THREE ESSAYS IN APPLIED MICROECONOMETRICS

A Dissertation

by

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ABSTRACT

This dissertation analyzes the effects of three public policies using quasi-experimental methods. These policies include U.S. cell phone bans that prevent drivers from using cell phones while driving, U.S. castle doctrine laws that justify using lethal force in self-defense situations, and China's marriage registration reform that simplifies the marriage and divorce process.

I first examine if cell phone bans change driver behavior. Using data on observed driver cell phone usage combined with a difference-in-differences approach that exploits the within-state variation in the adoption of bans, I find prohibiting drivers from texting and talking on handheld cell phones reduces each by 60 and 50 percent, respectively. This suggests the policy is effective at reducing the targeted behavior. Combined with findings that show cell phone bans do not reduce traffic accidents and casualties, I further discuss other factors and behavioral responses that may counteract the reduction in observed usage.

Next, I investigate if castle doctrine laws deter crime or escalate violence by lowering the expected cost of using lethal force and increasing the expected cost of committing violent crime. Using a similar difference-in-differences strategy, I find the laws do not deter burglary, robbery, or aggravated assault. In contrast, they lead to a statistically significant 8 percent net increase in the number of reported murders and non-negligent manslaughters.

Finally, I evaluate the marital consequences of easier access to divorce and marriage by exploiting a major policy change in China that simplify both. To distinguish the causal effect from the effect of other confounding factors, I use a regression discontinuity design to compare annual changes in divorce and marriage rates just before and just after the adoption

of the 2003 Regulations on Marriage Registration. Results indicate that lower cost of divorce and marriage immediately triggered faster growth in divorce and marriage rates.

DEDICATION

I dedicate this dissertation to my big family, especially…

to grandpa for sharing his wisdom;

to Mom and Dad for their selfless support and devotion;

to my wife, Xueyan, for her love, patience, and understanding.

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I can never finish this dissertation on my own, because there are so many people who have contributed to this dissertation directly or indirectly and supported me in ways that made it possible. This is an attempt to acknowledge and thank those people, though I understand it is impossible to list everyone or to thank those listed enough.

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CHAPTER I

INTRODUCTION

Understanding human behavior is an important aspect of applied microeconomics. This dissertation examines how people change their behavior in response to three public policies in the U.S. and China in order to evaluate policy effects. To obtain credible causal estimates, I adopt appropriate quasi-experimental methods to carefully distinguish the effects of these policies from the effects of other confounding factors.

The first policy analyzed in this dissertation is the U.S. cell phone ban. In response to concerns that distracted driving due to cell phone use has become a threat to roadway safety, many states have passed laws that prohibit drivers from texting and talking on handheld cell phones. Though recent evidence suggests cell phone bans do not reduce traffic accidents, it is not clear if these bans change driver behavior in the first place. Importantly, because cell phone bans increase the expected cost of using cell phones for drivers, one might expect these bans to alter driver behavior. Thus, the first paper asks whether cell phone bans reduce drivers' cell phone use. I use a difference-in-differences (DD) strategy to empirically address this question. Intuitively, I compare changes in drivers' cell phone use in states that have adopted cell phone bans from before and after cell phone ban adoption, relative to similar changes in non-adopting states. The corresponding identifying assumption requires adopting and non-adopting states would have followed similar trends in drivers' cell phone use in the absence of cell phone bans. Since credible estimates rely on the validity of this assumption, I empirically evaluate this assumption in different ways. First, I find there were similar trends in cell phone use before cell phone bans were adopted. Second, I show other factors of drivers' cell phone use do not

change in a way that is correlated with cell phone ban adoption. Finally, I perform a falsification test and find cell phone bans do not affect one outcome they are not supposed to affect: drivers' seat belt use. Collectively, evidence suggests the identification assumption is plausible. DD estimates indicate that cell phone bans significantly reduce drivers' cell phone use, which is the behavior cell phone bans target. These results are also robust to various robustness checks such as controlling for common regional shocks and allowing for state-specific linear time trends. Exercises looking into differential effects of cell phone bans provide further supporting evidence of the effectiveness of these bans: I find cell phone bans have more behavioral effect on subgroups (e.g., drivers with passengers and old drivers) which are expected to be mostly affected by these bans.

The second policy I examine is the U.S. castle doctrine laws. From 2000 to 2010, more than 20 states passed castle doctrine and stand-your-ground laws. These laws expand the legal justification for the use of lethal force in self-defense, thereby lowering the expected cost of using lethal force and increasing the expected cost of committing violent crime. Using a similar DD strategy to exploit within-state variation in law adoption, I find castle doctrine laws increase homicides by 8 percent but do not deter burglary, robbery, and aggravated assault. I also perform additional exercises to ensure DD estimates on homicides are based on correct statistical inference. Three different permutation tests using randomly assigned placebo castle doctrine laws indicate the significant estimate on homicides is not obtained by chance. In addition, I find castle doctrine laws also pass placebo tests since they do not affect crimes that are not supposed to be deterred by the laws, such as vehicle theft and larceny.

In the third paper, I estimate the effect of China's marriage and divorce registration reform on marital outcomes. Since this policy is enforced at the national level, unlike cell

phone bans and castle doctrine laws discussed in the first two papers, without proper control groups it is difficult to credibly exclude long-run effects of confounders. Therefore, I focus on the instantaneous effect using a regression discontinuity (RD) design. By doing so, I essentially compare annual changes in divorce and marriage rates just before and just after the policy adoption in 2003. The identification assumption requires changes in other determinants of divorce and marriage rates do not vary discontinuously in 2003. I provide graphical and regression evidence to show this assumption is reasonable. Results indicate that lower cost of divorce and marriage immediately triggered faster growth in divorce and marriage rates.

CHAPTER II

DO CELL PHONE BANS CHANGE DRIVER BEHAVIOR?

II.1 Introduction

In recent years, there has been increasing concern over distracted driving due to cell phone use. This stems from the substantial recent increase in cell phone usage while driving, as well as a body of research and official statistics suggesting that this behavior may lead to distraction and traffic accidents. One survey reports that over 60 percent of drivers regularly send text messages while driving, and that 66 percent of drivers report answering calls while driving (Tison, Chaudhary, and Cosgrove, 2011). A growing body of research including naturalistic studies and studies of simulated driving tasks indicates that cell phone usage does affect driver behavior by, for example, slowing drivers' reaction time or taking drivers' eyes away from the roadway more often.[1] While it is difficult to know for sure whether and how much driver cell phone use increases accidents and casualties, official estimates from the National Highway Traffic Safety Administration are that 995 people lost their lives in motor vehicle crashes in the United States in 2009 due to the use of cell phones while driving.

In response, states have started to pass cell phone bans—texting bans and handheld bans—that prohibit drivers from using cell phones behind the wheel. Texting bans prohibit drivers from sending or reading text messages on cell phones; handheld bans prohibit all drivers from engaging in phone calls, either talking or listening, on handheld cell phones when

[1] For example, see Housking, Young, and Regan (2009), Olson, Hanowski, Hickman and Bocanegra (2009), Just, Keller and Cynkar, (2008) and Olson, Hanowski, Hickman, and Bocanegra (2009).

operating motor vehicles. These bans impose significant penalties for violations, including fines (ranging from $20 to $500 in adopting states), license suspension, and even jail time. Therefore, by raising the expected cost of using cell phones while driving, one might expect the bans to reduce traffic accidents by reducing drivers' cell phone use. However, two recent studies show that cell phone bans have no meaningful effect on traffic accidents. Abouk and Adams (2013) focus on the effect of texting bans on single-vehicle-single-occupant accidents. They use monthly data from 2007 to 2010 and find that accidents are reduced only within a few months after the adoption of texting bans and then return to former levels. Bhargava and Pathania (2013) examine handheld bans instead. By analyzing data from 1989 to 2007, they show that banning drivers from talking on cell phones while driving has no effect on fatal crashes.[2]

These studies raise a question as to why cell phone bans do not reduce traffic accidents in general. Is it because cell phone bans are not effective at reducing driver cell phone usage? If so, it suggests that better policies or enforcement could still affect driver behavior in a meaningful way and thus subsequently reduce traffic accidents. On the other hand, if the bans do reduce observed driver cell phone usage significantly – which is the best outcome one could hope for given these bans – it suggests that other factors or driver responses may be responsible for the overall ineffectiveness of the bans in reducing accidents.

[2] In their main analysis, Bhargava and Pathania (2013) cleverly exploit the discontinuity in cellular plans that transit from "peak" to "off-peak" pricing at 9pm on weekdays from 2002 to 2005 in California. They find that although the call likelihood increases by 7.2% during 9-10pm from Mondays to Thursdays, this sharp local rise in call likelihood does not lead to more crashes.

While there have been several single-state studies that evaluate the impact of cell phone bans on driver behavior, to my knowledge this is the first multi-state study to address this question in a comprehensive fashion.[3] To do so, I apply a difference-in-differences (DD) strategy to two panel datasets from 2004-2010: individual-level observational survey data on drivers' visible cell phone usage from the National Occupant Protection Use Survey (NOPUS), and state-level data on traffic accidents and casualties from the Fatality Analysis Reporting System (FARS). Specifically, I exploit the within-state variation in the adoption of texting and handheld bans among the 23 adopting states. Intuitively, I compare the *relative* changes in outcome measures, including drivers' visible cell phone usage (handheld device manipulation and handheld cell phone usage), traffic accidents, and traffic casualties, between states that passed the cell phone bans (treatment states) and states that did not (control states), from before and after the adoption of cell phone bans. The identifying assumption is that states where drivers are prohibited from using cell phones would have followed similar trajectories in cell phone usage, accidents, and casualties to other non-adopting states, in the absence of the adoption of cell phone bans. To assess the validity of this assumption, I show that outcomes do not diverge between adopting and non-adopting states prior to the adoption of cell phone bans using graphical and regression analysis; I also do not find any evidence of historical divergence using data since 1975 by running rolling estimation. Finally, I also find evidence that the adoption of cell phone bans is as-good-as-random by showing that DD estimates are

[3] There is also a literature of single-state studies that evaluate the effectiveness of cell phone bans by using neighboring states as control groups. The majority of these studies focus on early adopters of handheld bans such as Connecticut (Cosgrove, Chaudhary and Roberts, 2010), New York (McCartt, Braver, and Geary, 2003; McCartt and Geary, 2004), and Washington DC (McCartt, Hellinga, and Geary, 2006; McCartt and Hellinga, 2007) and find that handheld bans do induce drivers to use cell phones less often. Relatedly, there is also larger literature focused on evaluating the impact of mandatory state laws on improving roadway safety (e.g., Cohen and Einav, 2003; Carpenter and Stehr, 2011).

robust to the inclusion of time-varying covariates that are used in previous studies, such as unemployment rate, median income, violent and property crime rates, and demographics (Abouk and Adams, 2013; Cohen and Einav, 2003; Carpenter and Stehr, 2011).

