

UC Riverside

UC Riverside Electronic Theses and Dissertations

Title

Three Essays on Institutional Reforms in Mexico

Permalink

<https://escholarship.org/uc/item/3vs843f7>

Author

Beleche, Trinidad

Publication Date

2010

Peer reviewed|Thesis/dissertation

UNIVERSITY OF CALIFORNIA
RIVERSIDE

Three Essays on Institutional Reforms in Mexico

A Dissertation submitted in partial satisfaction
of the requirements for the degree of

Doctor of Philosophy

in

Economics

by

Trinidad Beleche

June 2010

Dissertation Committee:

Dr. David Fairris, Co-Chairperson
Dr. Mindy S. Marks, Co-Chairperson
Dr. Jorge Agüero
Dr. Aman Ullah

Copyright by
Trinidad Beleche
2010

The Dissertation of Trinidad Beleche is approved:

Co-Chairperson

Co-Chairperson

University of California, Riverside

Acknowledgments

I would like to gratefully and sincerely thank my two co-advisers, Dr. David Fairris and Dr. Mindy Marks, for their guidance and understanding. I want to express my gratitude to Dr. Fairris for holding me to high research standards and for all his practical advice. Mindy's mentorship has been paramount in providing me a well-rounded experience consistent with my personal and career goals, and I would like to thank her for inspiring me to continue my journey as an academic. I am also indebted to members of my doctoral committee, Dr. Jorge Agüero and Dr. Aman Ullah, for their input, valuable discussions and accessibility.

The second chapter of this dissertation is co-authored with Dr. Nellie Lew at the Food and Drug Administration. In addition, this work also benefited from discussions with Libby Ashley, Kelley Bedard, Peter Kuhn, Rosemary Lavaty, Jon Sonstelie, Todd Sorensen and participants of the UCR Applied Economics Seminar and 2008 AEA Pipeline Conference. A childhood friend and lawyer, Marisol Cajero Garibay, patiently answered questions regarding legal institutions in Mexico.

The fourth chapter of this dissertation has greatly benefited from discussions with Jorge Agüero. I am grateful to Jorge for his insightful comments, constructive criticisms, and for always pushing me to think outside my comfort zone. I do hope that the current joint work in progress is the beginning of many more papers to come.

I owe everlasting gratefulness to many people who pleasantly involved themselves in encouraging me before and throughout my graduate studies. Especial thanks go to Nellie whose cherished friendship initiated during my studies at UCSB and whose

inexplicable sixth sense materialized into unexpected international phone calls, care packages, visits and many other acts of thoughtfulness and kindness that helped me keep the sanity. I would also like to acknowledge two others who believed in me when grad school was merely an idea and whose wholehearted advice I constantly seek: Dr. Lee Heavner and Mike Beauregard. Naming all others who remain may prove a futile exercise, but recognition goes to friends I have met at EAC, AG, and Saint Andrew's Newman Center.

My graduate studies would not have been the same without the social and academic diversions provided by two friends and office mates: Neha Raykar and Dr. Deniz Baglan. Those countless hours spent studying, or in discussions about teaching, the job market, research, dissertation writing, or any topic in general were greatly enriched by Deniz's breadth of knowledge and maturity. A million thanks goes to Neha Raykar, an awesome friend and listener. She proof-read parts of this dissertation, and has provided invaluable help and support through the writing and filing of this dissertation.

Last but not least, I would like to thank that fun-loving group of people I can always count on, who has contributed significantly in defining who I've become, and my greatest source of strength: *mi familia*. I especially thank my parents, Salvador and Angela, for everything. It was under their watchful eye that I gained so much drive and an ability to tackle challenges head on. Also, I thank my grandparents, Carmen and Obdulia. They, too, have provided me with unending encouragement and support.

Para mis papás y abuelitos.

ABSTRACT OF THE DISSERTATION

Three Essays on Institutional Reforms in Mexico

by

Trinidad Beleche

Doctor of Philosophy, Graduate Program in Economics

University of California, Riverside, June 2010

Dr. David Fairris, Co-Chairperson

Dr. Mindy S. Marks, Co-Chairperson

This dissertation is comprised of three essays on institutional reforms surrounding marital dissolution, domestic violence and education in Mexico. It is well accepted that a country's economic growth is greatly determined by its ability to innovate and to increase productivity. Understanding the role of family and education policy becomes relevant since there is evidence that domestic violence, divorce and education are linked to labor productivity and other socioeconomic outcomes such as health.

Using a difference-in-differences methodology, the second chapter exploits the variation in law adoption across states to estimate the impact of liberalized divorce laws on divorce rates. The findings do not support the claim that greater access to divorce has led to increasing divorce rates. In addition to the divorce laws examined in the second chapter, the third chapter estimates the impact of legal reforms that make domestic violence a punishable offense and that provide assistance programs to victims of domestic violence. In intra-household bargaining models, these laws potentially redistribute

bargaining power to the victims of domestic violence, which they can use to bargain for less violence. I find that improving the bargaining position through prevention programs and counseling decreases homicides, suicides and bodily injury crimes. Policies which may leave the victim without recourse if separated from the spouse seem to be less effective.

The final chapter exploits a quasi-experimental setting to estimate the impact of more instructional days on student performance. The sources of variation arise from state-mandated changes in test-administration and the start of the school calendar year. Using an unbalanced panel of all public elementary schools and controlling for a set of school quality measures, the analysis provides evidence to support increasing the number of schools days. The results also suggest that the effects are non-linear and that in impoverished schools, having more days of instruction may be beneficial to the extent that there are other school resources that assist in the production of learning.

Contents

List of Figures	xi
List of Tables	xii
1 Introduction	1
2 'Till Laws Do Us Part? The Impact of Divorce Laws on Divorce Rates in Mexico	5
2.1 Introduction	5
2.2 Background: Divorce Law Reform and Divorce Rates in Mexico	7
2.3 Divorce Rates and Divorce Law Reform	10
2.4 Data	12
2.5 Methodology	13
2.6 Results	16
2.7 Alternative Specifications	18
2.7.1 Dynamic Effects	18
2.7.2 Other Measures of Law Change Indicators	19
2.7.3 Pre-existing Trends	21
2.8 Discussion	22
2.9 Conclusion	25
3 Bargaining, Intra-family Violence Laws and Acts of Domestic Violence in Mexico	38
3.1 Introduction	38
3.2 Institutional Background and Economic Framework	41
3.2.1 Domestic Violence in the Mexican Legislature	41
3.2.2 Economic Framework	44
3.3 Empirical Strategy and Data	47
3.3.1 Identification	47
3.3.2 Model Specification	49

3.3.3	Data	51
3.4	Bodily Injuries	56
3.5	Homicides	58
3.6	Suicides	60
3.7	Robustness Checks and Discussion	63
3.7.1	Pre-existing Trends	63
3.7.2	Coding of Legal Reforms and Inclusion of Large States	64
3.7.3	Discussion	65
3.8	Conclusion	67
4	Estimating the Effects of School Year Length on Student Performance in Mexico	85
4.1	Introduction	85
4.2	Literature Review	88
4.3	The School Calendar and Standardized Student Tests in Mexico	92
4.3.1	ENLACE	92
4.3.2	Variation in the Number of Instructional Days	94
4.4	Data and Methodology	96
4.4.1	ENLACE Results, Grade and School Level Data	96
4.4.2	Methodology	101
4.5	Results	104
4.5.1	Overall Performance in Mathematics and Reading	104
4.5.2	Schools in Impoverished Localities	108
4.5.3	Other Specifications	109
4.6	Discussion	110
4.6.1	Law and Practice	110
4.6.2	Other	111
4.7	Conclusion	112
5	Conclusions	129
	Bibliography	133

List of Figures

2.1	Divorce Rates and Divorce Law Reform: 1993-2006	26
2.2	Divorce Rates and Total Number of Laws in 1993	27
3.1	IFV Crimes Reported as a Percent of All Crimes, 1997-2006	68
3.2	Male and Female Homicide Rates in 1996 and Adoption of Legal Reforms	69
3.3	Heterogeneity in Timing of Legal Reforms, 1996-2006	70
3.4	Effect of Assist Law and Contribution of Each Age Group to Bodily Injury Crimes	71
3.5	Effect of Assist Law and Contribution of Each Age Group to Suicide Rate	72
4.1	Instructional Days and Average Scores in Math and Reading	114
4.2	Cumulative Effects Differ by Marginalization Index	115

List of Tables

2.1	National Causes for Divorces and Divorce Types in 2001-2006	28
2.2	Demographic Characteristics by Law Status	29
2.3	Regression Results of Divorce Rates on Law Reform	30
2.4	Dynamic Effects of Law Reform on Divorce Rates	31
2.5	Divorce Rates on Law Reform: Number of Laws	32
2.6	Divorce Rates on Domestic Violence Reform	33
2.7	Divorce Rates on Incompatibility Reform	34
2.8	Divorce Rates on Separation Reform	35
2.9	Dynamic Effects of Law Reform on Divorce Rates	36
2.10	Divorce/1,000 persons (1993-2006) and Divorce Law Reform Years	37
3.1	Month and Year of Introduction of Legal Reforms by State	73
3.2	Lagged Outcomes on Law Indicator	74
3.3	Descriptive Statistics of Homicides by Victim's Gender	75
3.4	Descriptive Statistics of Suicides and Suicide Attempts	76
3.5	Descriptive Statistics of Criminal Suspects, 1997-2006	77
3.6	Effects of Legal Reforms on Bodily Injury Crimes Committed by Males	78
3.7	Effects of Legal Reforms on Bodily Injury Crimes Committed by Females	79
3.8	Effects of Assist Law on Homicide Rates: Place of Occurrence is Victim's Home	80
3.9	Effects of Assist Law on Male Suicide Rates	81
3.10	Effects of Assist Law on Female Suicide Rates	82
3.11	Effects of Assist Law on Homocides Committed in Public Areas	83
3.12	Effects of Laws on Annual Thefts per 100,000 People	84
4.1	Instructional Days Prior to ENLACE	116
4.2	Description of Variables	117
4.3	Student Performance on ENLACE	118
4.4	Summary Statistics of Student and School Characteristics	119
4.5	Summary Statistics of Student and School Characteristics: Various Samples	120

4.6	Estimates of Instructional Days on Student Performance on ENLACE: Third Grade	121
4.7	Estimates of Instructional Days on Student Performance on ENLACE: Fourth Grade	122
4.8	Estimates of Instructional Days on Student Performance on ENLACE: Fifth Grade	123
4.9	Estimates of Instructional Days on Student Performance on ENLACE: Sixth Grade	124
4.10	Student Performance in Math by Marginalization Level and Grade . . .	125
4.11	Student Performance in Reading by Marginalization Level and Grade . .	126
4.12	Instructional Days on Student Performance Using Various Samples . . .	127
4.13	Using Percent of Students Scoring at Least at the Basic Level	128

Chapter 1

Introduction

This dissertation is comprised of three essays on the impact of institutional reforms on marital dissolution, domestic violence and education in Mexico.

A country's economic growth is greatly determined by its ability to innovate and to increase productivity. Economists have linked education to economic growth and development (Hanushek and Kinko 2000; Hanushek and Woessmann 2009), which suggests the importance in understanding the role that public policy can have on both educational attainment and quality. Furthermore, there is evidence that marital dissolution and domestic violence are associated with labor market outcomes such as wages and employment. The World Health Organization reported that in countries like Nicaragua and Chile, domestic violence costs in terms of loss productivity have been estimated to be at least 1.6 percent of GDP (WHO 2005). Other than labor productivity, domestic violence and marital dissolution affect socioeconomic outcomes such as health. Hence,

considerable attention is also paid to identify factors that have greater implications on the socioeconomic well-being of individuals and society as a whole.

In developing countries the role of policies may vary significantly from that of similar policies applied in developed countries. For instance, the effectiveness of increasing educational inputs may be particularly high in a setting where educational resources are low or lacking, but may decrease as more of the input is purchased. It is also possible that low economic or institutional resources may dampen the intended effects of certain policies. The heterogeneity in the school-age population, school inputs, socioeconomic growth and institutional settings makes Mexico a good environment for which to investigate the effectiveness of family and education policies. The results may be applicable to countries with similar characteristics, and may shed light on other inputs that have been previously understudied.

Chapter 2 examines institutional reforms pertaining to marital dissolution in Mexico. Specifically, it investigates reforms to civil codes during the 1990s which resulted in states adopting or changing provisions that liberalized a state's divorce law. These legal changes include: allowing incompatibility of characters to be a cause for divorce, reducing the time required to file for divorce when one of the parties leaves the conjugal home and adopting procedures that, under certain conditions, grant a divorce within 15 days of filing. At the same time that divorce laws were changing, divorce rates in Mexico have been rising. For instance, in 1992, 5 percent of marriages ended in divorce, a figure which rose to 13 percent in 2006. Are these two trends linked? In particular, are the legal changes responsible for the increase in divorce rates? The heterogeneity in the adoption

of the law reforms provides a quasi-experimental opportunity to estimate the causal impact of legal reform adoption and divorce rates. The literature that investigates the role of legal reforms on divorce rates in developed countries is limited due to (i) divorce being a recent phenomenon, (2) lack of reliable data, or (3) little variation in the policy rule. The findings of this empirical exercise indicate that liberalization of divorce laws in Mexico cannot explain the near doubling of national divorce rates. These results are robust to various specifications of the law changes and specifications which allow the effects of the laws to adjust dynamically.

In addition to the legal reform which allows divorce on the grounds of domestic violence mentioned in Chapter 2, in Chapter 3, I examine changes in institutional reforms that make domestic a punishable offense and which establish assistance programs for victims of domestic violence. Can public policy create a mechanism through which domestic violence can be mediated? Access to divorce can lead to dissolution of violent relationships, thereby leading to a decrease in the incidence of domestic violence. However, evidence from Chapter 2 indicates that easier access to divorce did not have an impact on divorce rates in Mexico. Therefore, changes in domestic violence propensities could not be attributed to divorce regime changes. An application of intra-household bargaining models predicts that domestic violence should decrease in as far as these laws redistribute bargaining power to the victims, and victims use it to demand for less violence. Exploiting the variation occurring from the difference in the timing of domestic violence reform adoption I estimate the impact of these institutional changes on measures that capture domestic violence. I use rich data sets on suicides, homicides

and crimes which allow me to examine outcomes for the groups that are most likely affected by the reforms and to conduct a battery of robustness tests. I find evidence that providing access to assistance programs such as shelters and counseling lowered homicides, suicides and bodily injury crime rates. However, there is no evidence that reforms which provide access to police reporting and divorce have an impact on domestic violence, as measured by homicides, suicides and bodily injury crimes.

In addition to family law reform, Mexico has also acknowledged the need to reform its educational system. As national and international reports have indicated that Mexican students lag behind in math and science, there is a focus on educational reform that improves education quality. Does student performance on state-administered exams improve when they have more days to prepare for the assessment? In Chapter 4 I estimate the impact of instructional days on student performance in Mexico. To estimate the causal impact, I use a quasi-experiment that exploits two sources of variation in instructional days. The first source of variation arises from state-mandated changes in the administration of a standardized exam implemented at the national level for grades 3 to 6. The second source of variation arises from state-mandated changes in the start of the school calendar year in various states. Results suggest that having more days of instruction prior to examination does improve student achievement as measured on their performance on a standardized exam. However, the effectiveness appears to exhibit diminishing returns, that is, the benefit of an increase in the number of instructional days is high initially but decreases after a certain point. Moreover, there is evidence that the effects differ across areas with varying levels of economic development.

Chapter 2

'Till Laws Do Us Part? The Impact of Divorce Laws on Divorce Rates in Mexico

2.1 Introduction

During the 1990s many states in Mexico began to reform their civil codes on divorce by adopting or changing provisions that liberalized a state's divorce law. Legal changes include allowing domestic violence or incompatibility of characters to be a cause for divorce, eliminating the requirement for mutual consent in the case of separation, and enacting procedures that, under certain conditions, grant a divorce within fifteen days of filing. At the same time that divorce laws were changing, divorce rates in Mexico have been rising.

The impact of divorce laws on divorce rates is an important relationship to understand since there is a large body of literature documenting the consequences of divorce on other outcomes of interest, such as the educational attainment and future labor market earnings of children, emotional well-being of families, domestic violence, wealth, female labor supply and female labor force participation (Gruber 2004; Chase-Lansdale *et al* 1995; Gardner and Oswald 2006; Stevenson and Wolfers 2006; Stevenson 2008; Mammen 2008). To date, most of the divorce literature has focused on developed countries. In part, analysis of divorce in developing countries, especially in Latin America, has been limited because it is a fairly new phenomenon.¹ Although Mexico has been one of the earlier adopters of divorce law liberalization, there are no studies—to our knowledge—on the effects of these legal reforms on marital dissolution.

The advantages of using Mexico as a case study to inform us about the effects of divorce law changes in the developing countries of Latin America are the following. First, divorce laws in Mexico have been determined at the state level, whereas divorce reforms in other Latin American countries have occurred at the national level. Therefore, the variation in the timing of law adoption across states allows us to examine the effects of the law changes in a panel framework and eliminate some of the endogeneity issues that arise using cross-sectional data or a single national time series. Second, in terms of culture, religion, socio-economic characteristics, legal and social welfare institutions, Mexico is more representative of other Latin American countries compared to developed countries and therefore the results in this study may be applied more broadly. Third,

¹Mexico passed divorce laws in 1917, Brazil and Colombia in 1977, and Chile in 2004, (June 1982; Plata 1988; NotiSur 2004).

the findings of this analysis are also relevant as legislators weigh the costs and benefits of the recent approval of Mexico City's legislature on August 28, 2008 to eliminate all 21 grounds of divorce and institute unilateral divorce. Our results suggest that liberalization of divorce laws did not have a statistically significant impact on aggregate divorce rates and therefore does not explain the doubling of the divorce rate between 1993 and 2005.

2.2 Background: Divorce Law Reform and Divorce Rates in Mexico

In Mexico, like many Latin American countries, the laws of divorce and family relations are embedded in civil codes. Currently, each of the 31 states and Mexico City has its own laws stipulating the requirements and procedures for obtaining a divorce. The civil code of each state specifies: the entities (Civil Registrar or judicial court) where the divorce should be filed, the types of divorce that may be filed (administrative or judicial), the causes for divorce effective in each state, and filing requirements.

While states were adopting additional provisions that liberalized their divorce laws, they also retained many traditional grounds for divorce, such as mutual consent, adultery, mental illness, bigamy, incurable diseases, impotency, illegitimate children, and abandonment or separation of the conjugal home. Divorce provisions considered in this analysis are described below.

1. Domestic violence (DV): Where applicable, a state's civil code declares that intra-family violence—whether physical or psychological—imposed by a spouse on either the children or the other spouse, with the intent to harm or to humiliate, is grounds for divorce.
2. Incompatibility (Incomp): Incompatibility of characters may be invoked after a year of marriage by one party.
3. Separation (Sep): Either spouse may seek divorce if the couple has been separated for more than 2 years, independent of the reason that originated the separation.²
4. Administrative (Admin): This is a type of divorce filing with a state's Civil Registrar that can expedite the process of divorce within 15 days, in most cases, if the following requirements are satisfied: spouses mutually consent to divorce, there are no children—conceived or dependent—and both parties have agreed on the liquidation of their property.

Divorce law reforms (1)-(3) are additional grounds for divorce that allow one spouse to initiate divorce. If a spouse claims Domestic Violence or Separation as a cause for divorce, then he or she must go through a judicial process to prove his or her claim. Using Incompatibility as justification for divorce, the spouse initiating the divorce unilaterally must also go through the courts. On the other hand, an Administrative divorce requires mutual consent and it is filed with the Civil Registrar. In states where both Incompatibility and Administrative divorce are allowed, couples who fulfill the

²In some states, the period of separation begins from the time the law was passed as opposed to the time that the couple has actually been separated, (El Norte Newspaper, February 5, 2007).

Administrative divorce requirements would have an incentive to use Administrative divorce because it is fast and relatively inexpensive compared to Judicial divorce.

Figure 2.1 presents divorce rates and adoption of the various reforms since 1993, and suggests a relationship between the increase in divorce rate and divorce law reform. In 1997 less than 10 percent of the states allowed domestic violence as grounds for divorce, but by 2001 this proportion had increased to almost 50 percent. Reform of the separation requirement has followed a similar pattern so that by 2006 over 75 percent of the states had already incorporated this law. Although more than half of the states allowed administrative divorce since the inception of their civil code we do not observe many administrative divorce law reforms until the late 1990s.³

Coinciding with the passage of divorce legislation, divorce rates have also been increasing. In 1993 there were 0.41 divorces per thousand persons and this statistic rose to 0.76 divorces per thousand persons in 2005.⁴ According to Table 2.1, in 2001-2006 mutual consent accounted for over 70 percent of divorces, while separation or abandonment accounted for 5-10 percent. In addition, while the majority of divorces are judicial, there has also been a rise in the number of administrative divorce filings over time. Table 2.1 shows that relative to the other causes for divorce, the proportion of divorce filings listing domestic violence as the cause is slightly over one percent. Domestic

³Interestingly, some states eliminated administrative divorce claiming the civil registrar lacked the appropriate training to grant a divorce. Meanwhile others eliminated the provision arguing that it increased the number of divorce filings. (Palabra Newspaper, November 22, 2006) In Jalisco, administrative divorce was in effect since the inception of the civil code. This type of divorce was cancelled in 1995 and reinstated in 2007. In Coahuila, administrative divorce was eliminated in 1977 but reinstated by the state's congress in 2006.

⁴The divorce rates in the U.S. between 1993 and 2005 were 4.7 and 3.6 divorces per thousand persons, respectively (Clarke *et al* 1993 and Tejada-Vera and Sutton 2008).

abuse, however, is a major public health concern affecting Mexico. Based on household surveys in 2003, approximately 44 percent of women living with a partner reported having been a victim of domestic violence (ENDIREH 2003).⁵ When domestic violence is allowed as a reason for divorce, an abused spouse may threaten to use it against the other in order to obtain mutual consent to dissolve the marriage.

2.3 Divorce Rates and Divorce Law Reform

Early work of Becker (1981) and Peters (1986) examine the relationship between the change in divorce regimes and divorce rates in the US. Using cross-sectional data, Peters (1986) finds that divorce rates in the U.S. were not affected by a shift from mutual consent to unilateral divorce. By contrast, Allen (1992) finds that a change in divorce regime increased divorce rates and argues there are other factors (e.g. costs of divorce) not previously considered which are important for the analysis of divorce. Other work of Weiss and Willis (1997) and Allen and Brinig (1998) provide support for Allen's (1992) argument. Peters (1992) replies to Allen (1992) with an additional study and concludes that, after controlling for pre-existing differences in state divorce propensities, a shift to unilateral divorce did not affect divorce rates and that Allen's (1992) results suffered from omitted-variable bias.

Later work by Friedberg (1998) improves upon Peters' (1986, 1992) and Allen's (1992) methodology by using a panel of state-level administrative divorce data. Friedberg (1998) controls for state, year, and state-specific time trends to address concerns

⁵In the U.S., surveys of households in the 1970s and 1980s indicate approximately 12 percent of wives experienced violence from their husbands, (Stevenson and Wolfers 2006).

about the endogeneity of divorce reform. Her results suggest that adoption of unilateral divorce laws account for about one-sixth of the increase in the divorce rate since the late 1960s. In recent work, Wolfers (2006) extends Friedberg's (1998) results with a longer panel and a slightly modified methodology. He finds that unilateral divorce laws do not have permanent effects on the divorce rate in the U.S.

The work that investigates the changing trends of divorce rates in developing countries has been scant and the few that have been done have focused on finding the correlates of divorce rates using cross-sectional data. Furthermore, some of these studies examine developing societies characterized by permissive divorce customs, in which law reforms subsequently made divorce more difficult and were enacted at the national level (Jones 1981; Heaton *et al* 2001; Aghajanian, 1986). Most of these studies are also limited in the sense that they use either small samples or a specific regional area in their country of study (Jones 1981; Aghajanian 1986; Heaton *et al* 2001; Trent and South 1989).

This study contributes to the literature by examining panel data on a developing country, in which little is known about the role that divorce law liberalization has played in the rising divorce rate. Given similarities in cultural characteristics and demographic trends, the experience in Mexico may shed light on the effect of divorce reform in other Latin American countries. In addition, the estimation strategy exploits variation in the timing of law reforms across states to address the issue of endogeneity that limit some of the previous studies.

2.4 Data

We use state-level panel data drawn from each state's Statistical Yearbook, (Anuario Estadístico), which includes all divorces in Mexico from 1993-2005. Ideally, we would like to include more observations before some of the policy changes occurred in order to identify pre-existing state trends. However, prior to 1993, a divorce might have been reported to INEGI twice (once by the Civil Registrar and once by the Judicial Court) in the same year or in different years. Since then, Mexico's National Institute of Statistics and Geography (INEGI) has implemented a more consistent data collection methodology.⁶ The Statistical Yearbooks also include demographic information on birth rate, mortality, fertility, life expectancy, migration, population growth, and number of marriages. (See Table 2.2 for further description of these variables.)

Information on state divorce laws was collected by examining each state's civil code from its inception to April 2008. We identify when and if a state adopted each of the following divorce law reforms: Domestic violence (DV), Incompatibility (Incomp), Separation (Sep) and Administrative (Admin).

⁶Collection of divorce data began in 1926 and the methodology has changed eight times since then. INEGI reports that this problem was addressed and solved beginning in 1993. For comparison, in 1992, the count of divorces was 51,953 and in 1993 the figure dropped to 32,483. Currently, the Civil Registrar provides a copy of the divorce certificate while the Judicial Courts submit a ledger of divorces that were granted during a given month. Both Civil Registrars and Judicial Courts are now moving to reporting the data electronically.

2.5 Methodology

Our empirical strategy follows the approach taken by Friedberg (1998). Equation 2.1 models the impact of a divorce law change on divorce rates using a difference-in-difference (DID) technique.

DIV_{st} is divorce rate for state s in year t , calculated as the number of annual divorces per thousand persons in each state⁷ is an indicator variable equal to one if state s has the particular divorce law of interest in year t and zero otherwise. β is the

$$DIV_{st} = \alpha + \beta law_{st} + \sum_s \gamma_s S_s + \sum_t \eta_t T_t + \sum_s \psi_s S_s * time_t + X'_{st} \delta + \epsilon_{st} \quad (2.1)$$

coefficient that measures the average increase in the divorce rate that is due to the legal change, ceteris paribus. Included in the specification are state (s) and year (t) fixed-effects and linear state-specific time trends ($S * time$). State fixed-effects control for state-level time-invariant heterogeneity, while year fixed-effects account for unobserved factors affecting divorce rates that vary over time but affect states homogeneously.

Friedberg (1998) argues that there are unobservable variables changing within a state over time that affect both divorce rates and a state's likelihood to reform their divorce laws. Thus, the omission of state-specific trends, which would capture such

⁷In the literature, there is no consensus on whether to use divorces per 1,000 persons or divorces per 1,000 married persons. Divorces per 1,000 persons is sensitive to age and marital status composition of the population. If the laws are also affecting marriage rates (marriages/1,000 persons) the size of the population seeking divorce will change. Thus, when we use divorces per 1,000 persons our estimates on the impact of divorce law reforms implicitly capture changes in the legal structure and changes in marriage entry or exit.

unobservable determinants, will bias the coefficients. If changes in unobservable factors lead to higher divorce rates and increase the propensity of a state to adopt divorce law reforms, this will bias our results upward. Friedberg's (1998) empirical results indicate that omitting state and time trends creates an upward bias. To allow for unobservable factors influencing divorce rates to vary within a state over time, similar to Friedberg (1998), we include state-specific time trends in our specification.

However, this specification does not control for other demographic characteristics, and it is plausible there are systematic relationships between the trend in divorce rates and the adoption of unilateral divorce laws that are being omitted. For instance, if increases in the female labor force participation, not captured by state-specific time trends, lead to increased pressure for reform and raise divorce rates directly then the estimates will be upward bias. Moreover, the problem will be exacerbated if the omitted variable (and hence) is serially correlated, if so the standard errors will be inflated.⁸ Thus, in our preferred specification, equation (2.2), we include a matrix, X_{st} , of state-

$$DIV_{st} = \pi + \beta law_{st} + \sum_s \delta_s S_s + \sum_t \chi_t T_t + \sum_s \lambda_s S_s * time_t + X'_{st} \delta + u_{st} \quad (2.2)$$

level characteristics: real GDP (measured in thousands of pesos with 1993 as the base year), sex-ratio (defined as the population of males divided by the population of fe-

⁸In our analysis we will use heteroskedastic robust standard errors clustered at the state level.

males), marriage rate (annual marriages divided per thousand persons), fertility rate, internal and international migration rates.⁹ (See Table 2.2 for a full description of the demographic controls.)

