on Willingness to Reconcile, Conditional on Currently Married or Cohabiting.††

	(1) Probit	(2) Biprobit	(3) Probit	(4) Biprobit	(5) Probit	(6) Biprobit
Oportunidades	-0.026	-0.038	-0.016	-0.059	-0.056*	-0.029
	(0.019)	(0.100)	(0.022)	(0.175)	(0.033)	(0.125)
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	Yes	Yes	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	No	No	No	No
Sample	All	All	Rural	Rural	Urban	Urban
Observations	2980	2980	1736	1736	1244	1244
Municipalities	745	745	600	600	265	265

Marginal effects

Notes: All regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variables are the rollout of the densification process (C2 and C6) and the ratio of IMSS-Oportunidades over health providers (C2 and C4).
*, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.
+ Mean (SD) of Willing to Separate, Conditional on Currently Married: 0.22 (0.41).
++ Mean (SD) of Willing to Reconcile, Conditional on Currently Married: 0.16 (0.37).

Similarly, Oportunidades could change the threat point for some women —because of economic resources from the program—and thus could ease the burden of ostracism costs, created by social norms. In contrast to formal institutions, informal (social) institutions are not set in advance and, therefore, impose inefficiency costs that inhibit reporting IPV to the police (North, 1991; Posner, 2001). To proxy for informal institutions, I choose two variables related to the marriage market: willingness to leave the relationship (separate) and willingness to reconcile. ENDIREH-06 asks all married or cohabiting abused women about future plans to separate from their current partners. Likewise, the survey identifies those married or cohabiting women who separated from but reconciled with their current abusive partners.

Findings in Table 1.7 indicate that Oportunidades has an effect on the transformation of the marriage market but only in urban areas. Namely, the unbiased ATE of Oportunidades on willingness to separate for urban beneficiaries is 10.9% or 27.7% in absolute or relative terms, respectively (columns 5 and 6). Moreover, the results match the self-selection bias theory which predicts higher take-up rates for urban beneficiaries seeking to gain bargaining power. This positive effect, however, is not statistically different from zero for the rural and pooled samples.

By the same token, the bottom panel shows the effect of Oportunidades on willingness to reconcile. All of the estimations move in the "right" direction: less willing to reconcile after treatment. Moreover, all coefficients behave according to what the self-selection bias theory in my identification strategy predicts: attrition for the rural sample and take-up for the urban sample. However, here again the results are only statistically distinguishable from zero for the urban sample. *This could be because ostracism costs are much lower in urban areas than in rural areas.*

These findings are consistent with the conclusions of Bobonis (2011), who studies union dissolution among rural Oportunidades beneficiaries. He finds a rather modest treatment effect (0.32%) using randomized data. Most important, results for rural beneficiaries are in line with the "extractive-private information models" —in that the threat point for treated rural women does not change— just as in Angelucci (2008) and Bobonis et al. (2013).

1.6 Conclusion

In this chapter, I find a robust, positive effect of Oportunidades on reporting IPV to the police equal to 4.2% (30.2%). The channels through which the effect of Oportunidades works into the final outcome include increases in the operational effectiveness of formal institutions and changes in the market equilibrium of marriage or cohabitation. The former set of channels mainly take place in rural areas, whereas the latter channels do so in urban areas.

These inferential results have immediate policy applications. For instance, Oportunidades administrators could implement a randomized exercise in which women are incentivized to report IPV by means of information. This additional information could be conveyed during the monthly *pláticas*, where women and Oportunidades staff could discuss IPV health hazards and the advantages of using the police.

If this randomization exercise works for the treated population, then it can be expanded to the control group in Oportunidades and outside of Oportunidades as a CCT program of its own. The advantage of keeping this proposal within Oportunidades is the low budget needed to implement it. In theory, this policy should not cost a lot of money because all fixed costs (e.g. system, focalization, infrastructure, cash transfer) have already been covered. Most important, the benefits of establishing the rule of law are very great, economically speaking; particularly for a country like Mexico, where impunity prevails.

2 THE ECONOMIC CONSEQUENCES OF THE MEXICAN DRUG WAR

Mexico's previous federal administration (December, 2006 to December, 2012), headed by President Felipe Calderon, launched an unprecedented military strategy to capture as many drug-lords as possible. This was the government's response to an expansion in drug-related activity and violence. Between 2001 and 2006, prior to the launching of the Mexican Drug War, total yearly drug-related homicides increased 94%, from 1,080 to 2,100 (Chabat, 2010).

However, after the implementation of Calderon's military policy, yearly drug-related homicides spiked to levels above exponential rates. In 2010 alone, total drug-related homicides ascended to over 16,000 —eight times more than in 2006. The existing literature suggests that the main causal link between Calderon's military strategy and the spike in drug-related violence was the fragmentation of drug-trafficking organizations, hereinafter DTOs (Dell, 2011; Guerrero, 2011; Merino, 2011; Calderon et al., 2012).

Specifically, after the decapitation of DTOs, the level of competition rose significantly because these groups organized themselves horizontally (cells), as opposed to vertically (hierarchies). Also, competition grew because the federal government decided to fight all DTOs instead of the most violent groups. Worse yet, the government oftentimes dismantled and ultimately abandoned local enforcement institutions, eliminating the possibility of order in the medium run. In all, the strategy was inappropriate, and therefore, it backfired.

Calderon's administration, through its Economy Minister, contended fiercely that the conflict did not have any consequence on the economy (El Universal, 2012). However, the Economy Minister never provided scientific research to back up his claim. The purpose of this chapter is to fill this policy-knowledge gap by testing empirically whether the Mexican Drug War had an effect on economic development in treated states, using gross domestic product (GDP) per capita as the outcome of interest. The case study of the Mexican Drug War is unique in its kind because the conflict took place in states with different levels of GDP per capita.

In an attempt to estimate the impact of the Mexican Drug War on GDP per capita for treated states, the research design in this chapter consists mostly of synthetic control methods. This recently developed econometric technique compares a homogenous control state, produced by applying a two-step optimization procedure, with a "treated" state over time. I define a state as "treated" if, at any point during Calderon's administration (2006-2012), Mexico's federal forces had an executive mandate to be a primary security provider in that state.[40] To prove causality more systematically, I use panel data on statewide military operations conducted by the Mexican Army, as well as the geographic rollout of the war.

Findings indicate a reduction of 0.5% in GDP per capita for treated states, over the period 2003-2012, as a direct consequence of the Mexican Drug War. Determinants include a proportional reduction in consumption per capita for treated states. By the same token, the Mexican Drug War caused a decline in productive investment of at least 0.3%, driven by a drop of 3.2% in commercial credit (non-consumption and non-mortgage) per capita granted to the private sector.

The chapter proceeds as follows. Section 1 describes the Mexican Drug War, briefly. Section 2 identifies a proxy for the Mexican Drug War, and proves orthogonality in the assignment of treatment. Section 3 lays the foundations for the empirical strategy. Section 4 presents the results and robustness checks. Section 5 analyzes the economic determinants by which the Mexican Drug War hampered GDP per capita. Finally, I conclude with immediate policy applications.

2.1 The Mexican Drug War

2.1.1 Background

The true political reasons for launching a large-scale drug war remain unknown.[41] One fact, however, is that President Calderon encountered a more complicated national security threat than all

[40]The three legs of the federal forces are the Mexican Army, the Mexican Navy, and the Federal Police.

[41]Some scholars claim that the Mexican Drug War was a political strategy to legitimize Calderon's presidency after a rather narrow electoral victory of only 0.56%. Other experts suggest it was a desperate action to attend existing security concerns (see Chabat, 2010).

of his predecessors. Whereas most Mexican presidents were able to regulate drug-trafficking activities through a *"pax mafiosa,"* three structural factors damaged the Mexican tolerance policy (Chabat, 2010).

First, Mexico began cooperating with U.S. government agencies to eliminate the supply of illicit drugs. In September 1975, the Mexican Army, in coordination with the U.S. Drug Enforcement Administration (DEA), implemented "Operation Condor," the first military strategy against the supply of illicit drugs (Craig, 1980).[42] This partnership in law enforcement expanded after the assassination of DEA-agent Enrique Camarena in 1985, and the initial negotiations of the North American Free Trade Agreement in 1990 (Chabat, 2010). Subsequently, several kingpins were arrested during the presidential sexeniums of Salinas, Zedillo, and Fox (1989-2006). Specifically, the incarceration of Miguel Angel Felix Gallardo, the Mexican godfather, in 1989, changed the industrial organization of drug-trafficking services for decades to come (Chabat, 2010).

Second, cocaine-trafficking routes shifted to Mexico ("balloon effect") after the United States blocked the narrower Caribbean-trafficking corridor in the 1980s and early 1990s (Toro, 1995). This change in cocaine trafficking routes, and increasing enforcement efforts by the Colombian government —like the impeachment of corrupted politicians—, allowed Mexican DTOs to capture quasi-rents from Colombian drug cartels (Chabat, 2010). Soon, towards the end of the 1990s, Mexican DTOs became increasingly militarized and expanded their presence to many parts of Mexico (Valdés-Castellanos, 2013). Thanks to their newly acquired military capabilities, several Mexican DTOs like Loz Zetas and La Familia diversified their criminal enterprises into extortion, kidnapping, motor vehicle theft, and human-trafficking activities (Valdés-Castellanos, 2013).

Third, Mexico's democratization process decentralized political power and made it impossible to negotiate effectively with DTOs (Osorio, 2012). Furthermore, institutional reforms to the justice system —like the creation of the Mexican Intelligence Agency (CISEN) in 1989 and the National Executive Committee for Public Safety in 1995— transformed criminal enforcement practices in the country (Chabat, 2010). Mexico locked its political system into democracy with the election of

[42] Operation Condor tumbled Mexico's share of U.S.-bound heroin and marijuana from a record high of 80% to a low of 20%, although only for a short period of time (Toro, 1995).

Vicente Fox, the first president not to belong to the hegemonic political party, PRI. Nonetheless, this democratization process left severe power vacuums, for which DTOs competed. Consequently, drug-related violence increased across Mexico, although at very controllable levels; in fact, homicide rates for the population at large were at a historical low.

2.1.2 Joint Operations and the Spike in Drug-Related Violence

By the time Calderon took power, he faced a security threat of medium dimensions. Hence, soon after receiving complaints about the expansion in the activities of DTOs by the governors of Michoacan, Baja California, and Guerrero, he opted for sending Mexico's military forces out to the streets. In particular, Calderon's military strategy sought to capture kingpins from all DTOs. Since the federal government tried to pursue this goal in conjunction with the local governments, the military strategy became known as *"joint operations"* (JOs). However, because of politics, the federal government seldom coordinated with local enforcement agencies, many times taking over local public safety duties, permanently. For the rest of this chapter, I use the terms "Mexican Drug War", "military strategy", and "JOs", interchangeably.

In total, 11 out of the 32 Mexican states had a statewide JO at some point during the period 2006-2012. These states are Baja California, Chihuahua, Coahuila, Durango, Guerrero, Michoacan, Morelos, Nuevo Leon, Sinaloa, Tamaulipas, and Veracruz. Overall, the military strategy was very successful at accomplishing its target: Only 11 out of the 37 most wanted drug-lords were still at large by the end of Calderon's administration.

However, the decapitation of DTOs caused a "hydra effect." According to Guerrero (2011), the number of DTOs went from six to 16 during Calderon's presidency. This hydra effect responded to the organizational structure of the Mexican DTOs. Contrary to the Colombian Cartels and the Italian Mafia, the Mexican DTOs had a cellular organization rather than a vertical structure. That is, the Mexican DTOs were very much like a horizontal merger of enterprises or franchises, working within the same line-of-business in different Mexican states. The leader of the DTO coordinated and held together all corresponding cells, while managing international contracts for the transportation and final processing of drugs such as cocaine from Colombia and ephedrine from China.

For instance, the Sinaloa DTO, led by Joaquin Guzman, contained within its organization at least 12 different cells, operating in nine different states: "Gente Nueva" in Sinaloa, "Los Mexicles" in Chihuahua, "El Tigre" in Baja California, "Los Mata-Zetas" in Veracruz, and others. Once the leader of the DTO fell from the structure, turf wars erupted within and across DTOs to gain control of the trafficking-routes and major cities ("*plazas*").

Existing empirical evidence suggests that the Mexican Drug War had a significant effect on violence. Using a propensity score matching estimator, Merino (2011) concludes that the JOs caused 12,046 (52.4%) additional drug-related homicides between 2007 and 2010. By applying a regression discontinuity design on electoral results, Dell (2011) also finds supportive evidence for this causal hypothesis, with a very similar magnitude in municipalities that have a strong presence of DTOs (53.0%).[43] Last, Calderon et al. (2012) expand these findings by running a difference-in-difference regression with time-varying unobservable controls of intentional homicide rates on drug-lord and drug-lieutenant arrests. Their results show a temporary treatment effect as high as 46.9%.

Notwithstanding the unintended consequences of the Mexican Drug War, there would still have been a significant increase in drug-related homicides because of two foreign exogenous confounding events. First, the 2004 expiration of the U.S. Federal Assault Weapons Ban (AWB): which, according to Chicoine (2011) and Dube et al. (2013), made semi-automatic weapons more accessible to DTOs in Mexican states along the U.S. border, except for Baja California. These authors conclude that easier access to semi-automatic weapons had a marginal effect on intentional homicide rates within the range of 16.4% to 21.0% for the years 2005 and 2006, prior to the beginning of the Mexican Drug War.

The second foreign confounding event is an increase of cocaine seizure rates in Colombia, which became significantly larger beginning in 2006. More cocaine seizure in Colombia (less

[43]Dell (2011) uses variation in electoral results for municipalities where PAN, Calderon's political party, barely won, and where more federal enforcement assistance occurred at the beginning of the Mexican Drug War.

supply) led to higher revenues because of an inelastic demand for illicit drugs in the U.S. (Castillo et al., 2013). Subsequently, DTOs fought against each other to gain control over those additional revenues. According to Castillo et al., around 17.1% of all homicides can be explained by cocaine seizure rates in Colombia. Yet, the existing literature emphasizes Calderon's military strategy as the main driver of the variation in drug-related violence.

2.2 Identification Strategy

The primary objective of this chapter is to estimate the effect of the Mexican Drug War on economic development for treated states. Therefore, first, I identify a continuous proxy for the Mexican Drug War to describe its economic impact, thoroughly. A proper continuous proxy gathers variation in timing and intensity of treatment.

I exploit the rollout in the government's implementation of statewide JOs as the most direct measurement for the timing of treatment. Table 2.1 contains a timeline for all statewide JOs implemented during Calderon's administration. Evidently, Calderon launched some of these JOs as early as just days after assuming office in December 2006, and as late as weeks shy from leaving office in December 2012.

However, the rollout in the implementation of the JOs does not capture the intensity of the Mexican Drug War across treated states. To measure treatment intensity, I use data from Mexico's Freedom of Information Act System on statewide interception operations conducted by the Mexican Army.[44] These operations include cargo and fugitive interceptions in highways, ports, streets, and international bridges. Table 2.1 shows basic statistics on these interception operations.

Although the Army conducted a few interception operations prior to the Mexican Drug War, the number of these operations increased by tenfold during Calderon's administration. Most important, the nature of these operations was radically different after the implementation of the Mexican Drug War. For instance, the Army began using unconstitutional check-points, sophisticated detection technology (e.g. industrial ion scanners), and a larger number of soldiers traveling in convoys.

[44] Tracking file identification number in the Freedom of Information Act System is 0000700008513.

Table 2.1: Drug-Related Violence and Timing and Intensity of Treatment (2007-2012)

		Average (max.) rate p/ 100,000 inh. 2007-2012†	
State	Month/Year	Army Operations	Drug-Related Homicides
Michoacan*	January, 2007	0.10 (0.15)	11.67 (14.74)
Baja California	January, 2007	0.02 (0.03)	20.35 (34.04)
Guerrero	January, 2007	0.05 (0.12)	33.95 (54.31)
Nuevo Leon	January, 2008	0.06 (0.13)	12.64 (30.18)
Tamaulipas	January, 2008	0.08 (0.19)	13.19 (27.89)
Sinaloa	January, 2008	0.14 (0.22)	38.63 (67.71)
Chihuahua	April, 2008	0.12 (0.18)	78.94 (153.88)
Durango	May, 2008	0.12 (0.25)	28.72 (48.01)
Coahuila	September, 2011	0.07 (0.21)	11.74 (27.26)
Veracruz	October, 2011	0.06 (0.17)	3.87 (7.02)
Morelos	May, 2012	0.06 (0.18)	12.24 (23.28)
All Average	*2008*	*0.08*	*24.18*

†Data for Military Operations comes from the Freedom of Information Act System.
Data for drug-related homicides comes from the Bureau of Health Statistics (SINAIS).
* Michoacan's JO initiated on December 11th, 2006, being reinforced in January, 2007.

Given these fundamental differences in the nature of interception operations after the beginning of the conflict, the interaction between the rollout of JOs and the rate of interception operations constitutes my continuous proxy for the Mexican Drug War. According to the existing literature (Dell, 2011; Guerrero, 2011; Merino, 2011; Calderon et al., 2012), war-intensity variation, interacted with the rollout of JOs, should explain differences in drug-related violence. To verify the reliability of my continuous proxy, I run ordinary least squares (OLS) and fixed-effects regressions on drug-related homicide rates for treated states, for the period 2007-2012.[45] I approximate drug-related homicides using the mortality databases from Mexico's Bureau of Health Statistics (SINAIS). In particular, I track leading causes of death associated to drug-related violence (e.g. murder by hanging, murder by gunshot, and murder by mutilation). Compared to official records of drug-related homicides from Mexican Intelligence Agencies (available from 2007 to 2010,)

[45] I control for the identified confounding factors (a dummy for AWB-bordering states and cocaine seizure rates in Colombia). Some models also include the first-lag value of the dependent variable to account for dynamic tendency and simultaneity. For the model with state dummies and the first-lag value of the dependent variable, I use Arellano and Bond's generalized method of moments procedure (see Angrist and Pischke, 2009; ch.5.)

SINAIS data seem to approximate rather accurately drug-related homicides. The last row of Table 2.1 presents basic statistics of drug-related homicide rates for all treated state.

Table B.1 in the Appendix section indicates that, on average, the Mexican Drug War explains as much as 30.8% of the variation in drug-related homicide rates, during the course of the conflict.[46] Although smaller in magnitude than for previous chapters, these findings are consistent with the conclusions of the existing literature. Differences in point-estimation most likely come from endogeneity biases, a smaller sample (e.g. state vs municipality level), and the exclusion of operations conducted by the Federal Police and the Mexican Navy.

Finally, for a valid identification strategy, I must verify orthogonality between the outcome of interest and the assignment of treatment. In other words, I must make sure that the JOs did not take place in the poorest Mexican states. Otherwise, my estimations would be biased because the treatment coefficient would pick up unobservable institutional factors that determine economic development, like corrupt institutions.

Figure 2.1: Discrete Evidence for the Orthogonality in the Assignment of Treatment

Fortunately, Figure 2.1 depicts clear discrete evidence for the orthogonality between pre-treatment GDP per capita and the assignment of treatment. Seven out of the 11 treated states had GDP per capita values above the national median prior to the conflict.[47] Only Guerrero, Michoacan, Morelos, and Veracruz had pre-treatment GDP per capita values below the national median.