Results provide strong evidence that drivers reduce visible cell phone use when cell phone bans increase the expected cost of doing so. Specifically, cell phone bans significantly lower a driver's probability of talking and texting on a handheld cell phone while driving by 50 and 60 percent, respectively. These results are robust to various robustness checks such as allowing for region-year or region-year-quarter fixed effects to account for common regional shocks, including state-specific linear time trends to impose more flexible assumptions on unobservables, and using different definitions of cell phone bans. In addition, the results are robust to the inclusion of controls for the number of full-time equivalent police and government spending on both highway and public welfare. There is also no evidence that the bans affect seat belt usage, which suggests it is unlikely the results are driven by other concurrent policies aimed at improving driver safety. In addition, I am able to show that there is no reduction in traffic accidents even among the groups of drivers who reduce their cell phone use the most in response to the laws. One potential explanation for this seemingly contradictory finding is that the bans may cause more hidden cell phone use, which is likely more dangerous and could lead to an increase in accidents and casualties that offsets any reductions elsewhere. It is also possible that the use of cell phones does not necessarily result in more accidents and casualties

because drivers compensate for this distracted driving behavior by simply driving more carefully.[4]

These findings have significant welfare and policy implications. While current bans do alter driver behavior as intended – indeed, since police cannot observe hidden usage, the best the law can do is reduce observed usage – they do not have the intended effect on accidents and casualties. This suggests that these cell phone bans impose significant costs on drivers by distorting driver behavior, without generating measurable safety benefits to either those drivers or other drivers on the road. More importantly, the findings indicate that the ineffectiveness of the bans in reducing accidents is not merely a matter of enforcement. Rather, it suggests that a more complex set of factors is at play and need to be sorted out if policymakers are to succeed in reducing accidents and fatalities due to distracted driving.

II.2 Identification Strategy

I apply the difference-in-differences (DD) strategy to estimate the effect of state cell phone bans enacted between 2004 and 2010. Intuitively, I ask whether drivers in states that enact cell phone bans use cell phones less frequently and are involved in fewer accidents over time, relative to drivers in other states.

The annual state-level panel data model based on the individual observational survey of driver behavior is:

$$Outcome_{isy} = b_0 + b_1 Texting\ Ban_{sy} + b_2 Handheld\ Ban_{sy} + \mathrm{X}_{isy} g + c_s + u_y + e_{isy} \qquad \text{(II.1)}$$

[4] It is also possible that drivers switch to hands-free use as a result of the bans, which may be similarly distracting. I find little evidence of this, though I note that it is more difficult for the surveyors to observe hands-free usage.

where i indexes individuals, s indexes states, and y indexes years. The dependent variable $Outcome_{isy}$ is the dummy variable which equals one if the individual driver is observed using or manipulating a cell phone and otherwise equals 0. $Texting\ Ban_{sy}$ is an indicator variable that equals one if the texting ban is effective in state s in year y.[5] $Handheld\ Ban_{sy}$ is similarly defined. Individual-level control variables in X_{isy} include observed race and median age, as well as a set of indicators that correspond to weekday non-rush hour, weekend, rural and urban areas, and weather condition. c_s and u_y are state- and year- fixed effects. e_{isy} is the idiosyncratic term. The parameters of interest are b_1 and b_2, which measure the average effect of texting and handheld bans, respectively. Due to potential error correlations within states, standard errors are clustered at the state level (Bertrand, Duflo and Mullainathan, 2004).

The quarterly state-level panel data model of accidents and casualties is:

$$Outcome_{syq} = b_0 + b_1 Texting\ Ban_{syq} + b_2 Handheld\ Ban_{syq} + X_{syq} g + \Pi_{sy}\, l + c_s + u_{yq} + e_{syq} \quad \text{(II.2)}$$

where q indexes quarters. $Outcome_{syq}$ is the natural log of accident/casualty rate (count per 100 million vehicles miles travelled (VMT)). Formally, accident is defined as the number of vehicles involved in collisions, and casualty is defined as the sum of incapacitating injuries and fatalities in accidents. $Texting\ Ban_{syq}$ is an indicator variable that equals one if the texting ban is effective in state s in quarter q of year y. $Handheld\ Ban_{syq}$ is similarly defined. X_{syq} is a vector of quarterly time-varying covariates, including unemployment rate and median income.

[5] Since the survey is conducted in June each year, in the year when the texting ban is adopted, $Texting\ Ban_{sy}$ equals one if the texting ban is adopted before June and equals zero otherwise.

Π_{sy} is a vector of annual time-varying covariates, containing demographics, primary seat belt law dummy, violent and property crime rates, government spending on highway and public welfare, police and population. u_{yq} is year- fixed effects. e_{syq} is the idiosyncratic term.

The crucial identifying assumption for the DD strategy used here is that states that adopted cell phone bans and other non-adopting states would have *trended* similarly in outcomes, including cell phone usage, accidents, and casualties, in the absence of the adoption of cell phone bans. Therefore, if this assumption holds, differences in things like the rate of accidents between adopting and non-adopting states do not pose a threat to identification.[6] To provide evidence of the validity of this identifying assumption, I perform several tests. The first is to examine if there is any graphical evidence of divergence in outcomes before the adoption of cell phone bans, as well as formally test for divergence in the regression analysis by including leading indicators of both cell phone ban indicators. The second is to compare estimates of *Texting Ban* and *Handheld Ban* with and without controlling for time-varying covariates. If these two sets of estimates differ significantly, it indicates the time-varying determinants of outcomes varied systematically over time across treatment and control groups, which casts doubt on the identifying assumption. More specifically, this would cause me to worry that unobserved time-varying determinants, variables that I cannot directly control for, could vary in a similar way and potentially bias the DD estimates. In addition, to ensure that regional common shocks do not drive the results, I include region-year or region-year-quarter fixed effects, and therefore identify effects by comparing changes in outcomes between states

[6] In addition, the policy evaluation literature (e.g., Friedberg, 1998) has argued that even though *whether* a state adopted a law is correlated with initial levels of outcome measures that the law aims at, *when* such law is adopted is not.

that adopted cell phone bans and other non-adopting states within the same Census region of the country.

Finally, I will perform two more tests to provide more evidence that the estimated effects of cell phone bans are not confounded with other factors. First, I will show that VMT (vehicle miles travelled) is not affected by cell phone bans, indicating that cell phone bans indeed do not affect the overall level of driving. This suggests that any change in accidents and casualties will not be caused by changes in traffic congestion. Second, I will show that cell phone bans have no effect on drivers' safe-driving behavior, measured by use of safety seat belts, which suggests that the estimated effects of cell phone bans are not likely to be confounded by the effects of other concurrent policies that aimed at improving driving safety.

II.3 Data

II.3.1 Texting and Handheld Bans

In this paper, cell phone bans refer to texting and handheld bans that apply to all drivers in all locations.[7,8] Also, I focus on texting and handheld bans with primary enforcement and do not consider cell phone bans with secondary enforcement in the main analysis.[9] Primary

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[7] Due to their limited coverage, other cell phone bans that are only applicable to specific drivers and locations are not considered to be handheld or texting bans discussed in this paper. Some state bans only target inexperienced drivers who are either below the statutory age (usually 18 years old) or provisional license holders. For example, Missouri has a texting ban that only applies to drivers who are 21 years old and younger; Kansas's handheld ban is only applicable to learner's permit and intermediate license holders. In some other states the bans are only effective in specific areas. Illinois, for instance, restricts all drivers from talking on handheld devices in construction and school speed zones. Also, several states prohibit bus drivers from texting when a passenger of 17 years old or younger is present, such as Texas.

[8] Both bans do allow for the possibility of using cell phones for interactive communications in emergencies. Also, persons who perform their official duties such as certified law enforcement officers, firefighters, ambulance drivers are also exempt from the requirement of the cell phone bans.

[9] I also look at the effect when including secondary cell phone bans. Results are not statistically different from those reported in Section 4.

enforcement in the context of cell phone bans means that law enforcement officials can stop drivers who are observed violating cell phone bans by either making phone calls or texting while driving, without having other primary reasons such as running a red light or speeding. In contrast, under secondary enforcement drivers can only be pulled over for a primary offense first.[10] Therefore, *ex ante* it makes sense to focus on the primary cell phone bans that substantially increase the cost of using cell phones while driving due to the strong enforcement. In fact, most adopting states adopted primary cell phone bans. Up to 2010, only Nebraska, New York and Virginia adopted secondary texting bans; Maryland is the only state that has secondary handheld ban. During the examination period, two states also updated the enforcement level of their cell phone bans from secondary to primary: New Jersey upgraded its secondary handheld ban in 2008 and Washington upgraded its secondary texting ban in 2010.