Real GDP proxies for income level changes, while the fertility rate serves as a proxy for changing attitudes towards marriage and family as well as changing trends in labor force participation or educational attainment. An important trend in Mexico is the decline in the national fertility rate from 3.3 to 2.2 live children per woman between 1993 and 2005. This shift in women's fertility decisions may increase the proportion of couples who qualify for administrative divorce.

We also include controls for the marriage rate and the sex-ratio to address the possibility that changes in the marriage market affect the likelihood of divorce reform. If marriage rates are correlated with the liberalization of divorce then divorce rates may be affected as well and omitting them would lead to biased estimates. If easier divorce raises the marriage rate, reducing the average quality of marriages because exit costs are lower, our estimates would be biased upward. On the other hand, if liberal divorce laws reduce marriage rates by causing marriages to become less valued, a decline in marriage rates over time would lower the population at risk for divorce. Thus, omitting the marriage rate would lead to a downward bias. In separate analyses we find that the average national marriage rates have been decreasing over time, but the changes in the divorce laws have a positive and insignificant effect on marriage rates. Controlling for internal

⁹The results are robust to specifications where the control variables include the lagged marriage rate up to 4 years instead of the current marriage rate.

and international migration seems important as well since separation of the conjugal home is the second most cited reason for divorce in Mexico, after mutual consent, as described in Table 2.1. Some states in Mexico have experienced dramatic changes in migration relative to others. If an increase in migration creates marital instability and influences a state's decision to expand their divorce provisions to include separation as a cause, then omitting migration rates could bias our results upward.

2.6 Results

Table 2.3 shows the DID estimates under different specifications. Our law indicator, *law*, is defined to be equal to one if state s at time t has any of the following grounds for divorce: Domestic Violence, Incompatibility, or Separation.¹⁰ Our estimates suggest that states that expanded their grounds for divorce to include Domestic Violence, Incompatibility, and/or Separation, were not more likely to have higher divorce rates compared to states that did not. We find that these results are robust to specifications where we include linear state-trends and demographic characteristics. In general the coefficients tend to get smaller in magnitude and they remain insignificant when we add linear trends and demographic controls. In column (1) of Table 2.3 we

¹⁰The variable *Any Law* captures the effect of a state permitting domestic violence, incompatibility, or separation as cause for divorce. Reform of a state's civil code may involve bundling law changes. Table 2.10 shows the states of Coahuila, Michoacán, and Sinaloa bundled Domestic Violence and Separation in 1999, 2001 and 1998, respectively. We exclude the provision for administrative divorce because although it expedites the process of divorce, it requires mutual consent while the other law changes do not.

find that the estimated coefficient for *AnyLaw* is positive and statistically significant when omitting state and year fixed effects. However, the coefficients in columns (2)-(4) become statistically insignificant and smaller in magnitude when including these fixed effects, suggesting that the cross-sectional estimates of column (1) are upward biased. Comparing the estimates of column (2) to column (3) we find that adding our set of demographic control variables did not affect the results significantly. Except for the sex-ratio, the demographic controls are not individually statistically significant but are jointly statistically significant. Consistent with other studies, we observe that fertility rates appear to be negatively associated with divorce rates. On the other hand, the coefficients of the marriage and male-female ratios indicate a positive relationship, but it is only statistically significant for the male-female ratio. The robustness of our estimates to specifications that include demographic controls provides some evidence in favor of the exogeneity of the laws.

Our preferred specification, equation 2.2, is shown in column (4) of Table 2.3. When we add state-specific linear trends our estimate is 0.01 and it is not statistically significant. The male-female ratio shows a positive relationship but it is no longer statistically significant. We note that the standard errors are clustered at the the state level. We also examine this specification using Newey-West adjusted standard errors to consider serial correlation and autocorrelation up to lag 5. The standard errors become smaller, but the results of no effect remain.

2.7 Alternative Specifications

2.7.1 Dynamic Effects

Wolfers (2006) argues that the coefficient on the divorce law obtained in Friedberg's model does not adequately capture the full adjustment process of a policy change. He asserts that adding state-specific time trends will not only pick up the effects of pre-existing state trends, but they will also include some of the dynamic effects of the policy reform. In order to analyze the path of adjustment that occurs after a change in legal regime he suggests modifying equation (2.2) to include dummy variables indicating the number of years that the law has been in effect, as shown in equation (2.3).

$$DIV_{st} = \theta + \sum_{k \geq 1} \beta_k law_{kst} + \sum_s \eta_s S_s + \sum_s \eta_s S_s + \sum_t \rho_t T_t + \sum_s \mu_s * time_{st} + e_{st} \quad (2.3)$$

In equation (2.3), law_{kst} is a categorical variable equal to one if the law has been in effect for k periods. For example, suppose the law has been in effect for 10 years and we choose to divide this period into five periods, $k = \{1, 2, 3, 4, 5\}$. Thus, law_{1st} is equal one if the new regime has been in effect for 1 or 2 years in state s at time t and zero otherwise. law_{2st} is equal to one if the law has been in effect for 3 or 4 years in state s at time t , and so on. The estimated coefficient on law_{kst} identifies the response function of a law change. The advantage of the dynamic effects model is that it allows

us to examine both short-term and long-term effects of broadening allowable grounds for divorce rather than just the average difference before and after the law changes.

Table 2.4 displays the results for the model in equation (2.3) which traces the dynamic effects for *AnyLaw*. Similar to the results in Table 2.3, we find that once we control for state and year fixed effects the significance on the law coefficient disappears. In the preferred specification of column (4) the coefficient for the law change indicator *AnyLaw* is positive but statistically insignificant for all years that Domestic Violence, Incompatibility, and/or Separation have been in effect. In the specifications without state-specific linear trends, the coefficients in columns (2) and (3) are all statistically insignificant but negative for some years where the divorce provisions have been implemented longer than two years.

2.7.2 Other Measures of Law Change Indicators

In Table 2.5 we obtain the estimated coefficient by substituting the variable in equation (2.3) with an index, called *Number of Laws*, measuring the permissiveness of each state's divorce laws based on the number of provisions for divorce that it has in year t (Stetson and Wright, 1975). Therefore this variable can take on values 0 to 3. Again, our estimates are positive and insignificant for the preferred specification, similar to the results in Table 2.3.

We then next show results that capture the effect of a law change for each type of divorce provision run in separate regressions in Tables 2.6 to 2.8. Though not statistically different from zero, the estimated coefficient on Domestic Violence (Table

2.6) and Separation (Table 2.8) were 0.008 and 0.021, respectively. Surprisingly, we find that the coefficient for Incompatibility was negative and significant in the specifications of columns (2) to (4) of Table 2.7. The result was unanticipated because we would expect that adopting incompatibility as grounds for divorce, which allows divorce to be initiated unilaterally, would have a larger impact in liberalizing a state's divorce laws compared to the other types of divorce provisions in our analysis. One explanation is that there are currently only five states that have incompatibility grounds for divorce and that four of these states had adopted this cause before the period of our analysis. If these states had experienced an initial spike in their divorce rates shortly after the divorce reform occurred and subsequently returned to their steady state level of divorce it may appear that the addition of the incompatibility law caused a decline in the divorce rates.

In analysis not reported in this paper, we run specifications that include a law change indicator for each of type of divorce provision in a single regression. In this model we can examine the effect of adopting a particular divorce law while holding constant the effect of all other types of divorce provision.¹¹ We find that the results are similar

¹¹For example, in the specification in Table 2.3 we would not be able to compare the effect of states that adopt the Domestic Violence grounds for divorce but already have Separation and Incompatibility causes to other states that did not adopt the Domestic Violence law and also had Separation and Incompatibility. In a situation where a state has multiple provisions for divorce, a spouse may invoke the domestic violence cause for divorce as a threat against the other spouse to come to an agreement. Perhaps, there is also switching from one cause of divorce to another as states permit additional grounds for divorce. Therefore the coefficient on the law may be different than when we are not able to keep constant all other divorce laws a state may have.

to those found in Tables 2.5 to 2.8 except in some cases the magnitude of the coefficients became smaller. In Table 2.9 we estimate the models from Tables 2.5 to 2.8 under the dynamic effects model where we find similar results.

2.7.3 Pre-existing Trends

To examine whether there are pre-existing differences we present descriptive statistics of states that had adopted any law reform by 1993 and 2005 and compare them to control states—states that had not adopted any law reform in the given period. Table 2.2 shows that, except for population and the male-female ratio, the difference between reform and control states with respect to divorce rates, marriage rates, fertility, real GDP, and other demographic characteristics were not statistically significant in 1993. By 2005, reform states differed from control states in having higher divorce rates, but they were similar in the other demographic characteristics.

One might be concerned that rising divorce rates led states to reform the law as this may invalidate our identification strategy. Table 2.10 shows the average divorce rates by states overtime and the year in which states enacted the new laws.

In this table we also observe increasing divorce rates, but there is not a clear relationship between high divorce rates and divorce reform. For instance, Chiapas, Guerrero, Oaxaca, Puebla, Tlaxcala, and Veracruz have had below average divorce rates, but these states have also been active with respect to divorce reform. By contrast, Baja California, Colima, and Quintana Roo have adopted new grounds for divorce while their divorce rates have been higher than the national average.

Panel A of Figure 2.2 suggests there are regional differences with respect to divorce rates in 1993, with the states in the north and Yucatan area having higher divorce rates while the southern and central states have low to moderate levels of divorce. However, we do not see any systematic relationship between high divorce rates in 1993 and the total number of laws adopted by 2005 (see Panel B of Figure 2.2).

We also find that a state's 1993 divorce rate is not correlated with whether a state ever adopts any of the provisions for divorce. The correlation coefficients with significance level in parentheses are: -0.02 (0.90), 0.10 (0.57), -0.17 (0.34), and 0.06 (0.74) for Separation, Administrative, Domestic Violence and Incompatibility, respectively. Furthermore, the 1993 divorce rate is not correlated with a state's adoption of the law after 1993. The correlation coefficients and significance level in parentheses for Separation, Administrative, Domestic Violence and Incompatibility, respectively, are: 0.20 (0.27), -0.07 (0.69), -0.17 (0.34) and -0.04(0.81).

2.8 Discussion

Our results suggest that certain types of divorce provisions may not affect the likelihood of divorce and is consistent with the work of Peters (1988) and others. One explanation for this conclusion is that these types of law reforms have not sufficiently reduced the barriers to divorce. For instance, some states that have allowed domestic violence to be considered as grounds for divorce have subsequently amended this policy to require evidence of abuse. Aside from the possible reluctance some may feel in claiming domestic violence due to the associated stigmas, in effect, such a policy of

requiring proof would make it very difficult to use Domestic Violence as a cause for divorce. In the case of Administrative divorce, the cost of filing this type of divorce, which varies by state and municipality, may be prohibitive. From Table 2.1, we can see that there are relatively few divorces which cite domestic violence as the main cause compared to mutual consent and that the majority of divorces are judicial rather than administrative.

Of all the provisions for divorce, Separation appears to have a consistently positive effect on divorce rates, although usually statistically insignificant. Within our period of analysis, we observe that compared to the other provisions for divorce Separation has the most variation in the timing of law adoption, which may yield a positive correlation with divorce. The effectiveness of the Separation law may be dampened in some states that require couples to be separated for at least two years starting when the provision was adopted. Therefore couples who have been separated for two years or more prior to the implementation of the law would have to wait at least an additional two years to begin the divorce process using that particular cause.

The second explanation of what could be driving our results is that our period of analysis does not span far enough into the past to adequately control for trends in the divorce rates before some of the law changes occurred. As was previously discussed concerning the adoption of Incompatibility as grounds for divorce, we observe a similar trend for states that have added Administrative. More than half of the states that

eventually adopt Administrative divorce already had it before 1993, so the difference in the average divorce rate between the “reform” and “control” states is close to zero or negative from 1993 to 2005.

The divorce rates in the “control” states tend to be higher than those in the “reform” states during this period which may be due to differences in their divorce rate trajectories. While the “control” states are beginning to experience a relative rise in their divorce rates, the states in the “reform” group may have experienced an unobserved rise in divorce following the adoption of Administrative divorce and have returned to a steady-state level of divorce during our period of analysis. We are unable to identify the exact dates in which the Administrative law was adopted for most of the states that have had it since the inception of their civil code. Consequently we cannot differentiate among those that have had it longer than others.

Concerning the possible endogeneity of marriage rates, we find that the liberalization of divorce laws (*AnyLaw*) is positively correlated but statistically insignificant with the marriage rate, even though marriage rates have been declining over this time period.¹² This may suggest that if marriage rates are endogenous, the likely bias is that easier divorce raises marriage rates through lowering the average quality of marriages. The composition effect should therefore accentuate any positive effect of the divorce law changes on the probability of divorce given marriage. In a sense, this would strengthen our finding that the law changes had no statistically significant effect on divorce.

¹²Using marriage rates as the dependent variable in the preferred specification of equation (2.2) yielded a point estimate of 0.089 with a heteroskedastic robust standard error of 0.14 on the law coefficient.

Based on the available set of demographic control variables in our specification, it appears that the drop in fertility accounts for most of the rise in divorce rates. The decline in average fertility rates from 3.26 to 2.22 (measured as live children per woman) over this time period explains approximately 25 percent of the increase in divorce rates. This may suggest that other social changes are responsible for the rise in divorce rates, although it is also possible that liberalization of divorce laws caused a decrease in fertility as women lose security.

2.9 Conclusion

Over the past two decades, Mexico has experienced an increase in its divorce rates. Some state legislators have been concerned that broadening allowable grounds for divorce may have contributed to the rise in divorce and have considered repealing some of these laws. More recently, the adoption of unilateral divorce in Mexico City in August of 2008 has raised similar concerns. In this paper we do not find any evidence to support the claim that liberalizing divorce laws caused aggregate divorce rates to increase. In some cases we actually find that adoption of certain causes for divorce is correlated with a decrease in divorce rates. The results of this study may be more applicable than those found in the context of developed countries in predicting the magnitude and direction of the effects that divorce law reforms have in other Latin American countries as their laws continue to evolve.

Figure 2.1: Divorce Rates and Divorce Law Reform: 1993-2006

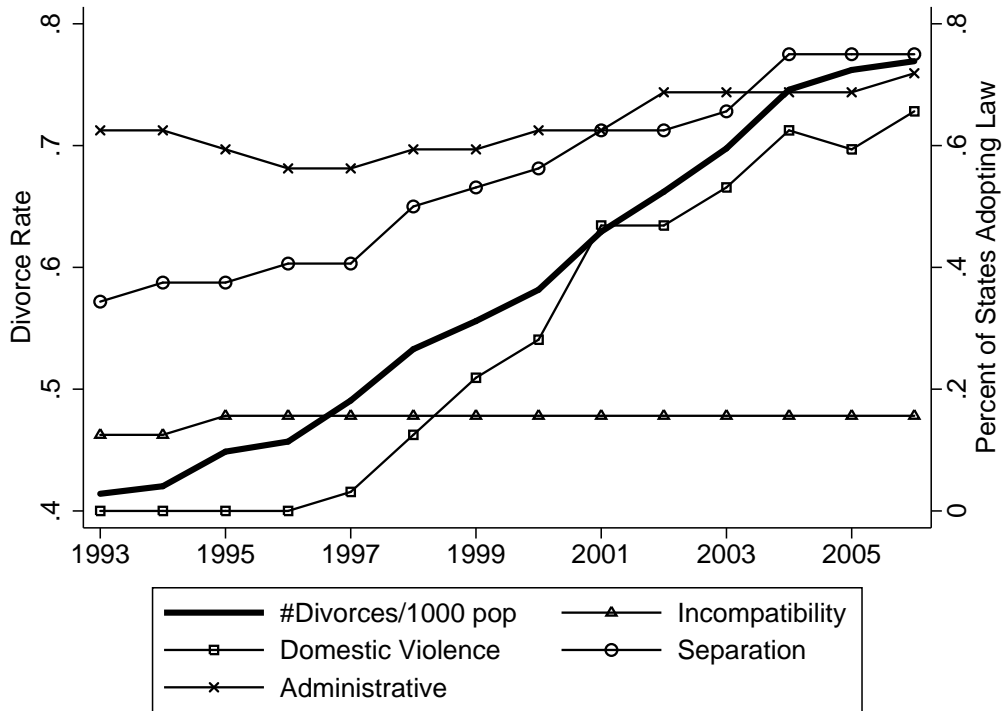


Figure 2.2: Divorce Rates and Total Number of Laws in 1993

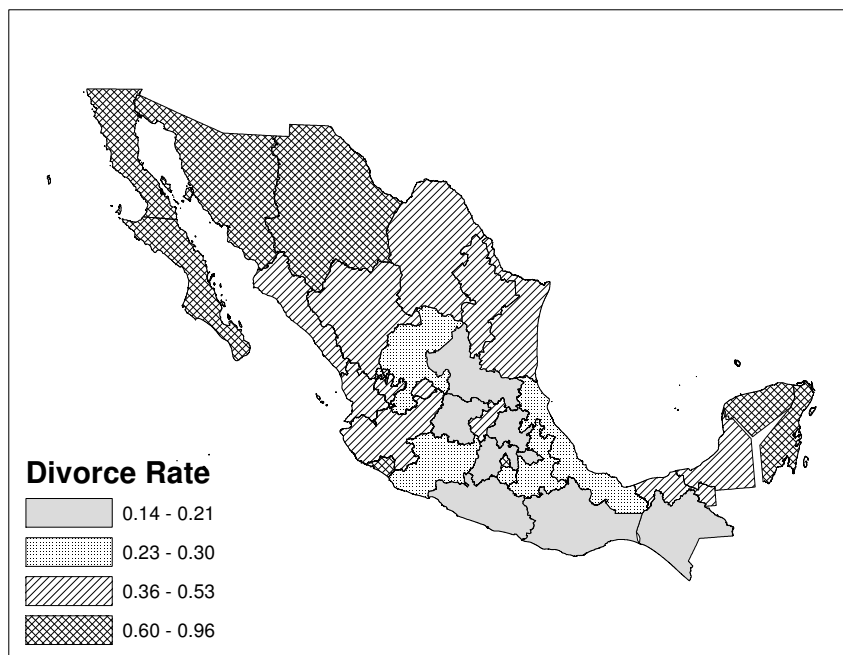


Table 2.1: National Causes for Divorces and Divorce Types in 2001-2006

Reported Cause	2001	2002	2003	2004	2005	2006
Mutual consent	40,796 (71.10)	43,351 (71.50)	46,285 (72.00)	49,046 (72.6)	51,091 (72.80)	52,712 (72.80)
Separation for more than 2 years in- dependent of the motive	3,454 (6.00)	3,344 (5.50)	5,783 (9.00)	6,497 (9.60)	6,787 (9.70)	7,250 (10.00)
Separation for more than 1 year with just cause	4,309 (7.50)	5,403 (8.90)	3,753 (5.80)	3,558 (5.30)	3,880 (5.50)	4,078 (5.60)
Abandonment of the home for more than 3 or 6 months without cause	5,111 (8.90)	4,744 (7.80)	4,802 (7.50)	4,932 (7.30)	4,944 (7.00)	4,886 (6.70)
Threats, domestic violence	985 (1.70)	1,091 (1.80)	1,068 (1.70)	1,077 (1.60)	974 (1.40)	949 (1.30)
Incompatibility of characters	660 (1.20)	658 (1.10)	427 (0.70)	361 (0.50)	315 (0.40)	380 (0.40)
Other	2,055 (3.60)	2,050 (3.40)	2,131 (3.30)	2,104 (3.10)	2,193 (3.10)	2,241 (3.10)
Divorce Type						
Administrative	9,431 (16.40)	10,290 (17.00)	11,395 (17.70)	11,686 (17.30)	11,850 (16.90)	12,163 (16.80)
Judicial	47,939 (83.60)	50,351 (83.00)	52,853 (82.30)	55,889 (82.70)	58,334 (83.10)	60,233 (83.20)
Total	57,370	60,641	64,248	67,575	70,184	72,396

Note: Percent of total in parentheses. Source: Statistics of Marriages and Divorces (INEGI).

Table 2.2: Demographic Characteristics by Law Status

Variable	Description	1993			2005		
		All States	Any Law	None	All States	Any Law	None
Divorce Rate	Divorces per 1,000 pop.	0.41	0.35	0.45	0.76	0.81	0.30**
Population Growth	Total population growth	2.00	2.03	1.98	1.02	1.04	0.83
Life Expectancy	Years	72.09	72.06	72.11	74.60	74.62	74.43
Birth Rate	Births per 1,000 pop.	27.60	27.22	27.81	19.54	19.49	20.00
Mortality	Deaths per 1,000 pop.	5.07	5.15	5.02	4.74	4.72	4.97
Fertility	Live children per woman	3.26	3.19	3.30	2.22	2.22	2.23
Marriage Rate	Marriages per 1,000 pop.	7.68	7.58	7.74	6.06	6.13	5.40
National Migration	National migrants per 1,000 pop.	3.06	2.02	3.60	1.90	1.88	2.17
International Migration	International migrants per 1,000 pop.	-4.41	-3.94	-4.66	-5.36	-5.13	-7.57
GDP	GDP per capita in 1993 pesos (in 1,000s)	12.70	12.28	12.93	15.14	15.59	10.79
Male-Female Ratio	Male/Female ratio	1.00	0.99	1.01*	0.98	0.98	0.96
Population	Population as of July 1 (in 1,000s)	2,774	3,909	2,179**	3,248	3,232	3,409
Undergraduate	Undergraduate enrollment	40,962	58,804	31,617	71,446	71,478	71,133
Graduate	Graduate enrollment	1,723	3,161	969	4,810	4,904	3,901
Number of States		32	11	21	32	29	3

Note: Any Law includes states that have adopted domestic violence, incompatibility or separation as of the given period. Any Law and None means are not equal at: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.3: Regression Results of Divorce Rates on Law Reform

	(1)	(2)	(3)	(4)
Any Law (DV, Incomp, or Separation)	0.136** (0.062)	0.057 (0.037)	0.041 (0.032)	0.010 (0.028)
Fertility Rate			-0.070 (0.147)	-0.159 (0.217)
GDP			0.012 (0.012)	-0.007 (0.010)
Internal Migration			-0.008 (0.004)	-0.016 (0.019)
International Migration			-0.003 (0.003)	0.005 (0.007)
Male-Female Ratio			5.430** (2.514)	5.849 (5.339)
Marriage Rate			0.015 (0.021)	0.005 (0.014)
Adjusted R-squared	0.039	0.906	0.919	0.957
Observations	416	416	416	416
State FE		✓	✓	✓
Year FE			✓	✓
State Trend, Linear				✓

Note: Robust standard errors, clustered at the state level are reported in parentheses.

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.4: Dynamic Effects of Law Reform on Divorce Rates

Any Law (DV, Incomp, Sep):	Dependent Variable: Annual Divorces per 1,000 Persons			
	(1)	(2)	(3)	(4)
1-2 years	0.168** (0.057)	0.036 (0.032)	0.023 (0.024)	0.018 (0.021)
3-4 years	0.090 (0.063)	0.014 (0.043)	-0.007 (0.037)	0.005 (0.031)
5-6 years	0.022 (0.070)	-0.004 (0.055)	-0.021 (0.048)	0.013 (0.046)
7-8 years	-0.015 (0.073)	-0.035 (0.057)	-0.048 (0.048)	0.001 (0.047)
9-10 years	0.027 (0.079)	-0.008 (0.065)	-0.025 (0.051)	0.039 (0.057)
11-12 years	0.112 (0.095)	0.046 (0.082)	0.045 (0.059)	0.011 (0.071)
13-14 years	0.058 (0.073)	-0.020 (0.082)	-0.026 (0.063)	0.074 (0.084)
15 or more years	0.175 (0.178)	-0.076 (0.099)	-0.087 (0.085)	0.086 (0.098)
Fertility Rate			-0.070 (0.142)	-0.159 (0.217)
GDP			0.014 (0.011)	-0.007 (0.010)
Internal Migration			-0.005 (0.004)	-0.023 (0.022)
International Migration			-0.001 (0.003)	0.005 (0.008)
Male-Female Ratio			5.775** (2.414)	5.173 (5.722)
Marriage Rate			0.020 (0.021)	0.006 (0.014)
Adjusted R-squared	0.056	0.908	0.923	0.959
Observations	416	416	416	416
State FE		✓	✓	✓
Year FE		✓	✓	✓
State Trend, Linear				✓

Note: Robust standard errors, clustered at the state level are reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.5: Divorce Rates on Law Reform: Number of Laws

	(1)	(2)	(3)	(4)
Number of Laws (DV, Incomp, Sep)	0.094* (0.052)	0.035 (0.024)	0.024 (0.022)	0.008 (0.020)
Fertility Rate			-0.093 (0.156)	-0.161 (0.218)
GDP			0.011 (0.011)	-0.008 (0.011)
Internal Migration			-0.007 (0.004)	-0.015 (0.019)
International Migration			-0.002 (0.003)	0.006 (0.007)
Male-Female Ratio			5.669** (2.461)	5.831 (5.280)
Marriage Rate			0.013 (0.021)	0.004 (0.014)
Adjusted R-squared	0.051	0.905	0.919	0.957
Observations	416	416	416	416
State FE		✓	✓	✓
Year FE		✓	✓	✓
State Trend, Linear				✓

Note: Robust standard errors, clustered at the state level are reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.6: Divorce Rates on Domestic Violence Reform

	(1)	(2)	(3)	(4)
Domestic Violence	0.214*** (0.064)	0.025 (0.031)	0.023 (0.031)	0.008 (0.020)
Fertility Rate			-0.097 (0.157)	-0.157 (0.215)
GDP			0.011 (0.011)	-0.008 (0.011)
Internal Migration			-0.006 (0.004)	-0.016 (0.215)
International Migration			-0.001 (0.003)	0.006 (0.007)
Male-Female Ratio			5.799** (2.515)	5.830 (5.304)
Marriage Rate			0.014 (0.021)	0.004 (0.014)
Adjusted R-squared	0.092	0.903	0.918	0.957
Observations	416	416	416	416
State FE		✓	✓	✓
Year FE		✓	✓	✓
State Trend, Linear				✓

Note: Robust standard errors, clustered at the state level are reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.7: Divorce Rates on Incompatibility Reform

	(1)	(2)	(3)	(4)
Incompatibility	-0.011 (0.185)	-0.143*** (0.021)	-0.227*** (0.049)	-0.079*** (0.020)
Fertility Rate			-0.126 (0.147)	-0.167 (0.211)
GDP			0.012 (0.012)	-0.008 (0.011)
Internal Migration			-0.006 (0.004)	-0.016 (0.019)
International Migration			-0.001 (0.003)	0.006 (0.007)
Male-Female Ratio			6.372** (2.427)	5.770 (5.369)
Marriage Rate			0.123 (0.020)	0.003 (0.014)
Adjusted R-squared	-0.002	0.904	0.920	0.957
Observations	416	416	416	416
State FE		✓	✓	✓
Year FE		✓	✓	✓
State Trend, Linear				✓

Note: Robust standard errors, clustered at the state level are reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.8: Divorce Rates on Separation Reform

	(1)	(2)	(3)	(4)
Separation	0.054 (0.084)	0.073 (0.043)	0.054 (0.037)	0.021 (0.033)
Fertility Rate			-0.082 (0.149)	-0.165 (0.217)
GDP			0.012 (0.011)	-0.008 (0.011)
Internal Migration			-0.007 (0.004)	-0.016 (0.019)
International Migration			-0.003 (0.003)	0.006 (0.007)
Male-Female Ratio			5.613** (2.482)	5.845 (5.295)
Marriage Rate			0.014 (0.020)	0.004 (0.011)
Adjusted R-squared	0.005	0.907	0.920	0.957
Observations	416	416	416	416
State FE		✓	✓	✓
Year FE		✓	✓	✓
State Trend, Linear				✓

Note: Robust standard errors, clustered at the state level are reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.9: Dynamic Effects of Law Reform on Divorce Rates