Similarly, before the beginning of the Mexican Drug War, six out of the 11 treated states had corrupt public institutions, based on the median bribery-index from the National Survey of Cor-

[46]Both confounding factors move in the predicted direction.

[47]I exclude Campeche and Tabasco from the sample because both economies are highly dependent on oil-drilling activity revenues, which belong to the federation (see below.)

ruption and Good Governance.[48] Not surprisingly, corrupt treated states, with the exception of Durango (marginally rich) and Nuevo Leon, were also "poor" treated states. The previous indicates that DTOs were located in states with easy access to trafficking routes rather than in poorer states with corrupt institutions, exclusively (Dell, 2011).

Table B.2 in the Appendix section expand this evidence on treatment orthogonality, using continuous indicators for treatment, pre-treatment GDP per capita, and pre-treatment corruption levels. All models in Table B.2 correspond to OLS estimators. Clearly, neither pre-treatment GDP per capita nor pre-treatment corruption are good predictors for the assignment, timing, and intensity of treatment.

2.3 Empirical Design

2.3.1 Synthetic Control Methods

A reliable continuous proxy for treatment, and orthogonality in its assignment, allow me to estimate the effect of the Mexican Drug War on GDP per capita for treated states. Being this a comparative case study at an aggregate level, with few units in the universe, the empirical design consists of synthetic control methods (SCMs). In the context of Rubin's model for inferential causality, SCMs use the scientific solution to solve for the Fundamental Problem of Causal Inference (FPCI).[49] Contrary to the statistical solution to the FPCI, the scientific solution depend on unit homogeneity between treated and control units, rather than on the independence assumption (Holland, 1986).[50]

Hence, under the scientific solution to the FPCI, the economic impact of the Mexican Drug War ($G_{s,t}$) for state s at year t is simply the difference between the GDP per capita of state s with a JO ($Y^T_{s,t}$) and the outcome of the identical untreated state ($Y^C_{s,t}$), so long as there are no

[48]Corruption rates are based on bribery frequency to obtain local public services (e.g. pay traffic violations, property registration, etc.), as measured by the 2001, 2003, and 2005 National Survey of Corruption and Good Governance.

[49]"It is impossible to observe the value[s] of [...] [treatment] and [control] on the *same* unit, and therefore, it is impossible to observe the effect" of whatever is being measure (Holland, 1984).

[50]Because of the low number of units in the universe, and the absence of randomization, is inappropriate to assume independence (e.g. common trends) across units, over time.

exogenous confounding factors driving the same causal mechanism (e.g. the expansion of drug-related violence):

$$G_{s,t} = Y_{s,t}^{T} - Y_{s,t}^{C}. \tag{2.1}$$

To accomplish unit homogeneity between treatment and control states, SCMs rely on economic theory and enough data variation from a pool of donor states, not exposed to treatment. Without loss of generality, assume that there are j donor states that can be observed for T years, and that there are T_0 years prior to treatment. Moreover, let $W = \left[w_1, ..., w_j\right]$ be a vector of non-negative weights that sum to one, where the components of the vector represent the weights assigned to each of the donor states.[51] Choosing a value of W creates a synthetic control for one particular treated state.

Abadie et al. (2003) suggest a two-step optimization procedure to find the weights that accomplish unit homogeneity. In particular, their procedure consists in minimizing the following equation for each of the treated states:

$$(X_s - X_0 W)' V (X_s - X_0 W), \tag{2.2}$$

where X_s and X_0 are vectors of pre-war characteristics for treated unit s and all donor states, respectively; and V is a symmetric, positive semidefinite matrix that assigns a relative-importance factor to each of the outcome predictors. These authors solve equation (2.2) conditional on V, which in turn seeks to minimize the mean square prediction error (MSPE) during the pre-treatment (matching) period:

$$MSPE_s = \left(Y_{s,t} - \sum_{i=1}^{j} w_i^{\star} Y_{s,t} \right)^2 for t \in \{1, ..., T_0\}. \tag{2.3}$$

In theory, Abadie et al. (2003) show that if the synthetic control resembles closely a given

[51] Weights are constraint to positive values between zero and one (inclusive); hence, there is no extrapolation (Abadie et al., 2003).

treated state prior to treatment, then these same weights ($W^{\star}$) can be used after period T_0 to estimate the treatment effect for that state:

$$\hat{G}_{s,t} = Y_{s,t} - \sum_{i=1}^{j} w_i^{\star} Y_{s,t} \, for t \in \{T_0 + 1, ..., T\} . \tag{2.4}$$

Evidently, equation (2.4) resolves in an effect with an upward bias if the counterfactual of a treated unit is underestimated, and vice-versa (Abadie et al., 2010). In practice, it is hard to find a perfect weight vector such that the MSPE is exactly equal to zero. As matter of fact, the fitting could be poor, in which case Abadie et al. (2010) advise against using SCMs.

Finally, since there are at least two identified confounding factors that simultaneously provoked the expansion of drug-related violence, equation (2.4) estimates the effect of the total expansion in drug-related violence on GDP per capita. This is not a problem for the overall results because the confounding factors are exogenous.[52] Most important, the continuous proxy for the Mexican Drug War helps to uncover causality once a proper counterfactual becomes available.

2.3.2 Data and Case Implementation

For the current comparative case study, the outcome of interest (Y) is GDP per capita. Observed covariates (X) are population density, gross fixed assets, economic sectoral shares, and human capital. I average the aforementioned variables over the matching period: 1993-2003, except for population density, which only contains the value for 2003. Additionally, I augment these variables by including the level of GDP per capita in 2003. This economic growth model, borrowed from Barro and Xavier Sala-i-Martin's (1995) work, is practically identical to the one used by Abadie et al. (2003). One important thing to notice is that the matching period for all treated states (1993-2003) stops four years before the beginning of the Mexican Drug War (2007) to avoid stiffer restrictions due to confounding shocks (the 2004 expiration of the U.S. Federal AWB), as well as potential spillover effects that may damage estimations otherwise.

[52] In the presence of negative spillovers from the two identified confounding factors on donor units, the effect of the Mexican Drug War on GDP per capita is a strict lower bound.

Disaggregate GDP data at the state level comes from Mexico's National Institute of Statistics and Geography (INEGI). This data is available from 1993 to 2012.[53] Records for gross fixed assets belong to the 1993, 1998, and 2003 economic census conducted by INEGI.[54] Population estimations for all years also come from INEGI. Moreover, I calculate human capital by state, for individuals older than 15 years of age, based on records from the Ministry of Education (SEP) and from INEGI.[55] Human capital data is available yearly from 1993 to 2012.

To build synthetic controls for all treated states, I use 19 of the 21 states (including the Federal District) from the donor pool, because two donor units (Campeche and Tabasco) have economies that depend on over 65% of their GDP in oil-drilling activity revenues, which belong to the federation. Relying on donor's data variation and on the outcome predictors above, I construct synthetic controls for all of the 11 treated units. In addition, I run iteratively the two-step optimization procedure on all donor states to obtain synthetic controls for fitting assessment and causal inference uses.[56]

Serving as an example, Table 2.2 reports the pre-treatment values of the outcome predictors for Chihuahua, its synthetic control, and the average of all donors states. Indeed, synthetic controls outperform simple averages of donor states at accomplishing unit homogeneity. To save space, I do not report the matching period characteristics for all other treated states; however, an identical conclusion can be drawn from the rest of the treated states.[57]

[53] There are two different GDP series: one that runs from 1993 to 2006, and another series that runs from 2003 to 2012. The latter series includes detailed regional measurements and new economic activities like agricultural services, oil and gas drilling, oil-related construction, land division services, new manufacturing divisions, as well as new tourism services, just to mention a few. This study uses the 2003-base series, and makes some adjustments to calculate the GDP for the earlier series.

[54] I use gross fixed assets instead of gross total investment because the latter variable is not available for Mexico at the state level. Gross fixed assets include only those productive assets with a durability higher than one-year.

[55] To build human capital variables by state, I apply Mas's (1995) methodology: $H_{r,t} = H_{r,t-1} + E_{r,t} + O_{r,t} + \delta_{r,t} H_{r,t-1}$, where $H_{r,t}$ is the level of schooling in state r, at time t; $E_{r,t}$ is the inflow of the adult population with the same level of education; $O_{r,t}$ is the outflow of individuals moving to a higher level of schooling; and $\delta_{r,t}$ is the morality rate at a certain level of education. INEGI provides the baseline levels and mortality rates by education, while the Ministry of Education (SEP) reports schooling inflows and outflows.

[56] I use the the nested optimization method to build all synthetic controls, with the exception of Distrito Federal, Nuevo Leon, Quintana Roo, Tamaulipas, Tlaxcala, and Veracruz, which show no improvement, or no available convex combination, over the data-driven regression based method.

[57] Matching period characteristics for all other treated states are available from the author upon request.

Table 2.2: Matching Period Characteristics for Chihuahua, Synthetic Chihuahua, and Donor Units

	Chihuahua	Synthetic Chihuahua	Mexico[1]
Real GDP per capita[a]	18604.90	18603.78	15556.32
Population Density[b]	12.86	545.19	420.4
Gross Fixed Assets[c]	27.61	29.70	35.05
Sectoral Shares (%)[d]			
Agriculture, Forestry, and Fishing	6.30	5.87	5.94
Energy, Mining, and Construction	10.18	10.18	10.98
Manufacturing Industry	28.14	23.26	19.20
Marketable Services	33.41	39.33	41.10
Nonmarketable Services	21.97	21.36	22.78
Human Capital (%)[e]			
Illiterates	5.18	8.67	10.81
Primary (0th-6th)	44.01	41.22	41.42
Secondary (7th-12th)	40.66	39.85	38.09
Higher Education (+12th)	10.15	10.26	9.68

[1] Excluding all treated states, Campeche, and Tabasco.
[a] GDP per capita in 2003, (1993) Mexican Pesos.
[b] Persons per square kilometer, 2003.
[c] Average gross fixed assets as percentage of the GDP for 1993, 1998, and 2003.
[d] Average percentage share of the GDP for the matching period 1993-2003.
[e] Mean percentage of working population (15+) for the matching period 1993-2003.

2.3.3 Normalization and Sample Selection

For comparability purposes, I calculate the normalized mean square prediction error (NMSPE) and the normalized treatment effect ($\hat{NG}$) *as percentages of the GDP per capita* from equations (2.3) and (2.4), since units depart from different levels of GDP per capita:

$$\begin{aligned} NMSPE_s &= \left(100 - \frac{\sum_{i=1}^{j} w_i^{\star} Y_{s,t}}{Y_{s,t}} \times 100\right)^2 for t \in \{1, ..., T_0\}, \\ \hat{NG}_{s,t} &= \left(100 - \frac{\sum_{i=1}^{j} w_i^{\star} Y_{s,t}}{Y_{s,t}} \times 100\right) for t \in \{T_0 + 1, ..., T\}. \end{aligned} \tag{2.5}$$

According to the distribution of the NMSPE, the economic model above proves to be a good predictor of economic growth for many Mexican states. In particular, the yearly median value of the NMSPE for the full sample (treated and donor units) during the matching period is equal to 7.6%. Unfortunately, not all treated states accomplish a strong fit during the matching period as to obtain a reliable counterfactual.

Certain treated states obtain NMSPEs above the full sample median, such as Baja California (10.1%), Coahuila (8.1%), Morelos (7.7%), Nuevo Leon (15.9%), Tamaulipas (39.3%), and Veracruz (10.0%). In stark contrast, Chihuahua (3.2%), Durango (3.9%), Guerrero (0.2%), Michoacan (7.1%), and Sinaloa (2.0%) attain NMSPEs below the full sample median. Luckily for this paper, the latter set of treated states experienced JOs early during Calderon's administration, as well as the highest rates of military operations and drug-related violence (see Table 2.1.)

For the sake of accuracy and brevity, I limit my discussion to Chihuahua, Durango, Guerrero, Michoacan, and Sinaloa. Provided that the proportion of pre-treatment poor treated states remains almost the same (e.g. 2 of 5 treated states), narrowing my analysis does not create a problem for causal inference, as orthogonality in the assignment of treatment continues to hold. Moreover, I emphasize the particular case of Chihuahua, because this state experienced homicide rates well above all other states.

2.4 Results

2.4.1 Effect of Drug-Related Violence on GDP Per Capita

Figures 2.2 and B.1 depict the results for those treated states with an accurate synthetic control. The panels on the left in Figures 2.2 and B.1 plot the trajectory of the GDP per capita for Chihuahua, Durango, Guerrero, Michoacan, Sinaloa, and their respective synthetic controls. Following equation (2.4), a simple visual comparison between treatment and synthetic control lines allows the impact assessment of drug-related violence —provoked by the Mexican Drug War and the identified confounding factors— on GDP per capita, for each of the treated states. In normalized terms, Chihuahua presents an outcome gap equal to -13.2%, Durango an outcome difference of -6.7%, Guerrero an outcome division of -3.6%, Michoacan an outcome gap of -3.4%, and Sinaloa an outcome difference of -3.6%.

Overall, the magnitude of the GDP per capita gap is directly proportional to the expansion of drug-related violence, presented numerically in Table 2.1. To illustrate this causal relationship,

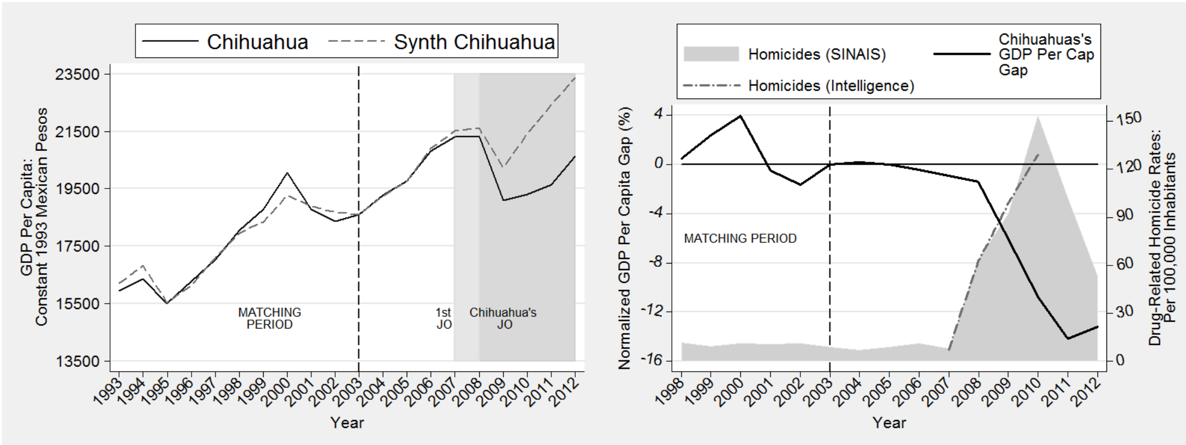

Figure 2.2: GDP Per Capita Gap between Chihuahua and Synthetic Chihuahua

the right-hand side panels in Figures 2.2 and B.1 plot drug-related homicide rates from SINAIS (shaded areas) and from the Mexican Intelligence Agencies (gray dashed-dotted lines), along with the GDP per capita gap (solid line), obtained from the left-hand side panels. As mentioned above, drug-related homicide rates from the Mexican Intelligence Agencies are only available from 2007 to 2010, and move rather closely to drug-related homicide rates from SINAIS.

For most treated states, these graphs show that the line for the GDP per capita gap descends very slowly between 2004 and 2006, after the identified confounding factors begin to expand drug-related violence. However, the GDP per capita gap only widens dramatically after the implementation of the JOs, when drug-related homicide rates spike. Furthermore, the GDP per capita gap stabilizes after drug-related homicide rates fall. This is readily observable for Chihuahua, where drug-related violence increases and falls drastically. To a lesser extent, this situation also occurs in Durango and Sinaloa, albeit the fall in drug-related violence for these states is more gradual.

Conversely, Guerrero and Michoacan, the poorest states among the treated units, continue to show increasing signs of drug-related violence beyond Calderon's administration. This behavior points to deeper governance issues (e.g. creation of paramilitary groups, political instability, etc.) Consequently, the slope of the GDP per capita gap for Guerrero and Michoacan seems to keep getting steeper. In line with the findings for the Basque and Italian conflict cases by Abadie et al. (2003) and Pinotti (2012), respectively, the main effect of drug-related violence on GDP per capita in Mexico occurs with a one-year lag.

Robustness Tests To determine the sensitivity of the findings above, I conduct four robustness tests. First, I expand the matching period up to the year prior to the rollout of the JOs, for each of the treated states. This first robustness check provides additional information for the construction of synthetic controls at the expense of spurious causal and inferential conclusions, due to the presence of confounding factors and spillover effects.

Table 2.3: Robustness Tests: GDP Per Capita Gap (%) between Treated and Synthetic Control Units

State	Original[a]	Matching Period[b]	Maximum Unit[c]	Pre-Homicide[d]	Geography[e]
Chihuahua	**-13.20**	-12.01	-11.93	-11.94	-14.89
Durango	**-6.73**	-6.91	-6.43	-7.67	-10.99
Guerrero	**-3.59**	-4.63	-4.57	-3.10	-3.09
Michoacan	**-3.37**	-3.12	-3.22	-2.70	-5.81
Sinaloa	**-3.63**	-2.23	1.65	-2.79	-3.63
Average	**-6.10**	-5.78	-4.90	-5.64	-7.68
T-test (p)[⋆]	-	0.510	0.327	0.282	0.140

[⋆]Ho: difference in means between models is indistinguishable from zero.

[a] Original model (see results above.)

[b] Sets the matching period from 1993 up to year prior to the rollout of the JO.

[c] Drops the donor unit with the highest weight from the donor pool.

[d] Adds per capita homicide rates to the original set of matching covariates (X).

[e] Includes the density-distance to the nearest U.S. border bridge in the outcome predictors (X).

Second, I drop the donor unit with the highest weight from the donor pool for each of the treated states, separately. This falsification test studies whether state-specific effects are only driven by one single donor unit. Nevertheless, when conducting this robustness check, the value of the MSPE in equation (2.3) necessarily increases because the optimal set of weights ($W^{\star}$) becomes unavailable by construction. For instance, Sinaloa's MSPE increases by half after dropping the principal donor unit from the optimal vector of weights ($W^{\star}$). Table 2.B.3 in the Appendix section presents the synthetic weights for treated states with an accurate synthetic control.

Third, I add drug-related homicide rates to the set of outcome predictors (X). This robustness check incorporates potentially endogenous structural trends of drug-related homicide rates. Similarly, I include an indicator of the average density-distance to the nearest U.S. border bridge in the set of matching covariates (X). This test seeks to control for the presence of DTOs, which tend to locate closer to the U.S. border, along the drug-trafficking routes.

Table 2.3 shows the results for all four robustness tests. Overall, the original model is robust to different checks. Statistically-speaking, all t-tests fail to reject the null hypothesis of a zero difference in the average of the treatment effects across models. The previous brings confidence to the original economic model, as well as to the results obtained thus far.