To determine if and when a state passed either a texting ban or a handheld ban between 2004 and 2010, I found the text of the actual laws or bills and then cross-checked with other sources such as Abouk and Adams (2013), Bhargava and Pathania (forthcoming), National Conference of State Legislatures (NCSL), the Insurance Institute for Highway Safety (IIHS), and Ibrahim, Anderson, Burris and Wagenaar (2011).[11] Table II.1 lists the 23 states that have passed primary cell phone bans as of 2010 along with the corresponding effective dates.[12] New

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[10] For example, in a state where cell phone ban has secondary enforcement and seat belt law has primary enforcement, a driver could be pulled over for not wearing seat belt and then get citations for not wearing seat belt and using cell phones. But the driver cannot be stopped for just using cell phones without any other primary enforcement offense.

[11] I thank Abouk and Adams (2013) for helpful conversations that corrected the coding of cell phone bans in some states in the earlier version of this paper.

[12] The effective dates are the dates when the full enforcement begins rather than the initial announcement dates. The reason to choose the effective dates based on whether the enforcement starts is consistent with the purpose of this paper: to evaluate if drivers respond to the incentive change in using cellphones while driving, which only makes sense when there is full

York was the first state to pass a handheld ban in 2001 and New Jersey became the first to pass a texting ban in 2008.[13] For the five states that passed both cell phone bans, all of them adopted handheld bans no later than they adopted texting bans. Therefore, it is difficult to distinguish between the effect of handheld bans on their own and the effect of handheld bans when texting bans are already in effect, which is important when interpreting the estimated effect of handheld bans.

Figure II.1 shows the geographic distribution of texting bans and handheld bans as of 2010: the 22 states with texting bans appear to be equally distributed across the U.S, and the 6 states with handheld bans are only located in the northeastern and western regions.

II.3.2 Outcome Measures

Empirical analysis in this paper relies on the outcome measures of state cell phone bans. To construct these variables, I use data from two distinct datasets. Descriptive statistics can be found in Table II.2.

The first set of outcome measures is the observed cell phone usage while driving by individual drivers, which essentially provides the first-stage evidence on the mechanism through which cell phone bans may affect accidents and casualties. The observed handheld cell phone usage contains two main categories. The first category is "handheld cell phone usage", which refers to the situation in which drivers hold cell phones to their ears while driving. The second is "visible handheld device manipulation". In this scenario, drivers are

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enforcement of cell phone bans. For example, Georgia's texting ban was supposed to become effective on July 1, 2010, but due to the ticketing issue the full enforcement began one month later.

[13] District of Columbia passed its both texting ban and handheld ban on July 1, 2004. But due to missing data, it is excluded from this analysis. Also, Washington was the first state to pass a texting ban with secondary enforcement on January 1, 2008.

Table II.1 State Texting and Handheld Bans with Primary Enforcement as of 2010

State	Effective Date	
	Texting Ban	Handheld Ban
Arkansas	10/01/2009	
California	01/01/2009	07/01/2008
Colorado	12/01/2009	
Connecticut	10/01/2010	10/01/2005
Georgia	07/01/2010	
Illinois	01/01/2010	
Louisiana	07/01/2008	
Maryland	10/01/2009	
Massachusetts	09/30/2010	
Michigan	07/01/2010	
Minnesota	08/01/2008	
New Hampshire	01/01/2010	
New Jersey	03/01/2008	03/01/2008*
New York		11/01/2001
North Carolina	12/01/2009	
Oregon	01/01/2010	01/01/2010
Rhode Island	11/10/2009	
Tennessee	07/01/2009	
Utah	05/12/2009	
Vermont	06/01/2010	
Washington	06/10/2010*	07/01/2008
Wisconsin	12/01/2010	
Wyoming	07/01/2010	

* States that have upgraded the enforcement type from secondary to primary

observed visibly text-messaging or manipulating other hand-held device while driving. The raw data come from the 2004-2010 National Occupant Protection Use Survey (NOPUS) and are kindly provided by the National Highway Traffic Safety Administration (NHTSA) for this

project.[14] NOPUS is the only nationwide probability-based observational survey of driver electronic device use in the United States. Unlike usual surveys that are done through interviews or questionnaires, NOPUS is based on the observations of sophisticated data collectors and is conducted at intersections controlled by stop signs or stoplights between 7 a.m. and 6 p.m. at different state observational sites each June. Observers collect information

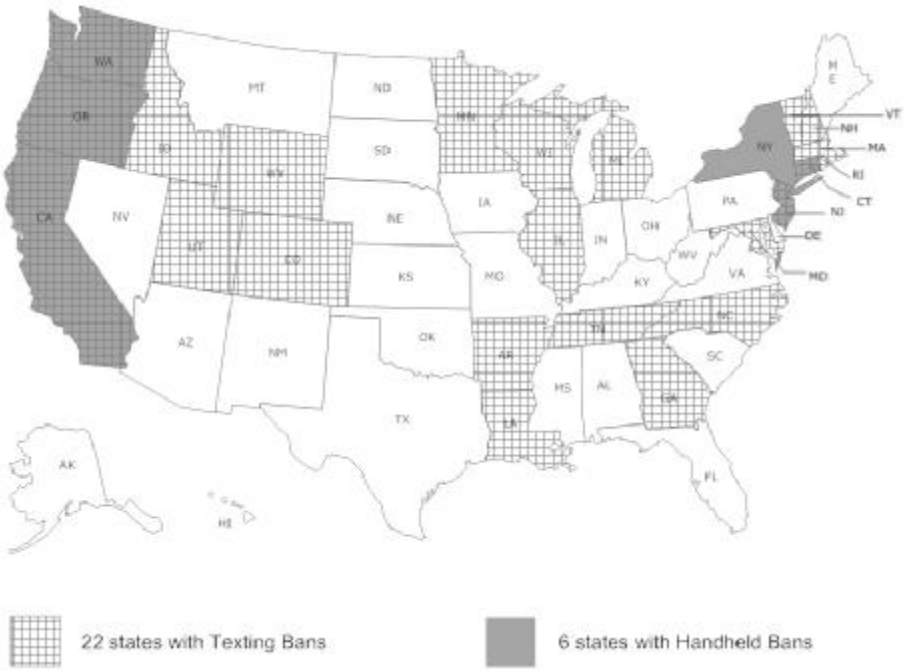


Figure II.1. State Texting and Handheld Bans with Primary Enforcement as of 2010

[14] NHTSA publishes the national estimates of driver electronic device use annually.

Table II.2 Descriptive Statistics

Panel A

Variable	Mean	Observations
Accident Rate (per 100 Million VMT)	2.01 (0.60)	1400
Casualty Rate (per 100 Million VMT)	1.81 (0.64)	1400
Fatality Rate (per 100 Million VMT)	1.39 (0.44)	1400
Injury Rate (per 100 Million VMT)	0.42 (0.25)	1400
Accidents	294 (313)	1400
Casualties	252 (255)	1400
Accident Rate by Drivers with at least One Passenger	0.81 (0.28)	1400
Accident Rate by Drivers without Any Passenger	1.21 (0.36)	1400
Accident Rate by Young Drivers	0.42 (0.15)	1400
Accident Rate by Adult Drivers	1.41 (0.44)	1400
Handheld Device Manipulation	0.0062 (0.08)	331883
Handheld Cellphone Usage	0.0541 (0.23)	331883
Headset Usage	0.0066 (0.08)	331883
Seat Belt Usage	0.84 (0.37)	331883
VMT (Millions)	14,741 (15136)	1400

Table II.2 Continued

Panel B

Variable	Mean	Observations
Texting Ban	0.08 (0.26)	1400
Handheld Ban	0.06 (0.24)	1400
Unemployment Rate (%)	5.88 (2.28)	1400
Median Income ($)	228,613 (268641)	1400
Primary Seat Belt Law	0.50 (0.50)	1400
Population	6,013,147 (6608285)	350
Police	19,916 (23805)	350
Government Spending on Highway	2,332,007 (2309445)	350
Government Spending on Public Welfare	8,428,193 (10800000)	350
Male Aged 15-24 (%)	7.23 (0.54)	350
Male Aged 25-44 (%)	13.66 (0.79)	350
Violent Crime Rate	400 (169)	350
Property Crime Rate	3164 (745)	350

Table II.2 Continued

Panel B Continued

Variable	Mean	Observations
Age	45 (12)	331883
White	0.79 (0.41)	331883
Black	0.11 (0.31)	331883
Male	0.58 (0.49)	331883
Weekend	0.23 (0.42)	331883
Weekday Non-Rush Hour	0.43 (0.49)	331883
Urban Area	0.20 (0.40)	331883
Rural Area	0.21 (0.41)	331883
Clear Weather	0.90 (0.31)	331883

Notes: Each cell contains the mean with the standard deviation in the parentheses. Quarterly state-level variables have 1400 observations and annual state-level variables have 350 observations.Variables from the individual observational survey have 331883 observations.

on multiple characteristics of drivers and observational sites, such as age range and weather condition. The survey covers around 30 states each year, though the composition of states covered varies slightly over time.[15] This dataset also includes data on individual use of headsets and seat belts, which can be used as further tests.

The second set of outcomes is the number of roadway accidents and casualties. They are aggregated to the state and quarterly levels using data from the Fatality Analysis Reporting System (FARS). FARS is a nationwide census provided by NHTSA, containing accidents and casualties information at the individual, vehicle, and the crash levels. Formally, accidents are measured as the number of motor vehicles involved in crashes; casualties are defined to be the sum of corresponding "incapacitating injuries" and "fatalities".[16] When I estimate rate models based on Model (2), the rate of accident and casualty is normalized using vehicle miles travelled (VMT). VMT data are from the Federal Highway Administration (FHA).