	Dependent Variable: Annual Divorces per 1,000 Persons							
	(1) DV	(2) Incomp	(3) Sep	(4) Admin	(5) DV	(6) Incomp	(7) Sep	(8) Admin
1-2 years	0.028 (0.027)	-0.161*** (0.027)	0.052* (0.029)	0.023 (0.016)	-0.010 (0.020)	-0.080*** (0.023)	0.032 (0.033)	0.009 (0.020)
3-4 years	0.005 (0.044)	-0.224*** (0.063)	0.059 (0.045)	-0.002 (0.024)	-0.057 (0.044)	-0.013 (0.039)	0.055 (0.056)	-0.013 (0.027)
5-6 years	-0.008 (0.071)	-0.275*** (0.067)	0.062 (0.059)	-0.034 (0.034)	-0.069 (0.072)	0.029 (0.041)	0.088 (0.078)	-0.024 (0.041)
7-8 years	-0.090 (0.115)	-0.310*** (0.084)	0.049 (0.060)	-0.065** (0.030)	-0.155 (0.099)	0.025 (0.043)	0.098 (0.082)	-0.057 (0.040)
9-10 years		-0.256*** (0.087)	0.085 (0.066)	-0.040 (0.031)		0.045 (0.067)	0.161 (0.100)	-0.046 (0.038)
11-12 years		-0.312*** (0.082)	0.148* (0.078)	0.012 (0.029)		0.059 (0.073)	0.251** (0.123)	-0.011 (0.026)
13-14 years		-0.312*** (0.082)	0.092 (0.082)	-0.015 (0.025)		-0.013 (0.075)	0.236 (0.141)	-0.031 (0.025)
15 or more years		-0.310*** (0.112)	0.023 (0.105)	-0.034 (0.039)		-0.024 (0.100)	0.268 (0.161)	-0.034 (0.048)
Adjusted R-squared	0.919	0.920	0.923	0.923	0.958	0.957	0.959	0.959
Observations	416	416	416	416	416	416	416	416
State Trend, Linear				√	√	√	√	√

Note: Robust standard errors, clustered at the state level are reported in parentheses. All regressions include state and fixed effects as well as demographic controls: fertility, internal and international migration rates, GDP, male/female ratio and marriage rate. * p < 0.10; ** p < 0.05; *** p < 0.01

Table 2.10: Divorce/1,000 persons (1993-2006) and Divorce Law Reform Years

State	1993	2000	2006	Admin ¹	Sep	DV	Incomp
North							
Baja California	0.96	0.78	1.43		2000	2004	
Baja California Sur	0.70	0.97	1.08	Yes	1996		
Coahuila ²	0.52	0.83	1.16	Yes	1999	1999	
Chihuahua	0.87	1.22	1.17	2006		2001	1974
Nuevo Leon	0.46	0.68	1.08	2002	2004	2000	
Sinaloa	0.41	0.61	1.00	Yes	1998	1998	
Sonora	0.60	0.85	1.01			2001	
Tamaulipas	0.41	0.54	0.61		1987	1999	
Average	0.62	0.81	1.07				
West Central							
Aguascalientes	0.48	0.81	1.12		2001	2001	
Colima	0.60	0.94	1.24	Yes	2003	2000	
Durango ²	0.44	0.72	0.87	Yes	2004	2001	
Guanajuato	0.18	0.44	0.71	1997	1989	2008	
Jalisco	0.36	0.40	0.54			2007	
Michoacan	0.24	0.39	0.66		2001	2001	1995
Nayarit	0.37	0.59	1.05	Yes	1990	2007	
San Luis Potosi	0.16	0.32	0.50	Yes	1990	1998	
Zacatecas	0.30	0.59	0.84	2000	2007	2003	
Average	0.35	0.58	0.84				
South-East							
Campeche	0.53	0.79	1.05	Yes	1994		
Chiapas	0.21	0.30	0.33	Yes	1998		
Guerrero	0.17	0.29	0.36	Yes	1990	2004	
Oaxaca	0.15	0.14	0.12	2002	2008	1999	1990
Quintana Roo	0.67	0.88	1.05	Yes	2004	2001	
Tabasco	0.42	0.52	0.71	Yes		2004	1980
Veracruz	0.25	0.43	0.47	Yes	1992	2003	
Yucatan	0.67	0.78	1.11	Yes	1993	1998	
Average	0.38	0.52	0.65				
Central							
Distrito Federal	0.68	0.84	0.79		1973	1997	
Hidalgo	0.14	0.20	0.31		1983		
Mexico	0.19	0.47	0.52	Yes		2007	
Morelos	0.30	0.39	0.42		1993	2006	
Puebla	0.23	0.33	0.35	Yes	1998	2007	
Queretaro	0.42	0.45	0.82	Yes	1990	2008	
Tlaxcala	0.15	0.15	0.16	2006		2006	1976
Average	0.41	0.58	0.77				

Note: 1. States that allowed for administrative divorce since the inception of divorce law in a state's civil code are noted as "Yes".

2. Coahuila and Durango adopted Admin reform in 12/15/2006 and 12/21/1997, respectively. In the analysis, these states were coded as having the reform in 2007 and 1998.

Chapter 3

Bargaining, Intra-family Violence Laws and Acts of Domestic Violence in Mexico

3.1 Introduction

Domestic violence is a public health issue present in both developed and developing countries, but its prevalence is more severe in developing countries. In Mexico, a survey indicated that 40 percent of married and cohabiting Mexican women over the age of 15 had been victims of domestic violence in 2006.^{1,2} Studies have shown

¹Encuesta Nacional sobre la Dinamica de las Relaciones en los Hogares, (ENDIREH 2006).

²In the U.S. the National Violence Against Women Survey found that 22 percent of women had been physically assaulted by an intimate partner at some point in their lives, and 1.3 percent reported such an event in the 12 months preceding a 1995 survey. <http://www.ncjrs.gov/txtfiles1/nij/183781.txt>, accessed May 10, 2010

that domestic violence is negatively associated with wages and health outcomes (Aizer forthcoming and Aizer 2009), and student performance (Carrell and Hoekstra 2010). Thus, determining whether public policy can affect behavior within the family becomes relevant because changes in domestic violence are likely to alter these outcomes for both current and future generations (Bowlus and Seitz 2006 and Pollak 2002).

In the last decade Mexico altered laws surrounding domestic violence and this event produces an opportunity to examine behavioral changes effected by legal institutional reform. The Mexican legal reforms provided access to divorce, police reporting and assistance programs to victims of domestic violence. These reforms redistribute bargaining power to the person who has the most to gain from exiting a relationship. In exit-threat bargaining models these legal reforms improve opportunities outside the relationship, thereby creating a credible threat to exit the abusive relationship. For example, if prior to the reforms, victims opted for costly alternatives, e.g. murder or suicide, to escape an abusive relationship, the legal reforms provide a cheaper or more attractive opportunity outside the marriage that can be used to bargain for less violence in the household.

Exploiting the heterogeneity in the adoption of legal reforms across states, I investigate whether these reforms explain changing trends in potentially lethal and non-lethal ends of domestic violence such as homicides, suicides and bodily injuries. I use a set of administrative data on homicides, suicides and crimes that contain detailed information which allows me to explore outcomes that reflect the group most likely affected by the treatment of the reform. For instance, I examine suicides by marital

status rather than overall suicides as has been done in studies on the U.S. Moreover, the richness of the data also provides me with other measures, e.g. other violent events that include information such as the age of the victim, and location where the event occurred that serve as placebo tests to probe into the robustness of the results. Lastly, I examine another outcome, injurious behavior, that is likely to reflect domestic violence behavior that does not result in a fatal outcome.

Although it has been more than a decade since the first intra-family violence (IFV) reform took place, no study has examined the impact of these reforms in Mexico, or in other Latin American countries such as Brazil, Chile, and Colombia that have adopted similar reforms at the national level. This study poses an opportunity to shed some light on the impact that this type of policy can have in developing countries where opportunities outside of the marriage may be limited by cultural and legal institutions.

The findings are suggestive of changes in household-bargained outcomes. I find evidence that male- and female-committed injury-related crimes were lower in states that provided assistance programs. I also find that states that adopted the reform to provide assistance programs saw a decline of about 6 percent in homicides committed against males. The effects on homicides represent half of the effect found on intimate partner homicides against women in the US. In the U.S., states that adopted unilateral divorce—thereby improving outside opportunities—experienced a decline in female suicides. By contrast, Mexican states that gave access to assistance programs had an 8-18 percent

decline in suicide rates of both men and women. While these effects are similar in magnitude with those found in the U.S., improving outside opportunities in Mexico had an impact on a broader population.

This paper is organized as follows. Section 3.2 presents the institutional background and economic framework. The empirical strategy and data are discussed in Section 3.2, followed by the results on bodily injuries (Section 3.4), homicides (Section 2.5) and suicides (Section 3.6). Robustness checks and a discussion on the results are presented in Section 3.7.3 The last section concludes.

3.2 Institutional Background and Economic Framework

3.2.1 Domestic Violence in the Mexican Legislature

Mexican laws of divorce and family relations are embedded in civil and penal codes that fall under each state's sovereignty. In the mid-1990s Mexican states³ began to reform their penal and civil codes in an effort to update statutes that had not changed since the 1920s.⁴

In 1996 Mexico City became the first state to introduce the "Law of Access, Assistance and Prevention against Intra-family Violence" (Assist Law). Assist Law defined intra-family violence (IFV) and established objectives and conditions under which inter-disciplinary agencies (Ministry of Education, Ministry of Health and Development,

³There are 31 states and the Federal District (Mexico City or Distrito Federal). Hereafter, I will refer to Mexico City as a state.

⁴As late as 1994, a Mexican Supreme Court ruled spousal rape as 'the undue exercise of a right.' www2.scjn.gob.mx/consultas/Comunicados, accessed March 2, 2009.

Ministry of Public Security, Department of Justice and a human rights commission) work together to increase public awareness and to provide prevention and assistance programs to victims of domestic violence. Under this law, IFV is defined as any act aimed to control or harm—physically, verbally or psychologically—any member of the family that is related by blood or affinity (whether married or cohabiting) living inside or outside the family residence. Some of the objectives of Assist Law include: training and educating judges, sensitizing law enforcement, health and social workers on domestic violence issues, running prevention and intervention programs, establishing shelters and centers that provide counseling, legal assistance and training to victims of IFV. Assist Law fosters a conciliatory process between the victim and the aggressor. Counselors serve as mediators suggesting solutions to prevent further domestic violence that are drawn into a contract agreed upon by the victim and the aggressor intended to prevent further domestic violence. Failure to fulfil the terms of the agreement can lead to prison, a restraining order, loss of alimony, and/or a fine.

Before states began to reform the penal code to explicitly define IFV, the penal codes covered crimes that were general, and which, for the most part, were consequences of aggressive behavior such as injuries or homicides. Sanctions for injuries were based on the severity of the wound, which was measured by the time it took to heal (15 days being the minimum for the injury to merit judicial intervention). Under the penal code reform (Penal Code) domestic violence (DV)⁵ is defined as “the use of physical or moral strength of one member of the family on another family member against his

⁵Generally, the penal codes discuss IFV while the civil codes use DV. Hereon I will use IFV and DV interchangeably.

or her physical or psychic integrity, independently of whether it results in injuries.” It further stipulates that the crime can be committed by the spouse, concubine, any blood relative or other relatives up to fourth degree, and adopted or adopting members living in the same household. The sanctions vary by state, but generally range from six months to four years in prison, loss of alimony or custody rights, a fine and/or counseling requirements.

While retaining the traditional grounds for divorce, states reformed their civil codes to allow “acts of domestic violence committed by the spouse against another or against the children” as grounds for divorce (Divorce Law). Although there were other causes, such as extreme cruelty or threats, that existed prior to this reform, they were seldom used,⁶ they did not explicitly define domestic violence, and the abusive spouse could still retain custody rights.

Table 3.1 lists the month and year in which each of the states adopted Assist Law, Divorce Law and Penal Code (the legal reforms) as of January 2007. This table shows that while some states adopted the legal reforms, others did not, and within the reforming states the changes occurred at various points in time. For instance, of the 32 states, Aguascalientes, Hidalgo, Chihuahua, and Yucatan had yet to adopt Assist Law, Tlaxcala Queretaro and Campeche had not adopted the Penal Code reform while there were six states without Divorce Law.

This study exploits the heterogeneity in the timing of the legal reform adoption across states to estimate the impact of IFV law reform on acts linked to domestic violence

⁶In the period 1992-1996, before the passage of the first reform, only 2.63 percent of all divorces used threats as a cause for divorce.

such as homicides, suicides and bodily injuries. The adoption of these reforms has occurred quite rapidly and evidence points to a change in behavior surrounding reform adoption. Figure 3.1 plots IFV crimes as a percent of total crimes committed after reform adoption. This figure illustrates that shortly after reform adoption, the percent of reported IFV crimes rose dramatically as access to reporting was facilitated by the reform. The percent of reported crimes continued to rise for approximately five years when it reversed to a downward trend. In Section 3.7, I discuss possible interpretations for why it may take time for shifts in behavior to occur; the next section presents the economic framework underlying this study.

3.2.2 Economic Framework

The modern literature on intra-household allocations focuses on bargaining models. Bargaining models relax the single household utility function and pooled budget constraint assumption of the Beckerian (Becker 1981) common preference approach and instead allow husband and wife to have distinct preferences in determining a bargained family outcome. In these models, the presence of threat points, which represent the highest level of utility attainable when no agreement is reached, determines intra-family distribution through Nash bargaining between the wife and the husband. Manser and Brown (1980) and McElroy and Horney (1981) present a cooperative (exit-threat) bargaining model in which the value of the threat point can be altered by changes in opportunities outside of the marriage. On the other hand, in Lundberg and Pollak's (1993) non-cooperative or separate spheres bargaining model, the threat point is de-

terminated within the marriage and the distribution of marital rents is maintained by a threat of reversion to a non-cooperative equilibrium. Their model predicts that changing opportunities outside the marriage will have little effect on the equilibrium outcome within the marriage.

The legal reforms can impact the incidence of domestic violence through changes in the external threat point. For instance, prior to Divorce Law, a victim wishing to dissolve the marriage could leave without the spouse's consent. But if the courts do not recognize domestic violence as legal justification for divorce the victim gives up the right to remarry and does not retain full legal child custody or alimony rights. Thus, under Divorce Law, the value of the exit threat point increases for the abused partner, as the rights to custody, alimony and remarriage are preserved. Furthermore, if the abuser stays in the marriage, the legal reforms create a credible threat that can be used to prevent abusive behavior. Changes in domestic violence behavior can occur even if the threat to divorce (available through Divorce Law), to use a shelter,⁷ (made possible through Assist Law) or to report the abuser to the authorities (available under Penal Code) is never exercised.

Domestic violence behavior can be affected by dissolution of the most violent relationships as marriage decisions might respond to divorce regime changes. However, as found in the second chapter above, there is evidence that Divorce Law had no impact on divorce or marriage rates. Thus, changes in domestic violence propensities could not be attributed to changes in divorce rates.

⁷Farmer and Tiefenthaler (1996) find that the use of shelters and other services can be used as signal of the victim's unwillingness to tolerate domestic violence.

If murder was being used to escape a violent relationship, and the legal reforms provide cheaper alternatives, abused spouses might substitute away from committing homicides. As such, one would expect to see a decrease in homicides or other forms of domestic violence because the threat to exit, if abused, becomes credible. Similarly, to the extent that abused partners resort to suicide to flee abusive relationships, the option to divorce or to incarcerate the abused partner may deter this course of action. On the other hand, if husbands feel that Divorce Law threatens their "right" to abuse their spouse, a substitution of private for public enforcement of their marriage contract (through more violence at home) could result in an increase in domestic violence.

Stevenson and Wolfers (2006) apply the exit-threat model to examine the impact of unilateral divorce laws in the U.S., which transfer bargaining power to the abused, on domestic violence, suicides and homicides. They find that unilateral divorce laws led to a decline in: women murdered by their partners, female suicide and domestic violence for both men and women. In the Mexican context, two empirical studies indicate that changes in a wife's income—brought by Mexico's flagship conditional cash transfer program Oportunidades—contributed to a decline in husband's aggressive behavior towards his wife (Angelucci 2008 and Bobonis *et al* 2009). Based on an observation that not many women use shelters or seek legal assistance, Rivera-Rivera *et al* (2003) claim that the legal reforms may have no significant impact on reducing domestic violence. However, as discussed above, in the exit-threat bargaining model, the threat does not have to be exercised to observe changes in intra-household bargained outcomes.

3.3 Empirical Strategy and Data

3.3.1 Identification

In order to identify the causal effect of the laws on measures of domestic violence the assumption is that the legal reforms are exogenous. Causal effects remain unidentified if the adoption of the legal reforms is correlated with time-varying unobservables that impact measures of domestic violence. An examination of the homicide rates in 1996 (the year in which the first legal reform occurred) and the intensity of the legal reform adoption suggests there is no systematic relationship between legal reform adoption and homicide rates (Figure 3.2). Some of the northern states with low homicide rates had higher intensity of law adoption than states with medium to high homicide rates.

Although not a test for exogeneity, one way to explore the timing of law adoption is by regressing the lagged outcome on the contemporaneous law dummy indicator. If high levels of domestic violence lead to the adoption of the legal reforms, the coefficient should be positive and statistically significant. Table 3.2 suggests that Assist Law did not come first for states with historically high rates of various measures of domestic violence. However, the timing evidence is somewhat inconclusive for Divorce Law and Penal Code adoption. That is, by chance, some coefficients are expected to be statistically significant, but I am finding slightly more statistically significant effects than would be expected under the null of no effect. Yet, there is no systematic relationship between the signs of the effects and the outcomes that would pose a major threat to

the identification of Penal Code and Divorce Law. The results suggest lower homicide rates occurred two years before Divorce Law adoption, but there is no effect for one year before law reform. Furthermore, the results point to a positive and statistically significant relationship between bodily injuries and male suicide rates two years before Penal Code adoption.

I will still present results for Assist Law, Divorce Law and Penal Code; however, the reader is left to judge whether decreasing homicide levels pre-dated Divorce Law or whether high bodily injury rates preceded Penal Code adoption and the extent to which these undermine the results. If evidence clearly indicated that states with higher incidence of domestic violence are adopting Penal Code reforms, the estimates on Penal Code would be upward biased. By contrast, one would be cautious about attributing any negative effects found on Divorce Law rather than to pre-existing trends.

Aside from establishing exogeneity, there needs to be enough variation to precisely identify the effects. Figure 3.3 shows there is variation, geographically and politically, in the adoption of the reforms and that there are no discernible patterns with respect to law adoption. For example, the northern border states do not show any systematic tendencies to adopt a particular law. There are northern border states that are early adopters of Assist Law but late adopters of Divorce Law or Penal Code, and vice versa. There is also variation with respect to the adoption of Assist Law, Divorce Law and Penal Code within the southern states. Some of these states are both early and late adopters and there is no indication that adoption of any of these reforms follows any particular pattern.

3.3.2 Model Specification

Using a difference-in-difference approach, this research design exploits the fact that not all states adopted the legal reforms and those that adopted the legal reforms did so at different points in time. The benchmark specification is equation (3.1):

$$DV_{st} = \beta law_{st} + X'_{st}\theta + \gamma_s + \delta_t + \epsilon_{st} \quad (3.1)$$

where DV_{st} is a measure of the domestic violence outcome of interest (homicide rate, suicide rate or bodily injury crime rate) in state s at time t . law_{st} is a categorical variable equal to one if state s had the reform for at least six months in year t . law_{st} can denote: allowing domestic violence as grounds for divorce (Divorce Law), defining IFV as a crime (Penal Code) or adopting an administrative law to assist victims of domestic violence (Assist Law). β is the average change in the outcome attributable to law adoption. γ_s represents state fixed effects that control for unobserved influences on measures of domestic violence that vary across states, while δ_t denotes year fixed effects that control for evolving unobserved national trends that affect measures of domestic violence.

X'_{st} is a vector of time-varying aggregate state level demographic controls for the sex ratio, population age structure, GDP per capita and percent of population enrolled in an undergraduate degree. Age structure represents the population ages 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older for men and women and they capture changes in the age population that may impact the adoption of the law

and the incidence of domestic violence. That is, if domestic violence behavior is more prevalent among a certain age group and a higher proportion of this age group in a state's population induces legal reform, then omitting it would lead to biased estimates. The sex-ratio serves as a proxy for changes in female empowerment which may affect both law adoption and changing attitudes towards domestic violence. If wealthier states or states with a higher proportion of educated people are more likely to adopt these reforms and these factors are associated with lower (higher) domestic violence propensity, then my estimates would be biased downward (upward). Finally, because the legal reforms are more likely to affect groups that are in a relationship and there could be differential attitudes or behavior surrounding the outcomes of interests, all the specifications are estimated separately for men and women, and where possible, by marital status. To address concerns of serial correlation (Bertrand *et al* 2004) standard errors are clustered at the state level.

One of the limitations with equation (3.1) is that the single dummy indicator captures the full adjustment process of the policy shock and does not map out the dynamic response of the adoption of the outcome to the legal reform. It is plausible that it takes time for information about the laws to be disseminated or for spouses to understand their new bargaining power; thus, a preferred specification—similar to Stevenson and Wolfers (2006)—is presented as equation (3.2). In this equation, ϕ_k , for

$k = 1, 2, \dots, 7$, is the estimated coefficient of a dummy law indicator that captures the effect of the law before and after law adoption. For instance, the first and second

$$DV_{st} = \sum_{k=1}^7 \phi_k law_{st} + X'_{st}\pi + \eta_s + \nu_t + \varsigma_{st} \quad (3.2)$$

dummy indicates, respectively, 2 and 1 year(s) before the law is adopted, the third denotes the year that a given law was changed, and the rest of the dummies is equal to one if the law has been in effect 1-2 years, 3-4 years, 5-6 years and more than 7 years.⁸ η_s and ν_t represent the state and year fixed effects, respectively.

3.3.3 Data

Information on the legislative changes was gathered by reviewing historical statutes of civil codes, penal codes and administrative laws for each of the 32 states available on the Mexican National Supreme Court of Justice's web site. The year of the legal reform was determined using the date in which the legal reform was published in the Official Newspaper (Diario Oficial). To allow for the possibility that it takes time for information to disseminate or for implementation to occur, I code the adoption of the legal reform to have occurred in the current year if it has been published for at least six months. The states of Colima and San Luis Potosi are excluded from the analyses because the publication dates for the adoption of Penal Code could not be verified.

Administrative data on homicides come from Mexico's Vital Statistics provided by the National Institute of Statistics and Geography (INEGI), which are compiled from

⁸I chose one-year intervals because I do not observe outcomes for four years prior to the law for some of the states.

the death certificate of all deceased persons in the country for 1994-2006. The individual level data used in this study contain a rich set of information that includes actual acts of domestic violence for a given period. The descriptive statistics of this subsample confirm relationships between domestic violence and demographic variables reported in other studies (WHO 2005). Moreover, the data also enable me to explore suicides by marital status, which capture groups that are more likely affected by the legal reforms, but which was not possible in the U.S. study by Stevenson and Wolfers (2006).

In the case of violent deaths, such as homicides, by law, a coroner or forensic authority must certify the cause of death before burial proceedings can take place; thus, it is unlikely that under-reporting is a major concern in this database. The extent to which homicides are reported as suicides introduces measurement error in the dependent variable, which does not affect the unbiasedness of the results. Assuming that some violent relationships end in homicides, intimate partner homicides would seem the appropriate measure to analyze. Yet, the death certificates do not contain information on the perpetrator or on the relationship of the murderer to the victim.⁹ An advantage of the data, however, is that IFV murders can be identified for 2000-2006, and these data reveal demographic characteristics about victims of IFV that are consistent with other studies.

Table 3.3 shows that IFV murder victims are more likely to be married or cohabiting, that they are more likely to work in non-professional occupations and have low levels of educational attainment. The data also show that 43 and 71 percent of

⁹Even if these data existed, problems would arise if classification of spousal relationships changed due to legal regime adoption. Stevenson and Wolfers (2006, p. 283).

male and female IFV homicides, respectively, occurred in the victim's home. Thus, for the period of analysis, I will use homicides committed in the victim's home to proxy for IFV homicides. It is worth noting the data also reveal that men are the victim in 55 percent of all IFV homicides. At first, this may seem at odds with the usual assumption that men are the perpetrators of domestic violence, however it brings forth the possibility that if men are the usual culprits of domestic violence, their acts do not result in a fatal outcome while women's acts do. This story is consistent with a study that found that in many cases Mexican women kill a relative as a result of persistent and long-lasting domestic violence (Azaola 1996, p. 117). When analyzing the homicide rate of all persons, not just those who are married or cohabiting, the estimates capture the effect of the legal reforms on murders that may not be related to domestic violence. However, due to sample size restrictions on the number of female murders I am not able to run the analysis by marital status for homicides committed against women.¹⁰

For suicide rates I have two sources of data: death certificates for 1994-2006 from Vital Statistics and deputy officer's reports on suicides and attempted suicides for 1996-2006 (Attempted Suicides). The advantage of the latter is that I can identify the cause (e.g. love, family arguments, financial, remorse) of the suicide for about fifty percent of the acts in the sample. In Table 3.4 I present descriptive statistics drawn from each database. The last two columns show that most of the events where the cause is reported was due to "love" or "family arguments," followed by illness. These data provide some evidence that suicides are plausibly associated with domestic violence.

¹⁰The counts by marital status for women show 117 and 34 state-year cells where there were no murders for single and married/cohabiting, respectively. This is unlikely due to non-reporting as the state shows murder counts for the entire year, but not for the given marital status.

There are a couple of disadvantages in using the Attempted Suicides database: INEGI did not begin collecting it until 1995 and there are cases in which three states do not report any suicides for 2006, while there are suicides reported in Vital Statistics for these states. Table 3.4 shows that the demographic characteristics are similar for the two databases, and to increase the sample size, the main specifications using suicides will draw from the death certificate data.

Crime data come from INEGI's Judicial Statistics in Penal Matter (Arrest data) available for 1997-2006, which are gathered from incidents reported to and investigated by law enforcement agencies (Ministerio Publico or public prosecutor). Arrest data include information for up to the first six¹¹ of all offenses associated with an investigation such as: the alleged criminal's state of residence, age, marital status, schooling, occupation, the psychic state of the individual at the time of the act (sober, under the influence of alcohol or drugs), the intent and degree of completion of each offense. Table 3.5 shows that a large portion of the perpetrators of IFV (crime code 171200) are married or cohabiting, but that IFV crimes are also committed by singles. Low levels of educational attainment are also associated with IFV crimes. Alcohol consumption also appears to be related with male-committed IFV crimes. Interestingly, compared to men, there is a higher percent of women who commit IFV crimes when sober and that are sent to prison. The first two columns in Table 3.5 show characteristics of female- and male-committed crimes related to "injuries", (crime code 170300¹²), and

¹¹In 2003, INEGI began collecting information for up to 99 counts. This change did not alter the composition of the counts. Before and after 2003, 99 percent of the arrests were associated with 3 counts.

¹²Injury crime is defined as an act "committed by any person who by any means infringes a harm against the health of another."

"threats" (crime code 210500¹³) Note that the demographic characteristics of the alleged suspects are also similar to those who committed IFV crimes. Because prior to Penal Code adoption, IFV crimes were likely to be reported as injuries and threats, I combine injuries, threats and intra-family violence crimes to construct a measure that I call "bodily injuries" to capture acts of domestic violence. Undoubtedly, issues of under-reporting are a concern because not all crimes are reported and not all reported crimes result in arrests.^{14,15} If the most abused victims or those that might be most likely to benefit from improved outside opportunities, are less prone to report then my estimates should be interpreted as a lower bound.

Finally, to construct a balanced panel of state level homicide, suicide and bodily injury crime rates, age structure and sex ratio measures, I use state level time-varying population counts available by gender and age group from the Mexican Population Council (CONAPO). GDP per capita is from INEGI's National Accounts, and educational measures are from the Ministry of Education.

¹³Threat is committed by anybody who "announces the intention to cause harm against another person, against their reputation, goods or rights, and is linked by any bonds or ties."

¹⁴The Mexican criminal procedure starts with a preliminary investigation by the Public Ministry, who acts as the highest authority. In order to formally begin a criminal procedure, the Public Ministry must compile evidence of probable responsibility and present the evidence and accused to a judge. If the judge finds probable cause, the procedure begins. If the Public Ministry does not compile enough evidence, it cannot prosecute the accused and the case is archived.

¹⁵In 2004, there were 77,961 new preliminary investigations (for all crimes), out of which 42 percent was sent to a judge. Of the 42 percent, 58 percent resulted in an arrest. www.pgr.gob.mx, accessed October 4, 2009.

3.4 Bodily Injuries

In this section I use bodily injury crimes to shed some light on injurious behavior closely related to domestic violence that did not result in a fatal outcome. The dependent variable is determined by combining the number of alleged injury, threats and IFV-related crimes committed per 100,000 persons for 1997-2006. As a first pass, I estimate the average impact of the legal reforms on bodily injury rates, and present the results in the first row of Table 3.6 (male-committed crimes) and Table 3.7 (female-committed crimes). Although the coefficients are imprecisely estimated, the findings suggest that reforming states experienced lower bodily injury crimes committed by the married and cohabiting group.