2.4.2 Inference: Placebo Studies

As in most comparative case studies, the small number of treated states in the universe and the absence of randomization do not allow the application of large sample inferential techniques. These limitations are common when using the scientific solution to the FPCI, as opposed to the statistical solution. Therefore, to statistically validate my findings, I apply a couple of "placebo" studies, based on the results of running iteratively the two-step matching procedure on all donor units (previously performed above to obtain the median value of the NMSPE for the full sample.)

The set of synthetic controls for donor units allows to construct placebo effects by taking the outcome difference between untreated states and their respective synthetic controls. As previously suggested by Abadie et al. (2010), the distribution of placebo effects can be used for the statistical assessment of the treatment effects: If treated states are outliers in the placebo distribution, then the treatment effects are statistically significant (Abadie et al., 2010).

Figure 2.3 presents the results for the first placebo test, which consists in comparing the treatment distribution against the placebo distribution, at one point in time during treatment. For this inferential exercise, I drop all donor (and treated) states that attain a matching period NMSPE above the full sample median (7.6%), because these units do not provide reliable information. All gray, solid lines represent placebo effects; while dark, dashed lines depict treatment effects. I emphasize the GDP per capita gap for Chihuahua using a solid line. The shaded area in Figure 2.2 indicates the treatment period.

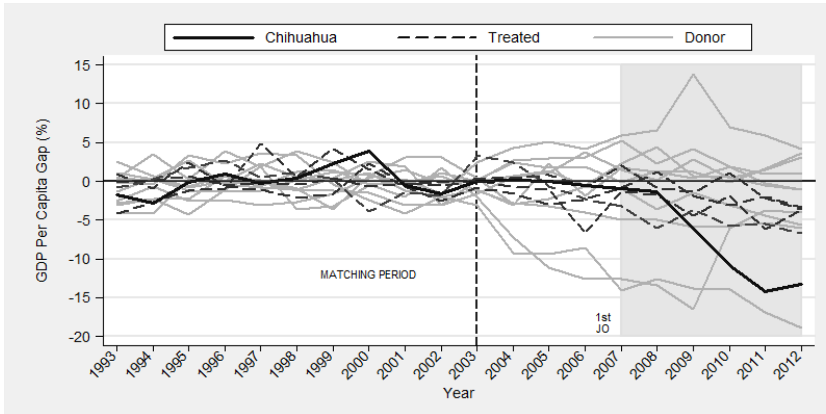

Figure 2.3: GDP per Capita Gap for Donor and Treated States with an Accurate Synthetic Control (MSPE<Median)

Seemingly, all treatment effects in this panel show an odd, negative behavior within the shaded area, in comparison to the placebo effects. By the end of the treatment period in 2012, five of the nine lowest GDP per capita gap lines are treated states. However, the binomial probability of this combination, under equal likelihood of outcomes, is only $\frac{9!}{5!\times 4!\times 2^9} = 0.246$, which does not let me infer causality on all of the treated states *together*, at conventional levels of confidence.

The second iterative placebo test builds p-values from the distribution of post/pre-treatment MSPE ratios to evaluate *individually* the significance of the treatment effects. I obtain the post-treatment MSPE from the squared values in equation (2.4). This placebo study includes those treated units with an accurate counterfactual, as well as all donor states.

Figure 2.4 presents the distribution of the post/pre-treatment MSPE ratios. The left panel plots those treated exposed to JOs in 2007, whereas the right panel shows states that became treated in 2008. For consistency, the placebo treatment period corresponds to the respective base year of the JOs in each of the panels.

Clearly, both distributions are skewed to the left, with the vast majority of donor states having ratios below 5. Conversely, Chihuahua, Durango, and Guerrero are all outliers in the distribution of ratios. Specifically, Chihuahua and Guerrero have the highest ratios at 19 and 43, respectively. The previous means that, under randomization, the probability of obtaining the highest ratio for either Chihuahua or Guerrero is $\frac{1}{20} = 0.05$. Consequently, the treatment effects for both of these states

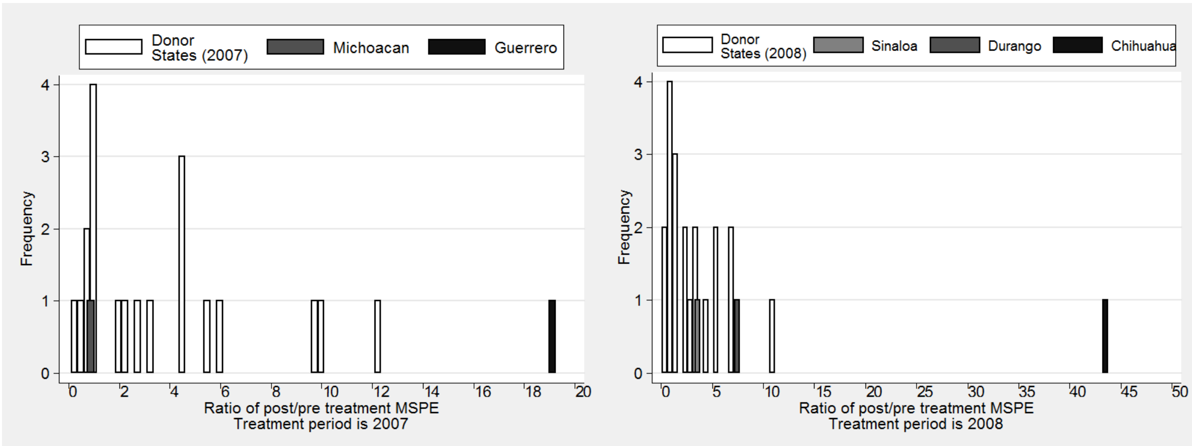

Figure 2.4: Distribution of Post/Pre Treatment MSPE Ratios by Rollout of JOs (2007 and 2008)

meet the conventional 5% level of confidence. The same logic applies to Durango, although this state presents an effect that is only statistically significant at the 10% level of confidence: $\frac{2}{20} = 0.10$. Finally, I cannot claim that findings for Michoacan and Sinaloa are statistically significant because neither of these treated states are outliers in the distribution of the post/pre-treatment MSPE ratios.

2.4.3 Causation: Effect of the Mexican Drug War on GDP Per Capita

Thus far, I have established that a spike in drug-related violence, driven partly by the Mexican Drug War, had an effect on the economy in those states where the federal government implemented JOs. Using placebo studies, I have also proven that, once treated, said effect is statistically significant for the majority of the treated states that display an accurate synthetic control. Moreover, in Section 1, I have acknowledged that the spike in drug-related violence was simultaneously provoked by two foreign confounding factors: the 2004 expiration of the U.S. Federal AWB, and a significant increase of cocaine seizure rates in Colombia after 2006. Therefore, the exogenous effect of drug-related violence on GDP per capita has not all come as a consequence of the Mexican Drug War.

To determine the direct causal effect of the Mexican Drug War on economic development, I run OLS on the variation of the normalized GDP per capita gap for the treated sample. Namely, the central model to evaluate the average treatment effect on the treated (ATT) is the following:

$$\hat{NG}_{s,t} = \alpha + \theta \hat{NG}_{s,t-1} + \beta JO_{s,t-1} + \gamma Z_{s,t-1} + \varepsilon_{s,t}, \tag{2.6}$$

where $\hat{NG}_{s,t}$ is the normalized GDP per capita gap, in percentage terms; $\hat{NG}_{s,t-1}$ is the lagged dependent variable, which controls for tendency; $JO_{s,t-1}$ is the lag value of my continuous proxy for the Mexican Drug War, the interaction term between the rollout of the JOs and the rate of interception operations; $Z_{s,t-1}$ is a vector with the lag values of the two aforementioned confounding factors —the interaction between the 2004 expiration of the U.S. Federal AWB and a dummy for AWB-bordering states (Chicoine, 2011; Dube et al., 2013), and cocaine seizure rates in Colombia (Castillo et al., 2012); and $\varepsilon_{s,t}$ are all other unobservables that influence the outcome. The coefficient of interest in equation (2.6) is β.

Alternatively, I include state fixed effects (λ_s) in equation (2.6) to control for possible systemic biases in the (normalized) GDP per capita gap, generated by the SCMs:

$$\hat{NG}_{s,t} = \lambda_s + \theta \hat{NG}_{s,t-1} + \beta JO_{s,t-1} + \gamma Z_{s,t-1} + \varepsilon_{s,t}. \tag{2.7}$$

However, the conditions for consistently estimating equation (2.7) are more complicated than OLS because, once state dummies are introduced, the error term ($\varepsilon_{s,t}$) becomes necessarily correlated with the lagged dependent variable ($\hat{NG}_{s,t-1}$). Following Angrist and Pischke (2009; ch.5), I apply Arellano and Bond's generalized method of moments procedure (ABGMM), which uses higher-lag values of the dependent variable as instruments, to solve for serial correlation.

Finally, I run an additional two-stage least squares (2SLS) model in which the indicator for the Mexican Drug War (JO_{t-1}), along with the two identified confounding variables (Z_{t-1}), enter equation (2.6) indirectly through exogenous drug-related violence:

$$\begin{gathered} \hat{NG}_{s,t} = \alpha + \theta \hat{NG}_{s,t-1} + \delta\, phomicides_{gap_{s,t-1}} + \varepsilon_{s,t} \\ homicides_{gap_{s,t-1}} = \alpha^F + \theta^F \hat{NG}_{s,t-1} + \pi JO_{s,t-1} + \gamma Z_{s,t-1} + \nu_{s,t-1}, \end{gathered} \tag{2.8}$$

where $phomicides_{gap}$ is the gap in drug-related homicide rates between treated and synthetic control units. In this specification, the parameter of interest is $\pi \times \delta$.

Established the mechanics of the minimization procedure in equations (2.2) and (2.3), I limit my sample observations from the end of the matching period onwards (2003-2012). I run equations (2.6), (2.7), and (2.8) for all treated states with a reliable synthetic control (Chihuahua, Durango, Guerrero, Michoacan, and Sinaloa), as well as for only those treated states that report a statistically significant GDP per capita gap (Chihuahua, Durango, and Guerrero). All together, my sample contains, at the most, 50 observations. Hence, equation (2.6) is more likely to provide the true ATT because OLS is consistent and unbiased for small samples, whereas the ABGMM and 2SLS estimators are only consistent in small-sample asymptotics (Angrist and Pischke, 2009; ch.4).

Table 2.4 presents the main results for the effect of the Mexican Drug War on GDP per capita gap, in percentage units. Columns 1 to 3 show the estimations for all treated units that have an accurate synthetic control, whereas columns 4 to 6 reduce the sample to treated states with a statistically significant GDP per capita gap. The last row in Table 2.4 presents the ATT of the Mexican Drug War on GDP per capita gap, in percentage terms. For most specifications, the coefficients for the Mexican Drug War and the confounding factors are statistically significant and move in the correct direction. What is more, there is little variation across estimators, implying no need for state dummies.

Given the properties of OLS and the number of states represented in the sample, my preferred specification is column 1. This specification explains around 69.1% of the outcome variation, and indicates a statistically significant ATT equal to -0.7% for Chihuahua, Durango, Guerrero, Michoacan, and Sinaloa, over the period 2003-2012. The 95% confidence interval of the ATT, under robust standard errors, is in the range of -1.4% and 0.4%. If there are zero spillovers, and a perfect linear relationship between the Mexican Drug War and GDP per capita, then an extrapolation of the ATT on all treated states amounts to a loss in GDP per capita equal to 0.5%, over the period 2003-2012. Given the share of treated states in Mexico's economy (over one-third), this is a considerable effect for a single policy, which partially explains the poor performance of

Table 2.4: Average Effect of the Mexican Drug War on GDP Per Capita Gap (%) between Treated States and Synthetic Controls (2003-2012)

	(1) OLS	(2) ABGMM	(3) 2SLS	(4) OLS	(5) ABGMM	(6) 2SLS
L p/Drug Homicide Gap			-0.097***			-0.102***
			(0.019)			(0.028)
L Roll x Intercep Ops	-10.366*	-11.604***		-18.078***	-18.949**	
	(5.494)	(3.912)		(6.402)	(7.891)	
L Colombia Seizures	-0.016	-0.006		-0.007	-0.000	
	(0.010)	(0.005)		(0.013)	(0.004)	
L Border x AWB	-1.999**	-2.105***		-1.730**	-1.726***	
	(0.864)	(0.585)		(0.785)	(0.438)	
L p/GDP Gap	0.605***	0.572***	0.251**	0.659***	0.645***	0.282
	(0.115)	(0.159)	(0.125)	(0.123)	(0.059)	(0.199)
FIRST-STAGE						
L Roll x Intercep Ops			125.944***			186.172***
			(34.312)			(70.983)
L Colombia Seizures			0.003			-0.023
			(0.062)			(0.100)
L Border x AWB			25.35**			20.257**
			(9.733)			(9.103)
L p/GDP Gap			-3.573***			-3.671***
			(1.114)			(01.296)
Restricted Sample	No	No	No	Yes	Yes	Yes
R-squared	0.691		0.710	0.828		0.795
Number of States	5	5	5	3	3	3
Observations	50	50	50	30	30	30
F-Stat Excluded Var			10.9			6.0
Drug War ATT (p.)	-0.70	-0.77	-0.82	-1.11	-1.16	-1.16

Notes: All regressions contain robust standard errors in parentheses.
Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level of confidence.

Mexico's economy during Calderon's administration.

2.5 Determinants

All of these results remain silent about the economic determinants by which the Mexican Drug War hampered economic development. Recent literature in the matter suggests a significant effect of the Mexican Drug War on the labor market. Specifically, the conflict provoked a fall of 1.5% in female labor participation rates, and a wage reduction for male workers in the informal sector equal to 2.3% (Dell, 2011). By the same token, BenYishay and Pearlman (2013) find a decrease in hours-worked equal to one unit per week as a consequence of the Mexican Drug War.

However, there is not any further empirical evidence on other possibly affected variables. In what follows, I measure the effect of the Mexican Drug War on two unexplored determinants for economic development: consumption and productive investment. Both of these variables are components of the GDP accounting equation, and contribute to economic growth by means of further production (Barro and Xavier Sala-i-Martin, 1995).

2.5.1 Consumption

The first potentially affected determinant for economic development is consumption. The logic for a possible decline in consumption as the result of the Mexican Drug War is as follows: If households become victims of drug-related violence, either directly through organized crime or indirectly through fear, then they are likely to hedge their exposure against further violence. Rational households may do so by changing their consumption behavior. In particular, households may avoid "risky consumption" activities like going out at night to have fun, or taking public transportation. These possible changes in consumption patterns erode the domestic market because economic resources, previously allocated to "risky consumption," are never spent in the same manner.

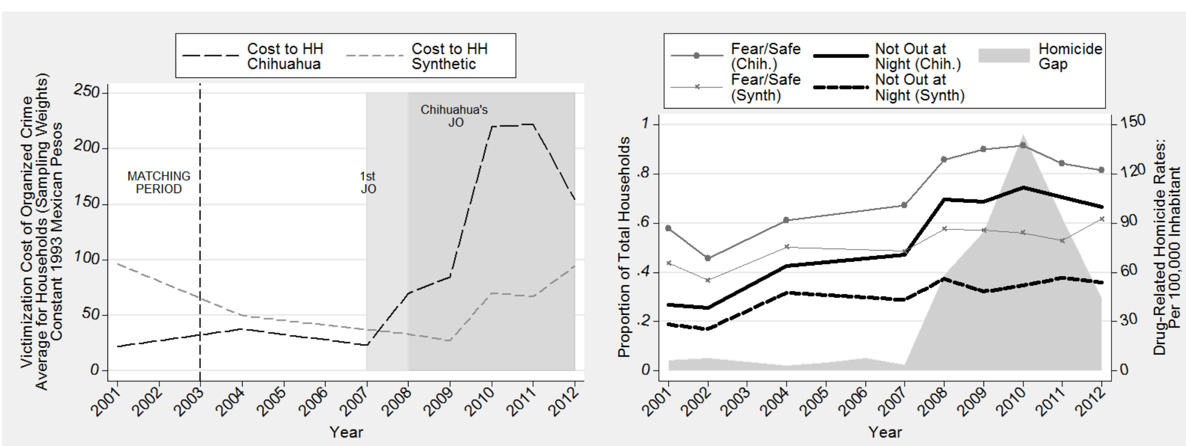

Figure 2.5: Mechanism for the Effect of the Mexican Drug War on "Risky" Consumption, for Chihuahua and Synthetic Chihuahua

To test for a decline in consumption, I use nine different cross-sectional waves of the Mexican Crime Victimization Survey, gathered by Mexico's Citizen Security Institute (ICESI) and INEGI.

Most of these surveys contain a representative sample of Mexico.[58] In addition, I collect aggregate records for local savings from Mexico's Federal Banking Regulator (CNBV) and the Central Bank of Mexico (BANXICO) to approximate aggregate consumption per capita, which is not available at the state level.[59] This information, together with the previously obtained weights ($W^{\star}$) from equations (2.2) and (2.3), allow me to build a panel database for treated states and their respective synthetic controls,[60] containing the mean victimization cost, fear for personal safety, average changes in "risky consumption", and savings per capita (as a measure of aggregate consumption per capita.)

Figures 2.5 and B.2 present graphically the mechanism for a potential drop in consumption, for treated states with a reliable synthetic control. The left panels include the pooled mean victimization cost of extortion, kidnapping, and motor vehicle theft for treated and synthetic control units, in 1993 Mexican Pesos (dashed lines). Similarly, the right panels plot fear for personal safety (shaped-lines) and changes in "risky consumption" activities like going out at night (darkest lines), both as percentages of the population.

Clearly, mean victimization cost and fear for personal safety increases radically for all treated states, in relation to synthetic controls. Because households internalize directly and indirectly drug-related violence, "risky consumption" (e.g. going out at night) decreases. Immediately visible is the strong dynamic relation between fear for personal safety and "risky consumption."

Table 2.5 presents numeric evidence for the effect of the Mexican Drug War on mean victimization cost gap, fear for personal safety gap, and savings per capita gap between treated and synthetic control units. Specifically, I run equations (2.6) and (2.8) for the normalized values of the aforementioned variables, in percentage terms. All specifications restrict the sample to those

[58] Specifically, ENSI-1 (2001) has a non-representative sample of 35,001 observations, ENSI-2 (2002) a non-representative sample of 35,174 households, ENSI-3 (2004) a representative sample of 66,000 households, ENSI-5 (2007) a non-representative sample of 44,977 households, ENSI-6 (2008) a representative sample of 71,370 households, ENSI-7 (2009) a representative sample of 73,324 households, ENVIPE-1 (2010) a representative sample of 78,179 households, ENVIPE-2 (2011) a representative sample of 95,903 households, and ENVIPE-3 (2012) a representative sample of 95,810 households. All surveys contain sampling weights.

[59] Aggregate savings per capita is inversely related to aggregate consumption per capita.

[60] Optimal weights for synthetic controls ($W^{\star}$) should remain valid for the comparison of household's aggregate consumption because the economic model in Section 3 incorporates demographic variables like human capital and population density.