II.3.3. Time-varying Control Variables

I have also obtained data on determinants of the outcome measures to serve as controls. To estimate the effect of cell phone bans on cell phone usage, I utilize the NOPUS data which contain characteristics of individual drivers (median age groups, race, and gender) and observational sites (whether the observation time is weekend or weekday rush hour, whether the site is urban or rural, and whether the weather is clear or not). To gauge the effect of cell phone bans on accidents and casualties, I have also collected quarterly and annual data on a set

[15] On average, 80% of the treatment states with handheld bans and 60% of the treatment states with texting bans are included each year.
[16] I exclude other minor or unknown injuries such as "possible injuries" and "non-incapacitating evident injuries".

of control variables. Quarterly variables include unemployment rate, median income, and primary seat belt law, with data collected from Bureau of Labor Statistics, the Census Bureau, and IIHS, respectively. Annual population and demographic (proportion of male in the 15-24 and 25-44 age groups) data are also obtained from the Census Bureau; annual violent crime and property crime data are from Federal Bureau of Investigation's Uniform Crime Reports (UCR). Importantly, I also include the number of full-time equivalent police from UCR to capture the effect of the enforcement of cell phone bans. In addition, I obtain data on government spending on highway infrastructure and public welfare (US Census Bureau) to distinguish the effect of cell phone bans from the effect of other policies that could have been implemented simultaneously.

II.3.4 Sample Period

The sample period used in this paper is the 2004-2010 period. I choose the year 2004 as the starting point because the quarterly state-level VMT data are only available starting from 2004, which are used in Model (2) to normalize the count of accidents and casualties.[17] This examination period covers all states that have passed texting bans and only leaves out New York's handheld ban, which was passed in 2001.[18]

[17] The 2004 data on "visible handheld device manipulation" are not available.
[18] As a check, I include New York's 2001 handheld ban by using the 2000-2010 sample and estimate the count model. I find that it does not drive the estimated effects of cell phone bans on accidents and casualties. .

II.4 Results

II.4.1 Do Cell Phone Bans Reduce Drivers' Cell Phone Usage?

In this section, I examine whether cell phone bans make drivers use cell phones less often. Regression results using the NOPUS data from Model (II.1) are reported in Table II.3. Panel A presents the effects of cell phone bans on the behavior of handheld cell phone usage while driving. In Column 1, in which state and year fixed effects are controlled for, estimates of *Texting Ban* are insignificant and close to 0, indicating that texting bans have no effect. In contrast, handheld bans reduce the drivers' probability of talking on handheld cell phones by 2.8 percentage points, which is significant at the 1% level. This represents a 50% drop from the rate of 5.6 percent in states adopting handheld bans before these bans are enacted. Column 2 is the preferred specification, in which I additionally control for time-varying covariates, including driver characteristics (e.g., gender, age, and race), observational site features (e.g., rural or urban areas) and weather condition. Estimates of *Texting Ban* and *Handheld Ban* remain almost unaffected after controlling for time-varying controls compared to estimates in Column 1. This indicates that the within-state variation in cell phone bans is orthogonal to known determinants of handheld device manipulation, which is consistent with the idea that the within-state variation in bans is as-good-as-random. It also gives me some reason to believe that the within-state variation would also be orthogonal to unobserved determinants (Altonji, Elder, and Taber, 2005).

In Column 3 I include leading indicators of *Texting Ban* and *Handheld Ban* to directly test if cell phone usage trends diverge one year before bans are enacted. The two insignificant leading indicator estimates show no evidence of such divergence. In Panel A of Figure II.2, I also show the estimated difference in handheld cell phone usage while driving between

Table II.3 The Effect of Cell Phone Bans on Drivers' Cell Phone Usage

	1	2	3	4	5	6	7	8	9	10
	Unweighted OLS					Weighted OLS				
Panel A. Handheld Cell Phone Usage (Talking on Handheld Cellphones)										
	Handheld Cell Phone Usage					Handheld Cell Phone Usage				
Texting Ban	-0.0056	-0.0072	-0.0097	-0.0054	-0.0140	-0.0117	-0.0118	-0.0178*	-0.0105	-0.0110
	(0.0086)	(0.0081)	(0.0095)	(0.0085)	(0.0101)	(0.0106)	(0.0097)	(0.0102)	(0.0103)	(0.0106)
One Year Before Adoption of Texting Ban			-0.0070					-0.0198		
			(0.0112)					(0.0130)		
Handheld Ban	-0.0284***	-0.0284***	-0.0254***	-0.0333***	-0.0297***	-0.0178	-0.0205**	-0.0152	-0.0246**	-0.0305***
	(0.0080)	(0.0075)	(0.0086)	(0.0064)	(0.0087)	(0.0107)	(0.0096)	(0.0093)	(0.0096)	(0.0106)
One Year Before Adoption of Handheld Ban			0.0090					0.0222		
			(0.0110)					(0.0147)		
Observations	331883	331883	331883	331883	331883	331883	331883	331883	331883	331883
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Region-Year Fixed Effects				Yes					Yes	
State Linear Trends					Yes					Yes

Table II.3 Continued

	1	2	3	4	5	6	7	8	9	10
	Unweighted OLS					Weighted OLS				
Panel B. Handheld Device Manipulation (Sending Text Messages, etc.)										
	Handheld Device Manipulation					Handheld Device Manipulation				
Texting Ban	-0.0140***	-0.0142***	-0.0067***	-0.0135***	-0.0325**	-0.0101***	-0.0102***	-0.0064***	-0.0095***	-0.0189*
	(0.0036)	(0.0037)	(0.0018)	(0.0034)	(0.0138)	(0.0034)	(0.0034)	(0.0018)	(0.0032)	(0.0096)
One Year Before Adoption of Texting Ban			0.0198**					0.0128		
			(0.0078)					(0.0078)		
Handheld Ban	0.0124***	0.0123***	0.0107***	0.0129***	0.0187	0.0098***	0.0096***	0.0102***	0.0092**	0.0093
	(0.0037)	(0.0038)	(0.0025)	(0.0045)	(0.0131)	(0.0030)	(0.0031)	(0.0016)	(0.0040)	(0.0094)
One Year Before Adoption of Handheld Ban			-0.0071					-0.0016		
			(0.0070)					(0.0073)		
Observations	293566	293566	293566	293566	293566	293566	293566	293566	293566	293566
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Region-Year Fixed Effects				Yes					Yes	
State Linear Trends					Yes					Yes

Notes: The table reports panel data estimates using linear probility model. Each column in each panel represents a separate regression. The unit of observation is an individual. Weighted OLS uses state population to reweight the sample. Standard errors are clustered at the state level. Time-varying controls include indicators for gender, age group, race, rural and urban areas, weekday non-rush hour, weekend, and clear weather.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

adopting and non-adopting states over time from before and after the adoption of handheld bans. Specifically, this figure plots coefficients from a DD model in which I control for state and year fixed effects, time-varying covariates, existence of texting bans, and then allow for divergence each year starting from the 4th year prior to the adoption of handheld bans. Estimates are therefore relative to the average difference in the average use of handheld cell phones 5 or more years prior to the adoption of handheld bans. The figure suggests that (1) there is no evidence of divergence prior to adoption and that (2) the difference in handheld cell phone use experiences a structural drop right after the adoption of cell phone bans. As the difference decreases from the average of 0.009 before ban adoption to the average of -0.021 after adoption, the change in the difference is -0.029 which corresponds to the estimate of *Handheld Ban* (-0.0284) in Column 2.

In Column 4 I also include region-year fixed effects. The estimates are unchanged, which suggests that the effects are not confounded by regional common shocks. Finally, in Column 5 I include state-specific linear time trends to relax the assumption that state-level unobservable covariates are constant from 2004 to 2010 and find that results stay robust (Friedberg, 1998).

From Column 6 to Column 10, I re-estimate the effects of cell phone bans using weighted OLS in which state population is used to adjust the sampling weight because the chosen sample depends on observational sites that are not necessarily representative. Results are not statistically different from estimates in the first six columns.

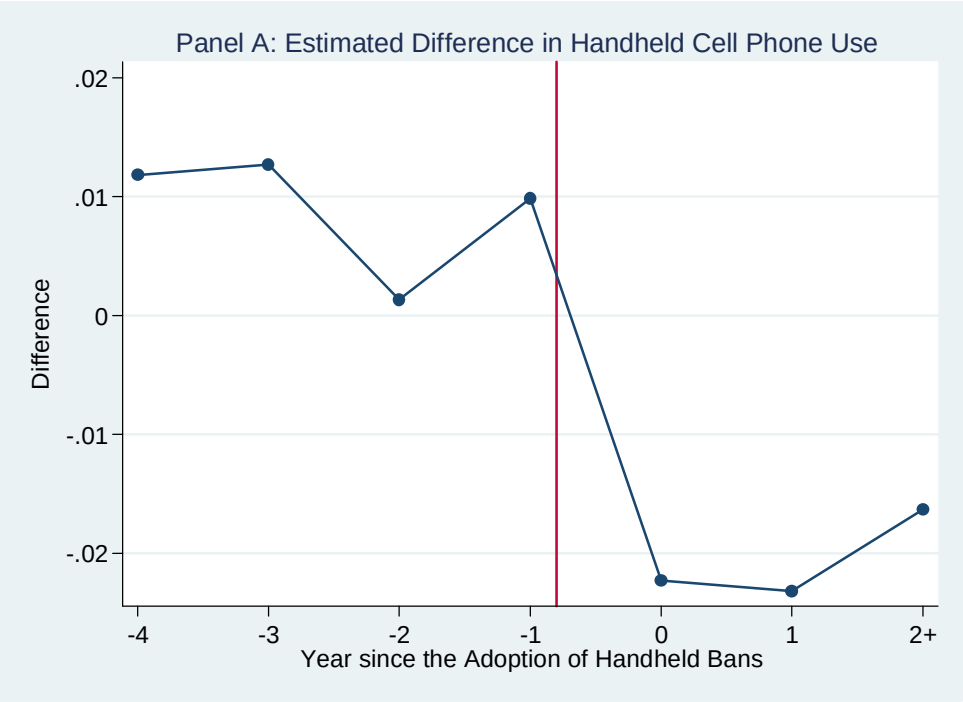
In panel B, I examine if cell phone bans have any effect on handheld device manipulation while driving, such as sending text messages. Results indicate that texting bans significantly reduce the probability of such behavior by about 0.6 to 1.9 percentage points,

which is about 40 to 70 percent drop compared to the manipulation rate before adopting states passed texting bans. These findings are also confirmed by Panel B of Figure II.2. Meanwhile, there appears to be a substitution from manipulating handheld devices towards using handheld cell phones caused by handheld bans as almost all the estimates of *Handheld Ban* are positive and significant at the 1% level.