Given data constraints I cannot observe pre-law adoption injuries for states that adopted the reforms on or before 1997; thus, equation (3.2), which estimates the evolution of the reforms, is estimated without pre-law adoption indicators. The estimates are presented from the second to the sixth row of Table 3.6 and Table 3.7 for male- and female-committed crimes, respectively. The effects of Divorce Law and Penal Code are not statistically significant and do not show a clear pattern on the direction of any possible effects. Notwithstanding, the results for Assist Law tell a different story. There are negative and statistically significant effects on bodily injury crime rates. In particular, the effects grow with time and affect the married and cohabiting group more than the single group. Although the effects are larger for men, the results suggest that

female-committed bodily injury crimes were lower in states that adopted Assist Law. Averaging the effects, Assist Law can explain 20 and 19 percent of the decline in male- and female-committed crimes, respectively, related to bodily injury crimes.

Surprisingly, Assist Law also reduced the crimes committed by single females. This result points to another public health concern in Mexico: violence among adolescent dating couples. A nationally representative survey revealed that 15 percent of individuals of ages 15-24 experienced at least one act of physical abuse in the current dating relationship, and reported that in 61 percent of these cases, the victims are women.¹⁶ If this group experiences acts of violence that induce them to commit other acts of violence, Assist Law seems to have provided alternatives to such behavior. Having found no effects on Divorce Law and Penal Code, hereon I will focus on Assist Law.

To further probe into the results I examine the evolution of Assist Law on bodily injury crimes by age groups and graph the estimates in Figure 3.4. I estimate the evolution with 2-year and 1-year pre-law adoption indicators, so effects are identified off the post-1999 reforming states. The coefficients are close to zero; thus providing some evidence of a causal interpretation. There is also indication of a decline in bodily injury crimes committed by both men and women 4 years after law adoption, results that are consistent with Figure 3.1 where an observed decrease in IFV crimes as percent of all crimes began to occur 4.5 years after law adoption. Most of the effects are driven

¹⁶Encuesta Nacional de Violencia en las Relaciones de Noviazgo (ENVINOV), 2007.

by the 20-44 year-olds, and no effects are found for the 55 and older age group. Again, the results suggest that there was a change in behavior in the groups more likely to be involved in a relationship and therefore to domestic violence.

3.5 Homicides

So far the evidence indicates that states that adopted Assist Law saw a decline in bodily injury crimes. Now, I explore whether there were other observed behavioral changes on potential acts of domestic violence that might have ended in a fatal outcome. The dependent variable is the homicide rate (number of homicides committed in the victim's home per 100,000 population) determined separately by gender for all persons and then by marital status for men. The estimates are presented in Table 3.8, and for expositional purposes I include the specifications with and without controls. While the findings indicate no discernible effect on female homicide rates, the negative sign on Assist Law is consistent with an argument that women's wellbeing might have improved due to better outside opportunities. For male homicides (row 1), the model without controls suggests that states which adopted Assist Law experienced lower male homicide rates, again, showing a larger effect on the married and cohabiting group. Once the controls are added, the standard errors are smaller and the coefficients remain negative but become statistically insignificant.

The coefficients of the controls (not shown) point to behavioral differences by marital status. For instance, among singles, higher proportions of 15-19 year olds are associated with more male homicides while among women, the proportion of 30-39 year

-olds is positively associated with female homicides. A higher proportion of the population obtaining an undergraduate degree is negatively associated with female murders. Individually, except for the proportion of the 20-29 year-olds in the specification of homicides of single males, and the 30-39 year-olds in the female murders, the controls are not statistically significant. However, I will keep the controls because an F-test shows these controls are jointly statistically significant, and irrespective of whether these controls are correlated with law adoption, they explain criminal or violent behavior.

The estimates of equation (3.2) are also shown in Table 3.8 in rows (2) to (8) for Assist Law.¹⁷ Consistent with estimates of equation (3.1), there are no discernible effects on female homicide rate, but there are negative and statistically significant effects for Assist Law on the homicide rate of the married and cohabiting group. Focusing on the married and cohabiting group a couple of things are worth noting. First, the estimates on the dummies indicating 2 years and 1 year prior to Assist Law are close to zero and statistically insignificant (individually or jointly). This points to a causal interpretation of the results. Second, the results are not sensitive to the inclusion of controls.¹⁸ Moreover, note that the largest effects are observed 5 years after law adoption, and that the estimates continue on a downward path.

Thus far, the findings suggest that if Mexican women were committing murders to escape domestic violence, access to counseling, shelters or legal assistance induced

¹⁷This analysis was done also for Divorce Law and Penal Code and the results show no discernable effect. Results are available upon request.

¹⁸The results are also robust when I exclude one state at a time. For the preferred dynamic specification of the married and cohabiting group, on average, the coefficient for 5-6 years and ≥ 7 years is -0.68 and -0.72, respectively.

behavioral changes in the household, just as the model predicted. Averaging the estimates over the period following adoption, Assist Law explains 6 percent of the decline in the average married and cohabiting male homicide rate. The magnitude of the effect is about half of that found by Stevenson and Wolfers (2006). Specifically, Stevenson and Wolfers found that improved outside opportunities available through unilateral divorce led to a decline in male murders but found no discernible effects on intimate murders committed against women. Can these differences be reconciled? U.S. data show there is a higher incidence of females being murdered by their spouse. Since there are no Mexican data on intimate homicides I cannot directly determine whether the husband or the wife is more likely to murder his or her spouse. Notwithstanding, arrest data show that Mexican women are equally likely as men to successfully murder a family member. Moreover, a study reported that 8 percent of males incarcerated for homicide had killed a family member while the rest of the convictions involved males killing strangers in street confrontations or assaults. By contrast, the same survey showed that 76 percent of incarcerated women had been convicted for killing a relative (Azaola 1996, p.64).

3.6 Suicides

If women commit suicide as a means to escape an abusive relationship, suicidal behavior can capture the result of a domestic violence act. The dependent variable is the suicide rate (number of suicides per 100,000 persons) of all persons and then it

is disaggregated by marital status.¹⁹ If marriage decisions respond to Divorce Law or Penal Code one would be concerned about endogeneity when using the suicide rates by marital status. However, previous work (Beleche and Lew 2009) found that Divorce Law had no impact on divorce rates. I also regressed the Penal Code law indicator on divorce rate and a set of state-varying controls such as education, GDP per capita, migration and fertility rates and found no evidence that Penal Code had any effects on marital dissolution.

The results from estimating equation (3.1) are shown in row (1) of Table 3.9 and Table 3.10 for males and females, respectively. Without controls, the coefficients on Assist Law are negative but not statistically significant, and when controls are added, the coefficient on Assist Law switches sign for the married and cohabiting male group. Given the wide confidence intervals, the estimates of the second column could go in either direction. One possible explanation for finding positive effects would argue that marital dissolution can lead to more unhappy spouses and hence to higher suicidal behavior. However, this scenario seems unlikely since I have not found evidence that marital dissolution is being directly affected by Divorce Law and Penal Code.

Examining the evolution of Assist Law adoption on suicide rates I find statistically significant effects which indicate that suicide rates of the married and cohabiting group began declining 3 years after the adoption of Assist Law (see rows (6)-(7)). Table

¹⁹Using linear interpolation, I estimated the suicide rates for the state of Tlaxcala that showed no suicides for the entire year in 2000. The results do not change when this state is excluded entirely from the analysis.

3.9 and Table 3.10 also show that inclusion of controls does not change the coefficients significantly, and that the drop in the suicide rate did not pre-date the adoption of Assist Law. Moreover, inclusion of the controls also reduces the standard errors.

As an additional check, I explore the overall suicide rates by age groups. The overall suicide rate captures the effect of the reform on suicides for those who remain in a relationship and for those who exit. If the legal reforms are directly affecting suicide propensities one might expect them to impact prime-age individuals more than teens or the elderly. Figure 3.5 presents the estimates of equation (3.2) with dummies to indicate periods before Assist Law for both men and women. Due to small sample sizes I include the elderly in the 55 and older category and create a separate category for those whose age cannot be determined. The top panel of Figure 3.5 illustrates that Assist Law had no impact on males ages 14 and younger, which may reflect both a relatively small number of suicides in this age group and little relationship between the legal reforms and suicide rate. As expected, the results indicate the male age groups 25-34 and 45-54 contributed the most to the decline. The 55 and older male age group seemed to have also contributed to the decline, although the standard errors are less precisely estimated. For women, the 35-44 age group contributed the most to the decline in female suicide, while the teens and the elderly did not.

Overall, evidence suggests that a shift in bargaining power through Assist Law created alternatives to exit the abusive relationship. Averaging the effects over the years following Assist Law reform points to a long-run decline of 11 and 19 percent in male and female suicide rates, respectively. Interestingly, Stevenson and Wolfers' (2006)

study found that improvements in bargaining power via unilateral divorce laws led to a decrease in female suicide rates of about 8-10 percent, but concluded there were no effects on male suicide rates. Yet, suicide prevalence and the circumstances surrounding the decision to commit suicide differ between Mexico and the U.S. First, religiosity may explain why suicide rates are, in general, lower in developing countries. Second, in Mexico, the main reason for suicide is love or family disputes, while in the U.S. the Center for Disease Control reports that at least half of the cases where the cause was known were related to mental illness.²⁰ Thus, improving outside opportunities for the group more vulnerable to suicide seems to be altering such behavior for both men and women.

3.7 Robustness Checks and Discussion

3.7.1 Pre-existing Trends

One concern is attributing changes on the outcomes of interest to Assist Law adoption rather than to pre-existing trends. To determine whether I have identified the effects of the legal reform separately from other confounding effects, I conduct a battery of placebo tests in which I use the same empirical strategy on a set of outcomes that are products of violent outcomes but should not be affected by the domestic violence legal reforms. As a first check, I conduct the same exercise using homicides committed

²⁰<http://www.cdc.gov/mmwr/preview/mmwrhtml/mm5526a1.htm>, accessed on September 24, 2009.

in public places,²¹ and present the results in Table 3.11. I present the static (row (1)) estimates for all legal reforms and the evolution (rows (2) to (8)) of Assist Law. Table 3.11 shows there is no relationship between homicides committed in public areas and legal reform adoption, and hence no indication that pre-existing trends are driving the results of Tables 3.6 to 3.10. The evolution of Assist Law provides further evidence of no relationship over time. Estimates of the evolution of Divorce Law and Penal Code adoption suggest that the Penal Code and Divorce Law are not associated with homicides of singles and married and cohabiting groups that were committed in public places.²² As an additional robustness check, I use thefts, which are the most common crime in Mexico, and represent more than 30 percent of total crimes reported during the period of analysis. Table 3.12 reports the estimates of equation (3.1) where annual thefts per 100,000 population is the dependent variable. As Table 3.12 shows, there is no indication that legal reform adoption is associated with thefts. The findings (not shown but available upon request) from the dynamic specification do not unduly change the results.

3.7.2 Coding of Legal Reforms and Inclusion of Large States

A couple of issues arise with respect to coding the year of legal reform adoption. First, recognizing that a reform is adopted sometime during the year raises the question of whether the given state should be considered a control or a treatment state. I address this by conducting the same analyses done in Tables 3.6 to 3.9 but excluding the year in

²¹These include homicides committed at work, public building or recreational area.

²²Results available upon request.

which the reform occurred. The modification (not shown) does not change the results discussed in Sections 3.6 to 3.9. For example, when either suicides or homicides is the outcome, the coefficients change slightly (from -0.67 to -0.511 for '5-6 years later' when homicide is the outcome), but the negative and statistically significant effects remain for the same years following law adoption. Moreover, the coefficients are not statistically significant and are close to zero in periods preceding law adoption. When bodily injuries is the outcome, excluding the year of reform results in estimates that are smaller for the married and cohabiting male group on Assist Law but the other results do not change significantly. Second, I ran all the specifications under two other cases, and find that the results are not sensitive to the coding. In the first case, the year of reform dummy is one if the reform has been in place for any period of time during the current year. In the second case, the year of reform dummy is one if the law has been in effect for 12 months in the current year.

In further robustness checks, and to address the concern that certain states are driving the results, I omitted individual states one at a time for each of the outcomes and the results remain.

3.7.3 Discussion

In light of the results, why are effects found for Assist Law but not for Divorce Law or Penal Code? Some interpretations are plausible. The first is that Assist Law provides an immediate (through a conciliatory contract) and less costly solution to domestic distress. Since most victims of domestic violence are women who are uneducated

and who do not work, victims might believe that incarcerating or divorcing the husband will leave them without recourse. Depending on the state, an IFV offender may be incarcerated for a period that can range from 3 months to 6 years, and it is possible that this punishment may be perceived as too harsh for victims who depend financially on the aggressor. A study by Kessler and Levitt (1996) found that punishments broader in scope and less punitive may prove more effective to fight crime.

Another explanation is that reputation of law enforcement may reduce incentives for police reporting. Studies suggest that reporting is more likely if there is high probability of punishment (Ehrlich 1996). Anecdotal evidence points to cases where the Public Ministry did not conduct a preliminary investigation required to proceed with judicial procedures even when the victim initiated legal action against the aggressor (Perez Contreras 2000 and COVAC 1995). Since counselors provide conciliatory alternatives and do not report the aggressor to the authorities unless the victim authorizes it, there is less of an association with the judicial system—and hence to low probability of law enforcement or punishability—attached to the services provided by Assist Law.

Even though Divorce Law stipulates that child custody, alimony and child support be given to victims of domestic violence, some evidence suggests that collection of these transfer payments might prove difficult in developing countries (Goode 1993, pp. 207-211). Furthermore, some states require evidence—a witness, medical report, and specific accounts on recurring domestic violence events—before it can be used as grounds for divorce. It is also likely that domestic violence is simply perceived as a spousal obligation rather than a crime or a reason to divorce. Thus, Assist Law—through public

awareness campaigns—may be an underlying mechanism through which social norms and intra-family behavior is changing. However, it takes time for effects to percolate through Mexican society. First, Assist Law requires coordination of multiple agencies which might slow down the time it takes for states to fully implement the law. Second, the time it takes to train law enforcement, social and health workers expands the time before behavioral changes can be observed.

3.8 Conclusion

Examination of the evolution of the legal reforms surrounding domestic violence consistently showed that improving opportunities outside a potentially abusive relationship, such as access to shelters and counseling, changed the trends in homicides, suicides and bodily injuries of married and cohabiting individuals. Specifically, bodily injury crimes are lower by about 20 percent in states that adopted the administrative law to provide support and assistance to victims of domestic violence. I found that homicide rates against men and suicides were also lower in states that adopted such reform.

A comparison of the results in this study with those of a study done on the US reveals that there are large effects to improving opportunities outside the relationship in an environment where the prevalence of domestic violence is more severe and where the outside opportunities without institutional reform might be lower than developed countries. The findings suggest that public policy can alter domestic violence behavior and thereby labor market outcomes.

Figure 3.1: IFV Crimes Reported as a Percent of All Crimes, 1997-2006

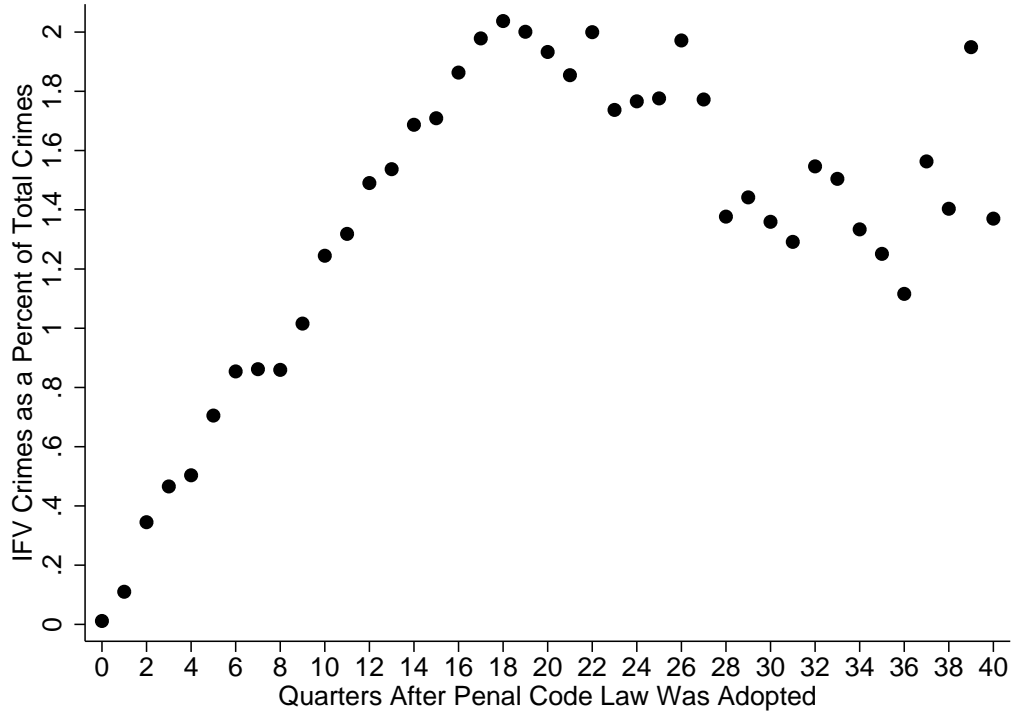


Figure 3.2: Male and Female Homicide Rates in 1996 and Adoption of Legal Reforms

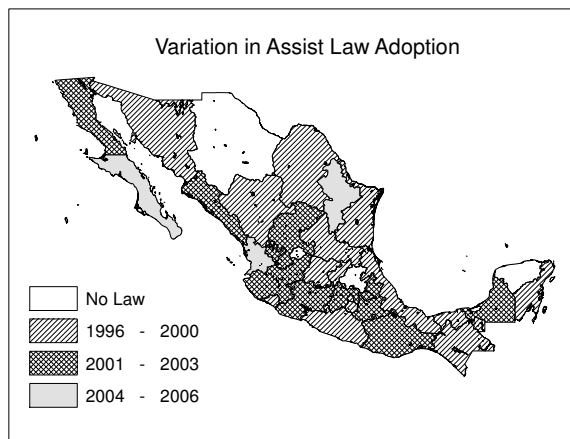
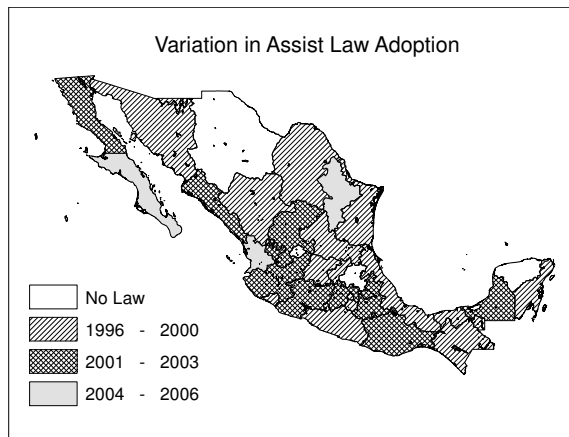
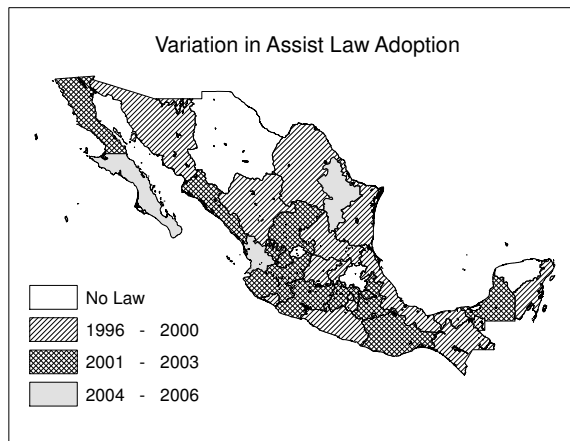


Figure 3.3: Heterogeneity in Timing of Legal Reforms, 1996-2006

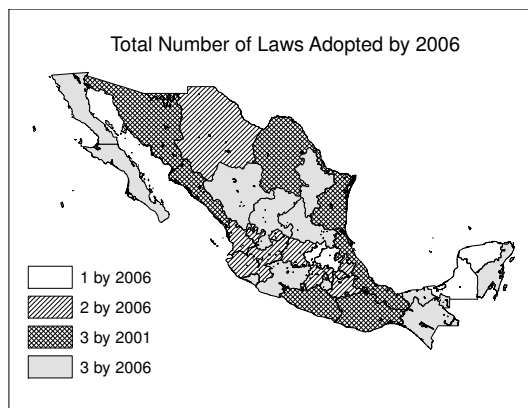
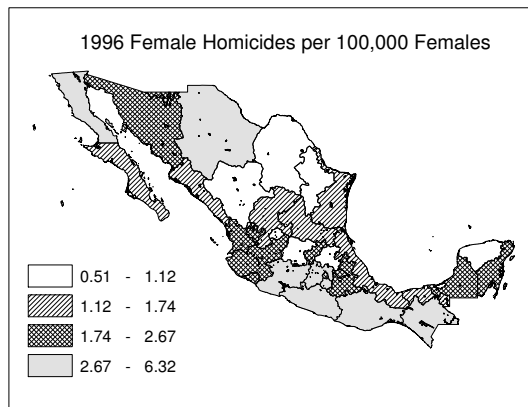
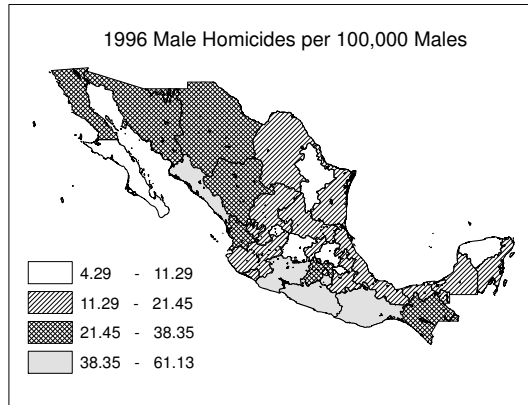


Figure 3.4: Effect of Assist Law and Contribution of Each Age Group to Bodily Injury Crimes

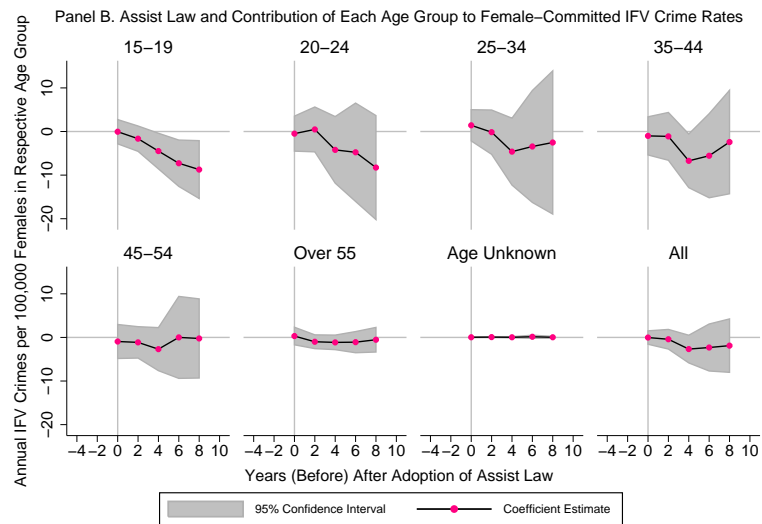
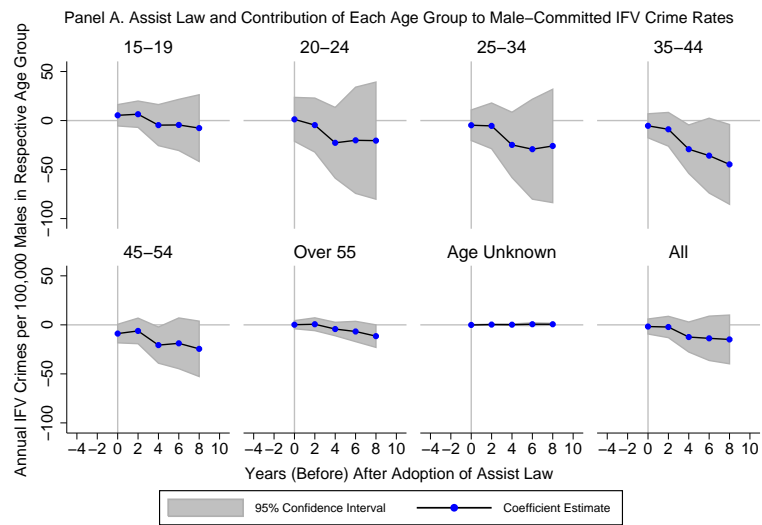
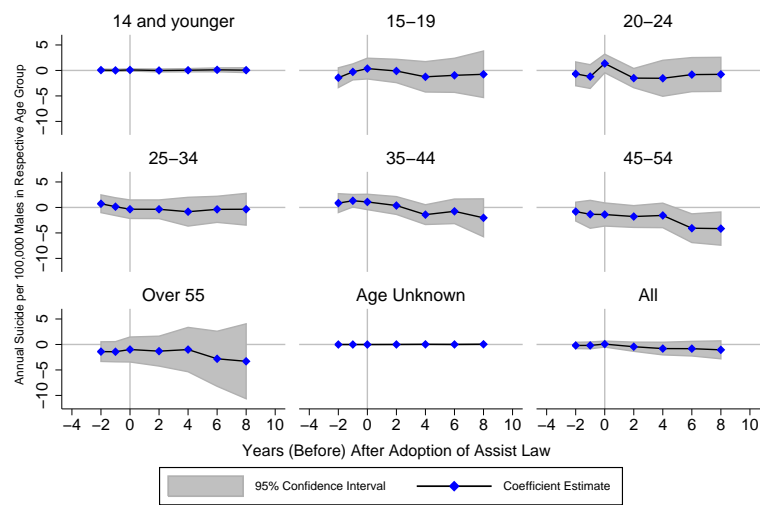


Figure 3.5: Effect of Assist Law and Contribution of Each Age Group to Suicide Rate

Panel A. Assist Law and Contribution of Each Age Group to Male Suicide Rates



Panel B. Assist Law and Contribution of Each Age Group to Female Suicide Rates

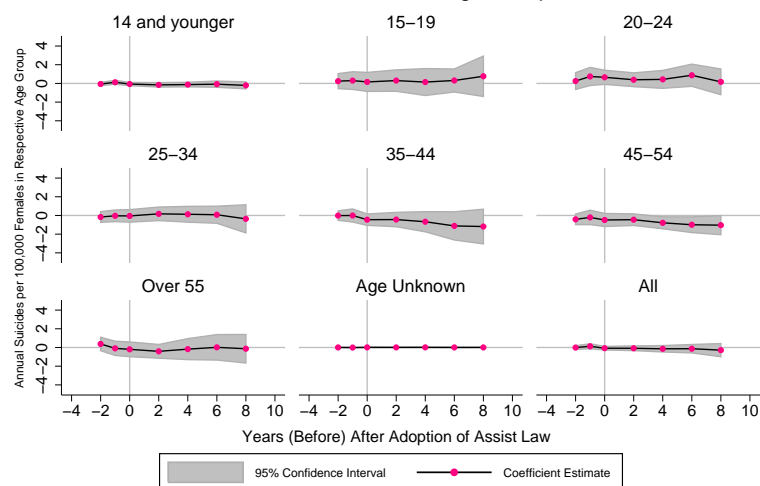


Table 3.1: Month and Year of Introduction of Legal Reforms by State

State	Assist Law	Divorce Law	Penal Code
Aguascalientes		11/2001	02/2001
Baja California	07/2003	09/2004	06/1998
Baja California Sur	03/2005	01/2002	03/2005
Campeche	06/2002	06/2007	
Coahuila	01/1997	06/1999	05/1999
Colima*	02/1998	03/2000	11/2005
Chiapas	07/1998	11/2004	08/2001
Chihuahua		09/2001	02/2001
Distrito Federal	07/1996	12/1997	12/1997
Durango	12/1999	05/1998	04/2004
Guanajuato	02/2000		11/2001
Guerrero	04/1999	11/1999	04/1999
Hidalgo			01/2002
Jalisco	12/2003	11/2007	09/2000
México	12/2002	01/2007	03/2000
Michoacán	02/2002	04/2001	04/2001
Morelia	01/1999	09/2006	06/2004
Nayarit	05/2004	05/2007	12/2004
Nuevo León	02/2006	01/2000	01/2000
Oaxaca	09/2001	02/1998	02/1998
Puebla	04/2001	11/2007	09/2003
Querétaro	12/1996		
Quintana Roo	06/2000	07/2004	06/2006
San Luis Potosí*	07/1998	10/1998	09/2000
Sinaloa	11/2001	10/1998	03/2003
Sonora	12/1999	05/2001	05/2001
Tabasco	04/1999	05/2003	05/2003
Tamaulipas	06/1999	06/1999	06/1999
Tlaxcala	05/2001	01/2006	
Veracruz	09/1998	09/1998	09/1998
Yucatán			03/2000
Zacatecas	02/2003	02/2003	08/2001

Note: * Publication date could not be verified for Penal Code.