Table 2.5: Average Effect of the Mexican Drug War on Victimization Cost Gap (%), Fear for Safety Gap (%), and Savings Rates Gap (%) between Treated States and Synthetic Controls (2003-2012)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	OLS	2SLS	OLS	2SLS
L p/Drug Homicide Gap		2.253*		9.030*		0.219*
		(1.185)		(5.452)		(0.126)
L Roll x Intercep Ops	261.756		446.357		66.175**	
	(206.918)		(2076.954)		(30.079)	
L Colombia Seizures	1.906*		0.673		-0.048	
	(1.082)		(4.187)		(0.095)	
L Border x AWB	47.179		633.421*		-18.089**	
	(37.229)		(323.773)		(8.134)	
L Victim Cost Gap	0.046	0.076				
	(0.053)	(0.064)				
L Fear-Safe Gap			0.363***	0.358***		
			(0.088)	(0.082)		
L p/Savings Gap					0.869***	1.075***
					(0.089)	(0.074)
Dependent Var	Cost	Cost	Fear-Safe	Fear-Safe	Savings	Savings
R-squared	0.352	0.074	0.463	0.415	0.887	0.846
Observations	30	30	30	30	45	45
F-Stat Excluded Var		20.8		23.2		8.3
Drug War ATT (p.)	65.04	30.32	29.75	118.61	4.41	2.92

Notes: All regressions contain robust standard errors in parentheses. Mean victimization cost and fear for personal safety are factored by inverse survey sampling weights. Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level of confidence.

treated states with a reliable synthetic control (Chihuahua, Durango, Guerrero, Michoacan, and Sinaloa), for the non-matching period (2003-2012).[61] For brevity, I exclude the first stage of the 2SLS estimator.

Findings indicate a statistically significant increase in mean victimization cost, although the spike in “wealth losses” is not proportional to the expansion in drug-related violence. Conversely, the percentage of the population feeling fearful for their personal safety in treated states increases by twofold, compared to synthetic controls. Therefore, during treatment, households internalize drug-related violence indirectly through fear, rather than directly through “wealth losses.” As a result of an increase in mean victimization cost and fear, the gap in savings per capita between treated states and synthetic controls increases by 4.4%, which means that aggregate consumption per capita declines simultaneously.

[61] To control for tendency in mean victimization cost gap and fear for personal safety gap, I take the values from 2002 in lieu of 2003 because this latter year is not available in the Mexican Crime Victimization Survey.

Extrapolating the results in Table 2.5 to all 11 treated states reduces the ATT on savings per capita to 2.9%. Considering that savings and consumption rates during the pre-treatment period (1993-2003) for all treated states are 12.6% and 68.3% of the GDP, respectively, the ATT of the Mexican Drug War on aggregate consumption per capita is equal to -0.5%. This effect is proportional to the effect of the Mexican Drug War on the GDP per capita.

2.5.2 Productive Investment

Another possibly affected determinant for economic development is productive investment. To proxy for this determinant, I use data for commercial credit granted to businesses from CNBV and BANXICO instead of gross domestic investment records, because the latter is not available yearly at the state level. Even if commercial credit granted to businesses is not a perfect proxy for gross domestic investments, the fraction of Mexico's capital market controlled by commercial financial intermediaries is crucial to the economy: According to the 2010 National Survey of Financial Competitiveness, conducted by CNBV and the Inter-American Development Bank, about 33% of all formal Mexican enterprises maintain banking loans at any point in time.[62] Most important, 24% of total commercial credit granted to the businesses goes towards investment.

Established the influence of bank lending on private investment, I examine a potential decline in commercial credit (non-consumption and non-mortgage) granted to the private sector as a consequence of the Mexican Drug War. Bonaccorsi di Patti (2009) proposes two reasons for a decline in commercial credit granted to businesses after a spike in drug-related violence: 1) The bank's inability to asses the quality of borrowers because of an uncertain propensity to victimization; and 2) a decrease in trust among parties in the domestic financial market.

The previous reasons do not apply to the local public sector because local governments can ultimately be bailed out by the federal government (e.g. lender of last resort). Subsequently, credit could move to the local public sector if the demand for it exists. Notwithstanding this possible substitution effect, productive investment would still decline because public investment, in contrast

[62] As a caveat, the 2010 National Survey of Financial Competitiveness includes loans from development and foreign banks, which hold less than 16% of the capital market.

to private investment, has not been a productive input for GDP since 1982, when Mexico adopted several privatization and decentralization reforms (see Ramirez, 2010.)

Table 2.6 contains the result for the effect of the Mexican Drug War on commercial credit (non-consumption and non-mortgage) by sector. Using again the optimal weights ($W^{\star}$) from above, I run equations (2.6) and (2.8) for the gap in commercial credit per capita granted to the private sector, the gap in commercial credit per capita granted to the public sector, and the gap in private-to-total credit ratio between treated and synthetic control units, in normalized terms. Just as in Tables 2.4 and 2.5, I limit my sample to those treated states with a reliable synthetic control, for the period 2003-2012.

Table 2.6: Average Effect of the Mexican Drug War on the Gap in Commercial Credit Per Capita Granted to the Private Sector (%), the Gap in Commercial Credit Per Capita Granted to the Public Sector (%), and the Gap in Private-to-Total Credit Ratio (%) between Treated States and Synthetic Controls (2003-2012)

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
L p/Drug Homicide Gap		-0.351*		0.659		-0.054
		(0.208)		(0.450)		(0.116)
L Roll x Intercep Ops	-37.775		83.970		20.186	
	(63.169)		(154.758)		(42.871)	
L Colombia Seizures	-0.004		-0.063		-0.167	
	(0.209)		(0.422)		(0.112)	
L Border x AWB	-19.582		44.751		-3.539	
	(14.806)		(33.077)		(5.552)	
L Priv Credit Gap	0.799***	0.808***				
	(0.111)	(0.096)				
L Pub Credit Gap			0.888***	0.834***		
			(0.134)	(0.126)		
L Priv Ratio Gap					0.698***	0.681***
					(0.073)	(0.069)
Dependent Var	Priv Credit	Priv Credit	Pub Credit	Pub Credit	Priv Ratio	Priv Ratio
R-squared	0.843	0.839	0.706	0.682	0.742	0.732
Observations	45	45	45	45	45	45
F-Stat Excluded Var		12.5		12.1		11.2
Drug War ATT (p.)	-2.52	-4.92	5.60	9.42	1.35	-0.75

Notes: All regressions contain robust standard errors in parentheses.
Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level of confidence.

The first two columns indicate a significant loss in commercial credit per capita granted to the private sector as high as 4.9%, as consequence of the Mexican Drug War. Expanding these results to all treated states reduces the ATT to -3.2%. According to records from BANXICO and the World Bank, commercial credit (non-consumption and non-mortgage) granted to businesses during the pre-treatment period accounts for 6.4% of GDP, whereas private gross domestic investment during the same period of time amounts to 18.5% of GDP.[63] Provided that 24% of all commercial credit granted to business goes towards investment, the ATT on productive investment for all treated states is equal to -0.3%. This effect, however, does not account for any changes in privately owned-capital investment.

Finally, there is a positive effect of the Mexican Drug War on commercial credit per capita granted to the public sector, even though the ATT is not statistically different from zero. In fact, there is little or no change on the private-to-total credit ratio, suggesting a null credit substitution effect. All of these findings are consistent with the evidence for a lower access to credit in Italy as a consequence of organized crime, found by Bonaccorsi di Patti (2009).

2.6 Conclusion

The main results in this chapter suggest a significant effect of the Mexican Drug War on GDP per capita for treated states equal to -0.5%, over the period 2003-2012. Economic determinants by which the Mexican Drug War hampered economic development include a proportional reduction in consumption, and a decline in productive investment equal to 0.3%. This latter determinant is driven by a drop of 3.2% on commercial credit (non-consumption and non-mortgage) per capita granted to the private sector as a consequence of the military conflict.

The results above reinforce the criticism from many academics and human rights activists against the military strategy implemented by President Calderon. Namely, President Calderon should have been more prudent in using the Mexican Army to conduct activities that belong to the police. Evidently, President Calderon did not calculate the unintended consequences of decapitating DTOs, prior to launching his military strategy. The negative outcomes of Mexico's failed drug war are palpable in the economy, as well as in the social fabric.

[63] These percentages are roughly equal across states.

3 BREAKING SAD: DRUG-RELATED HOMICIDES AND MENTAL WELL-BEING IN MEXICO

The level of violence recently exerted by Mexican drug-trafficking organizations (DTOs) surpasses that of many international armed conflicts. Decapitations, mass executions, and hanging of bodies are now common events in many parts of Mexico (Bunker, Campbell, and Bunker, 2010). Beyond any rhetoric from national and international media, drug-related violence has become a visible phenomena in public spaces. Although expressions of drug-related violence have always occurred sporadically in Mexico, the frequency and brutality of the violence that prevailed between 2007 and 2012 are incomparable to previous periods.

Such rates of violence, usually generated by war, are known to have a negative impact on mental well-being. For instance, Scholte et al. (2004) find an immediate increase in depression among Afghans after the launching of Bush's "War on Terror." Similarly, de Jong et al. (2003) and Priebe et al. (2010) observe a long-lasting significant effect of war exposure on anxiety, depression, and post-traumatic stress disorders for individuals living in Algeria, Cambodia, Ethiopia, Palestine, and the former Yugoslavia.

This chapter is a first attempt to estimate the effect of drug-related violence on depression among adults in Mexico, amid a conflict known as the "Mexican Drug War".[64] To infer causality, the empirical design consists mainly of first-differences in aggregate health outcomes at the municipality level before and after the beginning of the conflict. In addition, I employ two time-varying instrumental variables (e.g changes in net cocaine supply in municipalities with a drug-trafficking route and differences in federal-local enforcement cooperation) to account for potential endogene-

[64] A recent working paper by Michaelsen (2012) analyzes the effect of mental health on labor outcomes between 2002 and 2005, using changes in state-level homicide rates as instrumental variables. Although Michaelsen's approach is novel, the first-stage in her analysis misses the spike in drug-related violence (2007-2012). In this paper, I focus exclusively on the effect of drug-related violence on depression during the Mexican Drug War, while accounting for potential endogeneity between drug-related violence and depression.

ity issues.

Preliminary results suggest a statistical significant increase in clinical and non-clinical depression among women as consequence of drug-related violence. In stark contrast, men in Mexico seem largely unaffected by drug-related violence as far as mental health outcomes concern. Future versions of this paper will expand on explanations for gender difference in the treatment effect.

The chapter proceeds as follows. Section 1 describes the identification theory, and explains the relevant features of the Mexican Drug War. Section 2 provides the foundations for the empirical strategy to estimate the treatment effect. Section 3 presents preliminary results. Section 4 analyzes the robustness of the suggested findings. Finally, Section 5 discusses the economic implications of the preliminary results.

3.1 Identification Theory

3.1.1 Structural and Foreign Factors

Between 2004 and 2006, prior to the beginning of the conflict, Mexican DTOs expanded their activities significantly, even though homicide rates were at a historical low. This expansion in drug-trafficking activity was the result of several structural and foreign factors. Among the structural factors, there were three important events that fostered organized crime before the launching of Mexican Drug War.

First, U.S.-Mexico cooperation on law enforcement grew constantly after the murder of Enrique Camarena, an American drug-enforcement agent, in 1985. As a result of this multilateral cooperation, the Mexican government captured a few drug-lords. Even though these criminals continued to conduct business as usual from Mexican jails, the intensification of law enforcement began to change the industrial organization of drug-trafficking services towards more competition (Toro, 1995). Second, during the early 1990s, the U.S. government boosted crackdowns on cocaine shipments along the Caribbean-trafficking corridor (Toro, 1995), incentivizing cocaine-trafficking routes to shift towards Mexico (the “balloon effect”). Third and last, after the election of Vicente Fox in 2000, Mexico experienced a rapid democratization process, which left severe power vacuums as political decisions became increasingly decentralized (Osorio, 2012). The absence of a strong centralized State complicated a *pax narcotica*, the "explicit" coordination between DTOs and the State to maintain drug-related violence to a minimum (Osorio, 2012).

Additionally, there were two foreign events that developed simultaneously over the structural context. First, arguments on preserving the Second Amendment of the U.S. Constitution to its broadest interpretation led to the expiration of the U.S. Federal Assault Weapons Ban (AWB) in 2004. Lax gun regulations, just across the border, allowed Mexican DTOs to purchase semi-automatic weapons more easily (Chicoine, 2011; Dube et al., 2013). Second, after 2006, there was a dramatic shift in net cocaine supply from Colombia, provoked by increasing seizure efforts from the Colombian government (Castillo et al., 2014). Less total output in a market with an inelastic demand translated into higher rents for DTOs. The absence of property rights over these additional rents induced DTOs to exert violence as means of appropriation (Castillo et al., 2014).

3.1.2 Joint Operations and Local Enforcement Coordination

In response to increasing drug-trafficking activity, President Felipe Calderon took the decision of combating DTOs, using the Mexican Army. Specifically, the main goal of the military policy was to capture the most-wanted drug-lords. In total, the Mexican Army killed or captured 26 of the 37 targets during the Mexican Drug War.

However, instead of discouraging criminal activity, the military strategy caused a "hydra effect," attributable to the organizational structure of DTOs. In stark contrast to Colombian cartels, Mexican DTOs organized internally within cells, rather than as a vertical hierarchy.[65] Once the Army removed the central management of DTOs, cells within and across DTOs fought for the control of assets. Consequently, the number of DTOs went from six to 16 in a matter of five years (Guerrero, 2011).

At the beginning of the Mexican Drug War, the military strategy also sought to maintain cooperation with local enforcement institutions (e.g. state and municipal). Hence, military operations

[65]The main functions of the drug-lords were to coordinate all cells as franchises across trafficking-routes, and to maintain contracts with foreign supplier of cocaine and ephedrine.

became known as *"joint operations"* (JOs). In total, President Calderon launched JOs in 11 different states.[66]

Ultimately, politics became a factor for cooperation, mainly because of trust issues. Municipalities governed by PAN, President's Calderon political party, experienced greater cooperation than municipalities governed by PRI (or PRD). Although at first this translated into higher rates of violence for PAN-governed municipalities, empirical evidence shows "a diversion of drug traffic" away from PAN-governed municipalities (Dell, 2011). Hence, shifting some of the violence towards non-PAN-governed municipalities.

Worse yet, President Calderon dismantled many local enforcement institutions, and did not maintain political operators in raided municipalities governed by non-PAN. As a result, violence and chaos remained in non-PAN-governed municipalities for longer periods of time. A comparison often drawn by President Calderon, himself, was between Tijuana and Juarez, two border urban municipalities. Whereas the former municipality was governed by PAN, the latter municipality maintained a PRI mayor in office. Not surprising, President Calderon claimed "success" for his military strategy in Tijuana, whereas Juarez became the most violent city in the world during the Mexican Drug War (Associate Press, 2010).[67]

Provided the organizational structure of DTOs and the lack of political operators across the country, the military policy resulted in a counterproductive strategy. Figure 3.1 shows how drug-related homicide rates (solid and dashed lines) spiked tremendously after the launching of the Mexican Drug War (shaded area). Namely, rates rose from five drug-related homicides per 100,000 inhabitants, in 2006, to 16 drug-related homicides per 100,000 inhabitants, in 2012. Although foreign factors like net cocaine supply (lines with circles) also contributed to the spike in violence, empirical evidence suggest that the military strategy (droplines with crosses) was the main driver

[66] Some of these JOs occurred at the very beginning of the sexenium term, in 2007 (e.g. Baja California, Guerrero, and Michocan); some others in the middle of the administration, in 2008 (e.g. Chihuahua, Durango, Nuevo Leon, Sinaloa, and Tamaulipas); and the rest towards the very end of Calderon's presidency, in 2011 and 2012 (e.g. Coahuila, Morelos, and Veracruz).

[67] Specifically, Calderon said that "[i]n [...] Juarez, unfortunately, there has not been the same degree of collaboration and constructive attitude that we have found in other places, like Tijuana. [...] Instead of everyone working together, they [PRI] preferred the easy way out by blaming everything on the federal government [PAN] and the president" (Associate Press, 2010).

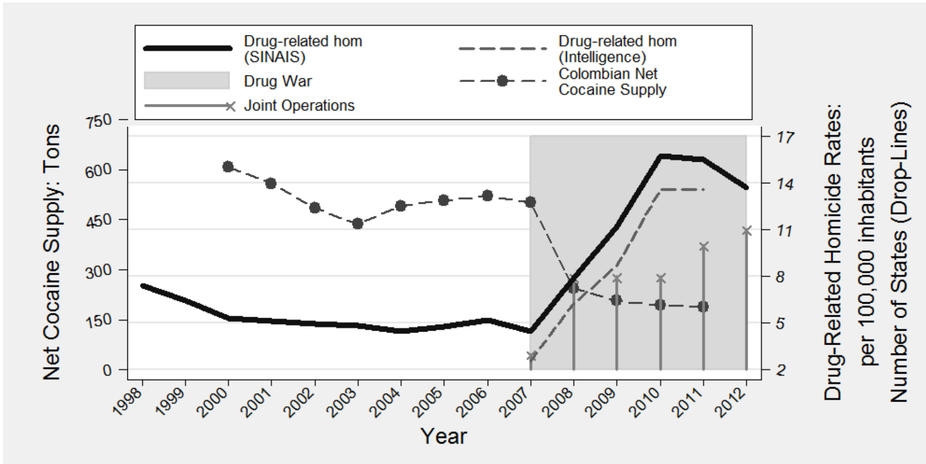

Figure 3.1: Evolution of Drug-Related Homicides

of the shock. Using a variety of methodologies (e.g. propensity score matching, regression discontinuity designs, and synthetic control methods), the existing literature finds a marginal effect of the Mexican Drug War on drug-related homicide rates between 46.9% and 52.4% (Dell, 2011; Merino, 2011; Calderon et al., 2012).

3.2 Empirical Strategy

3.2.1 Methodology

The primary objective of this chapter is to assess whether drug-related violence had an impact on depression among adults during the Mexican Drug War. To achieve the goal of this chapter, the empirical design consists in comparing aggregate mental health outcomes at the municipality level before and after the spike in drug-related homicide rates; hereinafter, pre-treatment and treatment period. Given the two-period empirical design of this chapter, the central model is an equation in first-differences:[68]

$$\triangle Y_m = \alpha + \beta \triangle T_m^L + \gamma \triangle X_m + \triangle \varepsilon_m, \tag{3.1}$$

where $\triangle Y_m$ is the change in depression outcomes between the pre-treatment and treatment pe-

[68] The first-differences estimator is identical to the within-estimator in the two-period case of the fixed-effects model.

riod, in municipality m; $\triangle T_m^L$ is the first difference in the lag values of yearly drug-related homicide rates between the pre-treatment and treatment period, for each municipality; $\triangle X_m$ are fluctuations in observable municipality characteristics; and $\triangle \varepsilon_m$ is the error term. The main parameter of interest, the average treatment effect (ATE) of drug-related homicide rates on depression, is β. I use ordinary least squares (OLS) to compute the magnitude and standard errors of the ATE in equation (3.1).