Thus, both graphical and regression evidence suggest that texting bans and handheld bans are quite effective in reducing visible handheld device manipulation and handheld cell phone use, respectively, by increasing the expected cost of using a cell phone while driving. Moreover, handheld bans appear to induce drivers to further shift towards more texting. One interpretation consistent with this finding is that it is much easier to avoid detection by police while texting than to avoid detection when talking on a handheld phone, as one can always text by holding the cell phone in a concealed way. But a caveat in interpreting the effect of handheld bans is that, as discussed in Section 3.1, it is difficult to distinguish between whether handheld bans matter on their own and whether banning both texting and talking while driving is what really works. In addition, given the way in which NOPUS data are collected, there is always the open question as to whether the relative declines in usage measured here extend to other locations and times.

II.4.2 Do Cell Phone Bans Reduce Accidents and Casualties?

After establishing the mechanism through which cell phone bans could matter, I now turn to the quarterly state-level panel data to investigate the effect of cell phone bans on accidents and casualties. Estimates in Table II.4 indicate that neither of the bans appears to have meaningful effects on traffic accidents and casualties, as the majority of the estimates are



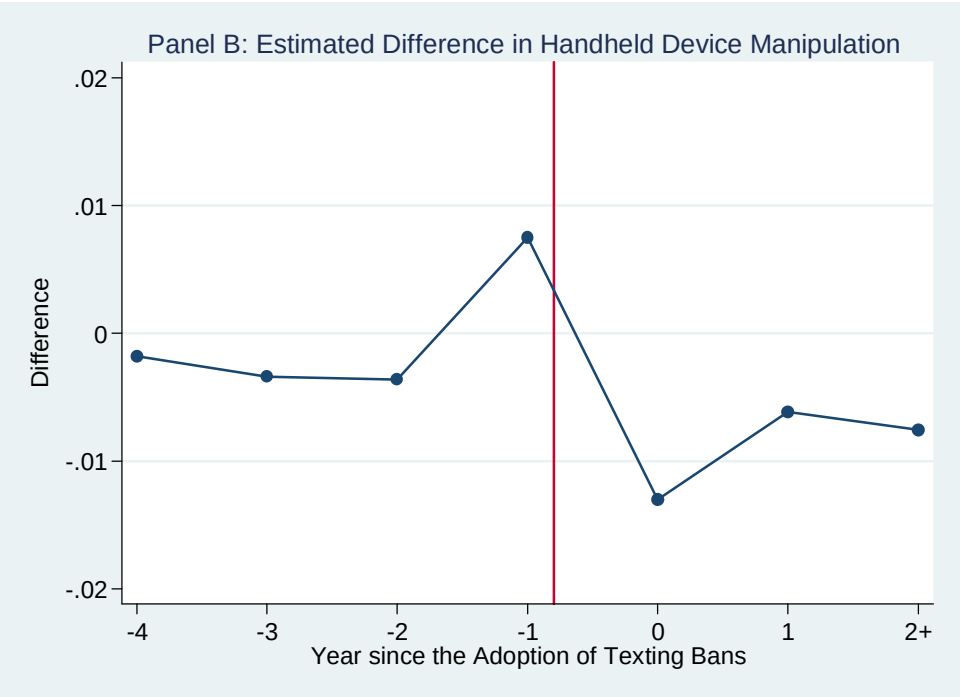


Figure II.2. Estimated Difference in Driver Cell Phone Use before and after the Adoption of Cell Phone Bans Between Adopting and Nonadopting States, Relative to the Difference 5 or More Years Before Adoption

statistically insignificant and close to zero.[19] I also look at incapacitating injuries and fatalities separately and find similar results. These findings are largely consistent with Abouk and Adams (2013) and Bhargava and Pathania (2013).[20]

Combined with the evidence from how drivers respond to texting bans and handheld bans, it seems quite interesting that cell phone bans have no effect on traffic accidents and casualties even though they reduce cell phone use while driving. There are two possible interpretations for this. One is that cell phone bans could reduce accidents and casualties by reducing some drivers' use of cell phones. But meanwhile they could also make drivers more likely to text or make phone calls in a concealed way. This type of behavior is likely more dangerous and would thus lead to more accidents and casualties, offsetting any reduction due to less overall cell phone use while driving. The other interpretation is that drivers compensate for distracted cell phone-related driving behavior by driving more cautiously in other ways, similar to the "Peltzman Effect" (Peltzman, 1975) in the setting of safety-belt use. Therefore, texting or talking on handheld cell phones does not necessarily lead to more accidents and casualties, because rational drivers simply drive more cautiously when doing so. Both of these interpretations are consistent with the finding that cell phone bans do not reduce accidents and casualties, even though they do reduce overall usage of cell phones by drivers.[21]

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[19] Only *Handheld Ban* estimates become significant in the specifications where linear trends are included.

[20] In addition, permutation tests in the spirit of Bertrand, Duflo, and Mullainathan (2004), Chetty, Looney, and Kroft (2009), Abadie, Diamond, and Hainmueller (2010) and Nunn and Qian (2011) further confirm the results of the null effect of cell phone bans on traffic accidents and casualties. Such tests are also called "refutability" tests in Angrist and Krueger (1999).

[21] Substituting other distracted driving behavior (e.g., talking to passengers, or fiddling with radios) with the use of cell phones could also explain why cell phone bans do not affect accidents and casualties for risk loving drivers (Hahn and Tetlock, 1999; Bhargava and Pathania, 2013).

Table II.4 The Effect of Cell Phone Bans on Traffic Accidents and Casualties

	1	2	3	4	5	6	7	8	9	10
	Unweighted OLS					Weighted OLS				
Panel A. Accident Rate										
	log(Accident Rate)					log(Accident Rate)				
Texting Ban	-0.0049	-0.0078	0.0117	-0.0047	0.0033	-0.0234	-0.0176	-0.0083	-0.0160	-0.0037
	(0.0225)	(0.0242)	(0.0276)	(0.0231)	(0.0289)	(0.0171)	(0.0174)	(0.0239)	(0.0176)	(0.0187)
One Year Before Adoption of Texting Ban			0.0480					0.0181		
			(0.0325)					(0.0218)		
Handheld Ban	-0.0213	-0.0038	-0.0119	0.0077	-0.0735**	-0.0373*	-0.0174	-0.0224	0.0174	-0.0415*
	(0.0338)	(0.0291)	(0.0273)	(0.0354)	(0.0338)	(0.0190)	(0.0189)	(0.0226)	(0.0330)	(0.0237)
One Year Before Adoption of Handheld Ban			-0.0072					-0.0047		
			(0.0300)					(0.0215)		
Observations	1400	1400	1400	1400	1400	1400	1400	1400	1400	1400
State and Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects				Yes					Yes	
State Linear Trends					Yes					Yes

Table II.4 Continued

	1	2	3	4	5	6	7	8	9	10
	Unweighted OLS					Weighted OLS				
Panel B. Casualty Rate										
	log(Casualty Rate)					log(Casualty Rate)				
Texting Ban	0.0081	0.0091	0.0349	0.0107	0.0162	-0.0163	-0.0077	-0.0008	-0.0039	0.0164
	(0.0263)	(0.0262)	(0.0303)	(0.0239)	(0.0323)	(0.0233)	(0.0217)	(0.0274)	(0.0201)	(0.0224)
One Year Before Adoption of Texting Ban			0.0639*					0.0136		
			(0.0328)					(0.0210)		
Handheld Ban	-0.0370	-0.0159	-0.0234	-0.0063	-0.0996**	-0.0614**	-0.0328	-0.0324	-0.0091	-0.0683***
	(0.0438)	(0.0359)	(0.0328)	(0.0430)	(0.0426)	(0.0237)	(0.0227)	(0.0239)	(0.0343)	(0.0253)
One Year Before Adoption of Handheld Ban			0.0000					0.0082		
			(0.0219)					(0.0182)		
Observations	1400	1400	1400	1400	1400	1400	1400	1400	1400	1400
State and Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects				Yes					Yes	
State Linear Trends					Yes					Yes

Notes: The table reports panel data estimates. Each column in each panel represents a separate regression. The unit of observation is state-year-quarter. Weighted OLS uses state population to reweight the sample. Standard errors are clustered at the state level. Time-varying controls include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

II.4.3 Differential Effects

To this point, I have shown the average effect of cell phone bans on drivers' cell phone usage, and subsequently on accidents and casualties. Here I investigate the differential effects of cell phone bans on different types of drivers and in different locations.

Two kinds of differential effects related to driver types are of particular interest. The first is that cell phone bans could affect a driver's behavior differentially depending on whether the driver is accompanied by passengers or not. For example, a driver may be able to substitute passengers' cell phone use for her own, thereby making it easier for her to comply with the ban. In short, raising the cost to using a cell phone while driving is likely to change behavior the most when there are good substitutes available, and drivers with passengers have more substitutes available. Similarly, risk-averse passengers who are aware of the cell phone bans are more likely to remind the drivers to comply with the bans. Thus, it is reasonable to expect that cellphone bans would have the biggest effect on drivers accompanied by passengers.