Blanks indicate that no law had been passed as of January 1, 2007.

Table 3.2: Lagged Outcomes on Law Indicator

Lag of:	Female Suspects/Victims			Male Suspects/Victims		
	Bodily Injuries	Suicides	Homicides	Bodily Injuries	Suicides	Homicides
	Assist Law					
[t-1]	0.004 (0.006)	0.014 (0.037)	-0.042 (0.056)	0.000 (0.001)	-0.007 (0.013)	0.009 (0.026)
[t-2]	0.002 (0.007)	-0.010 (0.035)	-0.047 (0.062)	0.000 (0.001)	-0.001 (0.009)	-0.009 (0.022)
	Divorce Law					
[t-1]	0.004 (0.005)	0.038 (0.033)	0.020 (0.039)	0.002 (0.002)	0.014* (0.008)	0.005 (0.017)
[t-2]	-0.007 (0.005)	0.016 (0.029)	-0.087** (0.041)	-0.001 (0.001)	-0.005 (0.011)	-0.037* (0.011)
	Penal Code					
[t-1]	0.009 (0.008)	0.036 (0.035)	-0.023 (0.047)	0.000 (0.002)	-0.007 (0.011)	-0.01 (0.025)
[t-2]	0.014** (0.006)	0.052* (0.027)	-0.043 (0.046)	0.003* (0.001)	-0.008 (0.010)	-0.028 (0.017)

Note: * p < .10; ** p < 0.05; *** p < 0.01. Sample 1996-2006 (n=275) for suicides and homicides, 1999-2006 (n=170) for bodily injuries. Standard errors clustered at the state level. The dependent variable is the respective law indicator for state s in year t. Suicide, homicide and bodily injuries are the annual state level counts per 100,000 population. States of Colima and San Luis Potosi are excluded. All specifications include state, year fixed effects and state controls. State controls include state GDP per capita, percent of population enrolled in an undergraduate degree, sex ratio, and age structure. Age structure is the proportion of a state's population 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older.

Table 3.3: Descriptive Statistics of Homicides by Victim's Gender

	1994-2006		2000-2006	
	All Homicides		IFV Homicides	
	Male	Female	Male	Female
Marital status (%)				
Single	34.61	29.80	23.57	11.22
Married/Cohabiting	54.31	41.91	57.89	64.16
Div/Sep/Widow	3.63	11.50	4.93	5.47
Unknown	7.45	16.79	13.60	19.15
Occupation (%)				
None	7.31	48.64	6.25	57.18
Agriculture	25.10	45.72	45.72	2.19
Prof/Tech/Admin	3.64	3.78	2.63	3.01
Manuf/Transport	21.40	2.38	17.76	2.46
Sales/Services	22.64	17.55	14.25	16.96
Unknown	19.91	26.09	13.38	18.19
Schooling (%)				
None	11.61	14.61	13.38	12.59
Primary	24.29	20.94	39.04	29.55
Secondary	38.07	30.73	27.41	29.00
High School	7.91	8.69	3.18	6.84
Professional	5.04	6.02	2.96	3.97
Unknown	13.08	19.01	14.04	18.06
Median Age (years)	32	30	33	29
Place of Occurrence (%)				
Home	12.44	36.59	43.31	71.00
Other	67.63	43.42	46.93	21.75
Unknown	19.93	20.00	9.76	7.25
Observations	135,533	17,484	912	731

Note: States of Colima and San Luis Potosi are excluded.

Table 3.4: Descriptive Statistics of Suicides and Suicide Attempts

	1994-2006		1995-2006	
	All Suicides		Suicides and Suicide Attempts	
	Male	Female	Male	Female
Marital status (%)				
Single	42.22	45.72	40.02	44.35
Married/Cohabiting	48.75	44.03	48.61	43.17
Div/Sep/Widow	5.96	7.52	5.65	5.66
Unknown	3.07	2.72	5.71	6.82
Occupation (%)				
None	17.21	71.20	19.32	60.80
Agriculture	20.50	0.76	17.61	0.80
Prof/Tech/Admin	4.32	4.23	2.77	2.75
Manuf/Transport	24.98	2.20	22.14	2.37
Sales/Services	23.75	13.37	21.53	12.92
Unknown	9.23	8.23	16.62	20.00
Schooling (%)				
None	8.22	7.67	14.10	11.20
Primary/Secondary	68.64	64.82	43.46	40.15
High School	9.61	13.63	6.80	8.69
Professional	6.16	8.31	4.99	5.87
Unknown	7.37	5.56	30.65	34.09
Median Age (years)	31	25	31	25
Place of Occurrence (%)				
Home	66.02	74.68	76.58	88.56
Other	20.20	12.47	22.63	10.60
Unknown	13.79	12.84	0.79	0.83
Had Children? (%)				
Yes			36.79	32.68
No			31.31	36.58
Unknown			31.90	30.74
Reason for Suicide (%)				
Love/Family			16.93	31.38
Financial			3.87	2.45
Other			19.80	19.57
Unknown			58.91	46.61
Observations	37,459	7,351	28,898	7,668

Note: States of Colima and San Luis Potosi are excluded. In 2006, Tlaxcala and Yucatan did not report any events in the Suicides and Attempted Suicides data.

Source: All Suicides are from Vital Statistics, Suicides and Attempted Suicides are from Suicides and Attempted Suicides Reports (INEGI).

Table 3.5: Descriptive Statistics of Criminal Suspects, 1997-2006

	Alleged Crime			
	Injuries and Threats		IFV	
	Male	Female	Male	Female
Marital Status (%)				
Single	34.50	30.59	14.91	25.57
Married/Cohabiting	61.70	62.27	79.83	65.59
Div/Sep/Widow	1.88	5.30	3.24	7.22
Unknown	1.92	1.84	2.01	1.62
Occupation (%)				
None	7.64	60.75	6.11	56.60
Agriculture	19.67	0.98	12.65	1.11
Prof/Tech/Admin	4.86	5.70	5.92	3.24
Manuf/Transport	38.24	3.75	44.36	6.12
Sales/Services	28.14	27.67	29.01	31.39
Unknown	1.46	1.15	1.96	1.55
Schooling (%)				
None	7.02	8.34	7.09	11.57
Primary	37.61	35.81	39.62	43.63
Secondary	30.23	27.28	28.66	24.32
High School	13.88	14.22	12.32	10.54
Professional	8.52	10.64	9.04	6.48
Unknown/Other	2.75	3.70	3.27	3.46
Median Age (years)	32	33	36	32
Number of Counts (%)				
1	65.38	79.52	54.10	56.82
2	26.69	17.49	38.07	37.80
> 2	7.93	2.99	7.83	5.38
Physical State (%)				
Sober	67.78	90.67	62.35	88.73
Drunk	24.35	3.59	28.71	4.20
Other	1.06	0.20	2.13	1.03
Unknown	6.80	5.55	6.82	6.04
Ruling (%)				
Prison	72.24	54.16	83.79	88.87
Subject to Process	19.92	38.93	2.27	1.77
Freed, No Evidence	4.77	3.90	8.60	8.03
Extinction of Law	3.07	3.00	5.34	1.33
Observations	295,445	55,268	13,439	1,357

Note: States of Colima and San Luis Potosi are excluded.

Source: Judicial Statistics on Penal Matter, (INEGI).

Table 3.6: Effects of Legal Reforms on Bodily Injury Crimes Committed by Males

	Assist Law			Divorce Law			Penal Code		
	All	Single	Married, Cohabiting	All	Single	Married, Cohabiting	All	Single	Married, Cohabiting
Mean Injury Crime Rate	82.83	28.38	51.01	82.83	28.38	51.01	82.83	28.38	51.01
Equation (1) (1) Average Effect	-0.315 (4.416)	0.659 (1.541)	-0.749 (2.884)	-1.090 (5.269)	-0.846 (1.691)	-0.477 (3.615)	-1.090 (5.269)	-0.846 (1.691)	-0.477 (3.615)
Equation (2) (2) Year of change	-1.742 (3.681)	-1.057 (1.483)	-2.258 (2.310)	-0.951 (4.122)	-0.641 (1.545)	-0.613 (2.787)	0.079 (3.322)	-0.951 (4.122)	-1.216 (2.332)
(3) 1-2 years later	-3.531 (4.569)	0.103 (1.502)	-3.405 (3.069)	-0.923 (6.844)	-1.133 (2.266)	0.035 (4.586)	-1.168 (5.677)	-0.923 (6.844)	-1.262 (3.925)
(4) 3-4 years later	-15.137** (6.382)	-2.883 (2.074)	-11.650** (4.337)	-1.154 (8.021)	-0.279 (2.569)	-0.990 (5.481)	1.696 (8.929)	-1.154 (8.021)	0.767 (6.147)
(5) 5-6 years later	-18.289* (10.388)	2.202 (3.234)	-15.476 (6.883)	-1.939 (9.038)	-0.287 (3.046)	-2.154 (6.277)	-2.477 (10.435)	-1.939 (9.038)	-2.886 (7.376)
(6) ≥7 years later	-20.212* (11.021)	-1.554 (3.617)	-17.248 (7.485)	0.766 (13.374)	-0.377 (4.632)	1.110 (9.007)	-9.497 (14.683)	0.766 (13.374)	-6.322 (9.918)

Note: * p < 0.10; ** p < 0.05; *** p < 0.01. Sample 1997-2006 (n=300). Standard errors clustered at state level. States of Colima and San Luis Potosi are excluded. The dependent variable is the annual state level injury crimes per 100,000 males. IFV related (171200), threats (210500), and injuries (170300). All specifications include demographic controls, state, and year fixed effects. Demographic controls include state GDP per capita, percent of population enrolled in an undergraduate degree, sex ratio, and age structure. Age structure is the proportion of a state's population 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older.

Table 3.7: Effects of Legal Reforms on Bodily Injury Crimes Committed by Females

	Assist Law			Divorce Law			Penal Code		
	All	Single	Married, Cohabiting	All	Single	Married, Cohabiting	All	Single	Married, Cohabiting
Mean Injury Crime Rate	13.32	3.97	8.34	13.32	3.97	8.34	13.32	3.97	8.34
Equation (1)									
(1) Average Effect	-0.084 (0.932)	-0.134 (0.346)	-0.034 (0.593)	0.199 (1.165)	-0.22 (0.410)	-0.073 (0.741)	-0.319 (0.783)	-0.355 (0.334)	-0.014 (0.483)
Equation (2)									
(2) Year of change	-0.066 (0.772)	-0.172 (0.298)	0.028 (0.558)	0.046 (0.953)	0.049 (0.400)	0.064 (0.638)	-0.338 (0.678)	-0.419 (0.278)	0.055 (0.483)
(3) 1-2 years later	-0.839 (1.067)	-0.417 (0.400)	-0.584 (0.681)	0.679 (1.539)	0.407 (0.553)	0.052 (0.923)	-0.341 (1.438)	-0.407 (0.572)	-0.025 (0.827)
(4) 3-4 years later	-3.692** (1.582)	-1.412** (0.583)	-2.281** (0.966)	0.290 (1.998)	0.483 (0.654)	-0.368 (1.252)	0.302 (2.083)	-0.061 (0.792)	0.301 (1.269)
(5) 5-6 years later	-3.867 (2.614)	-1.412** (0.583)	-2.464 (1.688)	0.882 (2.182)	0.337 (0.775)	0.136 (1.318)	-0.006 (2.544)	-0.436 (0.947)	0.318 (1.532)
(6) ≥ 7 years later	-3.920 (2.906)	-1.661* (0.936)	-2.541 (1.893)	2.126 (3.123)	0.781 (1.080)	0.854 (1.929)	-0.597 (3.484)	-0.719 (1.156)	0.054 (2.233)

Note: * p < 0.10; ** p < 0.05; *** p < 0.01. Sample 1997-2006 (n=300). Standard errors clustered at state level. States of Colima and San Luis Potosi are excluded. The dependent variable is the annual state level injury crimes per 100,000 females. IFV related (171200), threats (210500), and injuries (170300). All specifications include demographic controls, state, and year fixed effects. Demographic controls include state GDP per capita, percent of population enrolled in an undergraduate degree, sex ratio, and age structure. Age structure is the proportion of a state's population 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older.

Table 3.8: Effects of Assist Law on Homicide Rates: Place of Occurrence is Victim's Home

	Male Victims			Female Victims		
	All	Single	Married, Cohabiting	All	Married, Cohabiting	All
Mean Homicide Rate	20	7	11	2		
Equation (1)						
(1) Average Effect	0.331* (0.183)	-0.123* (0.072)	-0.203* (-0.118)	-0.133 (0.098)	-0.063 (0.059)	-0.037 (0.063)
Equation (2)						
(2) 2 years before	0.180 (0.204)	-0.043 (0.074)	-0.072 (0.133)	-0.006 (0.121)	0.038 (0.118)	0.078 (0.109)
(3) 1 year before	0.057 (0.267)	0.075 (0.098)	-0.016 (0.164)	0.059 (0.168)	0.064 (0.116)	0.113 (0.106)
(4) year of change	-0.238 (0.295)	-0.026 (0.115)	-0.145 (0.200)	-0.086 (0.194)	-0.015 (0.107)	0.031 (0.096)
(5) 1-2 years later	-0.492 (0.306)	-0.190* (0.099)	-0.299 (0.186)	-0.206 (0.163)	-0.013 (0.114)	0.057 (0.096)
(6) 3-4 years later	-0.581 (0.418)	-0.159 (0.140)	-0.428 (0.257)	-0.329 (0.214)	-0.048 (0.156)	0.028 (0.129)
(7) 5-6 years later	-1.047* (0.525)	-0.303* (0.159)	-0.733** (0.342)	-0.691** (0.267)	-0.103 (0.162)	-0.037 (0.162)
(8) ≥ 7 years later	-1.087 (0.646)	-0.366 (0.219)	-0.723* (0.387)	-0.723** (0.335)	0.093 (0.208)	0.178 (0.193)
Controls	$\sqrt{p=0.0488}$	$\sqrt{p=0.0405}$	$\sqrt{p=0.0019}$	$\sqrt{p=0.005}$		
F-test						

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Sample 1994-2006 ($n=390$). Standard errors clustered at state level.

States of Colima and San Luis Potosi are excluded. The dependent variable is the annual state level homicides committed at victim's home per 100,000 males (females). All includes single, married, cohabiting, divorced, widowed, separated, and unknown marital status. All specifications include demographic controls, state, and year fixed effects. Demographic controls include state GDP per capita, percent of population enrolled in an undergraduate degree, sex ratio, and age structure. Age structure is the proportion of a state's population 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older. The F-test of joint significance is for the set of demographic controls.

Table 3.9: Effects of Assist Law on Male Suicide Rates

	All		Single		Married, Cohabiting	
Mean Suicide Rate	7.04		2.85		2.61	
Equation (1)						
(1) Average Effect	-0.417 (0.358)	-0.052 (0.227)	-0.158 (0.186)	-0.010 (0.129)	-0.013 (0.154)	0.113 (0.120)
Equation (2)						
(2) 1 year before	-0.353 (0.365)	-0.010 (0.280)	-0.113 (0.170)	-0.113 (0.170)	-0.056 (0.162)	0.059 (0.143)
(3) 2 years before	0.428 (0.420)	0.011 (0.285)	0.111 (0.254)	0.111 (0.254)	-0.259 (0.211)	-0.113 (0.197)
(4) year of change	-0.248 (0.447)	-0.227 (0.237)	-0.084 (0.274)	-0.084 (0.274)	0.049 (0.207)	0.174 (0.172)
(5) 1-2 years later	-0.925 (0.598)	-0.291 (0.438)	-0.294 (0.319)	-0.294 (0.319)	-0.247 (0.235)	-0.079 (0.201)
(6) 3-4 years later	1.411* (0.783)	0.784 (0.623)	-0.429 (0.424)	-0.429 (0.424)	-0.493 (0.310)	-0.446* (0.248)
(7) 5-6 years later	-1.395 (0.954)	-0.876 (0.706)	-0.325 (0.509)	-0.325 (0.509)	-0.562 (0.386)	-0.668** (0.283)
(8) ≥ 7 years later	-1.919 (1.238)	-1.168 (0.825)	-0.631 (0.708)	-0.631 (0.708)	-0.978* (0.520)	-1.136*** (0.382)
F-test	p=0.003		p=0.002		p=0.000	

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Sample 1994-2006 ($n=390$). Standard errors clustered at state level. States of Colima and San Luis Potosi are excluded. The dependent variable is the annual state level suicides per 100,000 males. All includes single, married, cohabiting, divorced, widowed, separated, and unknown marital status. All specifications include demographic controls, state, and year fixed effects. Demographic controls include state GDP per capita, percent of population enrolled in an undergraduate degree, sex ratio, and age structure. Age structure is the proportion of a state's population 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older. The F-test of joint significance is for the set of demographic controls.

Table 3.10: Effects of Assist Law on Female Suicide Rates

	All		Single		Married, Cohabiting	
Mean Suicide Rate	1.26		0.52		0.40	
Equation (1)						
(1) Average Effect	-0.161 (0.103)	-0.138 (0.096)	-0.036 (0.045)	-0.037 (0.050)	-0.052 (0.064)	-0.039 (0.050)
Equation (2)						
(2) 2 years before	-0.022 (0.099)	-0.004 (0.099)	0.059 (0.065)	0.059 (0.137)	-0.068 (0.067)	-0.082 (0.067)
(3) 1 year before	0.105 (0.137)	0.129 (0.123)	0.155* (0.085)	0.329 (0.212)	-0.068 (0.070)	-0.062 (0.064)
(4) year of change	-0.114 (0.108)	-0.098 (0.094)	0.086 (0.068)	0.324* (0.200)	-0.115 (0.073)	-0.119* (0.064)
(5) 1-2 years later	-0.151 (0.138)	-0.112 (0.124)	0.012 (0.058)	0.023 (0.241)	-0.095 (0.093)	-0.097 (0.087)
(6) 3-4 years later	-0.248 (0.138)	-0.192 (0.171)	-0.001 (0.079)	-0.066 (0.341)	-0.203* (0.134)	-0.226* (0.111)
(7) 5-6 years later	-0.280 (0.222)	-0.214 (0.235)	0.105 (0.083)	0.081 (0.395)	-0.324** (0.158)	-0.361** (0.160)
(8) ≥7 years later	-0.488 (0.311)	-0.412 (0.342)	-0.025 (0.135)	0.025 (0.451)	-0.324* (0.180)	-0.382* (0.210)
F-Test	p=0.001		p=0.003		p=0.000	

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Sample 1994-2006 (n=390). Standard errors clustered at state level. States of Colima and San Luis Potosi are excluded. The dependent variable is the annual state level suicides per 100,000 females. All includes single, married, cohabiting, divorced, widowed, separated, and unknown marital status. All specifications include demographic controls, state, and year fixed effects. Demographic controls include state GDP per capita, percent of population enrolled in an undergraduate degree, sex ratio, and age structure. Age structure is the proportion of a state's population 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older. The F-test of joint significance is for the set of demographic controls.

Table 3.11: Effects of Assist Law on Homocides Committed in Public Areas

	Males			Females
	All	Single	Married, Cohabiting	All
Equation (1)				
Assist Law	-0.845 (0.716)	-0.269 (0.220)	-0.464 (0.455)	-0.037 (0.072)
Divorce Law	-1.097 (0.936)	-0.426 (0.289)	-0.695 (0.551)	-0.030 (0.092)
Penal Code	-0.240 (0.896)	-0.179 (0.276)	-0.045 (0.543)	-0.013 (0.096)
Equation (2)				
	Evolution of Assist Law			
2 years before	-0.525 (0.618)	-0.023 (0.211)	-0.351 (0.365)	-0.067 (0.057)
1 year before	-0.626 (0.749)	-0.094 (0.268)	-0.334 (0.439)	-0.053 (0.076)
year of change	-1.093 (0.874)	-0.294 (0.293)	-0.615 (0.539)	-0.060 (0.080)
1-2 years later	-1.289 (1.093)	-0.288 (0.314)	-0.729 (0.706)	-0.083 (0.105)
3-4 years later	-1.319 (1.271)	-0.186 (0.361)	-0.832 (0.858)	-0.090 (0.134)
5-6 years later	-0.961 (1.432)	-0.038 (0.399)	0.606 (1.011)	-0.124 (0.148)
≥7 years later	-0.778 (1.845)	0.060 (0.519)	-0.596 (1.295)	-0.008 (0.180)

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Sample 1994-2006 (n=390). Standard errors clustered at state level. States of Colima and San Luis Potosi are excluded. The dependent variable is the annual state level homicides per 100,000 males (females). All includes single, married, cohabiting, divorced, widowed, separated, and unknown marital status. All specifications include demographic controls, state, and year fixed effects. Demographic controls include state GDP per capita, percent of population enrolled in an undergraduate degree, sex ratio, and age structure. Age structure is the proportion of a state's population 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older.

Table 3.12: Effects of Laws on Annual Thefts per 100,000 People

	All	Single	Married, Cohabiting
Panel A. Thefts Committed by Males			
Assist Law	4.619 (6.586)	3.179 (4.316)	1.861 (2.508)
Divorce Law	-8.620 (5.975)	-0.874 (3.294)	-0.498 (2.739)
Penal Code	0.191 (5.622)	0.869 (3.663)	-0.683 (2.196)
Panel B. Thefts Committed by Females			
Assist Law	0.395 (0.440)	0.250 (0.219)	0.158 (0.201)
Divorce Law	0.717 (0.630)	0.428 (0.317)	0.293 (0.313)
Penal Code	-0.180 (0.429)	-0.200 (0.213)	-0.003 (0.221)

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Sample 1997-2006 (n=300). Standard errors clustered at state level. States of Colima and San Luis Potosi are excluded. The dependent variable is the annual state level thefts (crime code 200100) per 100,000 males (females). All includes single, married, cohabiting, divorced, widowed, separated, and unknown marital status. All specifications include demographic controls, state, and year fixed effects. Demographic controls include state GDP per capita, percent of population enrolled in an undergraduate degree, sex ratio, and age structure. Age structure is the proportion of a state's population 15-19, 20-29, 30-39, 40-49, 50-59, 60-64 and 65 and older.

Chapter 4

Estimating the Effects of School Year Length on Student Performance in Mexico

4.1 Introduction

Developing countries are increasingly identifying the expansion of instructional time as an important education policy. For example, Peru recently changed the start of the school calendar from April 1st to March 1st for all public schools across the country, and Argentina, Colombia and the Dominican Republic have lengthened their school days. A natural question that arises from students, teachers and administrators is whether more schooling will improve student achievement. Answering this question

can have further policy and economic implications as there is evidence that despite high levels of school attainment, poor growth performance in Latin America can be reconciled by low levels of cognitive skills (Hanushek and Woessmann 2009).

While there are several factors studied in the literature that examines the role of school quality on several economic and educational outcomes, the length of the school year is widely cited as an important input to student achievement. Although the work that examines the impact of instructional time on student performance is expanding, the vast majority of these studies have focused on developed countries. Most of this research finds that increasing instructional time has a positive impact on student achievement. However, little is said about the role of instructional time on student achievement when other inputs to the production process are low or lacking, as is the case in some developing countries like Mexico.

Cross-country studies show the low quality of education in Mexico as measured by student performance on international standardized exams. According to Santibañez *et al* (2005), 1995 unreleased results from Trends in International Mathematics and Science Study (TIMMS) tests showed that Mexican students in third and fourth grade scored 20 percentage points lower in math and science than students in other developing countries.¹ The dismal performance of Mexican students in international exams has raised concerns about school quality and accountability in Mexico. Several compensatory programs have been implemented to address low student achievement,

¹Mexico agreed to participate in the application of the Trends in International Mathematics and Science Study (TIMMS) 1995 which covered math and science. However, Mexico canceled the publication of the results.

however, there is little educational research and evaluation that can provide information on school quality improvement efforts in Mexico (Santibañez *et al* 2005), in particular with respect to changing instructional time.

We take advantage of a unique setting in which policies and other factors outside the school's control alter the number of instructional days, thereby introducing within state variation that creates a quasi-experiment setting that allows us to estimate the impact of more days of schooling on primary school student performance in Mexico. The source of variation arises from changes in the start of the calendar year and/or the implementation date of a standardized test. The standardized test has been administered in Mexico since 2006 by the Secretary of Education for all students in grades 3 to 6. In the first year of implementation, all the states administered the exam in June. In each of the following years, the administration of the test changed to early April or to early May for some states but not others. Furthermore, during the period under investigation, academic year 2005/2006 to 2008/2009, some states also changed the start of the school calendar year for reasons related to weather and tourism. These exogenous changes allow us to avoid possible confounding factors that could contaminate estimates of instructional time—typically endogenously determined—on student performance.

This study proceeds as follows. In Section 4.2 we summarize the literature that examines instructional time as an input to the education production function. Section 4.3 presents institutional background about education policy regarding school calendar and standardized exams in Mexico. Next we present our identification strategy. The methodology and data are discussed in Section 4.4, which are followed by the results.

Similar to studies that examine developed countries, we find a positive and statistically significant relationship between more days of instruction and student achievement. Our results indicate that ten additional days of instruction can improve student achievement from 1 to 2 percentage points in math and reading scores. The improvement is least pronounced for students whose schools are located in highly impoverished localities. Combined, our findings suggest that extending the school year can raise students' learning, as measured by student scores on the standardized exam. However, we note that extending the school year may not be beneficial for all groups, particularly those who may lack the resources, which combined with more days of school, assist learning and hence student achievement.

4.2 Literature Review

Researchers in the economics of education have shown that it is not only quantity of education but also quality that matters (Betts 1995). Yet there is still an ongoing debate on what measures to use to examine school quality. Traditional measures of school quality include school inputs such as class size, teacher training, and per-pupil expenditure. In the US, the evidence on whether school inputs have any impact on student outcomes is still mixed. For instance, some studies have shown that per-pupil expenditures have little effect on student outcomes (Coleman Report 1966), while others find that small class size improves student performance (Krueger 1999). Despite

the voluminous literature that examines the role that school resources play on student performance (see Card and Krueger 1998 and Hanushek 2002 for a review), the research that investigates instructional time as an input of the education production function has been, until recently, not widely studied. Initial work concentrated on estimating the impact of the length of the school year on labor market outcomes (Card and Krueger 1992, Grogger 1996). Focus since then has shifted to studying the impact of the variation in the length of the academic year on student outcomes. The findings of these studies have been also mixed. For example, Lee and Barro (2001) find international cross-country evidence that school year length has no impact test scores. However, it is possible that their findings are driven by attenuation bias due to measurement error since their study relies on instructional time data that even the authors acknowledge "tend to be unreliable." Moreover, the cross-sectional nature of their data draws into question the causality of their estimates.

Other studies address endogeneity bias by exploiting quasi-experimental settings. These studies examine the effects of reducing the length of the academic year on both educational and labor market outcomes. Pischke (2003) uses the variation introduced by a state-mandated change to shorten the school year which affected some cohorts but not others. His results indicate that a shorter school year in Germany increased grade repetition in primary schooling, but that there were no persistent, long-term effects on secondary schooling or earnings. Krashinsky (2006) exploits a similar exogenous policy change in Canada. He finds that cohorts exposed to four years of high school had substantially lower GPAs in college than those who attended high school for

five years. Using a panel of student level data, Eren and Millimet (2007) examine the effect of length of school year on student performance and find differential effects on the length of the school year and student outcomes across the performance distribution. Although these quasi-experiment studies improve from the cross-sectional studies that may be plagued by endogeneity issues, the policy change affected primarily a population of non-primary students.

Recent work has exploited other sources of exogeneity where there is more variation, and has begun examining student outcomes in both primary and secondary schooling. In particular, one such source of variation arises from unplanned school closures that occur due to inclement weather. Marcotte (2007) and Marcotte and Hemelt (2007) examine whether students in Maryland performed better in years in which there are more school days to prepare for a statewide examination. They find that the impact of unplanned school closures is smaller for 5th and 8th graders while 3rd graders perform nearly 3 percent lower in years with school closings than in years with no school closings. In the same spirit, but using a different methodology and more data, Hansen (2008) examines the variation arising from snow day closures in Colorado and Maryland and finds results similar to Marcotte's and Hemelt's.