Notwithstanding the advantages of first-differences regressions over cross-sectional analysis, equation (3.1) does not necessarily capture the true treatment effect because changes in depression and drug-related homicide rates could be caused by a third dynamic variable. For instance, recent literature contests a significant shift in migration patterns, motivated by drug-related violence. Specifically, Rios (2013) finds a gap of over 264,000 individuals, nationwide, between the population census and the demographic forecast by Mexico's Population Agency (CONAPO) for 2010. Based on records from the American Community Survey, Areceo-Gomez (2013) also suggests a new wave of wealthy well-educated Mexicans leaving the country as a consequence of drug-related violence. Using longitudinal survey data (MXFLS), Velasquez (2014) further predicts selective internal and international migration among self-employed men and single women living in troubled areas.

Insofar as migration occurs more frequently among those individuals with mental health distress, equation (3.1) provides a lower-bound of the ATE because $Cov\left(\triangle T_m^L, \triangle \varepsilon_m\right) \leq 0$. Conversely, if depressed individuals are less likely to migrate because of their mental health condition, then equation (3.1) provides an upper-bound of the ATE because $Cov\left(\triangle T_m^L, \triangle \varepsilon_m\right) \geq 0$. I address this migration endogeneity problem by introducing two instrumental variables: 1) the interaction of net cocaine supply from Colombia and the presence of a drug trafficking route (DTR) in a municipality, and 2) the interaction of a non-PAN-governed municipality dummy and the launching of a JO in any given state. As discussed in Section 1, the first instrument is inversely correlated to drug-related homicide rates (more net cocaine supply leads to less violence,) whereas the latter instrument is positively correlated with drug-related homicide rates (less enforcement coordination and the hydra effect lead to more violence.) Although hardly testable, it is unlike for both of these instruments to affect depression, other than through more drug-related violence.

Therefore, so long as the aforementioned instrumental variables are strong and exogenous, I am able to estimate the true effect of drug-related homicide rates on depression by conducting two-stage least squares (2SLS) in the first-differences model above:

$$\begin{aligned} \triangle Y_m &= \alpha + \beta \triangle T_m^L + \gamma \triangle X_m + \triangle \varepsilon_m \\ \triangle T_m^L &= \alpha^F + \pi \triangle Z_m^L + \gamma^F \triangle X_m + \triangle \varepsilon_m^F, \end{aligned} \tag{3.2}$$

where the first stage (F) incorporates the vector $\triangle Z_m^L$, which contains the first difference in the lag values of both instrumental variables.[69] To control for non-linearity, I include a second-order polynomial of drug-related homicide rates in equation (3.1) and (3.2), for some of the specifications. Finally, following the medical and economics literature, I present estimations by gender (Piccinelli and Wilkinson, 2000).

3.2.2 Data

To measure aggregate depression among adults, I use the National Health and Nutrition Survey (ENSANUT) for 2006 and 2012, the only available waves.[70] This repeated cross-sectional survey provides a timely framework to estimate the full effect of drug-related homicide rates on depression at the municipality level. Namely, I consider the 2006-wave as the pre-treatment period, whereas the 2012-wave serves as the treatment period (see Figure 3.1.)

ENSANUT contains four variables to capture the prevalence of depression among individuals older than 19 years of age. The first variable is a dummy for clinical depression (ever in lifetime);[71] the second variable is measure for "having felt sad for several days in the last week"; the third variable is an indicator for "currently feeling depressed"; and the fourth variable captures whether an

[69] Under the standard exclusion restriction: $Cov\left(\triangle Z_m^L, \triangle \varepsilon_m \mid \triangle X_m\right) = 0$.

[70] Mexico's Ministry of Health began collecting ENSANUT in October of the preceding year (2005 and 2011), and concluded all survey gathering in May of the base year.

[71] Diagnosis by a medical professional.

individual is "currently taking antidepressants". Additionally, ENSANUT provides several socio-economic indicators like crime victimization, ethnicity, education, health insured status, and labor outcomes.

The size of the cross-sectional samples are 45,240 and 46,277 adults for 2006 and 2012, respectively. To avoid mortality selection biases, I exclude adults older than 65 years of age.[72] What is more, given the aggregate level of the empirical analysis, I only keep observations for individuals living in municipalities that appear in both waves. The previous selection reduces the sample to 29,990 adults for 2006, and to 33,103 adults for 2012. After collapsing mental health outcomes and socio-economic information by municipality, the sample becomes a panel of 368 municipalities.[73] Although the number of municipalities in the sample represents a tiny fraction for the more than 2,450 municipalities in Mexico, these 368 municipalities amasses over two-thirds of the total Mexican population.

To determine drug-related homicide rates at the municipality level, I utilize Mexico's Mortality Databases from the Bureau of Health Statistics (SINAIS). These databases contain the universe of homicides for the period 1998-2012, along with detail information about the method of killing and the crime scene. I employ murders caused by gunshots, decapitations, and hangings as proxies for drug-related homicides. As shown in Figure 3.1, records from SINAIS move parallel to the officially identified drug-related homicide rates from the Mexican Intelligence Agencies. One disadvantage of official data for drug-related homicides is that these are only available from December, 2006 to September, 2011; thus missing the pre-treatment period. Consequently, I conduct my analysis using SINAIS data, exclusively.

To build cocain net supply data, as part of the identified instrumental variables, I gather information for cocaine production and cocaine confiscation in Colombia from the United Nations World Drug Report and Colombia's Ministry of Defense, accordingly. Net cocain supply data is available from 2000 to 2011. To assign DTR dummies across municipalities, I employ records of

[72] Older population accounts for 11% of the surveyed adult population.

[73] On average, the number of surveyed adults per municipality, by gender is 37.21 observations for males and 48.51 observations for females, for each of the survey waves. Consequently, there is more precision in municipality estimations for females than for males.

drug-shipment confiscations from Mexico's Ministry of Defense, for the period 2007-2011. Last, CIDAC's electoral database distinguishes municipalities where non-PAN mayors hold office, for the immediate electoral year that precedes the implementation of JOs.

3.2.3 Descriptive Statistics and Sources of Exogeneity

Table 3.1: Descriptive Statistics (Means) of Municipalities by Gender

		Means at the Municipality Level†			
		Females		Males	
		Pre-Treat (2006)	Treat (2012)	Pre-Treat (2006)	Treat (2012)
Treatment	Lag Drug-Related Homicide Rates‡	4.51	17.60	4.51	17.60
Outcomes	Clinical Depression (Ever in Lifetime)	16.90	16.53	5.12	5.80
	Sad for Several Days (Last Week)	40.78	46.69	22.33	28.20
	Currently Feeling Depressed	14.31	17.38	8.29	6.50
	Currently Taking Antidepressants	2.54	2.83	0.76	0.87
Covariates	Victim of Crime (Last 12 Months)	1.29	2.91	2.29	3.66
	Population Growth⋆	1.36	1.27	1.36	1.27
	Average Age	38.45	39.97	38.74	39.71
	Indigenous (Speaking) Population	4.27	4.03	4.08	3.85
	Health Insured	54.51	78.46	53.63	71.38
Instruments	Lag Cocaine Supply x DTR‡	374.93	139.90	374.93	139.90
	Joint Operations x Non-PAN-Governed‡	0	0.23	0	0.23

† Means are weighted by inverse population weights. N=368. ‡Pooled average at the municipality level.

Table 3.1 contains the average values of the treatment indicator, mental health outcomes, socio-economic covariates, and instrumental variables by gender and time period. Immediately noticeable is the differences in mental health outcomes across gender groups. In conformity with the existing literature, females report higher rates of depression than males for both time periods (Piccinelli and Wilkinson, 2000).

Across treatment periods, depression seems to increase for almost all proxies of depression. Similarly, crime victimization rates expand during the treatment period, although not nearly as much as drug-related homicide rates. The previous implies that, during the peak of the conflict, DTOs internalized the bulk of drug-related violence, directly.

Demographic covariates show a declining growth in population size, an aging adult population, and less indigenous (speaking) Mexicans across time, for both genders. Also, health insured rates rise dramatically after the 2007 expansion of "Seguro Popular," an universal health care program for disadvantaged households. This expansion in health insured rates is much greater for females than for males.

Table 3.2: Pre-Treatment Statistics by Instrument's Compliance Groups

Panel A. Pre-Treatment (2006) Difference in Means by Presence of a DTR: T-Tests

	Females			Males		
	DTR	No DTR	Difference	DTR	No DTR	Difference
Lag Drug-Rel. Homicide Rates‡	**4.58**	**4.33**	**0.24**	**4.58**	**4.33**	**0.24**
Clinical Depression (Ever in Life)	16.11	19.10	-2.99	5.11	5.13	-0.02
Sad for Several Days (Last Week)	41.28	39.37	1.91	23.11	20.16	2.95
Currently Feeling Depressed	14.55	13.66	0.89	8.71	7.10	1.61
Currently Taking Antidepressants	2.90	1.54	1.36***	0.79	0.69	0.10
Victim of Crime (Last Year)	1.37	1.09	0.28	2.13	2.75	-0.61
Population Growth	1.57	0.76	0.81***	1.57	0.76	0.81***
Average Age	38.56	38.17	0.39	38.65	38.98	-0.33
Indigenous (Speaking) Population	4.06	4.87	-0.81	3.87	4.68	-0.81
Health Insured	56.17	49.83	6.34**	54.99	49.80	5.19**
Lag Cocaine Supply x DTR‡	508.45	0.00	508.45***	508.45	0.00	508.45***

Panel B. Pre-Treatment (2006) Difference in Means by Non-PAN-Governed with a JO: T-Tests

	Females			Males		
	Non-PAN x JO	PAN or no JO	Difference	Non-PAN x JO	PAN or no JO	Difference
Lag Drug-Rel. Homicide Rates‡	**6.03**	**4.05**	**1.98**	**6.03**	**4.05**	**1.98**
Clinical Depression (Ever in Life)	12.90	18.12	-5.22***	4.47	5.32	-0.85
Sad for Several Days (Last Week)	38.80	41.39	-2.59	22.06	22.41	-0.35
Currently Feeling Depressed	12.84	14.77	-1.93	8.75	8.15	0.60
Currently Taking Antidepressants	2.25	2.63	-0.38	0.57	0.82	-0.25
Victim of Crime (Last Year)	1.06	1.37	-0.31	1.85	2.43	-0.58
Population Growth	1.48	1.32	0.16	1.48	1.32	0.16
Average Age	38.47	38.45	0.02	38.93	38.68	0.25
Indigenous (Speaking) Population	1.89	5.00	-3.11***	2.01	4.72	-2.71***
Health Insured	58.28	53.35	49.94*	57.35	52.48	4.87*
Lag Cocaine Supply x DTR‡	488.47	340.08	148.39***	488.47	340.08	148.39***

†Means are weighted by inverse population weights. Difference in means contain robust standard errors. N=368.
‡Pooled average at the municipality level. Connotations *, **, and *** mean significantly different from zero at the 90p, 95p, and 99p confidence level.

Finally, both instrumental variables move in accordance with the identification theory. In particular, during the treatment period, there is less net cocaine supply from Colombia and less enforcement coordination through the implementation of JOs in non-PAN-governed municipalities. Still, Table 3.1 does not test for instrumental exogeneity.

To verify the validity of the instrumental variables, Table 3.2 reproduces the previous statistics for the pre-treatment period, by instrument's complaince groups. The top panel presents statistics by the presence of a DTR across municipalities; while the bottom panel shows statistics by non-PAN-governed municipalities with a JO. The columns of interest in Table 3.2 contain the t-test difference in means between complaince groups.

Table 3.2 presents three crucial results for the identification strategy. First and most important, pre-treatment drug-related homicide rates (in-bold) are statistically indistinguishable between compliance groups, for both instruments. This result is the main source of exogeneity. Second, prior to treatment, mental health outcomes are practically the same, even though there might be a possible trend for females taking antidepressants in municipalities with a DTR. Third and last, compliers for both instruments have a smaller indigenous population (usually poor), and a bigger formal economy, approximated by health insured rates before the expansion of "Seguro Popular."

3.3 Preliminary Results

3.3.1 Clinical Depression

In Table 3.3, I present the main results for the effect of drug-related homicide rates on clinical depression (ever in lifetime) among adults. Columns 1 to 4 show estimations for females, while columns 4 to 8 present effects for males. The first two columns for each gender group correspond to linear estimations, whereas the last two columns for each gender group incorporate a second-order polynomial of drug-related homicide rates.

A rapid inspection of the main results suggests a significant linear effect of drug-related violence on clinical depression among females. In stark contrast, Mexican men are largely unaffected by drug-related violence as far as clinical depression concerns. Hence, I describe the different specifications of Table 3.3 for females, exclusively.

Table 3.3: Average Effect of Drug-Related Violence on Clinical Depression (Ever in Lifetime)

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
D Homicide	0.032*	0.097*	0.065	0.485	0.009	-0.035	0.062	-0.741
	(0.017)	(0.057)	(0.066)	(0.834)	(0.015)	(0.036)	(0.050)	(0.628)
D Homicide Sqr.			-0.000	-0.004			-0.000	0.007
			(0.000)	(0.008)			(0.000)	(0.007)
Joint Significance			[1.61]	[1.75]			[0.89]	[1.88]
Joint P-Value			0.201	0.417			0.412	0.391
FIRST-STAGE								
D Cocaine x DTR		-0.023***		-1.426**		-0.022***		-1.453***
		(0.006)		(0.545)		(0.006)		(0.532)
D non-PAN x JO		27.221***		2920.781*		27.253***		2919.909*
		(9.455)		(1519.394)		(9.243)		(1489.024)
Gender	Female	Female	Female	Female	Male	Male	Male	Male
R-squared	0.092	0.068	0.093	-0.038	0.039	0.012	0.044	-1.493
Municipalities	368	368	368	368	368	368	368	368
F-Stat Excluded		13.9		0.9		13.6		0.8
Chi-p: OLS-2SLS		0.145		0.320		0.227		0.096

Notes: All regressions contain robust standard errors in parentheses. Estimations factored by population weights. Joint test for non-linear models correspond to F-stats (OLS) and Chi-square stast (2SLS). Controls include first-differences in victimization, population growth, average age, indigenous population, and health insured rates by municipality. Connotations *, **, and *** mean significant at the 90p, 95p and 99p confidence level.

According to the OLS model in column 1, an one-standard deviation in the difference of yearly lagged drug-related homicide rates between pre-treatment and treatment period —or 34.9 drug-related homicides per 100,000 inhabitants— causes an enlargement of 1.0% on the proportion of females who report clinical depression. Relative to the pre-treatment mean of clinical depression for women, the ATE is equal to 5.9%. Said effect is statistically significant at the 90% level of confidence.

On the other hand, the 2SLS model in column 2 shows an effect three-times the size of the OLS model. This implies an absolute impact on clinical depression equal to 3.3% for every additional one-standard deviation expansion of yearly lagged drug-related homicide rates between treatment periods. Provided that the effect for the 2SLS model is also statistically distinguishable from zero, the lower-bound bias hypothesis on migration among clinically depressed women appears to be true.[74] However, statistically speaking, the p-value of the difference in magnitudes between the

[74] I present coefficients and F-statistics for the first-stage of the 2SLS estimator. Based on a large F-statistic for

OLS model and 2SLS model is not different from zero. Therefore, the results for the OLS model are preferable to the 2SLS model.

Regarding a non-linear relationship between drug-related violence and clinical depression among women, the joint significance test indicates a null effect. This is true across estimators. As a caveat, in columns 4 and 8, I only present the first stage for the squared value of drug-related homicide rates because the first stage for the linear value of drug-related homicide rates is exactly the same as in columns 2 and 6.[75]

3.3.2 Current Depression by Severity

Notwithstanding the relevance of the results for clinical depression, these estimations could miss the bigger picture for two reasons. First and foremost, the indicator for clinical depression does not inform whether the depression diagnosis by a medical professional occurred in the recent past. Second, not every person considers depression to be a serious or curable health problem, particularly in a developing country like Mexico. In fact, data suggest that a large portion of depressed Mexicans do not have a medical assessment of their mental health condition (Belló et al., 2005).[76]

Fortunately, both waves of ENSANUT contain proxies for current depression at various levels of severity. Specifically, the indicator for "having felt sad for several days in the last week" serves as a proxy for current mild depression; the measure for "currently feeling depressed" approximates current moderate depression; while the dummy variable for "currently taking antidepressants" represents current severe depression. As a caveat, the following results for current depression should be considered as mere approximations, because these findings are not based on a medically approved method of assessment like the Center for Epidemiologic Studies Depression Scale.

Table 3.4 conducts the same regressions as before, but for current depression among adults, and by severity. In particular, Table 3.4 contains three panels: the dependent variable in the top panel is mild depression; the outcome in the middle panel is moderate depression; while the results in the

excluded variables, I can say that the instrumental variables are strong predictors of drug-related homicide rates.

[75] The F-statistic for excluded variables drops significantly when the model is exactly identified.

[76] In Mexico, the percentage of depressed women without a medical diagnosis is 72.8%, while the proportion of undiagnosed depressed males is 81.0% (Belló et al., 2005).

bottom panel are for severe depression. For the sake of brevity, I omit the first-stage of the 2SLS estimator, which is identical to one presented in Table 3.3. Yet, the layout in Table 3.4 corresponds exactly to the same specifications as in Table 3.3.

In line with the results for clinical depression, males do not indicate any significant effect of drug-related homicide rates on current depression. What is more, females continue to report a significant enlargement in current depression as a consequence of drug-related violence. Thus, once again, I limit the subsequent discussion to current depression among females.

According the top panel of Table 3.4, the ATE of an additional one-standard deviation expansion in yearly lagged drug-related homicide rates on mild depression is equal to 1.8%. Moreover, there is little change in the treatment effect between the OLS and 2SLS models, implying no migration among mildly depressed women. Nonetheless, in contrast to the OLS model, the 2SLS model is not statistically different from zero. Based on the p-value of the joint significance test in the OLS model, in column 3, these results on mild depression hold after introducing a second-order polynomial of drug-related homicide rates, even though the coefficient for this latter term is equal to zero.

The middle panel of Table 3.4 shows a very similar enlargement in the proportion of moderately depressed women vis-à-vis clinical depression. Specifically, the OLS model in column 1 predicts a 1.2% increase in moderate depression among females for every additional one-standard deviation expansion in drug-related homicide rates. Most remarkable, the 2SLS model threefolds the effect for the OLS estimator, just as in the case of clinical depression. However, once again, there is no statistically significant difference between the magnitudes of the OLS model and the 2SLS model. Both linear specifications for moderate current depression among women are statistically significant at the 90% and 95% level of confidence. These findings remain constant in the non-linear specification for the OLS model.

Finally, the bottom panel of Table 3.3 reveals zero changes in the proportion of women who currently take antidepressants during the treatment period. These results on severe current depression for women are somehow contradictory to the linear results for clinical depression and mild-to-moderate current depression. However, this could be an indication for a limited effect of drug-related violence on current depression among women.

Table 3.4: Average Effect of Drug-Related Violence on Current Depression

Panel A. Dependent Variable: Sad for Several Days (Last Week).