In Panel A of Table II.5, results show that drivers who are driving with passengers do respond more to cell phone bans than those who are driving alone. Since the estimates in Columns 1 and 3 based on Model (1) only represent absolute changes in probabilities of handheld cell phone use and handheld device manipulation, respectively, in Columns 2 and 4 I convert them to proportional changes relative to the corresponding probabilities prior to cell phone bans were adopted in order to better compare these estimates. The results imply that texting bans reduce manipulation of handheld devices about 1.5 times as much for drivers with passengers as for single drivers. There is a similarly disproportionate effect for handheld bans. One somewhat peculiar finding is that texting bans appear to induce drivers with passengers

to be much less likely to talk on handheld cell phones. This could be because passengers text or talk on their phones in lieu of the driver doing so when a ban is in effect.

Along similar lines, it is interesting to know if cell phone bans also have differential effects on young and adult drivers as well since these two kinds of drivers generally have different driving behavior. Results from Panel B suggest that cell phone bans only differentially alter the way young and adult drivers send text messages, not whether or not they make phone calls while driving. In particular, texting bans significantly reduce the probability of manipulating handheld devices among adult drivers by 68 percent but have no significant effect on young drivers. Also, handheld bans induce more adult drivers to shift from talking to texting while driving than young drivers.

Results in Panel C further show that there are no differential effects on accidents with respect to the presence of passengers and drivers' age, as in each column estimates are insignificant and are not significantly different from each other. This is striking, and suggests that even for those subgroups whose behavior is most affected by the ban, there is no evidence of a reduction in accidents or casualties. Again, this could be explained by more hidden use of cell phones or by compensating driving behavior, as discussed in Section 4.2.

Finally, one might be concerned that drivers only change their behavior in locations observed by NOPUS such as traffic-controlled intersections, but not in other locations. As a result, one might not expect to see reductions in overall accidents and casualties. It is also possible that drivers substitute usage *away* from intersections and *toward* freeways, which could also explain why accidents do not decline overall. Therefore in Table II.6, I examine the differential effects of cell phone bans on accidents in different locations. Results in Columns 1 and 2 show that cell phone bans have no significant effect on accidents in

intersection areas. Thus, even in locations where we know driver cell phone usage is reduced, there is still no evidence of a decline in accidents. In addition, Columns 1 through 6 show no evidence of displacement from traffic-controlled intersection areas to non-intersection areas, intersections without traffic controls, or arterial and local roads.[22]

II.4.4 Additional Tests and Robustness Checks

In this section I perform different kinds of tests and robustness checks. First, to understand whether cell phone bans could affect traffic accidents and casualties through channels other than reducing drivers' cell phone usage, I look at if the general driving behavior is affected. To do so, I focus on the effect of cell phone bans on vehicle miles travelled (VMT), which is a measure of the total vehicle mileage. Estimates in the first six columns of Table II.7 are all insignificant and close to zero, which suggests that it is unlikely that the effect of cell phone bans on accidents and casualties could be due to differences in traffic congestion during the examination period.[23]

Second, the NOPUS data allow me to examine the effect of cell phone bans on drivers' seat belt usage, which I use as a proxy for general safe-driving behavior. If the estimated effects of cell phone bans also pick up effects of other concurrent policies that improve driving safety, then I expect to see cell phone bans significantly increase the use of seat belts. However,

---

[22] FARS data imply that 28% of the accidents happen near intersections, and 63% of these accidents are in traffic-controlled intersections. In particular, traffic controls include highway traffic signals (e.g., flashing traffic control signal), regulatory signs (e.g., stop signs), school zone signs (e.g., school speed limit sign), warning signs, and others (e.g., crossing guard).

[23] Carpenter and Stehr (2011) finds that state youth bicycle helmet laws have unintended consequence of reducing youth bicycling.

Table II.5 Differential Effects of Cell Phone Bans by Driver Type

	1	2	3	4
Panel A. Differential Effects by the Presence of Passengers on Cell Phone Use				
	Handheld Cellphone Usage (Talking on Handheld Cellphones)	% Change compared to Rate of Handheld Cellphone Use prior to Adoption of Cellphone Bans	Handheld Device Manipulation (Sending Text Messages, etc.)	% Change compared to Rate of Handheld Device Manipulation prior to Adoption of Cellphone Bans
Texting Ban x Driver with at least One Passenger	-0.0205***	-50%	-0.0171***	-77%
	(0.0070)		(0.0040)	
Texting Ban x Driver without Passenger	-0.0013	-2%	-0.0129***	-52%
	(0.0099)		(0.0036)	
Handheld Ban x Driver with at least One Passenger	-0.0329***	-70%	0.0104**	347%
	(0.0066)		(0.0043)	
Handheld Ban x Driver without Passenger	-0.0256***	-42%	0.0133***	190%
	(0.0084)		(0.0037)	
Observations	331883	-	2935666	-
Panel B. Differential Effects by Driver Age on Cellphone Use				
	Handheld Cellphone Usage (Talking on Handheld Cellphones)	% Change compared to Rate of Handheld Cellphone Use prior to Adoption of Cellphone Bans	Handheld Device Manipulation (Sending Text Messages, etc.)	% Change compared to Rate of Handheld Device Manipulation prior to Adoption of Cellphone Bans
Texting Ban x Young Driver (16<Driver Age<24)	-0.0034	-5%	-0.0066	-23%
	(0.0100)		(0.0050)	
Texting Ban x Adult Driver (Driver Age>25)	-0.0073	-16%	-0.0152***	-68%
	(0.0078)		(0.0036)	
Handheld Ban x Young Driver (16<Driver Age<24)	-0.0463***	-52%	0.0119***	108%
	(0.0083)		(0.0037)	
Handheld Ban x Adult Driver (Driver Age>25)	-0.0254***	-50%	0.0120***	240%
	(0.0074)		(0.0038)	
Observations	331883	-	2935666	-
Panel C. Differential Effects on Accidents				
	log(Accident Rate) (Drivers with at least One Passenger)	log(Accident Rate) (Drivers without Passenger)	log(Accident Rate) (16<Driver Age<24)	log(Accident Rate) (Driver Age>25)
Texting Ban	0.0150	-0.0223	-0.0198	-0.0041
	(0.0288)	(0.0296)	(0.0393)	(0.0268)
Handheld Ban	-0.0012	-0.0028	0.0032	-0.0272
	(0.0326)	(0.0311)	(0.0288)	(0.0326)
Observations	1400	1400	1400	1400
State and Year/Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes

Notes: The table reports unweighted panel data estimates. Standard errors are clustered at the state level. Time-varying controls for Panels A and B include indicators for gender, age group, race, rural and urban areas, weekday non-rush hour, weekend, and clear weather. Time-varying controls for Panel C include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

I find zero effect as shown in Columns 6 through 10, which suggests this confounding story is not likely to be true.

Third, results in Columns 11 through 15 indicate that cell phone bans do not change driver behavior with the hands-free technologies by using the usage of headsets as a proxy. While observing hands-free technologies is perhaps somewhat difficult, this result does suggest that substitution over this time period is only between talking and texting on handheld phones.

Finally, I check if different definitions of cell phone bans change the results in Table II.8. In Panel A I drop two states (New Jersey and Washington) where cell phone bans were upgraded to the primary enforcement. In Panel B, to single out the effect of texting bans, I exclude the six states that passed handheld bans. In Panel C, I keep the same state and year combinations used in NOPUS since NOPUS data are not balanced. In Panel D, I redefine *Texting Ban* and *Handheld Ban* to be fractions of days during a quarter rather than binary variables in order to better capture the effects of cell phone bans in the quarter when they are enacted.,[24] Results indicate that the estimated effects are robust to these changes in definition.

II.5. Conclusion

Texting and handheld bans are the major response from state legislatures to the widely-perceived increase in distracted driving. However, recent studies show that these bans do not reduce traffic accidents over the medium- or long-term. This paper asks if this is because the

[24] For example, California adopted handheld ban on July 1st, 2008. Therefore, in terms of fractions of days with texting ban in 2008, the value of *Handheld Ban* for California in 2008 should be 0.5, compared to 1 using the binary definition.

Table II.6 Differential Effects of Cell Phone Bans by Location

	1	2	3	4	5	6
	log(Accident Rate) (Accidents in Intersections)	log(Accident Rate) (Accidents not in Intersections)	log(Accident Rate) (Accidents in Intersections with Traffic Controls)	log(Accident Rate) (Accidents in Intersections without Traffic Controls)	log(Accident Rate) (Accidents in Arterial Roads)	log(Accident Rate) (Accidents in Local Roads)
Texting Ban	0.0224	-0.0174	0.0690	0.0070	-0.0113	-0.0244
	(0.0511)	(0.0250)	(0.0771)	(0.0837)	(0.0350)	(0.0930)
Handheld Ban	0.0749	-0.0302	0.1605*	-0.0506	0.0439	-0.1907
	(0.0456)	(0.0283)	(0.0813)	(0.1283)	(0.0434)	(0.2188)
Observations	1400	1400	1400	1400	1400	1400
State and Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports unweighted panel data estimates. Standard errors are clustered at the state level. Time-varying controls include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table II.7 Additional Tests: The Effect of Cell Phone Bans on VMT, Seat Belt Usage and Headset Usage

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
	log(VMT)					Seat Belt Use					Headset Use				
Texting Ban	-0.0099	-0.0065	-0.0075	-0.0044	-0.0054	0.0102	0.0133	0.0215	0.0168	0.0011	0.0032	0.0030	0.0040	0.0020	0.0017
	(0.0083)	(0.0066)	(0.0087)	(0.0063)	(0.0084)	(0.0138)	(0.0135)	(0.0156)	(0.0132)	(0.0220)	(0.0035)	(0.0035)	(0.0038)	(0.0038)	(0.0052)
One Year Before Adoption of Texting Ban			-0.0027					0.0252					0.0043		
			(0.0061)					(0.0160)					(0.0026)		
Handheld Ban	-0.0035	0.0035	0.0006	0.0044	-0.0046	-0.0089	-0.0114	-0.0126	-0.0078	-0.0179	0.0003	0.0005	0.0036	0.0021	-0.0059
	(0.0110)	(0.0095)	(0.0135)	(0.0100)	(0.0086)	(0.0189)	(0.0181)	(0.0196)	(0.0202)	pre	(0.0032)	(0.0033)	(0.0040)	(0.0026)	(0.0060)
One Year Before Adoption of Handheld Ban			-0.0098					-0.0086					0.0066		
			(0.0128)					(0.0174)					(0.0043)		
Observations	1400	1400	1400	1400	1400	331883	331883	331883	331883	331883	331883	331883	331883	331883	331883
State and Year/Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Region-Year/Region-Year-Quarter Fixed Effects				Yes					Yes					Yes	
State Linear Trends					Yes					Yes					Yes