Another source of variation examined exogenous policy changes that alter the number of instructional days that students have before a state-mandated assessment.² Hansen (2008) finds that having more instructional days due to changes in the implemen-

²Eren and Millimet (2007) examine the effect of length of school year on student performance but there is very little variation (the authors can only identify whether the school had more than 180 days of schooling) in instructional time

tation of state-mandated test improved student performance in Minnesota. Combined, these studies find evidence for benefits in increasing the school calendar year (Marcotte and Hansen 2010).

Since developing countries are increasingly identifying instructional time as an important education policy, a natural question that arises is whether the results from the existing literature on instructional time and student performance on developed countries can be generalized to developing countries. For instance, while Peru has changed the start of the school calendar year from March to April, Colombia, the Dominican Republic and Argentina have lengthened their school days. The work of Llach *et al* (2009) and Bellie (2009) shed some light on the effect of lengthening the school day on secondary school student outcomes in Argentina and Chile, respectively. However, there is still no consensus on which inputs are more important in developing countries (Hanushek 2006), and with the current research it is not clear what one may expect to find with respect to instructional time and student achievement in Mexico.³

This paper contributes to the literature on instructional time in several dimensions. First, it is the first empirical work, to our knowledge, that estimates the effect of more instructional days on primary school student performance in Mexico. Second, unlike other studies, this paper broadens the population and subject areas studied. We have a rich panel dataset that includes average performance outcomes in math and read-

³For instance, Bacolod and Tobias (2006) find evidence that in developing countries, school resources have a much stronger effect on outcomes than traditional measures such as per-pupil expenditure. In the Mexican context, it has been discovered that providing classroom software (Lopez Acevedo 2002), and grants to improve school infrastructure (Skoufias and Shapiro 2006) have positive effects on student outcomes.

ing scores for all students in grades 3 to 6. Third, given our database we will be able to explore the importance of this input, instructional time, to the production function of students in relation to other inputs.

4.3 The School Calendar and Standardized Student Tests in Mexico

4.3.1 ENLACE

Mexico's Federal General Law of Education grants the Secretary of Education, Secretaría de Educación Pública (SEP), responsibility to establish a school calendar made up of 200 days of instruction and to release it to the public weeks before the start of each academic year. The law establishes that all public and affiliated private pre-school, elementary, and secondary institutions must follow the school calendar or be subject to sanctions. Though seldom exercised, this law also grants state authorities the right to petition for school calendar changes. However, the SEP must still approve the petition, and authorizations are only granted in extraordinary cases. Note that changes are only made about the dates when instruction begins or ends, and that this does not mean altering the curriculum or the total number of instructional days at the end of the academic year. The law specifies that any unscheduled closings must be made up. Although, the law does not indicate when these days must be made up, any lost days are typically made up at the end of the calendar year. Furthermore, the SEP began

implementing a national standardized test (Evaluación Nacional del Logro Académico en Centros Escolares or ENLACE) in mathematics and reading for all students in grades 3 to 6 and 9 since the 2005/2006 academic year.⁴ With the implementation of this exam, it also became the SEP's responsibility to schedule the evaluation period into the school calendar before it gets published. Thus, before classes begin, students, teachers and parents are aware of the examination dates.

Covering more than 11 million students in all public, private, and indigenous primary schools,⁵ ENLACE is the first standardized exam implemented at the national level in Mexico. For the first time, parents are given a report of their children's performance on math and reading relative to others, and the public has access to aggregated measures of these results. ENLACE is comprised of 110 multiple choice questions and the scores can range from 200 to 800 points. Since the contents of the tests are publicly available, one may assess how well the tests reflect the curriculum.

Several measures are taken to assure the integrity of the ENLACE results, e.g., teachers do not proctor their own students,⁶ and there are parent volunteers and other community members who serve as external auditors during the implementation of the exam. Although short-lived, ENLACE scores are becoming more and more important for policy makers and parents alike. Parents associate schools that report high ENLACE

⁴Since 2008 new subjects (rotating among science, civics, history and geography) have been added to ENLACE, and the population of students has expanded to include 7th and 8th grade.

⁵Average cost of implementing this exam to the SEP is approximately \$2 USD per student. Note that students do not pay to take the exam.

⁶An agreement with the teachers' union was made to guarantee that teachers do not proctor their own students and that attendance is encouraged. Poy Solano, Laura, "Busca la SEP dar credibilidad y transparencia a la prueba ENLACE," April 20, 2007, <http://www.jornada.unam.mx>, accessed May 1, 2010.

results with better school quality, and policy makers are considering rewarding teachers based on students' performance on ENLACE.⁷ Furthermore, a recent study for example, uses the ENLACE results as a direct measure of education quality (de Janvry *et al* 2010).

In the next section we discuss our identification strategy, which exploits the changes discussed above in order to evaluate whether a school exposed to more instructional days experienced better scores in ENLACE.

4.3.2 Variation in the Number of Instructional Days

In the academic years from 2005/2006 to 2008/2009, several Mexican states experienced changes in either the implementation date of ENLACE or the start date of the calendar year. Table 4.1 lists the start dates of the school calendar years as well as the evaluation dates for all the states in Mexico. The variation in instructional days is mostly driven by changes in the states of Aguascalientes, and Sinaloa, but there is some variation which occurs in the other states, too.

In the first year of ENLACE we observe variation in the number of days for the states of Aguascalientes and Sinaloa. Aguascalientes had historically been on vacation during April due to an international festival that has been an important source of tourism,⁸ and to end the calendar year at the same time that the rest of the country,

⁷Bacaz, Veronica, "Premiarán a Maestros de Prueba ENLACE," *Diario de Morelos*, Sección Local, 3/3/2010. <http://www.diariodemorelos.com>, accessed March 30, 2010.

⁸Historically, this festival, the Feria Nacional de San Marcos, had been held in mid-April and lasts on average of 30 days. The festival started in 1828 in the month of November to expand commercial opportunities after the harvest. However, it was later changed to April to concur with a religious holiday of patron Saint Marcos on April 25.

the start of the school year was moved up. On the other hand, Sinaloa—a coastal state—had been authorized to start classes a week after most of the country to avoid exposing students to high temperatures.

In the second year of ENLACE, academic year 2006/2007, the SEP moved up the implementation of the exam from June to April. This reduced the number of potential days of instruction prior to examination by 28 days. However, the new exam schedule conflicted with the time that Aguascalientes was authorized to be off for the international festival. Thus, to accommodate the local economic and traditions, Aguascalientes was authorized to administer ENLACE at a different date. This change reduced the number of school days prior to examination by 15 days.

Finally, in the third year of ENLACE, the implementation of the exam was moved up by a week, reducing again the number of potential instruction time before examination. Furthermore, the state of Sinaloa changed its academic calendar so that schools start classes the same day as most of the country. The change was to accommodate time off during religious holidays such as Christmas.

The top and bottom panel of Figure 4.1 present the association between instructional days and the unconditional means of student performance in Aguascalientes (A), Sinaloa (S), and the remaining states (O), for math and reading, respectively. Assuming a linear relationship, the overall association between average scores and instructional days appears to be negative. However, when we consider different range of days or compare the means within separate groups, the association is not clear. For instance, in the range of 140 to 160 days, the relationship seems to be positive and

then negative for instructional time beyond 160 days. Moreover, within the state of Sinaloa, the relationship appears to be positive but then negative. Noting that over time the number of days of instruction were reduced, another possible interpretation is that scores may be improving. In the next section we discuss how we address these possibilities.

4.4 Data and Methodology

4.4.1 ENLACE Results, Grade and School Level Data

A key contribution of this empirical exercise is the use of an exogenous source of variation to infer the causal effect of more instructional days on student achievement. We combine the information on this change on the number of instructional days with publicly available data from ENLACE for school grades 3 to 6 in mathematics and reading. One advantage of our data is that ENLACE is administered in all public and private schools in Mexico; thus, our results can be generalized to the population in Mexico while minimizing sample selection problems.

Another advantage of our data is with respect to the observable outcomes. One outcome is the percent of students in a given grade for which their performance was "Unsatisfactory," "Basic," "Good," and "Excellent." Unlike previous studies (Hansen 2008, Marcotte 2007, Marcotte and Hansen 2010, and Marcotte and Hemelt 2007) that were limited to observing only the percent of students in the relevant grade who perform at least satisfactorily on a subject test, we also observe the means for students within

grades (note that a grade can be a group of classes, e.g. two groups or classes of third graders) in a given school. The availability of these two outcomes will enable us to examine the sensitivity of our estimates.

It is possible that the exam material did not arrive in time; thus, some schools may not be observed in all three academic years.⁹ We begin our analysis with an unbalanced panel data composed of all general public primary schools with morning shifts¹⁰ in academic years 2005/2006 to 2007/2008. Furthermore, we exclude the states of Michoacán, Oaxaca and Tabasco for reasons discussed below.

First, teachers' strikes occurred in the states of Michoacán and Oaxaca in the later part of the 2005/2006 academic year which led to school closures that we cannot identify. Although Michoacán stopped the strike in time for the implementation of ENLACE, teachers in Oaxaca did not. Despite the strike, there were schools in Oaxaca that did not close and which administered the test, but for the most part, coverage was low in both Oaxaca and Michoacán.¹¹ In addition to school closures from the teachers' strike in 2006, on August 22, 2007, the SEP and the state of Michoacán agreed to cancel the ENLACE 2007 results when analysis showed the results were not reliable.¹²

⁹We will discuss attrition later in the section

¹⁰Multiple-shift schools are established to meet the excess demand; however, it is known that evening shift schools can have a large proportion of disadvantaged (academically and socioeconomically) students. (Parker *et al*, 2008). This makes evening shift schools interesting in their own right, and as such we leave them for future research.

¹¹For the period 2006-2009, coverage in Oaxaca and Michoacán has been 10.5 and 35.5 percent, respectively. Secretaría de Educación Pública, "ENLACE en Educación Básica. Aplicación 2009" http://enlace.sep.gob.mx/ba/docs/presentacion_aplicacion2009.pdf, accessed on May 3, 2010.

¹²On August 22, 2007, the SEP and the state of Michoacán agreed to cancel the results when analysis showed the results were not reliable, <http://www.cambiodemichoacan.com.mx/vernota.php?id=67506>, accessed April 4, 2010.

Second, on April 2008, the state of Tabasco experienced severe floods for which the SEP mandated that ENLACE be postponed until June 2008. Although this event by nature would add more exogenous variation in the number of instructional days, it is also possible that some schools suffered infrastructure damages that could also affect student achievement and the number of instructional days. At the moment we cannot identify which of the schools were damaged and when they reopened their doors; thus we also exclude Tabasco from the analysis.¹³ Finally, we exclude the academic year 2008/2009 since the outbreak of the H1N1 influenza virus in April 2009 led to school closures that we cannot currently identify.

We have time-varying grade level data for some information and school level data for others. These grade and school characteristics come from annual school censuses (Formato 911) which are completed by an authorized school representative and then sent to the Ministry of Education (SEP). At the grade level we observe the number of female and male students. At the school level, we have information on the number of students who are indigenous, gifted, in special education or handicapped. Variables that capture characteristics about teachers (and possibly school infrastructure) in a given school include, the number of teachers teaching art, technology, foreign languages, and physical education, number of administrative personnel, number of groups taught by the principal, total number of teachers and the number of teachers participating in Carrera Magisterial (CM)—a program that compensates teachers for participating in professional development courses. In addition, we include time-varying state level data

¹³Notimex. "Aplican Prueba ENLACE en el país; en Tabasco hasta junio," April 18, 2008. www.tabascohoy.com.mx, accessed April 24, 2010.

such as real GDP and percent of GDP in educational services expenditures, which come from Mexico's National Institute of Statistics and Geography (INEGI). We further limit our sample to schools with non-missing information in all of these variables (see Table 4.2 for a description of these variables).

With the criteria discussed above our initial sample includes 633,596 grade-year observations. Dropping the states of Michoacán, Tabasco and Oaxaca excludes 45,800 grade-school observations.¹⁴ When we exclude observations with values of zero or missing on the outcome variables, we exclude 18,821 observations.¹⁵ Our final unbalanced panel, after excluding observations with missing school characteristics, includes 543,568 grade-year observations.

We note that we also have marginalization indices provided by Mexico's National Council of Population (CONAPO 2005), which we will use to explore additional specifications. The marginalization indices are available at the locality level and are able to map it to 93 percent of the schools in our sample. The marginalization index is a function of measures of education (percent of population that are illiterate or without primary education), housing (percent of houses without water, sewage, electricity, non-dirt floors, and access to goods (having a refrigerator) in a given locality. CONAPO's marginalization index are categorized as *Very Low*, *Low*, *Moderate*, *High*, and *Very High*, where *Very High* denotes highly impoverished localities. Table 4.3 presents a summary

¹⁴Michoacán, Tabasco, and Oaxaca are southern states with higher proportions of indigenous populations and/or high poverty levels.

¹⁵Examination of this excluded sample indicates we dropped schools that have small student body populations and few teachers.

of average student performance on ENLACE. The descriptive statistics show that most of the students perform at an elementary or basic level in both reading and math. The percent of students who do not perform satisfactorily is almost 25 percent for all grades and subjects, and it is almost 30 percent in reading for 4th grade students. Overall, these statistics draw a similar picture of low student achievement by Mexican students on international standardized tests.

Since student achievement can be affected by student and school characteristics, we present descriptive statistics of the schools and students in our sample in Table 4.4. We provide these summaries for the states of Aguascalientes and Sinaloa separately to assess whether there are observable differences in the states driving most of the variation in school days. We observe that the average number of students in a grade is composed of 31 students, with slightly more males than females. Most of the schools do not have teachers in physical education, arts or foreign languages, but, on average, at least half of the teachers in a given school seem to be participating in CM. Examining the states separately we find that, on average, Aguascalientes has the largest grade groups and most number of teachers teaching foreign language and participating in Carrera Magisterial. Moreover, both Aguascalientes and Sinaloa have more students who are handicapped or are in special education. On average, a state spends 4.5 percent of its GDP on educational services is 4.5 percent; examining Aguascalientes and Sinaloa, this figure represents 1 and 2 percent, respectively. We also note that the proportion of schools located in impoverished areas is higher in Sinaloa than Aguascalientes. Examination of the sample of observations dropped (column 1 of Table 4.5)

indicates that schools without exam scores are comprised on small-sized groups (average of 9 students per group) or schools (average of 2 teachers in the school). These statistics may be suggestive of schools located in remote areas. We find a similar pattern for the sample of schools that appear in one period ($T=1$). Moreover, for the schools that appear in one time period we observe, that on average, they have the lowest proportion of students performance at least at the basic level. The schools appearing in two time periods ($T=2$) are also very similar to those that appear in one period.

4.4.2 Methodology

The “natural experiment” described in Section 4.3.2 creates an important difference in the number of instructional days at the state level, thereby allowing us to estimate a causal effect of the number of instructional days on student achievement.

In our study, the changes in the school calendar led to an average of 16 fewer days of instruction prior to examination over the period, which is 6 fewer days compared to days lost in studies done in the U.S. If a significant difference between developed and developing countries is that developed countries start with high levels of the inputs that determine the production of student achievement, it is plausible that the marginal effect of additional increases in the input can be larger for those (i.e developing countries) with low input levels. However, it is also possible that being exposed to more days of low quality inputs (e.g. low quality of instruction, or poor infrastructure) can translate into (unintended) negative effects. For instance, with more days to prepare for an exam, students may initially be better able to absorb and learn the material. However, the

students' learning experience may be negatively affected if they must attend school for more hot/cold days if the school does not have windows, AC, etc. Thus, *a priori*, the magnitude and direction of more days of school on Mexican student outcomes is largely an empirical question.

We estimate Model (4.1) separately by grade g (for $g = 3, 4, 5, 6$) and subject $s = m, r$, where ($m = mathandr = reading$). $score_{jkt}^{g,s}$ is the average ENLACE score on subject m of students in grade g attending school j in state k during academic year

$$score_{jt}^{m,g} = f(days)_{st} + X'\delta + \alpha_j + \tau_t + \varepsilon_{jt} \quad (4.1)$$

t . $f(days)_{st}$ is a function of the number of potential days of instruction (excluding weekends, and holidays) prior to ENLACE in state s in academic year t since the beginning of the school year. We will explore a functional form that allows for non-linearity in instructional time as Figure 4.1 suggests the presence of diminishing returns. If more days of instructions mean the students have more time to prepare for the exam, then we should expect a positive relationship between the instructional form of instructional time and student achievement. All models are estimated using standard errors clustered at the state level, which allow the possibility of serial correlation in the error term.

In addition to the student-teacher ratio, at the grade level we control for the gender composition to capture possible variation in classroom dynamics that affect student performance. In X we also include school level covariates such as the number of students who are indigenous, gifted, handicapped and in special education programs.

We also control for the number of teachers who teach physical education, arts, technology, foreign language. These variables may capture differences in the availability of resources or infrastructure in a given school. Including the number of administrative staff and the number of groups being taught by a principal can proxy for school resources and/or teacher effectiveness in a given school. That is, more administrative staff and/or not having to teach while being a principal may reallocate time away from non-teaching activities that may negatively impact student learning. Though the impact of CM on student achievement is not clear, we control for it to proxy for a measure of teacher quality or experience. Since educational quality may vary across wealthier states, we also control for a state GDP's and percent of GDP expenditures on educational services.

Even with a rich set of covariates, a comparison of student achievement across states, that is, estimating Model (4.1) using pooled OLS will lead to biased estimates. For instance, if the average ability of students is low (e.g. struggling schools) and this influences a state's authority to petition for a change in the start of the calendar year that leads to more days of instruction prior to examination then pooled OLS estimates will be downward biased. Alternatively, if the average motivation of students is high and this leads state authorities to petition for more days of schooling, then our estimates would be upward biased. Thus, school fixed effects permit a comparison that control for all other possible confounding factors (observed and unobserved) that remain constant at the school and grade level. α_j represents school fixed effects which control for time-invariant school and state level characteristics that affect average scores. τ_t is the year fixed effects which control for any national trends that can alter student scores and school

days. ε_{sgt} is the error term that includes unobserved school and state characteristics in academic year t . The coefficient(s) of interest, allow us to compare the results in the standardized test of children living in state k and attending school j in grade g in academic year t with those who attended the same grade and school in a different year and therefore experienced different number of instructional days.

4.5 Results

4.5.1 Overall Performance in Mathematics and Reading

We estimate Model (4.1) separately by subject and present the results in Tables 4.5 to 4.8 for grades 3 to 6, respectively. All the specifications include time-varying grade and school-level covariates discussed in Section 4.4. Columns 1 and 5 of each table present our benchmark specification, namely pooled OLS estimates, for math and reading, respectively. The OLS results provide mixed and surprising results. The estimated coefficients suggest a negative, and some cases statistically insignificant, relationship between an additional days of potential instruction and student performance. The estimates for 5th graders indicate a positive relationship between additional school days and student achievement in math. Except for 4th grade, we find negative (but statistically insignificant in most cases) effects on reading (see column (5)).

Except for percent of GDP spent on educational services, the signs of the coefficients on the grade, school and state level covariates have the expected signs. The female-male student ratio has a positive and statistically significant effect on both math and reading performance. The effects are larger for reading than math (2 to 4.5 points). This may point to other findings that girls out-perform boys in measures of reading achievement while generally under-performing in science and mathematics (Dee 2007). The magnitude of the effect is larger for higher grade groups. Interestingly, we observe a positive and statistically significant effect on having foreign language teachers in the school.

One concern is that over time, there could be annual tendencies at the national level that alter student achievement and number of school days, e.g. pressure to improve student achievement. If this were the case, our estimates would be biased. In columns 2 and 6 we control for year fixed effects to account for unobservable national trends that may affect both student performance and the likelihood of having more days of instruction prior to ENLACE. The results provide a different picture: all the coefficients are positive and strongly statistically significant. There is also support for a quadratic functional form (the coefficient on days squares is negative and statistically significant). The direction and magnitude of the covariate coefficients do not change much. When we estimate Model (1) using only covariates and school fixed effects (columns 3 and 6), the coefficient becomes negative and decreases in absolute terms. This is suggestive of the importance of controlling for both school and year fixed effects. As discussed

in Section 4.4, the OLS estimates may be biased due to possible school time-invariant omitted variables. Thus, in columns 3 and 7, we control for both school and year fixed effects. Note that adding school fixed effects absorbs state fixed effects so the school fixed effects also control for any unmeasured time-invariant factors that vary across states. In these specifications, the coefficients become larger—suggesting previous coefficients were biased downward. The coefficient of the quadratic term is also negative and statistically significant, indicating the presence of diminishing returns.¹⁶

Furthermore, the estimates indicate that the effects differ slightly by grade and subject matter. For example, for 5th grade math, the results indicate that within school, adding one more day of instruction to an average group of students that starts with 140 days of instruction (compared to those in the same school who did not) increases the average math scores by 2.35 points ($22.09 - 2 \cdot 0.07 \cdot 141$) or 0.48 percentage points. By contrast, the effect of adding a fifth day is 1.79 points (0.37 percent). For the most part, the effects of a fifth day of schooling is slightly larger for math than reading—for this same group, the fifth day improves reading scores by 0.17 percent compared to 0.20 percent in math. Across grades 3rd and 5th graders seem to benefit more than 4th and 6th graders.

Taken together, the cumulative effect of ten additional school days improves average math scores from 1.37 to 2.42 percentage points, and average reading scores from 0.73 to 2.58. These estimates are in range of what is found in the literature that examines lengthening the school year in the US. For instance, Marcotte (2007)

¹⁶Bellei (2009) finds evidence of diminishing returns in instructional time. Betts and Johnson (1997) find diminishing returns in school spending.

and Hansen (2008) find that students who lost an average of ten school days due to snow saw a 1 to 2 percent (or up to 0.15 standard deviations) decrease in math test scores. Moreover, Bellei (2009) and Llach *et al* (2009) investigate high school students in Chile, and Argentina, respectively, and they both find positive effects to lengthening the school day. Specifically, Bellei (2009) finds larger effects for math (up to 0.12 standard deviations) than reading (up to 0.07 standard deviations).¹⁷

When we control for both year and school fixed effects, the statistical significance of the coefficients on GDP and percent of GDP become positive, albeit statistically insignificant for most grades. We also observe that the student-teacher ratio becomes strongly statistically significant and indicates that high student-teacher ratio decreases student achievement. The coefficients on CM loses statistical significance. There is also evidence that having an additional teacher participating in Carrera Magisterial has little to no effect on student performance on ENLACE. Although evaluating CM is not our main focus, these results are consistent with other studies that conclude that this pay-per-performance program does little to create teacher incentives that improve student achievement (Lopez Acevedo 2002; McEwan and Santibañez 2005; Santibañez 2006). Since there is some indication that school resources or other measures of school quality can have different effects on student performance. In the next section we explore whether having more school days affects impoverished schools differently from non-impoverished ones.

¹⁷Llach *et al* (2009) find that lengthening the school day increased graduation rates by 21 percent.

4.5.2 Schools in Impoverished Localities

If more affluent localities have access to better resources that complement the learning experience of the students, the benefits of having additional days of instruction prior to ENLACE may vary across impoverished localities. Table 4.10 presents the estimates of our preferred specification, which include school and year fixed effects, for math across the marginalization index categories. The results indicate that there are indeed differences in how instructional time affects students by grade and subject. Specifically, we continue to find a positive and statistically significant effect of more school days on student performance and evidence of diminishing returns in instructional time. The largest effects appear to be for schools in *Moderate* localities and the gap is wider between the two extremes, *Very Low* and *Very High*. This pattern, with respect to the magnitude and the gap across impoverishment levels, prevails when we examine the effects on reading scores, see Table 4.11.

Although the estimates provide useful information, the overall effects will depend on (i) the grade, (ii) the subject, (iii) the initial number of days, and (iv) how many more days are added to the benchmark. Thus, Figure 4.2 presents the cumulative effect as a percent of the average scores on math and reading over the range of the days of school observed in our sample, 140 to 160.

First, there is evidence that more instructional time benefits students attending schools in localities that have an advantage with respect to socioeconomic measures that the marginalization index captures. In particular, except for 6th graders, there is evidence that the effects of more days of instruction do accumulate. By contrast,

the effects are smaller and erode faster for students attending the schools located in less-advantaged areas. In most cases, the gap is larger between the least and most impoverished areas.

The extent to which the marginalization index is proxying for differences in infrastructure, teacher quality or school resources beyond those we control for, these results indicate that changing the input of instructional time can have significant effects on narrowing or increasing the achievement gap. Furthermore, it is suggestive of complementarities between inputs of student achievement production. That is, students can benefit from more days of instruction up to a certain point and only to the extent that infrastructure or other inputs that aid in the learning process are accessible.

4.5.3 Other Specifications

We further check that our results are not sensitive to the sample of schools that appear in two periods versus those that appear in three periods (see Table 4.12). Summary statistics of these two samples show that schools appearing for only two periods had smaller group of students and scored lower than those schools appearing in all three years. To the extent that this sample represents school groups whose marginal benefit of additional instruction is larger (smaller groups may give teachers the ability to spend more time explaining concepts), we may expect to find larger effects on this sub-sample of schools that appear in two periods. Estimating our preferred specification on these samples separately, we find that, except for sixth graders, generally, the effects are

consistently larger for the schools that appear only in two periods.¹⁸ In columns 3 and 6 of Table 4.12, we also show the estimated coefficients for the sample that includes the states that were originally dropped. Adding the states of Oaxaca, Michoacan and Tabasco does little to change the findings in Tables 4.6 to 4.9.

Since we also have information on the percent of students who perform over a certain threshold (Non-satisfactory, Basic, Good and Excellent), we conduct our analysis in Section 4.5.1 replacing the average score by the percent of students performing at least at the Basic level. Our results (see Table 4.13) are consistent with what we have presented so far. Having an additional school day has positive effects on student performance. There is also indication of the presence of diminishing returns.

4.6 Discussion

4.6.1 Law and Practice

Law and reality may not be in sync. Although the law states that sanctions be brought against those that do not adhere to the school calendar, the sanctions are not clearly spelled out. There are reports that some states are better able to monitor (or punish lack of) adherence to the school calendar than others. Recently, a SEP's committee proposed guidelines to promote adherence to the school calendar year. Part of the suggestions included: avoiding teacher meetings to be held during scheduled instructional time, educating public about the importance of adhering to the

¹⁸Eren and Millimet (2007) and Bellei (2009) find that instructional time have varying effects along the performance distribution.

school calendar through mass media, and establishing monitoring committees made up of parents, teachers and public organizations. However, discussions of sanctions were still not clear.¹⁹

Newspaper articles report cases in which certain schools went on vacation a couple of days earlier than scheduled. However, authorities disclosed being aware of the problem but claim that they could not identify the schools that did not adhere to the calendar and hence sanctions could not be put in place.²⁰ This situation raises two issues. First, nonrandom measurement error in the number of days of instruction will lead to attenuation bias, which means that our estimates would be a lower bound. Second, bias can also be created when schools do not adhere to the school calendar in a time-varying manner.

4.6.2 Other

An important assumption is that additional days of instruction translate into more days teachers have for instruction and for students to learn. We do not address the number of instructional days that students choose to attend nor the number of instructional days that teachers conduct classes. Our estimates would be biased upward if changes in the start of the calendar year lead to more student or teacher absenteeism that causes some classmates to fall behind. For instance, if parents or teachers extend

¹⁹“Seguimiento al Cumplimiento del Calendario Escolar,” *Séptima Reunión de Trabajo de Autoridades Educativas Responsables de la Planeación y Evaluación*, April 2008. www.upepe.sep.gob.mx, accessed April 17, 2010.

²⁰“Escuelas Arrancas Hojas al Calendario de la SEP,” yucatanalamano.com, Accessed January 1, 2010.

summer vacation into the calendar year or go on vacation earlier than supposed to. Unfortunately, due to lack of data availability we cannot control for absenteeism in our analysis.

We focused our analysis on public schools because the majority of children attend these schools. Our findings indicate that the effects of more days of instruction are smaller for students attending schools in localities with high levels of marginalization. Thus, we would expect the results from examining private schools to follow a similar pattern of that found in schools located in low levels of marginalization.