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
D Homicide	0.048**	0.030	0.021	-1.112	0.001	-0.056	-0.111	-1.630
	(0.020)	(0.063)	(0.086)	(0.855)	(0.027)	(0.075)	(0.092)	(1.149)
D Homicide Sqr.			0.000	0.011			0.001	0.015
			(0.001)	(0.009)			(0.001)	(0.013)
Joint Significance			[6.57]	[1.83]			[2.28]	[2.68]
Joint P-Value			0.002	0.400			0.104	0.262
Gender	Female	Female	Female	Female	Male	Male	Male	Male
R-squared	0.262	0.261	0.262	-0.506	0.166	0.157	0.255	-0.995
Municipalities	368	368	368	368	368	368	368	368
Chi-p: OLS-2SLS		0.895		0.282		0.544		0.309

Panel B. Dependent Variable: Currently Feeling Depressed.

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
D Homicide	0.035**	0.090*	0.128**	-0.127	0.002	-0.021	-0.020	-0.275
	(0.017)	(0.052)	(0.060)	(0.532)	(0.012)	(0.038)	(0.043)	(0.519)
D Homicide Sqr.			-0.001*	0.002			0.000	0.002
			(0.000)	(0.005)			(0.000)	(0.005)
Joint Significance			[2.62]	[1.28]			[.6]	[.37]
Joint P-Value			0.074	0.526			0.552	0.832
Gender	Female	Female	Female	Female	Male	Male	Male	Male
R-squared	0.143	0.124	0.150	-0.012	0.093	0.086	0.093	-0.040
Municipalities	368	368	368	368	368	368	368	368
Chi-p: OLS-2SLS		0.143		0.441		0.519		0.750

Panel C. Dependent Variable: Currently Taking Antidepressants.

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
D Homicide	0.009	-0.006	0.027	-0.297	0.004	-0.010	0.023	-0.296
	(0.007)	(0.017)	(0.023)	(0.242)	(0.005)	(0.013)	(0.016)	(0.239)
D Homicide Sqr.			-0.000	0.003			-0.000	0.003
			(0.000)	(0.003)			(0.000)	(0.003)
Joint Significance			[.98]	[1.76]			[1.02]	[1.89]
Joint P-Value			0.375	0.415			0.360	0.389
Gender	Female	Female	Female	Female	Male	Male	Male	Male
R-squared	0.067	0.058	0.069	-0.734	0.022	-0.001	0.028	-2.137
Municipalities	368	368	368	368	368	368	368	368
Chi-p: OLS-2SLS		0.380		0.176		0.496		0.124

Notes: All regressions contain robust standard errors in parentheses. Estimations factored by population weights. Joint test for non-linear models correspond to F-stats (OLS) and Chi-square stast (2SLS). Controls include first-differences in mean victimization, population growth, average age, indigenous population, and health insured rates by municipality. Connotations *, **, and *** mean significant at the 90p, 95p and 99p confidence level.

To verify that this is the case, I use suicide rates as an alternative proxy for severe depression. SINAIS provides the universe of suicides from 1998 to 2012. As mentioned previously, SINAIS also contains information for all drug-related homicides, for the same time period. Therefore, the number of time periods available for this proxy is much greater than in the previous analysis, which only allows for first-differences. Consequently, I run a t-period fixed-effects regression of suicide rates on drug-related homicide rates.[77] For consistency purposes, I also conduct the following analysis using OLS and 2SLS models; however, for brevity, I limit my results to linear effects, exclusively.

Table 3.5: Average Effect of Drug-Related Violence on Suicide

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	2SLS	OLS	OLS	2SLS
L Drug Homicide Rate	-0.001	-0.001	0.003	-0.002	-0.001	-0.007
	(0.000)	(0.000)	(0.004)	(0.001)	(0.001)	(0.011)
Gender	Female	Female	Female	Male	Male	Male
Linear Time Trends	No	Yes	Yes	No	Yes	Yes
R-squared	0.010	0.014	0.010	0.009	0.019	0.017
Municipalities	2453	2453	2453	2453	2453	2453
Observations	34342	34342	29436	34342	34342	29436
1stg: L Cocaine x DTR			-0.011***			-0.011***
			(.002)			(.002)
1stg: L Non-Pan Mayor x JO			12.267*			12.267*
			(6.831)			(6.831)
F-Stat for Excluded Var.			13.2			13.2

Notes: All regressions contain robust standard errors, clustered by municipality, in parentheses. Estimations factored by population weights. Pre-treatment mean suicide rates are 0.30 per 100,000 inhabitans for women and 2.05 per 100,000 inhabitants for men. Connotations *, **, and *** mean significant at the 90p, 95p and 99p confidence level.

Table 3.5 indicates a null effect of drug-related homicides on suicide rates across gender groups. These results imply that, indeed, the spike in violence had a limited effect on current depression, bringing confidence to the proxies for current depression and all of the results above. Finally,

[77] Specifically, the t-period fixed-effects model is as follows:

$$Y_{mt} = \beta T^L_{mt} + \gamma X_{mt} + \delta_m + \alpha_t + \varepsilon_{mt},$$

where Y is yearly suicide rates at the municipality level, in period t;, T^L is the lag value of drug-related homicides at the municipality level, in period t; X is a vector of municipality time-varying observables, δ is a full set of municipality dummies that controls for time-invariant unobservables, α is a matrix of year dummies that captures common shocks across municipalities, and ε are all other time-varying unobservables that influence suicide rates. For this regression, I use the within-estimator of the fixed-effects model.

as the number of time periods increases, the F-statistic for excluded variables remains practically unchanged.

3.4 Robustness Test

To evaluate the sensitivity and validity of the findings above, I conduct five robustness tests. First, I perform a non-parametric analysis, using the Center for Epidemiologic Studies Depression Scale (CES-D), available only for the 2012-wave of ENSANUT. Based on a host of medically approved questions, CES-D computes a depression score that runs from zero to 21, in which higher values indicate more severe depression. In particular, I run a first-order local polynomial smoothing regression of individual CES-D scores on municipality lagged drug-related homicide rates, by gender.[78]

Figure 3.2 presents graphically the non-parametric results for individuals that live in municipalities with drug-related homicide rates below an one-standard deviation expansion over the pre-treatment mean (e.g. 39.41 drug-related homicides per 100,000 inhabitants).[79] The left-hand side panel contains results for females, whereas the right-hand side panel shows findings for males. In line with the results above, females report higher CES-D scores than males.

For females, the CES-D score line increases constantly all throughout the horizontal axis, becoming slightly steeper around extreme values of drug-related homicide rates. Conversely, the CES-D score line for males is basically flat along the x-axis, corroborating the results in Tables 3.3 and 3.4. This check presumes a robust linear causal relationship of drug-related homicides on depression among females, in its rawest sense.

Second, I verify the robustness of the linear 2SLS model by dropping one of the instruments, for each of the outcomes in Table 3.3 and 3.4. Table C.1 in the Appendix section include the results of this robustness check. The top panel tests the instrumental validity for the interaction of net cocaine supply from Colombia and the presence of a DTR in a municipality, while the bottom

[78]This locally weighted OLS contains an Epanechnikov kernel and optimized bandwidths.

[79]Over 90% of the surveyed adult population live in municipalities with a drug-related homicide rates below the aforementioned threshold.

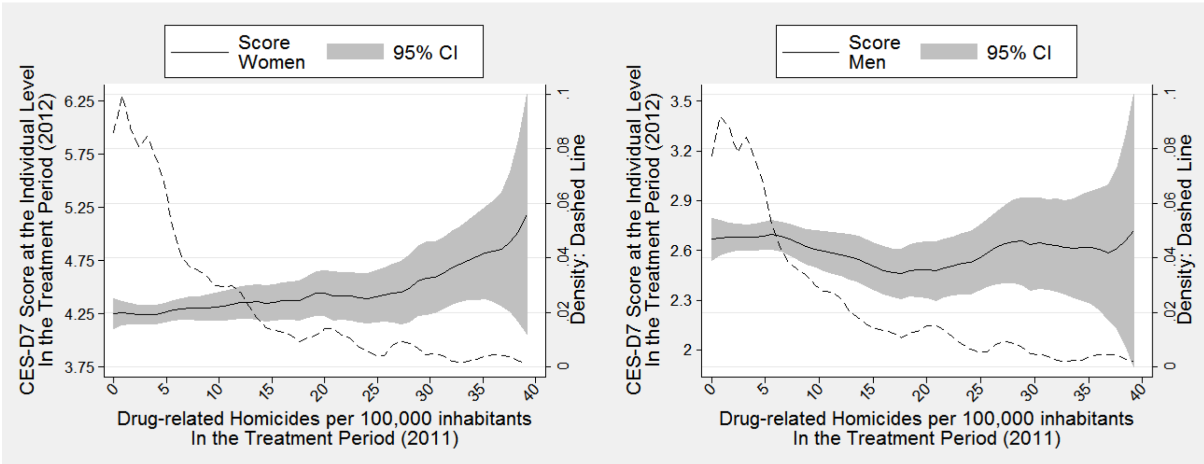

Figure 3.2: Relationship between Drug-Related Homicide Rates and CES-D for the Treatment Period

panel does so for the interaction of a non-PAN governed municipality and the launching of a JO in a given state.

In the case of clinical depression among women, the linear 2SLS model continues to be much higher than the OLS estimator, after dropping one of the instrumental variables. Similarly, moderate current depression for females also shows the same behavior. Conversely, the linear 2SLS model for mild and severe current depression among females are not robust to the exclusion of one of the instrumental variables. However, the linear 2SLS model for both of these outcomes, in females, are not statistically significant. Most important, both instrumental variables continue to report a large F-statistic value, separately.

The last three robustness tests consists of including additional covariates, applying a fake out-of-synch treatment, and using official records of drug-related homicides from the Mexican Intelligence Agencies, instead of SINAIS data. To conduct this last robustness test, I assume official drug-related homicides to be zero during the pre-treatment period, which is missing. Table C.2 in the Appendix section contains the results for all three robustness checks. Each of the panels in Table C.2 contains one robustness test for the linear OLS model.

The top panel reports coefficients that are robust to the inclusion of a host of covariates (e.g. education levels, prevalence of chronic diseases, labor outcomes, and recent accidents,) even though some statistical power is lost for clinical depression among females. The middle panel confirms the nature of this natural experiment by regressing drug-related homicides that are out-of-synch, and which do not correspond to the spike in drug-related violence (e.g. five years earlier). Finally, the bottom panel indicates very similar results when using official records of drug-related homicides from the Mexican Intelligence Agencies, instead of SINAIS data.

3.5 Conclusion

Preliminary findings in this chapter suggest a statistically significant increase of 1.0% in clinical depression among women for every one-standard deviation expansion in yearly lagged drug-related homicide rates, after the beginning of the Mexican Drug War. Also, drug-related homicides seem to have a very similar impact on self-assessed mild-to-moderate depression among women vis-à-vis clinical depression. In stark contrast, Mexican men appear largely unaffected by drug-related violence. These results are robust to a variety of specifications, falsification tests, and data sources of drug-related homicides. However, additional research into the mechanisms that create differential effects for females and males is needed. Future versions of this paper will seek to find these gender differential mechanisms.

The economics consequences of a higher depression prevalence in the adult population are numerous. For instance, there are immediate effects on labor supply (Michaelsen, 2012). Moreover, depression can lead to a series of intrafamily problems, ultimately being reflected back into the social fabric. Hence, an extrapolation to all possible economic consequences could easily indicate slower economic development in Mexico because of the absence of the rule of law, and the social conditions to maintain citizens free of fear.

A APPENDIX OF CHAPTER 1 (THE EFFECT OF OPORTUNIDADES ON REPORTING VIOLENCE AGAINST WOMEN TO THE POLICE)

Table A.1: July-December 2005 Monthly Transfer for *Oportunidades*

Scholarship (MXN)	Level	*Grade*	*Boy*	*Girl*
		3rd	115	115
	Primary	*4th*	135	135
		5th	170	170
		6th	230	230
		7th	335	355
	Middle	*8th*	355	390
		9th	370	430
		10th	560	645
	High	*11th*	605	685
		12th	640	730
Food Grant (MXN) = 170 (MXN)				
Max per family with children in 3th-9th = 1045 (MXN)				
Max per family with children in 10th-12th = 1775 (MXN)				

Source: Rules of operation for Oportunidades 2005

Table A.2: Spousal Abuse Classification

Type	*Question:since the beginning of your relationship with your partner how many times has he:*	*Severity*
	Pushed you or grabbed you by the hair?	High
	Tied you up?	High
	Kicked you?	High
	Thrown an object at you?	High
Physical	Slapped you with his hand or with an object?	High
	Tried to choke you or to strangle you?	High
	Attacked you with a knife or blade (white arm)?	High
	Shot you with a firearm?	High
	Threatened you with a deadly weapon (knife, switchblade, gun or rifle)?	High
	Threatened to kill you, kill himself, or kill the children?	High
	Demanded that you have sex with him against your own will?	High
Sexual	Forced you to do sexual things that you do not like to do?	High
	Used physical force to have sexual relations?	High
	Made you feel ashamed or belittled you (compared you to other women)?	Low
	Ever ignored you, did not take you into account, or did not give you affection?	Low
	Accused you of cheating on him?	Low
	Made you feel fear?	High
Emotional	Threatened to leave you, hurt you (economically), take your children away or kick you out?	High
	Locked you in, forbidden you from going out or receiving visitors?	High
	Turned your relatives against you?	High
	Spied on you?	Low
	Stopped speaking to you?	Low
	Became very angry because the domestic chores are not done?	Low
	Destroyed, thrown away, or hidden things that belong to you or to your household?	High
	Complaint about your money spending?	Low
	Even though he had money, did not provide for household expenses?	Low
Patrimonial	Threatened to not contribute for household expenses?	Low
	Spent money that was needed for household necessities?	Low
	Has taken away your money or your estate?	Low
	Forbidden you from studying or working?	Low

Table A.3: Changes in Beneficiary Population: Exclusion of women who begin experiencing IPV after receiving Oportunidades

	Exclusion of women				Exclusion of women		
Covariate	No	Yes	Diff.	Covariate	No	Yes	Diff.
Community>14999	0.16	0.17	0.01	Free-Union	0.26	0.27	0.01
Mun. Empower.	0.49	0.49	0	Years cohabiting	20.93	21.12	0.19*
Mun. Develop.	0.71	0.71	0	Predicted Puntaje	1.84	1.83	-0.01
Indigenous woman	0.24	0.24	0	Asset index	-1.27	-1.26	0.01
Indigenous men	0.24	0.24	0	Dirt floor	0.33	0.32	-0.01
Age	40.7	40.79	0.09	Drainage	0.71	0.72	0.01
No schooling	0.23	0.23	0	Crowding index	2.45	2.46	0.01
Primary Incomplete	0.4	0.4	0	Wage	1.9	1.91	0.01
Primary Complete	0.2	0.2	0	Family member harmed	0	0	0
Secondary	0.16	0.16	0	Seriously Harmed	0.14	0.14	0
>Secondary	0.02	0.01	-0.01	Harmed	0.38	0.4	0.02
Children<11y.	0.69	0.69	0	Hospitalized	0.2	0.2	0
Family size	5.21	5.23	0.02	IPV Frequency	0.23	0.23	0
Divorced	0.07	0.07	0	Childhood IPV	0.56	0.57	0.01

Notes: Means are weighted by inverse survey sampling weights. Difference in means are clustered at the municipality level. Connotations *, **, and *** mean significantlydifferent from zero at the 90 p., 95 p. and 99 p.

Table A.4: IPV and Reporting IPV rates for Beneficiary and Non-beneficiary Women

	Pre-treatment		Post-treatment		
Sample	Non-Ben.	Benef.	Non-Ben.	Benef.	Diff-in-Diff
All	0.253	0.250	0.266	0.268	0.005***
	(0.125)	(0.108)	(0.129)	(0.113)	(0.000)
(S-) Rural	0.208	0.228	0.225	0.248	0.003***
	(0.115)	(0.108)	(0.123)	(0.116)	(0.001)
(S-) Urban	0.331	0.354	0.337	0.363	0.002**
	(0.129)	(0.122)	(0.132)	(0.125)	(0.001)

Notes: Means are weighted by inverse survey sampling weights. Difference in means are clustered at the municipality level. Connotations *,**, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.5: First-stage: Effects of IVs on the assignment of Oportunidades

	1st	1st	1st	1st	1st	1st	1st	1st	1st
IMSS-ratio	0.128**	0.126**	0.127**	0.160***	0.157***	0.161***			
	(0.054)	(0.054)	(0.053)	(0.060)	(0.059)	(0.057)			
Densification	0.025***	0.025***	0.024***				0.013*	0.012*	0.011*
	(0.007)	(0.007)	(0.007)				(0.007)	(0.007)	(0.006)
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	Yes	No	No	Yes
Sample	All	All	All	Rur	Rur	Rur	Urb	Urb	Urb
Observations	3444	3444	3444	1920	1920	1920	1524	1524	1524
Municipalities	776	776	776	627	627	627	283	283	283
R-squared	0.243	0.243	0.259	0.132	0.134	0.150	0.120	0.124	0.154

Notes: All regressions contain robust standard errors, clustered by municipalities, in parentheses.
Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p. level

Table A.6: Rollout of States's Specialized IPV Laws

State	Intra-household abuse	Date	Additional Law Provisons	All Unions	Sentence (yrs)
Aguascalientes	Artículo 36 A.-	November, 2007	Artículo 36 B.-		1-4
Baja California	Artículo 242 Bis.-	**July, 2003**	Artículo 242 Bis		0.5-6
Baja California Sur	Artículo 240.-	**March, 2005**	NONE	x	0.5-4
Campeche	NONE	-	NONE		-
Coahuila de Zaragoza	Artículo 310.	**October, 2002**	Artículo 311.-	x	0.5-6
Colima	Artículo 191 Bis.-	**February, 1998**	Artículo 191 Bis 1.-	x	1-5
Chiapas	Artículo 198.-	**July, 1998**	Artículo 202.-	x	3-7
Chihuahua	Artículo 193.-	January, 2007	NONE	x	0.5-6
Distrito Federal	Artículo 200.-	**July, 1996**	Artículo 201 Bis.-	x	0.5-4
Durango	Artículo 300.	**December, 1999**	NONE	x	0.5-6
Guanajuato	Artículo 221.-	**June, 2005**	Artículo 221.-	x	0.4-4
Guerrero	Artículo 194 A.-	**April, 1999**	NONE	x	0.5-5
Hidalgo	Artículo 243 Bis.-	December, 2007	Artículo 243 Ter.-	x	0.5-3
Jalisco	Artículo 176-Ter.-	**December, 2003**	Artículo 176-Ter, .-		0.3-3
Edo. de México	Artículo 218.-	**December, 2002**	NONE	x	2-5
Michoacán de Ocampo	Artículo 224 Bis.-	**February, 2002**	NONE	x	0.5-4
Morelos	Artículo 202 Bis.-	December, 2007	Artículo 202 Ter.-	x	0.5-4
Nayarit	Artículo 273 bis.-	**May, 2004**	Artículo 273 Ter.-	x	0.5-4
Nuevo León	Artículo 287 Bis.-	September, 2007	Artículo 287 Bis 2.-	x	1-4
Oaxaca	Artículo 404.-	**September, 2001**	NONE		0.5-4
Puebla	Artículo 284 Bis.-	**April, 2001**	Artículo 284 Ter.-	x	1-6
Querétaro de Arteaga	Artículo 142 Bis.-	April, 2010	NONE		0.3-3
Quintana Roo	Artículo 176 Bis.-	**June, 2000**	NONE	x	0.5-5
San Luis Potosí	Artículo 177.-	July, 2008	Artículo 178.-	x	0.5-3
Sinaloa	Artículo 241 Bis.-	**December, 2001**	Artículo 241 Bis A.-	x	0.5-4
Sonora	Artículo 234-a.-	**December,1999**	Artículo 234-b.-	x	0.5-6
Tabasco	Artículo 208 Bis.-	**May, 1999**	Artículo 208 Bis 1.-		0.3-2
Tamaulipas	Artículo 368 bis.-	**June, 1999**	Artículo 368 ter.-	x	0.5-4
Tlaxcala	NONE	-	NONE		-
Veracruz de Ignacio de la Llave	Artículo 233.-	**September, 1999**	Artículo 234.-	x	2-6
Yucatán	Artículo 228.-	**August, 1999**	Artículo 229.-	x	0.5-4
Zacatecas	Artículo 254 A.-	**February, 2003**	Artículo '254 D.-		0.5-4