Notes: The table reports unweighted panel data estimates. Standard errors are clustered at the state level. Time-varying controls include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table II.8 Additional Robustness Checks

	1	2	3	4
	Handheld Cellphone Usage	Handheld Device Manipulation	Log (Accident Rate)	Log (Casualty Rate)
Panel A. Excluding States with Cellphone Bans Enforcement Upgrade				
Texting Ban	-0.0072	-0.0142***	-0.0058	0.0130
	(0.0081)	(0.0036)	(0.0252)	(0.0270)
Handheld Ban	-0.0290***	0.0121***	0.0256	0.0017
	(0.0074)	(0.0040)	(0.0385)	(0.0559)
Observations	314133	277208	1344	1344
Panel B. Excluding States with Handheld Bans				
Texting Ban	-0.0051	-0.0152***	0.0056	0.0281
	(0.0091)	(0.0040)	(0.0269)	(0.0292)
Observations	242025	211844	1232	1232
Panel C. Keeping State/Year Combinations Used in NOPUS				
Texting Ban	-	-	-0.0279	-0.0049
	-	-	(0.0238)	(0.0330)
Handheld Ban	-	-	0.0241	0.0197
	-	-	(0.0406)	(0.0495)
Observations	-	-	668	668
Panel D. Defining Cellphone Ban Dummies as Fractions				
Texting Ban	-	-	-0.0099	0.0065
	-	-	(0.0249)	(0.0270)
Handheld Ban	-	-	-0.0002	-0.0148
	-	-	(0.0290)	(0.0360)
Observations	-	-	1400	1400
State and Year/Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes

Notes: The table reports unweighted panel data estimates. Each column in each panel represents a separate regression. Standard errors are clustered at the state level. Time-varying controls for Column 1 through Column 3 include indicators for gender, age group, race, rural and urban areas, weekday non-rush hour, weekend, and clear weather. Time-varying controls for Column 4 through Column 5 include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

laws fail to change driver behavior, which might be the case if the bans were not enforced. In contrast, results indicate driver behavior is very responsive: texting bans reduce visible texting while driving by around 60% and handheld bans reduce the probability of talking on handheld cell phones while driving by around 50%. In addition, cell phone bans seem to have a larger effect on adult drivers and drivers accompanied by passengers. However, these apparent changes in behavior do not lead to meaningful reductions in accidents or casualties, which is consistent with existing literature. This is true even for subgroups who reduce cell phone usage while driving the most.

This apparent puzzle can be resolved by several different explanations. One is that handheld cell phone use while driving is less dangerous than commonly believed. Similarly, it is possible that the risk of handheld use is offset by driving more carefully while using handheld phones. Alternatively, the bans may induce more hidden and likely dangerous use of cell phones while driving. While it is difficult for me to shed light on which of these potential interpretations is driving the results, it is clear that while current bans are effective in changing drivers' behavior, they do not achieve the ultimate policy goal. This suggests that improving the effectiveness of the laws in reducing accidents is considerably more complex than merely improving enforcement. Perhaps more importantly, the results also have important social welfare implications in that the cell phone bans impose significant social costs on drivers, without yielding the intended social benefits.

CHAPTER III

DOES STRENGTHENING SELF-DEFENSE LAW DETER CRIME OR ESCALATE VIOLENCE? EVIDENCE FROM CASTLE DOCTRINE*

III.1 Introduction

A long-standing principle of English common law, from which most U.S. self-defense law is derived, is that one has a "duty to retreat" before using lethal force against an assailant. The exception to this principle is when one is threatened by an intruder in one's own home, as the home is one's "castle". In 2005, Florida became the first in a recent wave of states to extend castle doctrine to places outside the home, and to expand self-defense protections in other ways. Since then, more than 20 states have followed in strengthening their self-defense laws by passing versions of "castle doctrine" or "stand-your-ground" laws. While the specific components vary across states, the laws eliminate the duty to retreat from a list of specified places, and often remove civil liability for those acting under the law and establish a presumption of reasonable fear for the individual claiming self-defense. For ease of exposition, we subsequently refer to these laws as castle doctrine laws.

These laws alter incentives in two important ways. First, the laws reduce the expected cost of using lethal force. Castle doctrine lowers the expected legal costs associated with defending oneself against criminal and civil prosecution, as well as the probability that one is ultimately found criminally or civilly liable for the death or injury inflicted. In addition, the

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laws increase the expected cost of committing violent crime, as victims are more likely to respond by using lethal force. The purpose of our paper is to examine empirically whether people respond to these incentives, and thus whether the laws lead to an increase in homicide, or to deterrence of crime more generally.

In doing so, our paper also informs a vigorous policy debate over these laws. Proponents argue these statutes provide law-abiding citizens with additional necessary protections from civil and criminal liability. They argue that since the decision to use lethal force is a split-second one that is made under significant stress, the threatened individual should be given additional legal leeway. Critics argue that existing self-defense law is sufficient to protect law-abiding citizens, and extending castle doctrine will unnecessarily escalate violence. These potential consequences have been of particular interest recently following some highly publicized cases.[25] In examining the empirical consequences of these laws, this study informs the debate over their costs and benefits.

We use state-level crime data from 2000 to 2010 from the FBI Uniform Crime Reports to empirically analyze the effects of castle doctrine on two types of outcomes. First, we examine whether these laws deter crimes such as burglary, robbery, and aggravated assault. In doing so, we join a much larger literature on criminal deterrence generally (e.g., Becker, 1968; Ehrlich, 1973; Di Tella and Schargrodsky, 2004; Donohue and Wolfers, 2009). More specifically, however, we join a smaller literature focused on whether unobserved victim precaution can deter crime. For example, Ayres and Levitt (1998) examine whether LoJack

[25] The most publicized case is that of Trayvon Martin, an unarmed teenager who was shot and killed by a neighborhood watch volunteer (Alvarez, 2012).

reduces overall motor vehicle thefts, while others have examined whether laws that make it easier to carry concealed weapons deter crime (Bronars and Lott, 1998; Dezhbakhsh and Rubin, 1998; Lott and Mustard, 1997; Ludwig, 1998).[26]

We then examine whether lowering the expected cost of using lethal force results in additional homicides, defined as the sum of murder and non-negligent manslaughter. We also examine the effects of the laws on other outcomes in order to shed light on *why* homicides are affected by the laws.

To distinguish the effect of the laws from confounding factors, we exploit the within-state variation in the adoption of laws to apply a difference-in-differences identification strategy. Intuitively, we compare the within-state *changes* in outcomes of states that adopted laws to the within-state *changes* in non-adopting states over the same time period. Moreover, we primarily identify effects by comparing changes in castle doctrine states to other states in the same region of the country by including region-by-year fixed effects. Thus, the crucial identifying assumption is that in the absence of the castle doctrine laws, adopting states would have experienced changes in crime similar to non-adopting states in the same region of the country.

Our data allow us to test and relax this assumption in several ways. First, graphical evidence and regression results show that the outcomes of the two groups did not diverge in the years prior to adoption. In addition, we show that our findings are robust to the inclusion

[26] Our view is that relative to shall-issue concealed carry laws, the potential for castle doctrine law to deter crimes is quite large. For example, in Texas only 1.5 percent of adults age 18 and older have a concealed carry permit, and presumably only a fraction of those carry a gun on a regular basis (Texas Department of Public Safety, 2006; Texas Department of State Health Services, 2006; and authors' calculations). In contrast, Gallup polls indicate that from 2000 to 2009, 44 percent of households own a gun that could be used in self-defense against a burglar or assailant (Saad, 2011). Moreover, strengthened self-defense laws lower the cost of using a concealed carry weapon.

of time-varying covariates such as demographics, policing, economic conditions, and public assistance, as well as to the inclusion of contemporaneous crime levels unaffected by castle doctrine laws that proxy for general crime trends. This suggests that other known determinants of crime rates were orthogonal to the within-state variation in castle doctrine laws. Along similar lines, we offer placebo tests by showing that castle doctrine laws do not affect crimes that ought not be deterred by the laws, such as vehicle theft and larceny. Failing to find effects provides further evidence that general crime trends were similar in adopting and non-adopting states. Finally, we allow for state-specific linear time trends.

Results indicate that the prospect of facing additional self-defense does not deter crime. Specifically, we find no evidence of deterrence effects on burglary, robbery, or aggravated assault. Moreover, our estimates are sufficiently precise as to rule out meaningful deterrence effects.

In contrast, we find significant evidence that the laws lead to more homicides. Estimates indicate that the laws increase homicides by a statistically significant 8 percent, which translates into an additional 600 homicides per year across states that adopted castle doctrine. The magnitude of this finding is similar to that reported in a recent paper by McClellan and Tekin (2012), who examine these laws' effect on firearm-related homicide

using death certificate data from Vital Statistics.[27,28] We further show that this divergence in homicide rates at the time of castle doctrine enactment is larger than any divergence between the same groups of states at any time in the last 40 years, and that magnitudes of this size arise rarely by chance when randomly assigning placebo laws in similarly-structured data sets covering the years prior to castle doctrine. In short, we find compelling evidence that by lowering the expected costs associated with using lethal force, castle doctrine laws induce more of it.