Furthermore, our study investigates morning shift schools, which can draw students from higher SES or ability levels (Parker *et al* 2008). Multiple shift schools are more common in urban, crowded areas where the demand is high and the supply is low. Due to the time and facility constraints, multiple shift schools may also be less likely to offer academic programs or resources that can aid student learning. If evening shift schools have higher proportion of students of lower academic ability, and these students benefit less (more) from more school days, then we would expect our estimates would be smaller (larger) effects for the evening shift schools.

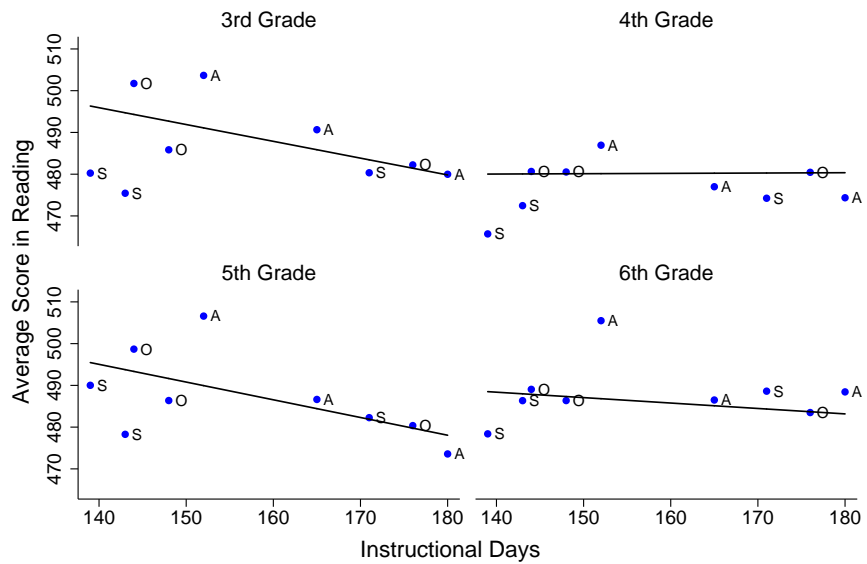
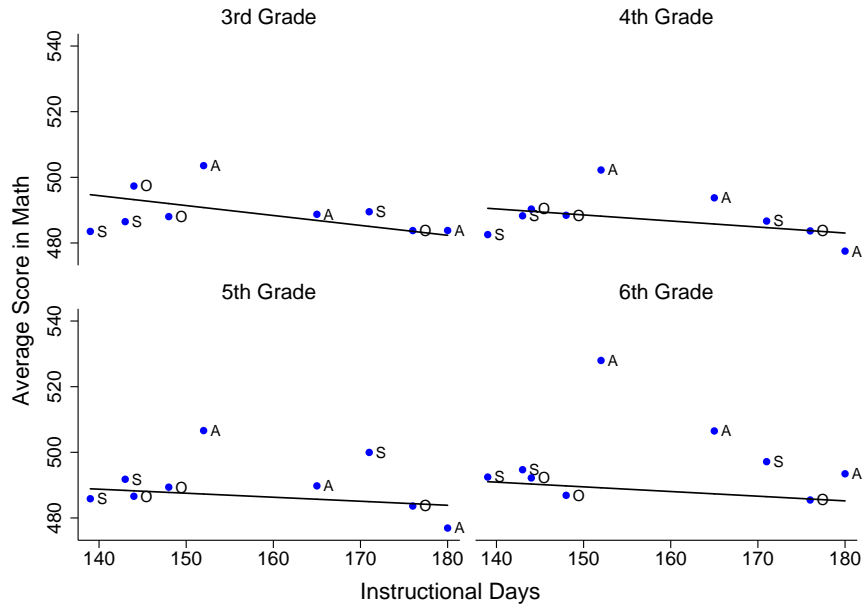
4.7 Conclusion

State mandated changes in the start of the calendar year and the implementation of an standardized exam provide a quasi-experimental opportunity to estimate the impact of more instructional days on student achievement in Mexico.

Our results are consistent with the argument that having more days to prepare for an examination can translate into higher average student scores in both math and reading. The impact of more days of instruction varies by subjects. For instance, the third graders benefit the most from more instructional days. On the other hand, 6th graders benefit the most from having more days of instruction prior to examination. We also observe diminishing returns in instructional days. Estimating the cumulative effect as a percent of average scores we find that ten additional days of instruction can increase scores from 1 to 2 percent.

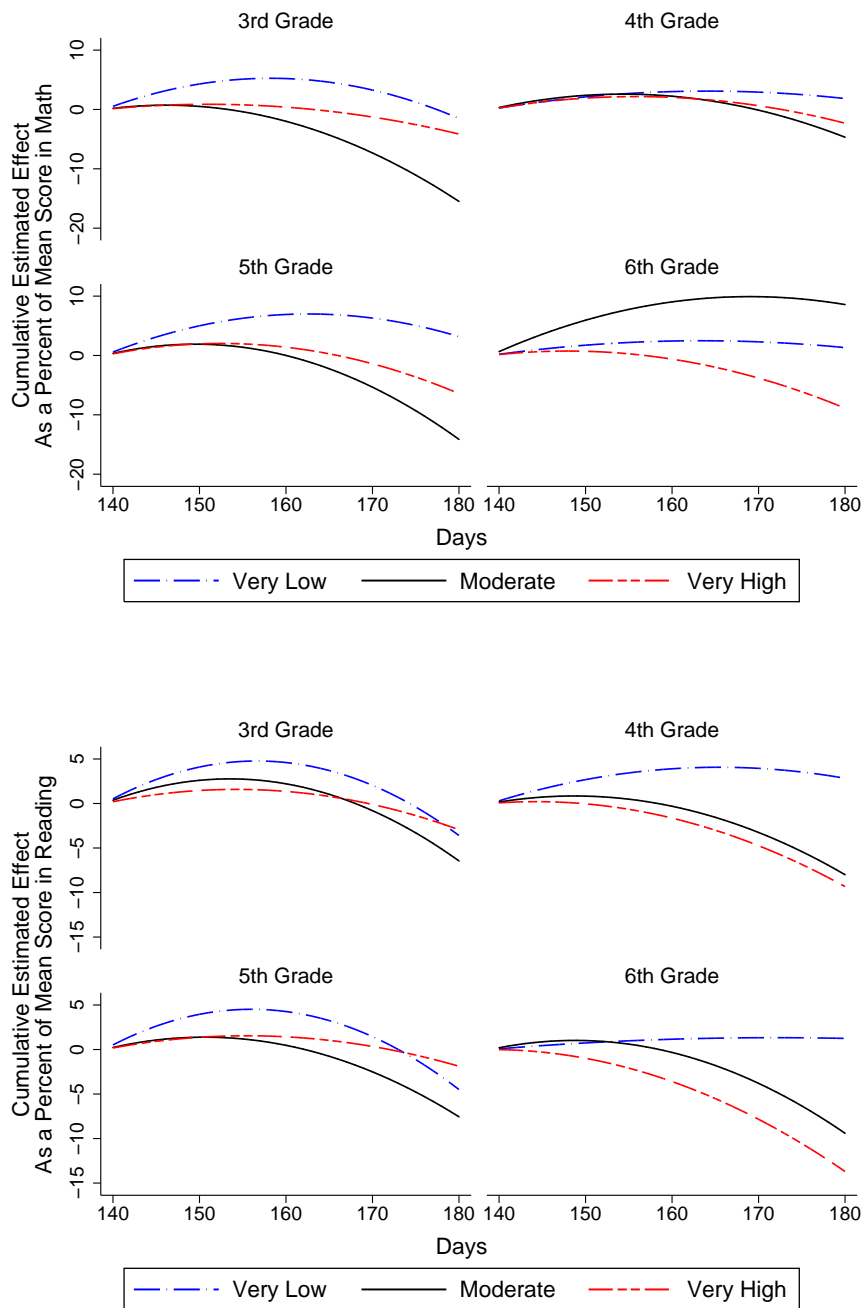
Taken together, there is support for the potential benefits of extending the school year. However, we also note that this does not necessarily mean doing so for all the schools as we observe differential effects across marginalization levels. Our results suggest that for students attending schools in highly marginalized areas, more important than having more days of instructions may be having access to resources that aid learning.

Figure 4.1: Instructional Days and Average Scores in Math and Reading



Note:
A = Aguascalientes S = Sinaloa O = Other

Figure 4.2: Cumulative Effects Differ by Marginalization Index



Note: Marginalization levels as determined by Mexico's National Council of Population, (CONAPO).

Table 4.1: Instructional Days Prior to ENLACE

State	Academic Calendar			Average
	2005-2006	2006-2007	2007-2008	
Aguascalientes				
Instruction Begins	15-Aug	21-Aug	20-Aug	
Examination Date	05-Jun	21-May	14-Apr	
Days of Instruction	180	165	152	166
Change in Days		-15	-13	-14
Sinaloa				
Instruction Begins	29-Aug	28-Aug	20-Aug	
Examination Date	05-Jun	23-Apr	14-Apr	
Days of Instruction	171	143	144	153
Change in Days		-28	-4	-16
Other				
Instruction Begins	22-Aug	21-Aug	20-Aug	
Examination Date	05-Jun	23-Apr	14-Apr	
Days of Instruction	176	148	144	156
Change in Days		-28	-4	-16

Table 4.2: Description of Variables

Variable	Description
<i>Grade Level</i>	
<i>Basic and Higher</i>	Proportion of students in a given grade scoring at least at basic level
Number of Students	Total number of students in a given grade
Male Students	Total number of male students in a given grade
Female Students	Total number of female students in a given grade
<i>School Level:</i>	
Special Ed Students	Total number of students in special education in a given school
Indigenous Students	Total number of indigenous students in a given school
Handicapped Students	Total number of handicapped students in a given school
Gifted Students	Total number of gifted students in a given school
Number of Teachers	Total number of teachers in a school
Carrera Magisterial	Total number of teachers who participate in Carrera Magisterial in a given school
Physical Education	Total number of physical education teachers in a school
Arts	Total number of arts teachers in a school
Technology	Total number of technology teachers in a school
Language	Total number of foreign language teachers in a school
Administrators	Total number of administrators in a school
Principals Teaching	Total number of groups taught by a principal in a school
Student-Teacher Ratio	Students-teacher ratio in a school
<i>State/Locality Level:</i>	
GDP	Gross domestic product (millions of 1993 Mexican pesos)
%GDP on Ed Spending	Percent of GDP spent on educational services
High Marginalized	Proportion of schools in moderate, high or very high marginal level localities

Table 4.3: Student Performance on ENLACE

	3rd Grade	4th Grade	5th Grade	6th Grade
	Math			
Mean	489.66 (65.83)	487.48 (66.98)	486.83 (65.64)	488.16 (69.27)
<i>Proportion:</i>				
Non-Satisfactory	0.28 (0.25)	0.26 (0.24)	0.27 (0.25)	0.26 (0.25)
Basic	0.49 (0.22)	0.57 (0.23)	0.56 (0.22)	0.61 (0.23)
Good	0.20 (0.19)	0.15 (0.18)	0.15 (0.17)	0.12 (0.17)
Excellent	0.03 (0.08)	0.02 (0.06)	0.02 (0.06)	0.01 (0.06)
	Reading			
Mean	489.58 (62.20)	480.16 (60.46)	488.32 (60.58)	486.10 (60.81)
<i>Proportion:</i>				
Non-Satisfactory	0.25 (0.23)	0.29 (0.25)	0.24 (0.23)	0.25 (0.24)
Basic	0.49 (0.21)	0.55 (0.22)	0.60 (0.21)	0.59 (0.22)
Good	0.24 (0.22)	0.15 (0.17)	0.15 (0.17)	0.15 (0.17)
Excellent	0.02 (0.06)	0.01 (0.04)	0.01 (0.04)	0.01 (0.04)
Observations	136,224	136,060	135,978	135,306

Note: Standard deviations in parentheses.

Table 4.4: Summary Statistics of Student and School Characteristics

	Aguascalientes	Sinaloa	Other	All
<i>Grade Level</i>				
<i>Basic and Higher</i>				
Math	0.74 (0.20)	0.73 (0.25)	0.73 (0.24)	0.73 (0.24)
Reading	0.74 (0.19)	0.72 (0.24)	0.74 (0.23)	0.74 (0.23)
Number of Students	38.55 (28.14)	24.13 (23.70)	30.99 (31.25)	30.81 (31.01)
Male Students	19.61 (14.49)	12.36 (12.24)	15.81 (16.00)	15.72 (15.88)
Female Students	18.94 (14.39)	11.76 (12.01)	15.17 (15.79)	15.08 (15.67)
<i>School Level:</i>				
Special Ed Students	2.77 (6.10)	3.53 (8.66)	1.74 (5.96)	1.82 (6.09)
Indigenous Students	0.01 (0.13)	0.42 (5.22)	4.62 (29.86)	4.41 (29.17)
Handicapped Students	6.32 (11.91)	4.26 (9.92)	3.32 (10.42)	3.39 (10.43)
Gifted Students	0.16 (1.08)	0.19 (2.10)	0.18 (2.30)	0.18 (2.28)
Number of Teachers	7.07 (4.79)	4.58 (4.41)	6.32 (5.65)	6.26 (5.61)
Carrera Magisterial	5.47 (4.47)	2.99 (3.71)	3.84 (4.49)	3.82 (4.47)
Physical Education	0.72 (0.57)	0.34 (0.56)	0.39 (0.62)	0.39 (0.62)
Arts	0.28 (0.67)	0.13 (0.43)	0.07 (0.31)	0.07 (0.32)
Technology	0.01 (0.12)	0.18 (0.42)	0.07 (0.40)	0.07 (0.39)
Language	0.64 (0.93)	0.09 (1.46)	0.06 (0.34)	0.07 (0.36)
Administrators	1.43 (1.36)	0.65 (0.50)	1.02 (1.74)	1.01 (1.73)
Principals Teaching	0.34 (0.47)	0.56 (0.34)	0.48 (0.51)	0.48 (0.51)
Student-Teacher Ratio	31.41 (0.10)	27.23 (0.34)	26.70 (2.96)	26.77 (2.93)
<i>State/Locality Level:</i>				
GDP	93,071 (2,602)	169,809 (5,891)	351,272 (285,871)	341,914 (282,246)
%GDP on Ed Spending	1.11 (0.04)	2.20 (0.01)	4.61 (3.86)	4.48 (3.81)
High Marginalized	0.14 (0.34)	0.46 (0.49)	0.57 (0.49)	0.57 (0.49)
Observations	5429	20,309	517,830	543,568

Note: Standard deviations in parentheses. Other excludes Aguascalientes and Sinaloa.

Table 4.5: Summary Statistics of Student and School Characteristics: Various Samples

	Invalid Obs	T=1	T=2	T=3
<i>Grade Level:</i>				
<i>Basic or Higher</i>				
Math	-	0.66 (0.34)	0.67 (0.32)	0.73 (0.23)
Reading	-	0.67 (0.33)	0.69 (0.31)	0.74 (0.22)
Number of Students	9.06 (18.55)	9.97 (16.57)	11.53 (17.81)	32.79 (31.39)
Male Students	4.64 (9.58)	5.15 (8.32)	6.03 (9.12)	16.72 (16.09)
Female Students	4.42 (19.25)	4.82 (8.84)	5.50 (9.04)	16.07 (15.88)
<i>School Level:</i>				
Special Ed Students	0.54 (2.97)	0.34 (2.90)	0.51 (2.98)	1.96 (6.31)
Indigenous Students	2.84 (20.81)	4.68 (24.66)	2.41 (17.99)	4.58 (30.00)
Handicapped Students	1.09 (5.75)	0.86 (5.20)	1.03 (4.95)	3.63 (10.80)
Gifted Students	0.08 (1.61)	0.03 (0.64)	0.07 (1.35)	0.20 (2.36)
Number of Teachers	2.11 (3.68)	1.60 (3.34)	2.10 (3.65)	6.70 (5.61)
Carrera Magisterial	1.30 (2.49)	0.87 (2.04)	1.24 (2.43)	4.10 (4.55)
Physical Education	0.12 (0.38)	0.06 (0.26)	0.12 (0.38)	0.42 (0.63)
Arts	0.02 (0.16)	0.01 (0.11)	0.02 (0.17)	0.08 (0.33)
Technology	0.02 (0.18)	0.01 (0.11)	0.02 (0.34)	0.08 (0.41)
Language	0.01 (0.14)	0.01 (0.12)	0.01 (0.14)	0.08 (0.38)
Administrators	0.27 (0.91)	0.15 (0.65)	0.28 (0.97)	1.09 (1.77)
Principals Teaching	0.80 (0.41)	0.86 (0.35)	0.16 (0.39)	0.45 (0.51)
Student-Teacher Ratio	26.64 (2.75)	26.49 (2.67)	26.30 (2.88)	26.81 (2.93)
<i>State/Locality Level:</i>				
GDP	309,568 (216,162))	310,993 (208,586)	302,782 (216,667)	345,767 (287,883)
%GDP on Ed Spending	3.86 (2.76)	3.96 (2.64)	3.90 (2.83)	4.54 (3.89)
High Marginalized	0.72 (0.44)	0.76 (0.42)	0.76 (0.42)	0.55 (0.49)
Observations	18,821	7,348	42,786	493,434

Note: Standard deviations in parentheses.

Table 4.6: Estimates of Instructional Days on Student Performance on ENLACE: Third Grade

	Math			Reading				
Days	-11.1422 (7.5333)	14.0623*** (1.9137)	-10.5018 (7.9997)	16.7063*** (2.4033)	-20.1933* (11.6244)	16.1889*** (2.1321)	-22.4376** (10.8202)	17.2716*** (3.5075)
Days Squared	0.0337 (0.0234)	-0.0472*** (0.0059)	0.0319 (0.0248)	-0.0555*** (0.0082)	0.0616* (0.0361)	-0.0519*** (0.0055)	0.0686* (0.0335)	-0.0556*** (0.0117)
Special Ed	0.1605***	0.1624***	-0.0203	-0.0214	0.1349***	0.1514***	-0.0192	-0.0217
Indigenous	-0.1356***	-0.1364***	0.0037	0.0038	-0.1183***	-0.1198***	-0.0037	-0.0037
Gifted	-0.0603	-0.0599	-0.0226	-0.0133	-0.0585	-0.0527	-0.049	-0.0342
Handicapped	0.0585*	0.0598*	-0.0121	-0.0087	0.0595*	0.0608**	-0.0116	-0.0062
Female/Male	1.7562***	1.7720***	2.4508*	2.4883***	3.1139***	3.1394***	3.8321***	3.8909***
Physical Ed	2.2763*	2.2244*	0.9684	0.6652	2.6110**	2.4696**	0.962	0.4954
Arts	4.7792	4.8233	1.8052*	1.4349	3.7055	3.7249	1.8551	1.2800
Technology	2.1002	2.0919	0.7418	0.5545	1.1607	1.3850	0.8651	0.5885
Languages	5.7140***	5.7253***	0.1013	-0.498	6.7966***	6.4109***	0.7799	-0.1596
Administrative Staff	0.4847	0.4833	0.3259	0.2545	1.3140***	1.2785**	0.1792	0.0676
Carrera Magisterial	3.2112***	3.2217***	0.0951	0.2067	3.0019***	3.0165***	0.0736	0.2448
Groups Principal Teaches	-13.2820*	-13.2574***	-0.2263	-0.1025	-14.8956***	-14.8645***	-0.4257	-0.2425
Student-Teacher Ratio	0.3711	0.3716	-4.1822	-7.7330**	0.4666	0.4270	-2.9848	-7.9763***
GDP	0.0001***	0.0001***	0.0001	0.0001	0.0000***	0.0000***	0.0001	0.0000
% GDP on Ed Spending	-3.9328***	-3.9223***	3.7127	4.0661	-2.8952***	-2.9008***	3.9572	3.9227
Observations	136,224	136,224	136,224	136,224	136,224	136,224	136,224	136,224
Year FE		√		√		√		√
School FE			√	√			√	√

Note: Significant at *10 %, **5 %, ***1 %. Robust standard errors in parentheses. Standard errors clustered at state level.

Table 4.7: Estimates of Instructional Days on Student Performance on ENLACE: Fourth Grade

	Math			Reading				
Days	-0.4001 (2.8945)	9.8289*** (2.9588)	3.1105 (3.9477)	13.8908*** (1.6848)	1.6738 (3.2499)	9.4749*** (2.4711)	3.6396 (3.0551)	10.1513*** (1.4424)
Days Squared	0.0006 (0.0091)	-0.0336*** (0.0096)	-0.0099 (0.0123)	-0.0442*** (0.0057)	-0.0052 (0.0102)	-0.0316*** (0.0097)	-0.0111 (0.0096)	-0.0339*** (0.0048)
Special Ed	0.1559***	0.1509***	0.0259	0.0255	0.1637***	0.1588***	0.0251	0.0254
Indigenous	-0.0990***	-0.0991***	-0.0116	-0.0116	-0.1059***	-0.1060***	-0.0132	-0.0131
Gifted	-0.0139	-0.0159	-0.0767	-0.0725	-0.095	-0.0969	-0.0816	-0.0799
Handicapped	0.0357	0.0365	-0.0009	0.0004	0.0766**	0.0773**	0.0146	0.0152
Female/Male	2.5834***	2.5854***	3.0469***	3.0468***	4.2247***	4.2268***	4.8844***	4.8850***
Physical Ed	1.9719	1.9791	1.3414*	1.2173	2.7643**	2.7751**	1.3301**	1.2703**
Arts	4.7629	4.801	2.1258**	1.9768**	4.4173	4.4498	0.7738	0.7053
Technology	1.3109	1.209	0.3170	0.2386	0.7851	0.6886	0.1271	0.0815
Languages	5.8058***	5.9779***	0.2832	0.0468	7.3135***	7.4764***	0.4009	0.2993
Administrative Staff	0.2035	0.2166	0.6294	0.598	1.2073**	1.2199**	0.4616	0.4472
Carrera Magisterial	3.4923***	3.4968***	0.1788	0.2252*	3.0987***	3.1022***	0.0946	0.1172
Groups Principal Teaches	-16.7375***	-16.7244***	-1.6928*	-1.6433*	-17.9094***	-17.8992***	-1.7114*	-1.6831*
Student-Teacher Ratio	0.1240	0.1412	-6.5545*	-7.9240**	0.2689	0.2852	-4.8803	-5.8294*
GDP	0.0000***	0.0000***	0.0003**	0.0003**	0.0001***	0.0001***	0.0002*	0.0002*
% GDP on Ed Spending	-3.3475***	-3.3339***	19.847	19.9637	-2.9458***	-2.9338***	15.4977	15.9154
Observations	136,060	136,060	136,060	136,060	136,060	136,060	136,060	136,060
Year FE		√		√		√		√
School FE			√	√			√	√

Note: Significant at *10 %, **5 %, ***1 %. Robust standard errors in parentheses. Standard errors clustered at state level.

Table 4.8: Estimates of Instructional Days on Student Performance on ENLACE: Fifth Grade

	Math			Reading				
Days	7.3007*** (1.7261)	16.4051*** (2.8981)	12.7621*** (3.0971)	22.0971*** (2.2878)	-2.1915 (3.6469)	10.2073*** (1.6605)	0.4732 (4.6736)	13.5738*** (1.6849)
Days Squared	-0.0232*** (0.0054)	-0.0570*** (0.0082)	-0.0397*** (0.0096)	-0.0728*** (0.0078)	0.0064 (0.0114)	-0.0346*** (0.0059)	-0.0015 (0.0145)	-0.0472*** (0.0058)
Special Ed	0.1664***	0.1477***	0.0183	0.0188	0.0900**	0.0858**	0.0041	0.0046
Indigenous	-0.0735**	-0.0731**	-0.0258*	-0.0257*	-0.0949***	-0.0951***	-0.0026	-0.0024
Gifted	-0.0500	-0.0571	-0.1260	-0.1235	-0.1473	-0.149	-0.1729	-0.1699
Handicapped	0.0344	0.0358	0.0180	0.0189	0.0716*	0.0725*	0.0153	0.0167
Female/Male	1.8150***	1.8067***	2.2975***	2.2869***	5.9065***	5.9085***	6.3685***	6.3776***
Physical Ed	2.0671	2.1410	1.1282**	1.0473**	2.5965**	2.5954**	1.5872**	1.4733**
Arts	4.6992	4.7763	1.9465**	1.8549**	4.5013	4.5429	0.4047	0.2682
Technology	1.2975	0.9645	-0.8860	-0.9497	0.8906	0.7986	0.1270	0.0380
Languages	5.1590**	5.7239***	0.0858	-0.0543	6.6493***	6.8033***	1.4160	1.2056
Administrative Staff	0.2005	0.2459	0.6768	0.6597	0.9410*	0.9528*	0.9280**	0.9042**
Carrera Magisterial	3.5952***	3.6002***	0.0655	0.0971	3.1412***	3.1465***	-0.2167	-0.1712
Groups Principal Teaches	-16.5989***	-16.5861***	-0.5513	-0.5088	-15.0839***	-15.0649***	-1.0692	-1.0114
Student-Teacher Ratio	0.0403	0.0958	-6.5885**	-7.9670**	-0.2834	-0.2671	-4.7678	-6.6256*
GDP	0.0000*	0.0000*	0.0003***	0.0003***	0.0000**	0.0000**	0.0003**	0.0003**
% GDP on Ed Spending	-2.6939**	-2.6612**	19.7345*	20.3961*	-1.3492	-1.3361	14.6275	15.4095
Observations	135,978	135,978	135,978	135,978	135,978	135,978	135,978	135,978
Year FE		√		√		√		√
School FE			√	√			√	√

Note: Significant at *10 %, **5 %, ***1 %. Robust standard errors in parentheses. Standard errors clustered at state level.

Table 4.9: Estimates of Instructional Days on Student Performance on ENLACE: Sixth Grade

	Math		Reading	
Days	-7.8942* (4.4745)	7.5119** (3.0541)	-4.5582 (6.0649)	15.6934*** (3.2919)
Days Squared	0.0240* (0.0140)	-0.0258*** (0.0075)	0.0141 (0.0189)	-0.0511*** (0.0110)
Special Ed	0.0414	0.0407	0.0006	0.0000
Indigenous	-0.0331	-0.0335	-0.0058	-0.0057
Gifted	0.0148	0.0145	-0.1455	-0.1385
Handicapped	-0.0045	-0.0036	0.0032	0.0058
Female/Male	4.2239***	4.2264***	4.5823***	4.5948***
Physical Ed	2.0179	1.9949	2.1230**	1.9066**
Arts	5.1977	5.2352	0.9335	0.6664
Technology	0.9452	0.9096	0.9897	0.8463
Languages	5.8335**	5.8916**	2.4536**	2.0267*
Administrative Staff	0.4102	0.4137	1.2419**	1.1938**
Carrera Magisterial	3.8338***	3.8402***	-0.2029	-0.1185
Groups Principal Teaches	-15.1626***	-15.1400***	-1.4938	-1.4040
Student-Teacher Ratio	-0.2113	-0.2042	-8.4754*	-11.0413***
GDP	0.0000	0.0000	0.0003**	0.0003*
% GDP on Ed Spending	-1.1446	-1.1352	25.8193*	26.1457*
Observations	135,306	135,306	135,306	135,306
Year FE	✓	✓	✓	✓
School FE		✓	✓	✓

Note: Significant at *10 %, **5 %, ***1 %. Robust standard errors in parentheses. Standard errors clustered at state level.

Table 4.10: Student Performance in Math by Marginalization Level and Grade

	Marginalization Level				
	Very Low	Low	Moderate	High	Very High
3rd Grade					
Days	22.1242*** (4.8718)	23.8419*** (3.1506)	20.3256*** (1.8072)	15.1002*** (3.1811)	8.8817*** (0.8630)
Days Squared	-0.0697*** (0.0155)	-0.0857*** (0.0113)	-0.0693*** (0.0063)	-0.0512*** (0.0107)	-0.0293*** (0.0030)
Observations	7,291	46,286	19,037	21,285	33,426
4th Grade					
Days	7.8990** (3.7407)	19.6048*** (2.0749)	16.6281*** (2.1801)	12.9529*** (2.0478)	11.6930*** (1.2164)
Days Squared	-0.0240* (0.0118)	-0.0637*** (0.0074)	-0.0537*** (0.0074)	-0.0425*** (0.0069)	-0.0374*** (0.0040)
Observations	7,268	46,238	19,003	21,266	33,397
5th Grade					
Days	19.9618*** (4.3462)	29.8073*** (2.9580)	25.2017*** (2.6886)	20.9314*** (2.5191)	16.6140*** (1.3552)
Days Squared	-0.0612*** (0.0135)	-0.1104*** (0.0104)	-0.0840*** (0.0091)	-0.0706*** (0.0085)	-0.0543*** (0.0045)
Observations	7,288	46,237	18,968	21,265	33,363
6th Grade					
Days	6.6861 (6.0066)	14.2687*** (3.2805)	18.2691*** (3.7582)	16.6661*** (1.4476)	13.5669*** (3.7553)
Days Squared	-0.0204 (0.0190)	-0.0494*** (0.0116)	-0.0539*** (0.0126)	-0.0550*** (0.0049)	-0.0457*** (0.0122)
Observations	7,127	45,953	18,922	21,234	33,241

Notes:

Significant at *10 %, **5 %, ***1 %.