Table A.7: Confounding Effects: Specialized IPV Laws for Pooled Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.038*	0.042**	0.042**	0.031*	0.147	0.039***	0.223*	0.040**	0.207
	(0.019)	(0.019)	(0.019)	(0.018)	(0.138)	(0.015)	(0.134)	(0.016)	(0.134)
IPV-Law time	0.003	0.004*	0.005**	0.001	0.003	0.002	0.004	0.002	0.005*
	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	3444	3444	3444	3444	3444	3444	3444	3444	3444
Municipalities	776	776	776	776	776	776	776	776	776
GOF p-stat				0.000		0.000		0.000	
Overidentif.					0.293		0.399		0.392
Endogeneity					0.489		0.200		0.243
F-stat IV					9.84		9.66		9.70
Redundacy					0.018		0.019		0.016

Marginal effects

Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variables are the rollout in the densification process and the ratio of IMSS-Oportunidades over health providers. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Redundancy is an LM test for an invalid IV. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.8: Confounding Effects: Specialized IPV Laws for Rural Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.007	0.011	0.013	0.041	-0.051	0.055*	0.066	0.049	0.046
	(0.020)	(0.020)	(0.019)	(0.033)	(0.230)	(0.029)	(0.235)	(0.037)	(0.227)
IPV-Law time	0.002	0.003	0.003	0.001	0.003	0.002	0.003	0.002	0.004
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.003)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	1920	1920	1920	1920	1920	1920	1920	1920	1920
Municipalities	627	627	627	627	627	627	627	627	627
GOF p-stat				0.000		0.000		0.000	
Endogeneity					0.803		0.814		0.883
F-stat IV					6.79		6.64		7.44

Marginal effects

Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variable is the ratio of IMSS over health providers. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.9: Confounding Effects: Specialized IPV Laws for Urban Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.125***	0.118***	0.112***	0.040***	0.574	0.043**	0.769	0.048**	0.769
	(0.040)	(0.037)	(0.038)	(0.013)	(0.539)	(0.019)	(0.547)	(0.023)	(0.574)
IPV-Law time	0.004	0.005	0.006	0.000	0.003	0.001	0.003	0.000	0.004
	(0.004)	(0.004)	(0.004)	(0.001)	(0.006)	(0.001)	(0.006)	(0.001)	(0.006)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	1524	1524	1524	1524	1524	1524	1524	1524	1524
Municipalities	283	283	283	283	283	283	283	283	283
GOF p-stat				0.000		0.000		0.000	
Endogeneity					0.351		0.145		0.161
F-stat IV					3.20		3.11		2.98

Marginal effects

Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variable is the rollout in the densification process. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.10: Additional Covariates for Pooled Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.040**	0.044**	0.043**	0.033*	0.160	0.041***	0.213*	0.043***	0.190
	(0.020)	(0.019)	(0.019)	(0.018)	(0.139)	(0.014)	(0.129)	(0.015)	(0.125)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	3444	3444	3444	3444	3444	3444	3444	3444	3444
Municipalities	776	776	776	776	776	776	776	776	776
GOF p-stat				0.000		0.000		0.000	
Overidentif.					0.228		0.293		0.299
Endogeneity					0.500		0.232		0.282
F-stat IV					9.86		9.96		10.39
Redundacy					0.017		0.017		0.012

Marginal effects

Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variables are the rollout in the densification process and the ratio of IMSS-Oportunidades over health providers. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Redundancy is an LM test for an invalid IV. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.11: Additional Covariates for Rural Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.008	0.014	0.016	0.039	-0.046	0.047	0.080	0.040	0.063
	(0.020)	(0.019)	(0.019)	(0.035)	(0.226)	(0.031)	(0.236)	(0.045)	(0.225)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	1920	1920	1920	1920	1920	1920	1920	1920	1920
Municipalities	627	627	627	627	627	627	627	627	627
GOF p-stat				0.000		0.000		0.000	
Endogeneity					0.813		0.774		0.828
F-stat IV					6.56		6.18		7.25

Marginal effects
Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variable is the ratio of IMSS over health providers. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.12: Additional Covariates for Urban Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.119***	0.115***	0.111***	0.037*	0.516	0.043**	0.736	0.053**	0.785
	(0.038)	(0.035)	(0.035)	(0.022)	(0.750)	(0.022)	(0.704)	(0.026)	(0.812)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	1524	1524	1524	1524	1524	1524	1524	1524	1524
Municipalities	283	283	283	283	283	283	283	283	283
GOF p-stat				0.000		0.000		0.000	
Endogeneity					0.570		0.287		0.302
F-stat IV					1.81		1.91		1.63

Marginal effects
Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variable is the rollout in the densification process. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.13: Exclusion of the "Quasi Poor" for Pooled Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.024	0.027	0.030	0.027	0.089	0.038*	0.189	0.039	0.168
	(0.022)	(0.021)	(0.021)	(0.027)	(0.180)	(0.021)	(0.169)	(0.024)	(0.167)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	2651	2651	2651	2651	2651	2651	2651	2651	2651
Municipalities	720	720	720	720	720	720	720	720	720
GOF p-stat				0.000		0.000		0.000	
Overidentif.					0.145		0.190		0.195
Endogeneity					0.917		0.417		0.490
F-stat IV					7.21		7.05		7.23
Redundacy					0.033		0.033		0.030

Marginal effects

Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variables are the rollout in the densification process and the ratio of IMSS-Oportunidades over health providers. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Redundancy is an LM test for an invalid IV. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.14: Exclusion of the "Quasi Poor" for Rural Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	-0.010	-0.006	-0.002	0.015	-0.169	0.042	-0.044	0.031	-0.062
	(0.022)	(0.021)	(0.020)	(0.053)	(0.265)	(0.042)	(0.266)	(0.065)	(0.260)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	1642	1642	1642	1642	1642	1642	1642	1642	1642
Municipalities	588	588	588	588	588	588	588	588	588
GOF p-stat				0.005		0.000		0.000	
Endogeneity					0.533		0.883		0.818
F-stat IV					5.98		5.91		6.36

Marginal effects

Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variable is the ratio of IMSS over health providers. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.15: Exclusion of the "Quasi Poor" for Urban Sample

	(1) Probit	(2) Probit	(3) Probit	(4) Biprobit	(5) 2SLS	(6) Biprobit	(7) 2SLS	(8) Biprobit	(9) 2SLS
Oportunidades	0.127***	0.117***	0.116***	0.049***	0.784	0.052**	1.186	0.061	1.184
	(0.048)	(0.044)	(0.044)	(0.018)	(0.960)	(0.021)	(1.163)	(0.050)	(1.222)
Instruments	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Muni. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	No	No	Yes	Yes
Observations	1009	1009	1009	1009	1009	1009	1009	1009	1009
Municipalities	245	245	245	245	245	245	245	245	245
GOF p-stat				0.000		0.000		0.000	
Endogeneity					0.404		0.159		0.178
F-stat IV					1.66		1.55		1.37

Marginal effects
Notes: Regressions contain robust standard errors, clustered by municipalities, in parentheses. Excluded instrumental variable is the rollout in the densification process. Murphy's goodness-of-fit score tests for excess kurtosis in the errors. The Hansen J statistic reports a test of overidentifying restrictions. The C statistic presents the result of the Hausman endogeneity test. Connotations *, **, and *** mean significantly different from zero at the 90 p., 95 p. and 99 p.

Table A.16: Within-Municipalities FE: Effects of Oportunidades on Reporting IPV to the Police

	FE	FE	FE	FE	FE	FE	FE	FE	FE
Oportunidades	0.074*	0.074*	0.072*	0.025	0.029	0.025	0.112	0.107	0.111
	(0.041)	(0.039)	(0.041)	(0.068)	(0.057)	(0.062)	(0.094)	(0.085)	(0.091)
Muni. dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPV controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Puntaje-Assets	No	No	Yes	No	No	Yes	No	No	Yes
Sample	All	All	All	Rural	Rural	Rural	Urban	Urban	Urban
Observations	2156	2156	2156	790	790	790	1183	1183	1183
Municipalities	259	259	259	163	163	163	105	105	105
R-squared									

Marginal effects
Notes: All regressions contain robust standard errors, clustered by municipalities, in parentheses.
Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level

B APPENDIX OF CHAPTER 2 (THE ECONOMIC CONSEQUENCES OF THE MEXICAN DRUG WAR)

Table B.1: Effect of the Mexican Drug War on Drug-Related Homicide Rates for Treated States (2007-2012)

	(1) OLS	(2) OLS	(3) Fixed	(4) ABGMM
Roll x Inter Ops	77.424***	71.545***	102.969***	84.906**
	(21.267)	(23.067)	(37.343)	(37.109)
Border x AWB		2.841		
		(3.983)		
Colombia Seizures	0.080**			
	(0.035)			
L p/Homicide	0.736***	0.756***		0.501***
	(0.163)	(0.160)		(0.031)
State dummies	No	No	Yes	No
Year dummies	No	Yes	Yes	Yes
R-squared	0.710	0.736	0.753	
Number of States	11	11	11	11
Observations	66	66	66	66
Drug War ATT (Hom. Rates)	5.60	5.17	7.44	6.14

Notes: All regressions contain robust standard errors in parentheses. Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level of confidence.

Table B.2: Continuous Evidence for the Orthogonality in the Assignment of Treatment

	(1) OLS	(2) OLS	(3) OLS
Pre-Treatment p/GDP	0.003		0.002
	(0.009)		(0.009)
Pre-Treatment Corruption		-0.003	-0.002
		(0.017)	(0.017)
R-squared	0.002	0.001	0.002
Number of States	30	30	30
Observations	30	30	30

Notes: All regressions contain robust standard errors in parentheses. Connotations *, **, and *** mean significant at the 90p, 95p and 99p confidence level. Sample excludes Campeche and Tabasco.

Table B.3: Synthetic Weights for Treated States with an Accurate Synthetic Control

Donor Pool[†]	Chihuahua	Durango	Guerrero	Michoacan	Sinaloa
Aguascalientes	0	0.187	0	0	0
Baja California Sur	0	0	0.016	0	0.260
Chiapas	0	0	0.166	0	0
Colima	0	0	0	0	0
Distrito Federal	0.077	0	0.011	0	0.025
Guanajuato	0.323	0.088	0	0.148	0
Hidalgo	0	0	0	0	0
Jalisco	0.219	0.162	0	0	0
Mexico	0	0	0	0	0
Nayarit	0	0	0.099	0	0.520
Oaxaca	0	0	0.690	0.266	0
Puebla	0	0	0	0	0
Queretaro	0.122	0.215	0	0	0
Quintana Roo	0	0	0.018	0	0
San Luis Potosi	0	0	0	0	0
Sonora	0.259	0	0	0	0.019
Tlaxcala	0	0	0	0	0
Yucatan	0	0	0	0.319	0
Zacatecas	0	0.348	0	0.267	0.176

[†] Sample excludes all treated states, Campeche, and Tabasco.

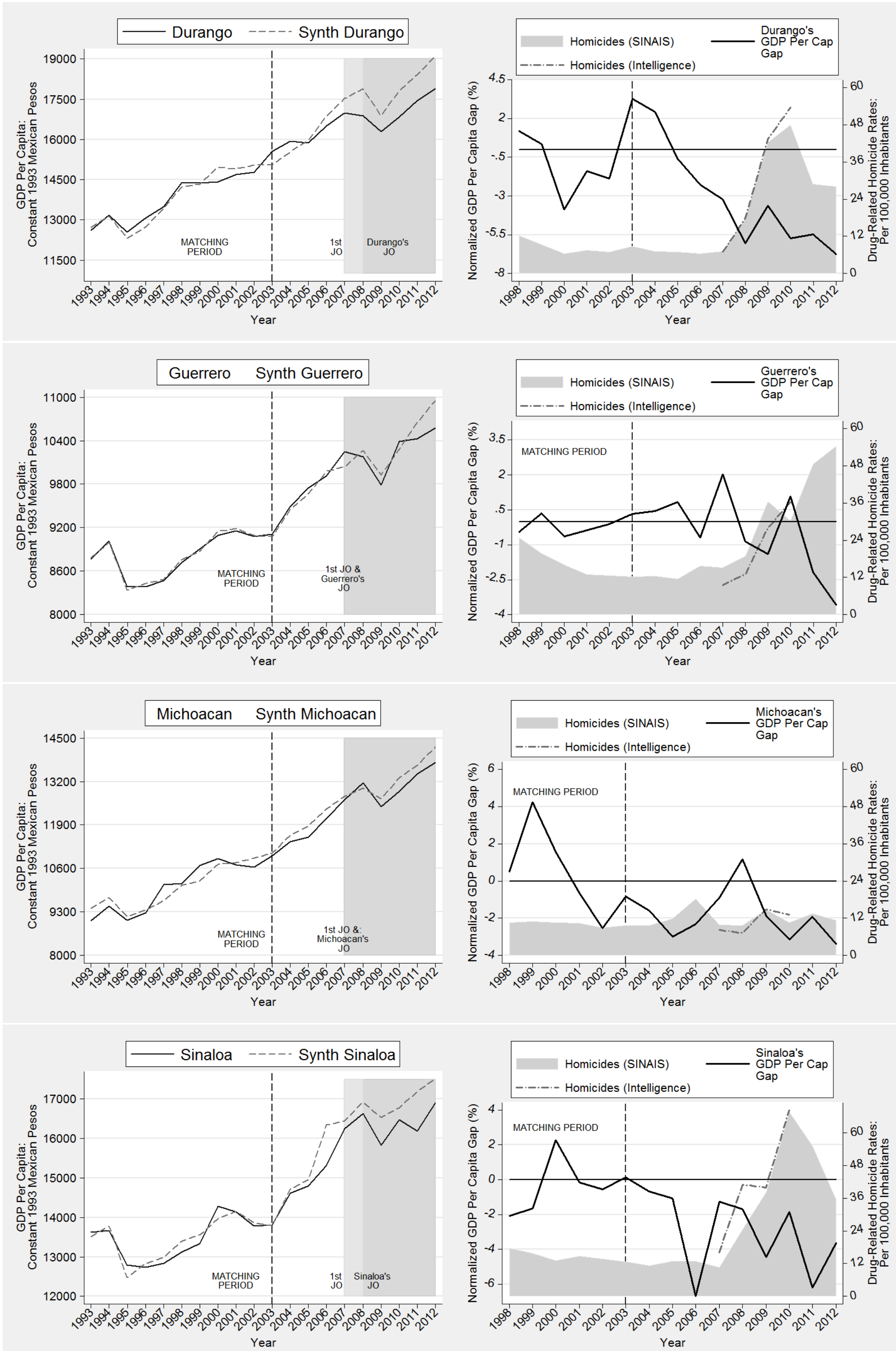

Figure B.1: GDP per Capita Gap for Treated States with an Accurate Synthetic Control

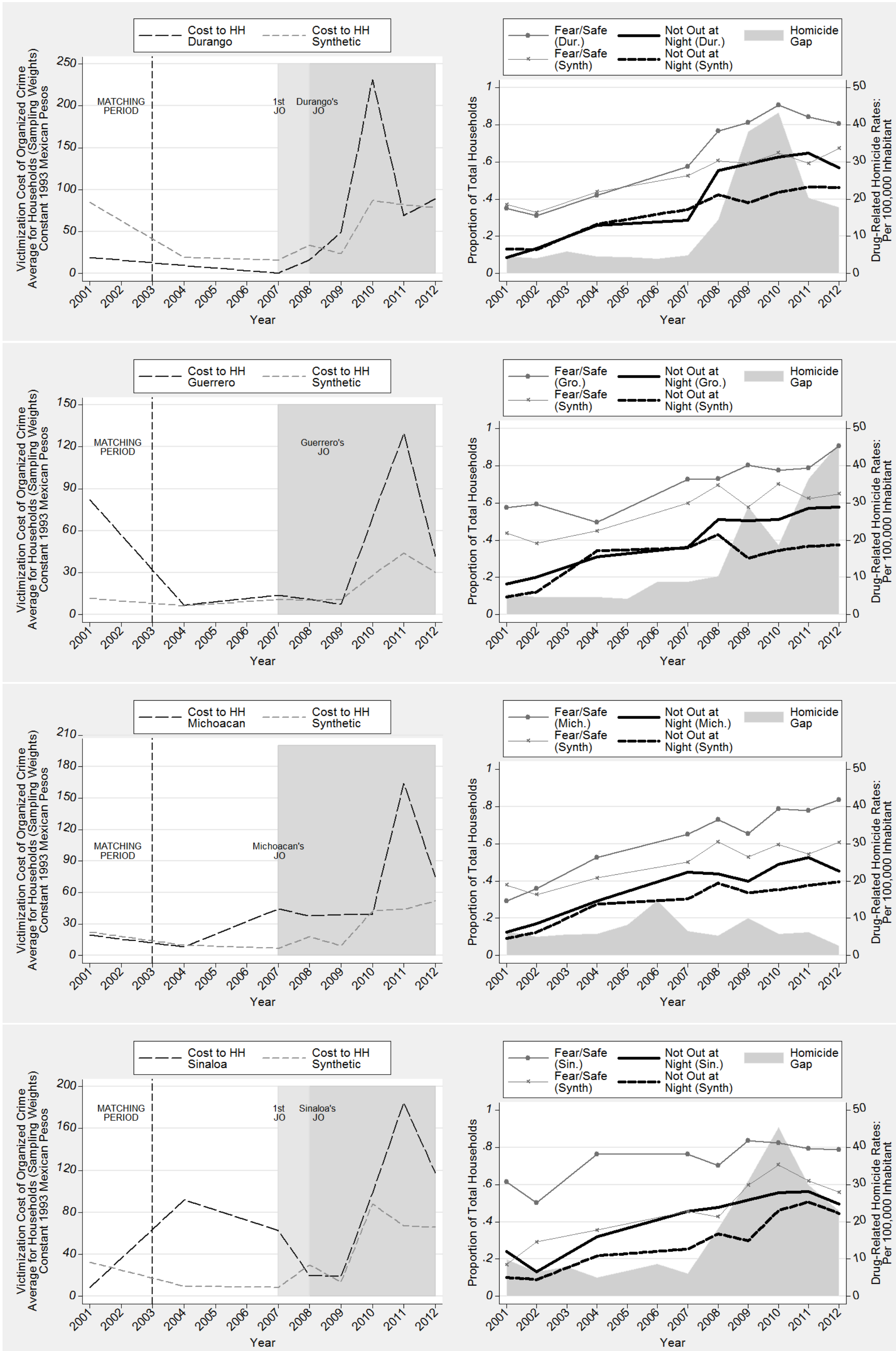

Figure B.2: Mechanism for the Effect of the Mexican Drug War on "Risky" Consumption, for Treated and Synthetic Control Units

C APPENDIX OF CHAPTER 3 (BREAKING SAD: DRUG-RELATED HOMICIDES AND MENTAL WELL-BEING IN MEXICO)

Table C.2: Robustness and Falisication Checks

Panel A. Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Diff L Drug Homicide Rate	0.026	0.012	0.043**	0.009	0.034*	0.007	0.008	0.005
	(0.018)	(0.013)	(0.019)	(0.025)	(0.020)	(0.012)	(0.008)	(0.004)
Dependent Variable	Clinical	Clinical	Mild	Mild	Moderate	Moderate	Severe	Severe
Gender	Female	Male	Female	Male	Female	Male	Female	Male
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.170	0.150	0.356	0.296	0.214	0.183	0.168	0.092
Municipalities	368	368	368	368	368	368	368	368

Notes: All regressions contain robust standard errors in parentheses. Estimations factored by population weights. Controls include first-difference in mean victimization, population growth, average age, indigenous population, health insured rates, education levels, chronic diseases, labor participation, and recent accidents by municipality. Connotations *, **, and *** mean significant at the 90p, 95p and 99p confidence level.

Panel B. Fake Treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Diff L Drug Homicide Rate	0.123	0.033	0.048	0.080	0.074	0.087	0.013	-0.006
	(0.145)	(0.106)	(0.219)	(0.192)	(0.116)	(0.122)	(0.053)	(0.031)
Dependent Variable	Clinical	Clinical	Mild	Mild	Moderate	Moderate	Severe	Severe
Gender	Female	Male	Female	Male	Female	Male	Female	Male
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.085	0.038	0.258	0.167	0.139	0.092	0.064	0.022
Municipalities	368	368	368	368	368	368	368	368

Notes: All regressions contain robust standard errors in parentheses. Estimations factored by population weights. Controls include first-differences in mean victimization, population growth, average age, indigenous population, and health insured rates by municipality. Signs *, **, and *** mean significant at the 90p, 95p and 99p confidence level.

Panel C. Official Records of Drug-Related Homicide Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Diff L Of Drug Hom Rate	0.026*	-0.001	0.043**	-0.004	0.037**	-0.000	0.004	0.001
	(0.016)	(0.012)	(0.017)	(0.026)	(0.018)	(0.012)	(0.006)	(0.003)
Dependent Variable	Clinical	Clinical	Mild	Mild	Moderate	Moderate	Severe	Severe
Gender	Female	Male	Female	Male	Female	Male	Female	Male
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0	0	0	0	0	0	0	0
Municipalities	368	368	368	368	368	368	368	368

Notes: All regressions contain robust standard errors in parentheses. Estimations factored by population weights. Controls include first-differences in mean victimization, population growth, average age, indigenous population, and health insured rates by municipality. Signs *, **, and *** mean significant at the 90p, 95p and 99p confidence level.

Table C.1: Robustness Test for Instrumental Variables

Panel A. Instrumental Variable: Interaction of Net Cocaine Supply from Colombia and DTR

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) OLS	(8) OLS
Diff L Drug Homicide Rate	0.166	-0.144	-0.173	-0.300	0.051	-0.060	-0.057	-0.054
	(0.168)	(0.096)	(0.161)	(0.185)	(0.105)	(0.096)	(0.042)	(0.038)
Dependent Variable	Clinical	Clinical	Mild	Mild	Moderate	Moderate	Severe	Severe
Gender	Female	Male	Female	Male	Female	Male	Female	Male
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	-0.008	-0.290	0.124	-0.084	0.141	0.044	-0.122	-0.383
Municipalities	368	368	368	368	368	368	368	368
1stg: Diff L Cocaine x DTR	-0.0460***	-0.045***	-0.0460***	-0.045***	-0.0460***	-0.045***	-0.0460***	-0.045***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
F-Stat for Excluded Var.	17.6	17.1	17.6	17.1	17.6	17.1	17.6	17.1

Panel B. Instrumental Variable: Interaction of a Statewide Joint Operation and non-PAN Mayor

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) OLS	(8) OLS
Diff L Drug Homicide Rate	0.088*	-0.021	0.059	-0.024	0.095*	-0.016	0.002	-0.004
	(0.052)	(0.034)	(0.063)	(0.070)	(0.054)	(0.036)	(0.017)	(0.011)
Dependent Variable	Clinical	Clinical	Mild	Mild	Moderate	Moderate	Severe	Severe
Gender	Female	Male	Female	Male	Female	Male	Female	Male
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.075	0.027	0.262	0.165	0.120	0.089	0.065	0.015
Municipalities	368	368	368	368	368	368	368	368
1stg: Diff L non-PAN x JO	29.206***	29.218***	29.206***	29.218***	29.206***	29.218***	29.206***	29.218***
	(9.398)	(9.201)	(9.398)	(9.201)	(9.398)	(9.201)	(9.398)	(9.201)
F-Stat for Excluded Var.	9.6	10.0	9.6	10.0	9.6	10.0	9.6	10.0

Notes: All regressions contain robust standard errors in parentheses. Estimations factored by population weights. Controls include first-differences in mean victimization, population growth, average age, indigenous population, and health insured rates by municipality. Connotations *, **, and *** mean significant at the 90p, 95p and 99p confidence level.

REFERENCES

[1] Abadie, Alberto, and Javier Gardeazabal. 2003. "The Economic Costs of Conflict: A Case Study of the Basquer Country". *American Economic Review*. 93 (1): 113-132.

[2] Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2010. "Synthetic control methods for comparative case studies: Estimating the effect of California's Tobacco control program". *Journal of the American Statistical Association*. 105 (490): 493-505.

[3] Álvarez, Carola, Florencia Devoto, and Paul Winters. 2008. "Why do Beneficiaries Leave the Safety Net in Mexico? A Study of the Effects of Conditionality on Dropouts". *World Development*. 36 (4): 641-658.

[4] Amarante, V., and A. Vigorito. 2009. "The impact of PANES on Social capital and Empowerment". Paper presented at the 2011 PEGNet Conference, The Hague.

[5] Angelucci M. 2008. "Love on the rocks: Domestic violence and alcohol abuse in rural Mexico". *B.E. Journal of Economic Analysis and Policy*. 8 (1).

[6] Angelucci, Manuela, and Orazio Attanasio. 2009. "Oportunidades: Program Effect on Consumption, Low Participation, and Methodological Issues". *Economic Development and Cultural Change*. 57 (3): 479-506.

[7] Angrist, Joshua David, and Jörn-Steffen Pischke. 2009. *Mostly harmless econometrics: an empiricist's companion*. Princeton: Princeton University Press.

[8] Areceo-Gomez, Eva O. 2013. "Drug-Related Violence, Forced Migration and the Changing Face of Mexican Migrants in the United States". *G. Genna, D. Mayer - The North American Institutional Void: The Dilemmas of Migration, Security, and Development.* Routledge.

[9] Associate Press. 2007. Felipe Calderon Declares Tijuana 'A Success' Amid Mexican Drug War. *Huffington Post*. December, 11. Retrieved from http://www.huffingtonpost.com

[10] Attanasio, Orazio, Luca Pellerano, and Sandra Polanía Reyes. 2009. "Building Trust? Conditional Cash Transfer Programmes and Social Capital". *Fiscal Studies*. 30 (2): 139-177.

[11] Barro, Robert J. 2001. *Determinants of economic growth: a cross-country empirical study*. Cambridge, Mass: The MIT Press.

[12] Behrman, Jere R., Jorge Gallardo-Garcia, Susan W. Parker, Petra E. Todd, and Viviana Velez-Grajales. 2012. "Are Conditional Cash Transfers Effective in Urban Areas? Evidence from Mexico". *Education Economics*. 20 (3): 233-259.

[13] Belló M, E Puentes-Rosas, ME Medina-Mora, and R Lozano. 2005. "Prevalencía y diagnóstico de depresión en población adulta en México". *Salud Pública De México*. 47: 4-11.

[14] Benería, Lourdes, and Martha Roldán. 1987. *The crossroads of class & gender: industrial homework, subcontracting, and household dynamics in Mexico City.* Chicago: University of Chicago Press.

[15] BenYishay, Ariel, and Sarah Pearlman. 2013. "Homicide and Work: The Impact of Mexico's Drug War on Labor Market Participation", unpublished mimeo. University of New South Wales, Department of Economics.

[16] Black, Donald J. 1976. *The behavior of law*. New York: Academic Press.

[17] Bobonis, Gustavo J. 2011. "The Impact of Conditional Cash Transfers on Marriage and Divorce". *Economic Development and Cultural Change*. 59 (2): 281-312.

[18] Bobonis, Gustavo J. and Roberto Castro. 2010. "The Role of Conditional Cash Transfers in Reducing Spousal Abuse in Mexico: Short-Term vs. Long-Term Effects", unpublished mimeo. Department of Economics, University of Toronto.

[19] Bobonis, Gustavo J, Melissa González-Brenes, and Roberto Castro. 2013. "Public Transfers and Domestic Violence: The Roles of Private Information and Spousal Control". *American Economic Journal: Economic Policy*. 5 (1): 179-205.

[20] Bonaccorsi di Patti, Emilia. "Weak institutions and credit availability: the impact of crime on bank loans". *Bank of Italy Occasional Paper.* 52.

[21] Bunker, Pamela L., Lisa J. Campbell, and Robert J. Bunker. 2010. "Torture, beheadings, and narcocultos". *Small Wars & Insurgencies*. 21 (1): 145-178.

[22] Calderón, Gabriela, Alberto Diaz-Ceyros, and Beatriz Magaloni, Gustavo Robles, and Jorge Olarte. 2012. "The Temporal and Spatial Dynamics of Violence in Mexico", unpublished mimeo. Stanford, Department of Polical Science.

[23] Campos Vázquez, Raymundo M., Carlos Chiapa, and Alma S. Santillán. (2012). ''Análisis de Trayectorias de los Hogares Beneficiarios del Programa Oportunidades.'' *Estudios Económicos*, 27(2), 295-346.

[24] Castillo, Juan Camilo, Daniel Mejía, and Pascual Restrepo. 2013. "Illegal drug markets and violence in Mexico: The causes beyond Calderón". *Seminario ITAM.* 3(10). http://cie. itam. mx/SEMINARIOS/Marzo Mayo_2013/Mejia (accessed 24 June 2013).

[25] Castillo, Juan Camilo, Daniel Mejía, and Pascual Restrepo. 2014. "Scarcity without Leviathan: The Violent Effects of Cocaine Supply Shortages in the Mexican Drug War". *CGD Working Paper*, 356. Washington, DC: Center for Global Development.

[26] Chabat, Jorge. 2010. *Combatting drugs in Mexico under Calderon: The inevitable war*. México, D.F: CIDE.

[27] Chiburis, R.C., J. Das, and M. Lokshin. 2012. "A practical comparison of the bivariate probit and linear IV estimators". *Economics Letters*. 117 (3): 762-766.

[28] Chicoine, Luke. 2011. “Exporting the Second Amendment: U.S. Assault Weapons and the Homicide Rate in Mexico”, unpublished mimeo. Notre Dame, Department of Economics.

[29] Coker A.L., P.H. Smith, R.E. McKeown, and M.J. King. 2000. "Frequency and correlates of intimate partner violence by type: physical, sexual, and psychological battering". *American Journal of Public Health*. 90 (4): 553-9.

[30] Craig, Richard. 1980. "Operation Condor: Mexico’s Antidrug Campaign Enters a New Era". *Journal of Inter-American Studies and World Affairs*. 22 (3): 345-363.

[31] de Jong JT, Ivan H Komproe, and Van Ommeren Mark. 2003. "Common mental disorders in postconflict settings". *Lancet*. 361 (9375): 2128-30.

[32] Dell, Melissa. 2011. “Trafficking Networks and the Mexican Drug War”, unpublished mimeo. MIT, Department of Economics.

[33] Dube, Arindrajit, Oeindrila Dube, and Omar García-Ponce. 2013. “Cross-Border Spillover: U.S. Gun Laws and Violence in Mexico”. *American Political Science Review*, 107 (3): 397-417.

[34] Durose, Matthew R. 2005. *Family violence statistics including statistics on strangers and acquaintances*. Washington, D.C.: U.S. Dept. of Justice, Office of Justice Programs, Bureau of Justice Statistics. http://purl.access.gpo.gov/GPO/LPS72873.

[35] *El Universal*. 2012. “Combate al narco no afectará economía: Ferrari”. 16 March, A1.

[36] Ellsberg, Mary, Lori Heise, Rodolfo Pena, Sonia Agurto, and Anna Winkvist. 2001. "Researching Domestic Violence Against Women: Methodological and Ethical Considerations". *Studies in Family Planning*. 32 (1): 1-16.

[37] Farmer, Amy, and Jill Tiefenthaler. 1996. "Domestic Violence: The Value of Services as Signals". *The American Economic Review*. 86 (2): 274-279.

[38] Felson, R. B., S. F. Messner, A. H. Hoskin, and G. Deane. 2002. “Reasons for reporting and not reporting domestic violence to the police”. *Criminology*. 40 (3): 617-648.

[39] González-Flores, Mario, Maria Heracleous, and Paul Winters. 2012. "Leaving the Safety Net: An Analysis of Dropouts in an Urban Conditional Cash Transfer Program". *World Development*. 40 (12): 2505-2521.

[40] Guerrero, Eduardo. 2011. “Security, Drugs, and Violence in Mexico: A Survey”. *7th North American Forum*, Washington D.C.

[41] Hernández Franco D.H., M.O. Corona, and S.V. Baez. 2008. "Métodos de focalización en la política social en México un estudio comparativo". *Economia Mexicana, Nueva Epoca.* 17 (1): 101-137.

[42] Hevia de la Jara, F. (2009). ''De Progresa a Oportunidades: efectos y límites de la corriente cívica en el gobierno de Vicente Fox.'' *Sociológica*, 24 (70): 753-763.

[43] Holland, Paul W. 1986. "Statistics and Causal Inference". *Journal of the American Statistical Association*. 81 (396): 945-960.

[44] Knack, Stephen, and Philip Keefer. 1995. "Institutions and Economic Performance: Cross-Country Tests Using Alternative Institutional Measures." Econ*omics and Politics*. 7 (3): 207-227.

[45] Manacorda, Marco, Edward Miguel, and Andrea Vigorito. 2011. "Government Transfers and Political Support". *American Economic Journal: Applied Economics*. 3 (3): 1-28.

[46] Mas, Matilde. 1995. *Capital humano, series históricas: 1964-1992*. Valencia: Fundación Bancaixa.

[47] Merino, G. 2003. "Descentralización del sistema de salud en el contexto del federalismo. Caleidoscopio de la salud: de la investigación a las políticas y de las políticas a la acción." *FUNSALUD*, 1: 195-207.

[48] Merino, José. "Los operativos conjuntos y la tasa de homicidios: Una medición". *Nexos*, June 1, 2011.

[49] Michaelsen, Maren M. 2012. "Mental health and labour supply: Evidence from Mexico's ongoing violent conflicts". *Ruhr Economic Papers*, 378.

[50] Morrison, A. R., and M. B. Orlando. 1999. "Social and Economic Costs of Domestic Violence: Chile and Nicaragua." *Too Close to Home: Domestic Violence in Latin America*. Ch. 3 in: Morrison, A., and L. Biehl. Inter-American Development Bank, Washington, D.C.

[51] Osorio, Javier. 2012. "Democratization and drug violence in Mexico", unpublished mimeo. Notre Dame, Department of Polical Science.

[52] Piccinelli M, and G Wilkinson. 2000. "Gender differences in depression. Critical review". *The British Journal of Psychiatry.* 177: 486-92.

[53] Pinotti, Paolo. 2012. "The Economic Costs of Organized Crime: Evidence from Southern Italy". *Banca D'Italia: Temi di Discussione*. 868.

[54] Priebe S, M Bogic, D Ajdukovic, T Franciskovic, GM Galeazzi, A Kucukalic, D Lecic-Tosevski, et al. 2010. "Mental disorders following war in the Balkans: a study in 5 countries". *Archives of General Psychiatry*. 67 (5): 518-28.

[55] Programa de las Naciones Unidas para el Desarrollo. 2008. *Índice de desarrollo humano municipal en México*. México: Programas de las Naciones Unidas para el Desarrollo.

[56] Ramirez, Miguel D. 2009. "Are Foreign and Public Capital Productive in the Mexican Case A Panel Unit Root and Panel Cointegration Analysis". *Eastern Economic Journal*. 36 (1): 70-87.

[57] Sahn, David E., and David Stifel. 2003. "Exploring Alternative Measures of Welfare in the Absence of Expenditure Data". *Review of Income and Wealth*. 49 (4): 463-489.

[58] Scholte WF, M Olff, P Ventevogel, de Vries GJ, E Jansveld, BL Cardozo, and CA Crawford. 2004. "Mental health symptoms following war and repression in eastern Afghanistan". *The Journal of the American Medical Association*. 292 (5): 585-93.

[59] Secretaria de Desarrollo Social (SEDESOL). 1999. *Programa de Educación, Salud y Alimentación: más oportunidades para las familias pobres : evaluación de resultados del Programa de Educación, Salud y Alimentación : primeros avances 1999*. México: Secretaría de Desarrollo Social.

[60] Secretaria de Desarrollo Social (SEDESOL). 2000. *Mas oportunidades para las familias pobres: evaluación de resultados del Programa de Educación, Salud y Alimentación : alimentación, 2000*. México: Secretaría de Desarrollo Social.

[61] Skoufias, Emmanuel. 2005. *PROGRESA and its impacts on the welfare of rural households in Mexico*. Washington, DC: International Food Policy Research Institute.

[62] Tauchen, Helen V., Ann Dryden Witte, and Sharon K. Long. 1991. "Domestic Violence: A Nonrandom Affair". *International Economic Review*. 32 (2): 491-511.

[63] Thompson, Martie P. and J. B. Kingree. 2006. "The Roles of Victim and Perpetrator Alcohol Use in Intimate Partner Violence Outcomes." *Journal of Interpersonal Violence*. 21 (2): 163–177.

[64] Toro, María Celia. 1995. *Mexico's" war" on Drugs: Causes and Consequences*. (Vol. 3). Lynne Rienner Publishers.

[65] United Nations. 2010. *The World's Women: Trends and Statistics*. United Nations: New York.

[66] Valdés-Castellanos, Guillermo. 2013. *Historia del narcotráfico en México*. Santillana Ediciones: Mexico, D.F.