Robust standard errors in parentheses. Standard errors clustered at state level.

All regressions include female-male student ratio, total number of students that are in special education, gifted, handicapped, and indigenous, total number of arts, technology, foreign language and physical education teachers, total number of administrative staff and teachers participating in Carrera Magisterial, student-teacher ratio, state level of GDP and percent of GDP on educational services. Marginalization levels as determined by Mexico's National Council of Population, (CONAPO).

Table 4.11: Student Performance in Reading by Marginalization Level and Grade

	Marginalization Level				
	Very Low	Low	Moderate	High	Very High
3rd Grade					
Days	23.6037*** (5.4253)	25.6042*** (2.5875)	19.8064*** (1.7068)	14.7297*** (3.9588)	10.3373*** (2.4683)
Days Squared	-0.0751*** (0.0171)	-0.0855*** (0.0092)	-0.0643*** (0.0060)	-0.0471*** (0.0132)	-0.0334*** (0.0082)
Observations	7,291	46,286	19,037	21,285	33,426
4th Grade					
Days	9.2919** (3.4391)	11.9358*** (1.7999)	12.9197*** (2.0352)	9.3450*** (1.1495)	10.3955*** (0.9700)
Days Squared	-0.0280** (0.0107)	-0.0380*** (0.0062)	-0.0433*** (0.0068)	-0.0325*** (0.0039)	-0.0359*** (0.0032)
Observations	7,268	46,238	19,003	21,266	33,397
5th Grade					
Days	23.9129*** (4.6804)	15.3050*** (2.3782)	15.2604*** (2.1654)	14.6387*** (1.4276)	8.6724*** (1.3473)
Days Squared	-0.0764*** (0.0146)	-0.0595*** (0.0083)	-0.0505*** (0.0073)	-0.0476*** (0.0050)	-0.0278*** (0.0045)
Observations	7,288	46,237	18,968	21,265	33,363
6th Grade					
Days	2.0681 (6.1666)	13.6614*** (2.5362)	15.5248*** (3.2274)	14.9902*** (2.8012)	11.0482*** (1.8861)
Days Squared	-0.0060 (0.0194)	-0.0459*** (0.0089)	-0.0520*** (0.0108)	-0.0525*** (0.0094)	-0.0396*** (0.0060)
Observations	7,127	45,953	18,922	21,234	33,241

Notes:

Significant at *10 %, **5 %, ***1 %.

Robust standard errors in parentheses. Standard errors clustered at state level.

All regressions include female-male student ratio, total number of students that are in special education, gifted, handicapped, and indigenous, total number of arts, technology, foreign language and physical education teachers. Total number of administrative staff and teachers participating in Carrera Magisterial, student-teacher ratio, state level of GDP and percent of GDP on educational services. Marginalization levels as determined by Mexico's National Council of Population, (CONAPO).

Table 4.12: Instructional Days on Student Performance Using Various Samples

	Math			Reading		
	t = 2	t = 3	All States	t = 2	t = 3	All States
3rd Grade						
Days	29.4825* (3.1637)	16.0499* (2.3612)	16.6028* (2.5355)	25.8596* (3.2879)	16.8795* (3.6551)	17.2115* (3.5853)
Days Squared	-0.1012* (0.0109)	-0.0533* (0.0081)	-0.0551* (0.0086)	-0.0822* (0.0113)	-0.0544* (0.0122)	-0.0553* (0.0120)
Observations	10,810	123,621	145,062	10,810	123,621	145,062
4th Grade						
Days	30.5614* (2.3401)	13.1845* (1.7763)	12.7679* (2.0990)	22.9874* (2.1687)	9.6268* (1.5786)	9.4295* (1.6241)
Days Squared	-0.0974* (0.0081)	-0.0420* (0.0060)	-0.0404* (0.0071)	-0.0785* (0.0072)	-0.0321* (0.0053)	-0.0314* (0.0055)
Observations	10,596	123,666	144,841	10,596	123,666	144,841
5th Grade						
Days	19.9321* (3.2484)	21.9659* (2.3540)	20.8247* (2.7214)	12.4936* (2.1257)	13.9276* (1.2758)	12.9977* (1.4764)
Days Squared	-0.0702* (0.0111)	-0.0724* (0.0080)	-0.0685* (0.0092)	-0.0449* (0.0073)	-0.0452* (0.0044)	-0.0421* (0.0051)
Observations	10,470	123,687	144,743	10,470	123,687	144,743
6th Grade						
Days	5.6269 (5.6868)	15.9738* (3.0474)	15.1316* (3.2702)	0.3698 (4.8497)	14.1016* (1.6102)	12.6898* (1.7973)
Days Squared	-0.0246 (0.0189)	-0.0519* (0.0102)	-0.0492* (0.0109)	-0.006 (0.0162)	-0.0489* (0.0056)	-0.0442* (0.0062)
Observations	10,910	122,460	144,027	10,910	122,460	144,027

Notes:

Significant at *10 %, **5 %, ***1 %.

Robust standard errors in parentheses. Standard errors clustered at state level.

All regressions include female-male student ratio, total number of students that are in special education, gifted, handicapped, and indigenous, total number of arts, technology, foreign language and physical education teachers. Total number of administrative staff and teachers participating in Carrera Magisterial, student-teacher ratio, state level of GDP and percent of GDP on educational services. Marginalization levels as determined by Mexico's National Council of Population, (CONAPO).

Table 4.13: Using Percent of Students Scoring at Least at the Basic Level

	Math			Reading		
	3rd Grade					
Days	-0.0015 (0.0137)	0.0388*** (0.0061)	0.0061 (0.0180)	0.0505*** (0.0086)	-0.0087 (0.0192)	0.0442*** (0.0066)
Days Squared	0.0000 (0.0000)	-0.0001*** (0.0000)	0.0000 (0.0001)	-0.0002*** (0.0000)	0.0000 (0.0001)	-0.0001*** (0.0000)
Observations	131,063	131,063	131,063	131,063	133,115	133,115
	4th Grade					
Days	0.0543*** (0.0107)	0.0325*** (0.0099)	0.0697*** (0.0084)	0.0449*** (0.0070)	0.0654*** (0.0153)	0.0311** (0.0148)
Days Squared	-0.0002*** (0.0000)	-0.0001*** (0.0000)	-0.0002*** (0.0000)	-0.0001*** (0.0000)	-0.0002*** (0.0000)	-0.0001** (0.0000)
Observations	131,198	131,198	131,198	131,198	131,930	131,930
	5th Grade					
Days	0.0500*** (0.0128)	0.0452*** (0.0122)	0.0729*** (0.0081)	0.0603*** (0.0101)	-0.0512** (0.0217)	0.0422*** (0.0051)
Days Squared	-0.0002*** (0.0000)	-0.0002*** (0.0000)	-0.0002*** (0.0000)	-0.0002*** (0.0000)	0.0002** (0.0001)	-0.0001*** (0.0000)
Observations	131,028	131,028	131,028	131,028	133,019	133,019
	6th Grade					
Days	0.0384** (0.0141)	0.0150** (0.0058)	0.0536*** (0.0105)	0.0318*** (0.0066)	0.0284*** (0.0082)	0.0304*** (0.0062)
Days Squared	-0.0001** (0.0000)	-0.0001*** (0.0000)	-0.0002*** (0.0000)	-0.0001*** (0.0000)	-0.0001*** (0.0000)	-0.0001*** (0.0000)
Observations	130,796	130,796	130,796	130,796	131,882	131,882

Note: Significant at *10 %, **5 %, ***1 %. Robust standard errors in parentheses. Standard errors clustered at state level. All regressions include female-male student ratio, total number of students that are in special education, gifted, handicapped, and indigenous, total number of arts, technology, foreign language and physical education teachers. Total number of administrative staff and teachers participating in Carrera Magisterial, student-teacher ratio, state level of GDP and percent of GDP on educational services. Marginalization levels as determined by Mexico's National Council of Population, (CONAPO).

Chapter 5

Conclusions

Identifying factors that affect, either directly or indirectly, labor market outcomes becomes relevant to identify causes underlying economic well-being. This dissertation investigated the impact of institutional reforms surrounding marital dissolution, domestic violence and education—all of which have implications on labor market outcomes—in Mexico.

The dissertation addressed the following questions. 1) Did the expansion of divorce provisions contribute to the rise in divorce rates? 2) Can public policy create a mechanism through which mediate domestic violence? 3) Does having more days of instruction prior to an exam improve student achievement?

Over the past two decades, Mexico has experienced an increase in its divorce rates. Some state legislators have been concerned that broadening allowable grounds for divorce may have contributed to the rise in divorce and have considered repealing some of these laws. More recently, the adoption of unilateral divorce in Mexico City

in August of 2008 has raised similar concerns. The results in the second chapter of this dissertation do not support the claim that liberalizing divorce laws led to a rise in aggregate divorce rates during the period 1993-2006. In fact, in some cases, the findings suggest that certain causes for divorce is associated with a decrease in divorce rates.

In Chapter 3, the examination of the evolution of the legal reforms surrounding domestic violence consistently show that improving opportunities outside a potentially abusive relationship, such as access to shelters and counseling, changed the trends in homicides, suicides and bodily injuries of married and cohabiting individuals. The results are consistent with bargaining models that predict that improved bargaining can be used to demand for less violence. Specifically, the estimates show that states which established assistance and prevention programs for victims of domestic violence saw a 6 percent decline in homicides committed against males and a decrease of 8-19 percent in suicide rates. Evidence also indicates that bodily injury crime rates declined by about 20 percent among the reforming states.

The analysis in Chapter 4 exploits a quasi-experimental setting arising from state-mandated changes to the start of the school calendar year or the implementation of a standardized exam. Since these changes occurred in some states but not in others, there is within state variation in the number of instructional days prior to an assessment. The model specifications include a rich set of grade and school level covariates that control for possible spurious correlation or omitted variable bias. The quasi-experimental setting and the panel nature of the data allow one to address the question, "Did students who had more days to prepare in the exam do better in comparison to other students in the

same school who did not?” The results are consistent with the argument that students can benefit from having more days of instruction prior to an examination. However, the benefits become even slightly negative after a certain point, that is, there is evidence of diminishing returns to instructional days. Examination of the effects across subjects shows that the benefits of more instructional days are larger for math than reading. Finally, there is evidence that the effects differ across various levels of socioeconomic development. In particular, while the cumulative effects increase with more days of instruction for students attending schools in the most advantaged areas, the effects become even negative for those attending schools in the least advantaged ones.

In sum, this dissertation demonstrated that institutional reform can play a role in altering outcomes that are closely associated with the labor market. The results of this dissertation may be more applicable than those found in the context of developed countries. In particular, the results in Chapter 2 can shed some light in predicting the magnitude and direction of the effects that divorce law reforms have in other Latin American countries where divorce is a relatively new phenomenon.

A comparison of the results in Chapter 3 with those of a study done on the US reveals that there are large effects to improving opportunities outside the relationship in an environment where the prevalence of domestic violence is more severe and where the outside opportunities without institutional reform might be lower than developed countries. Finally, the evidence in Chapter 4 provides support for the potential benefits of extending the school year. However, the results also indicate that this does not necessarily mean doing so for all the schools as there are heterogeneous effects across

levels of economic development. The results suggest that for students attending schools in highly impoverished areas, more important than having more days of instructions may be having access to resources that aid learning.

Bibliography

- [1] Aghajanian, A. (1986). "Some Notes on Divorce in Iran." *Journal of Marriage and the Family*, Vol. 48, No. 4: 749-756.
- [2] Aizer, A. "The Gender Wage Gap and Domestic Violence," *The American Economic Review* (forthcoming).
- [3] Aizer, A. "Poverty, Violence and Health," Manuscript, Brown University and NBER, October 2009.
- [4] Allen, D. W. (1992). "Marriage and Divorce: Comment." *American Economic Review*, Vol. 82, No.3: 679-695.
- [5] Allen, D. W. and M. Brining. (1998). "Sex, Property Rights and Divorce." *European Journal of Law Economics*, Vol. 5: 211-233.
- [6] Anderson, J. B. (2006). "Improving Latin America's School Quality: Which Special Interventions Work?" *Comparative Education Review*, Vol.49, No. 2: 205-229.
- [7] Anderson, K., M.A. Hill, and J.S. Butler. (1987). "Age at Marriage in Malaysia." *Journal of Development Economics*, Vol. 26: 223-234.
- [8] Angelucci, M. (2008). "Love on the Rocks: Alcohol Abuse and Domestic Violence in Rural Mexico," *The B.E. Journal of Economic Analysis & Policy*, Vol. 8, Article 43.
- [9] Asociación Mexicana contra la Violencia hacia las Mujeres, (COVAC). (1995). "Encuesta de Opinión Pública sobre la Incidencia de Violencia en la Familia," Mexico City.
- [10] Azaola, E. (1996). "El Derecho de Ser Mujer. Hombres y Mujeres Homicidas en la Ciudad de México: Historias de Vida," México, D.F.: Editorial Plaza y Valdes.
- [11] Bacalod, M. and J. L. Tobias. (2006). "Schools, School Quality and Achievement Growth: Evidence from the Philippines" *Economics of Education Review*, Vol. 25, No. 6: 619-632.

- [12] Becker, G. (1981). *A Treatise on the Family*. Harvard University Press: Cambridge.
- [13] Beleche, T. and N. Lew. "Till Laws Do Us Part? The Impact of Changing Divorce Laws on Divorce Rates in Mexico," Unpublished Manuscript, November 2009.
- [14] Bellei, C. (2009). "Does Lengthening the School Day Increase Students' Academic Achievement? Results from a Natural Experiment in Chile," *Economics of Education Review*, Vol. 28: 629-640.
- [15] Bertrand, M., E. Duflo, and S. Mullainathan. (2004). "How Much Should We Trust Differences-in-Differences Estimates?," *Quarterly Journal of Economics*, Vol. 119: 249-275.
- [16] Betts, Julian. (1995). "Does School Quality Matter? Evidence from the National Longitudinal Survey of Youth" *The Review of Economics and Statistics*, Vol. 77, No. 2: 231-250.
- [17] Betts, Julian and Eric Johnson. "A Test for Diminishing Returns to School Spending," Manuscript, University of California, San Diego, 1997.
- [18] Bobonis, G., R. Castro and M. Gonzalez-Brenes. "Public Transfers and Domestic Violence: The Roles of Private Information and Spousal Control," mimeo, University of Toronto, UNAM and University of Massachusetts-Amherst, July 2009.
- [19] Bowlus, A. J., and S. Seitz. (2006). "Domestic Violence, Employment, and Divorce," *International Economic Review*, Vol. 47: 1113-1149.
- [20] Buttner, B. and S.L. Thomsen "Are We Spending Too Many Years in School? Causal Evidence of the Impact of Shortening Secondary School Duration," ZEW Discussion Paper 10-011, February 2010.
- [21] Card, D. and A. Krueger, (1992). "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States," *Journal of Political Economy*, Vol. 10: 1-40.
- [22] Card, D. and A. Krueger, (1996). "School Resources and Student Outcomes: An Overview of the Literature and New Evidence from North and South Carolina," *Journal of Economic Perspectives*, Vol 10, No. 4: 31-50.
- [23] Carrell, S. E., and M. L. Hoekstra. (2010). "Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone's Kids," *American Economic Journal: Applied Economics*, Vol. 2: 211-228.
- [24] Chase-Lansdale, P. Lindsay, A. J. Cherlin and K. E. Kiernan. (1995). "The Long-Term Effects of Parental Divorce on the Mental Health of Young Adults: A Development Perspective." *Child Development*, Vol. 66, No. 5:1614-1634.

- [25] Clarke, S., T. Yannicos, and B. Smith. (1993). Births, Marriages, Divorces, and Deaths for September 1993. "In Monthly Vital Statistics Report," Vol. 42, No. 9. Hyattsville, Maryland: National Center for Health Statistics.
- [26] Coase, R. H. (1960). "The Problem of Social Cost," *Journal of Law and Economics*, Vol. 3: 1-44.
- [27] Coleman, J. *et al.* "Equality of Educational Opportunity," Washington : Government Printing Office, 1966.
- [28] Dee, T. S. (2007). "Teachers and the Gender Gaps in Student Achievement, " *Journal of Human Resources*, Vol 42, No. 3: 528-554.
- [29] De Janvry, A., A. Dustan and E. Sadoulet (2010). "The Role of School Quality in the Mexico City Choice-Based Tracking System, " Manuscript, University of California, Berkeley.
- [30] Diaz Aranda, E. (2006). "Criminality and Criminal Law in Mexico," *Mexican Law Review*, Vol. 6: 163-174.
- [31] Ehrlich, I. (1996). "Crime, Punishment, and the Market for Offenses," *Journal of Economic Perspectives*, Vol. 10: 43-67.
- [32] Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares (ENDIREH), 2003.
- [33] Eren, Ozkan and Daniel L. Millimet, (2007). "Time to Learn: The Organizational Structure of Schools and Student Achievement," *Empirical Economics*, Vol. 32: 301-322.
- [34] Farmer, A. and J. Tiefenthaler. (1996). "The Value of Shelters as Signals," *American Economic Review*, Vol. 6, No. 2: 274-279.
- [35] Friedberg, L. (1998). "Did Unilateral Divorce Raise Divorce Rates?" *American Economic Review*, Vol. 88, No. 3: 608-627.
- [36] Fuller, B., (1987). "What School Factors Raise Achievement in the Third World?" *Review of Educational Research*, Vol. 57, No. 3: 255-292.
- [37] Gardner, J., and A. J. Oswald. (2006). "Do Divorcing Couples Become Happier by Breaking Up?" *Journal of the Royal Statistical Society Series A*, Vol. 169, No. 2: 319-336.
- [38] Goode, W. J. (1993). "World Changes in Divorce Patterns," New Haven and London: Yale University Press.
- [39] Grogger, Jeff, (1996). "Does School Quality Explain the Recent Black/White Wage Trend?," *Journal of Labor Economics*, Vol 14, No. 2: 231-253.

- [40] Gruber, J. (2004). "Is Making Divorce Easier Bad for Children?" The Long-Run Implication of Unilateral Divorce." *Journal of Labor Economics*, Vol. 22, No.4: 799-833.
- [41] Hansen, Benjamin, "School Year and Student Performance: Quasi-Experimental Evidence," University of California, Santa Barbara, February, 2008.
- [42] Hanushek, E. A., "Publicly Provided Education" in Handbook of Public Economics Vol. 4, edited by A.J. Auerback and M. Feldstein, 2002.
- [43] Hanushek, E. A., "School Resources " in Handbook of Economics of Education Vol. 2, edited by E. Hanushek and F. Welch, 2006.
- [44] Hanushek, E. A., and D.D. Kinko (2000). "Schooling, Labor Force Quality and the Growth of Nations " *American Economic Review* Vol. 90, No. 5: 1184-1208.
- [45] Hanushek, E. A., and L. Woessmann, "Schooling, Cognitive Skills, and the Latin American Growth Puzzle," Unpublished Manuscript, May 2009.
- [46] Heaton, T. B., M. Cammack and L. Young. (2001). "Why is the Divorce Rate Declining in Indonesia?" *Journal of Marriage and the Family*—, Vol. 63, No. 2) 480-490.
- [47] INEGI. (2004). "Marco Metodológico para la Generación de las Estadísticas Vitales: Divorcios." <http://www.inegi.gob.mx>
- [48] Jones, G. W. (1981). "Malay Marriage and Divorce in Peninsular Malaysia: Three Decades of Change." *Population and Development Review*, Vol. 7, No. 2: 255-278.
- [49] June E. H. (1982). "Women's Place' in Politics and Economics in Brazil since 1964." *Luso-Brazilian Review*, Vol. 19, No. 1: 83-91.
- [50] Kessler, D., and S.D. Levitt. (1999). "Using Sentence Enhancements to Distinguish Between Deterrence and Incapacitation," *Journal of Law and Economics*, Vol. 42: 343-363.
- [51] Krashinsky, Harry. "How Would One Extra Year of High School Affect the Academic Performance in University? Evidence from a Unique Policy Change," Manuscript, 2006.
- [52] Krueger, A. B. (1999). "Experimental Evidence of Education Production Functions," *Quarterly Journal of Economics*, Vol. 114, No. 2: 497-532.
- [53] Lee, Jong-Wha and Robert Barro, (2001). "School Quality in a Cross-Section of Countries," *Economica*, Vol. 68, No. 271: 465-488.
- [54] Ley General de Protección Civil, Diaro Oficial de la Federación, April 24, 2006. <http://www.proteccioncivil.gob.mx/upLoad/Publicaciones/LGPC06.pdf> accessed March 28, 2010.

- [55] Llach, J., C. Adroque, and M.E. Gigaglia, “Do Longer School Days Have Enduring Educational, Occupational or Income Effects? ” LACEA Paper, April 2009.
- [56] Lopez Acevedo, Gladys, (2002). “Teachers’ Incentives and Professional Development in Schools in Mexico,” World Bank Policy Research Working Paper 2777, February 2002.
- [57] Lundberg, S., and R. Pollak. (1993). “Separate Spheres Bargaining and the Marriage Market,” *Journal of Political Economy*, Vol. 101: 988-1010.
- [58] Lundberg, S., and R. Pollak. (1994). “Noncooperative Bargaining Models of Marriage,” *American Economic Review AEA Papers and Proceedings*, Vol. 84: 132-137.
- [59] Lundberg, S., and R. Pollak. (1996). “Bargaining and Distribution in Marriage,” *Journal of Economic Perspectives*, Vol. 10: 139-158.
- [60] Mammen, K. (2008). “The Long Term Effects of the Divorce Revolutions: Health, Wealth and Labor Supply,” Unpublished Manuscript, Columbia University.
- [61] Manser, M., and M. Brown. (1980). “Marriage and Household Decision-Making: A Bargaining Analysis,” *International Economic Review*, Vol. 21: 31-44.
- [62] Marcotte, Dave E. (2007). “Schooling and Test Scores: A Mother-Natural Experiment,” *Economics of Education Review*, Vol. 26: 629-640.
- [63] Marcotte, Dave E., and Steven W. Hemelt. (2007). “Unscheduled School Closings and Student Performance,” IZA Discussion Paper 2923.
- [64] Marcotte, Dave E., and Benjamin Hansen. “Time for School,” *Education Next*, Winter 2010: 53-59.
- [65] McElroy, M. B., and M. J. Horney. (1981). “Nash-Bargained Household Decisions: Toward a Generalization of the Theory of Demand,” *International Economic Review*, Vol. 22: 333-349.
- [66] McEwan, Patrick, J. and L. Santibanez, “Teacher Incentives and Student Achievement: Evidence from a Mexican Reform,” Manuscript, Wellesley College and RAND, 2005.
- [67] NotiSur. “Chile: Divorce Legal After Eight Years of Debate.” May 21, 2004.
- [68] Orden Jurídico Nacional. (2008). “Diarios, Periodicos y Gacetas Oficiales.” <http://www.ordenjuridico.gob.mx> (last updated September 2008).
- [69] Palafox, J. C., J. Prawda and E. Velez. (1994). “Primary School Quality in Mexico,” *Comparative Education Review*, Vol. 38, No. 2: 167-180.
- [70] Parker, Susan W., J. Behrman, and L. Rubalcava “The Quality of Education in Latin America and the Caribbean Region: The Mexican Case,” IDB Research Network Paper, February 2008.

- [71] Perez Contreras, M. (2004). "La Ley de Asistencia y Prevencion de la Violencia Familiar para el Distrito Federal: Comentarios en Torno al Contenido de sus Reformas," *Boletín Mexicano de Derecho Comparado*, Vol. 38: 909-924.
- [72] Peters, H. E. (1986). "Marriage and Divorce: Informational Constraints and Private Contracting." *American Economic Review*, Vol. 76, No. 3: 437-454.
- [73] Peters, H. E. (1992). "Marriage and Divorce: Reply." *American Economic Review*, Vol. 82, No. 3: 686-692.
- [74] Pischke, J. S. (2003). "The Impact of Length of the School Year on Student Performance and Earnings: Evidence from the German Short School Years," NBER Working Paper 9964.
- [75] Plata, M. I. (1988). "Family Law and Family Planning in Colombia." *International Family Planning Perspectives*, Vol. 14, No. 3: 109-111.
- [76] Pollak, R. A. (2004). "An Intergenerational Model of Domestic Violence," *Journal of Population Economics* Vol. 17: 311-329.
- [77] Rivera-Rivera, L., E. Lazcano-Ponce, J. Salmeron-Castro, E. Salazar-Martinez, R. Castro and M. Hernandez-Avila. (2003). "Prevalance and Determinants of Male Partner Violence Against Mexican Women: A Population-based Study," *Salud Publica de Mexico*, Vol. 46: 113-122.
- [78] Santibanez, Lucrecia. (2006). "Why We Should Care If Teachers Get A's: Teacher Test Scores and Student Achievement in Mexico," *Economics of Education Review*, Vol. 25: 510-520.
- [79] Santibanez, Lucrecia, G. Vernez, and P. Razquin, "Education in Mexico. Challenges and Opportunities," RAND Documented Briefing, 2005.
- [80] Schultz, Paul T. (2004), "School subsidies for the poor: evaluating the Mexican Progresa poverty program," *Journal of Development Economics*, Vol. 74, No. 1: 199-250, June.
- [81] Shapiro, Joseph, and Jorge Moreno Trevino. "Compensatory Education for Disadvantaged Mexican students : An Impact Evaluation Using propensity Score Matching," World Bank Policy Research Working Paper 3334, June 2004.
- [82] Skoufias, Emmanuel and Joseph Shapiro. "Evaluating the Impact of Mexico's Quality Schools Program: The Pitfalls of Using Nonexperimental Data," World Bank Policy Research Working Paper 4036, October 2006.
- [83] Stetson, D. M. and G. C. Wright, Jr. (1975). "The Effects of Laws on Divorce in American States." *Journal of Marriage and the Family*, Vol. 37, No. 3: 537-547.
- [84] Stevenson, B. (2008). "Divorce Law and Women's Labor Supply," NBER Working Paper, No. 14346.

- [85] Stevenson, B. and J. Wolfers. (2006). "Bargaining in the Shadow of the Law: Divorce Laws and Family Distress." *Quarterly Journal of Economics*, Vol. 121, No. 1: 267-288.
- [86] Subsecretaría de Asuntos Jurídicos y Derechos Humanos. 2008. "Registro Nacional de Avisos de Testamento: Códigos Civiles." <http://www.testamentos.gob.mx> (last updated September 2008).
- [87] Suprema Corte de Justicia de la Nación. (2008). "Legislación Estatal." <http://www2.scjn.gob.mx/legislacionestatal> (last updated September 2008).
- [88] Tauchen, H. V., D. Witte and S. K. Long. (1991). "Domestic Violence: A Nonrandom Affair," *International Economic Review*, Vol. 32: 491-511.
- [89] Tauchen, H. V., and A. D. Witte. (1995). "The Dynamics of Domestic Violence," *The American Economic Review AEA Papers and Proceedings*, Vol. 85: 414-418.
- [90] Tejada-Vera B, and P. D; Sutton. (2008). "Births, Marriages, Divorces, and Deaths: Provisional Data for January 2008." National Vital Statistics Reports; 57(3). Hyattsville, MD: National Center for Health Statistics.
- [91] Trent, K., and S. J. South. (1989). "Structural Determinants of the Divorce Rate: A Cross-Sectional Analysis," *Journal of Marriage and the Family*, Vol. 51, No. 2: 391-404.
- [92] Villarreal, A. (2007). "Women's Employment Status, Coercive Control and Intimate Partner Violence in Mexico." *Journal of Marriage and Family*, Vol. 69: 418-434.
- [93] Weiss, Y., and R. Willis. (1997). "Match Quality, New Information, and Marital Dissolution." *Journal of Labor Economics*, Vol. 15: S293-S329.
- [94] Wolfers, J. (2006). "Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results." *American Economic Review*, Vol. 96, No. 5: 1802-1820.
- [95] World Health Organization, WHO. (2005). "WHO Multi-country Study on Women's Health and Domestic Violence Against Women," Geneva: World Health Organization.