Finally, we perform several exercises to examine the possibility that the additional reported criminal homicides induced by the laws were in fact legally justified, but were misreported by police to the FBI. We conclude on the basis of these findings that it is unlikely, albeit not impossible, that all of the additional homicides were legally justified but were misreported by police as murder or non-negligent manslaughter.[29]

Collectively, these findings suggest that incentives do matter in one important sense: lowering the threshold for the justified use of lethal force results in more of it. On the other hand, there is also a limit to the power of incentives, as criminals are apparently not deterred when victims are empowered to use lethal force to protect themselves.

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[27] One advantage of using FBI UCR data is that it allows us to assess both how the laws affect the use of lethal force and whether they deter violent crime. In addition, the nature of the UCR data enables us to measure all homicides, rather than just those caused by firearms. The data also allow us to examine homicide subclassifications and relative changes in reported justifiable homicide from the SHR, along with assumptions about the degree of underreporting, to address the issue of whether the additional homicides are legally justified. The primary disadvantage of the UCR homicide data is that while the annual state-level data we use are regarded as accurate and there is no reason to believe that any total homicide reporting issue at any level should be systematically correlated with changes in castle doctrine law, the monthly data from Vital Statistics are more complete. However, we obtain nearly identical estimates to those reported when we exclude observations in the year in which the state adopted the law, indicating that this is not a problem.

[28] Our findings contrast with those of Lott (2010), who reports that castle doctrine laws adopted from 1977 through 2005 reduced murder rates and violent crime.

[29] Of course, there is also the issue of whether all legally justified homicides under castle doctrine are socially desirable, which is beyond the scope of this paper.

These findings also have significant policy implications. The first is that these laws do not appear to offer any hidden spillover benefits to society at large in the form of deterrence. On the other hand, the primary potential downside of the law is the increased number of homicides. Thus, our view is that any evaluation of these laws ought to weigh the benefits of increased leeway and protections given to victims of actual violent crime against the net increase in loss of life induced by the laws.

III.2 Castle Doctrine Law and Identification

III.2.1 Castle Doctrine Law

U.S. self-defense law, which stems from English common law, has long favored the principle of "retreat to the wall", which means that only after no longer being able to retreat safely could one respond to an attacker with deadly force (Vilos and Vilos, 2010). The exception to this rule is if the attack is inside one's home, or "castle", in which case there is no longer a duty to retreat. In 2005, a wave of states began passing laws that put this "castle doctrine" into state statute. More importantly, these laws also removed the duty to retreat from places outside the home, and strengthened self-defense laws in other ways. For example, most laws added language that explicitly states individuals are justified in using deadly force in certain circumstances when they reasonably believe that they face a serious risk of imminent death or serious bodily harm. In addition, castle doctrine laws removed the duty to retreat in a list of special places such as one's vehicle, place of work and, in some cases, any place one has a legal right to be. Many of these laws also added a presumption of reasonable fear of imminent serious injury or death, which shifts the burden of proof to the prosecutor to show

someone acted unreasonably.[30] Similarly, many laws also grant immunity from civil liability when using defensive force in a way justified under law. Collectively, these laws lower the cost of using lethal force to protect oneself, though they also lower the cost of escalating violence in other conflicts.[31]

Our understanding is that the main rationale for these laws was to provide additional legal leeway to potential victims in self-defense situations, not to deter crime. Thus, there is little reason to believe that the enactment of these laws coincided with either other policies expected to affect crime or homicides, or with expectations about future crime.[32]

To determine if and when states passed castle doctrine laws, we searched news releases and other sources such as the Institute for Legislative Action of the National Rifle Association to determine whether a state appeared to have passed a law that strengthened self-defense law these ways. Specifically, we coded the specific attributes of each state statute found, and classified whether the law i) removed the duty to retreat from somewhere outside the home, ii) removed the duty to retreat from any place one has a legal right to be, iii) added a presumption of reasonable fear for the person using lethal force, and iv) removed civil liability for those acting under the law. We then classified a state as having a castle doctrine law if they remove the duty to retreat in some place outside the home. Our goal in doing so

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[30] For example, the law passed in Florida states that "a person is presumed to have held a reasonable fear of imminent peril of death or bodily injury to himself or herself or another when using defensive force that is intended or likely to cause death or bodily injury to another."

[31] These laws also typically state that the protections do not apply to those who are committing a crime at the time, or who instigated the conflict.

[32] The National Rifle Association (NRA) was a major proponent of these laws (Goode, 2012). We are unaware of any statement by the NRA that suggests their support for the laws is due to a belief that the law will deter crime, or that the law is a necessary response to recent changes in violent crime. Rather, our understanding is that supporters view castle doctrine/stand-your-ground as an issue of individual and victim rights.

was to create a list of states that extended castle doctrine and generally passed meaningful changes to their self-defense law that would be widely reported.[33]

Table III.1 shows the states classified as having enacted castle doctrine laws between 2000 and 2010. We classify 21 states as having passed castle doctrine laws, as each of these states extended the castle doctrine to places outside the home.[34] Of these, 17 states removed the duty to retreat in any place one has the legal right to be, 13 included a presumption of reasonable fear, and 18 explicitly removed civil liability. Our main analysis groups all of these laws together, and thus captures the average effect of passing a law similar to those passed in these 21 states. However, since that approach is perhaps unnecessarily blunt, in appendix Table B1 we show results from different subgroups and find that the results are largely similar to the average effects. We note, however, that due to the high degree of collinearity and the potential for interaction effects, we do not attempt to distinguish between the effects caused by the different attributes of these laws.

III.2.2 Crime Data

Outcome data come from the FBI Uniform Crime Reports (UCR) and cover all 50 states from 2000 – 2010.[35] Specifically, we use homicide, burglary, robbery, and aggravated

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[33] We are aware of four states that passed laws removing civil liability that that made no other changes to self-defense law over this time period, including Idaho (2006), Maryland (2010), Maine (2007), and Illinois (2004). We do not code those states as castle doctrine states. We also do not classify Wyoming as having passed a castle doctrine law, though we note that they removed civil liability and added a presumption of reasonable fear (provisions that removed the duty to retreat were stripped out prior to passage) (Vilos and Vilos, 2010). We thank McClellan and Tekin (2012) for helpful conversations about the specific attributes of laws passed in different states.

[34] To avoid confusion over which states are driving the within-state variation used in our study, we intentionally leave states off Table III.1 if they had passed a law that extended castle doctrine prior to 2000 or after 2010, which are outside our sample period.

[35] There are relatively few cases of missing data. Data on whether robbery was committed with a gun were missing from 2000 to 2005 for Illinois. Justifiable homicide data were missing for Florida, so we requested and received those data directly from

assault data from the official UCR data published online by the FBI.[36] In addition, for the other variables not available from the online UCR, we use data from the FBI's Master files (Return A and Supplemental Homicide Report).

We use these data to test whether making it easier for individuals to use lethal force in self-defense deters crime or increases homicide. For deterrence, we focus on three criminal outcomes. The first is burglary, which is defined as "the unlawful entry of a structure to commit a felony or a theft" (FBI, 2004). The second is robbery, defined as "the taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear" (FBI, 2004). Finally, we also examine aggravated assault, which the FBI defines as "an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury", and is typically accompanied by the use of a weapon (FBI, 2004).[37] In all cases, one might expect rational criminals to be less likely to commit such crimes under castle doctrine, as the increased scope for the use of justifiable lethal force on the part of the victim raises the expected cost to the criminal.

The homicide measure we use is total homicides, defined as the sum of murder and non-negligent manslaughter. We also look at murder separately to determine exactly how police are classifying the additional homicides.

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the Florida Department of Law Enforcement Office.

[36] These data include corrections by the FBI to adjust for under-reporting by police agencies. We note, however, that results are qualitatively and quantitatively similar if we instead use data from the Supplemental Homicide Report and Return A from the FBI Master files, which were acquired directly from the FBI and include statistics reported after the deadline, but do not correct for under-reporting. For example, estimates corresponding to the homicide estimates in the 6 columns of Panel A in Table III.5 are 0.0875, 0.0928, 0.0854, 0.0967, 0.0910, and 0.0729, respectively. All estimates but the last are significant at the 5 percent level.

[37] Results are similar using data on all assaults, including simple assault, which were obtained from Return A of the FBI Master files. We also note that it is possible that the laws could increase aggravated assaults by escalating violence in conflicts.

Table III.1 States that Extended Castle Doctrine Between 2000 and 2010

State	Effective Date	Removes duty to retreat somewhere outside home	Removes duty to retreat in any place one has a legal right to be	Presumption of reasonable fear	Removes civil liability
Alabama	2006/06/01	Yes	Yes	No	Yes
Alaska	2006/09/13	Yes	No	Yes	Yes
Arizona	2006/04/24	Yes	Yes	Yes	Yes
Florida	2005/10/01	Yes	Yes	Yes	Yes
Georgia	2006/07/01	Yes	Yes	No	Yes
Indiana	2006/07/01	Yes	Yes	No	Yes
Kansas	2006/05/25	Yes	Yes	No	Yes
Kentucky	2006/07/12	Yes	Yes	Yes	Yes
Louisiana	2006/08/15	Yes	Yes	Yes	Yes
Michigan	2006/10/01	Yes	Yes	No	Yes
Mississippi	2006/07/01	Yes	Yes	Yes	Yes
Missouri	2007/08/28	Yes	No	No	Yes
Montana	2009/04/27	Yes	Yes	Yes	No
North Dakota	2007/08/01	Yes	No	Yes	Yes
Ohio	2008/09/09	Yes	No	Yes	Yes
Oklahoma	2006/11/01	Yes	Yes	Yes	Yes
South Carolina	2006/06/09	Yes	Yes	Yes	Yes
South Dakota	2006/07/01	Yes	Yes	No	No
Tennessee	2007/05/22	Yes	Yes	Yes	Yes
Texas	2007/09/01	Yes	Yes	Yes	Yes
West Virginia	2008/02/28	Yes	Yes	No	No

An increase in criminal homicide could represent the escalation of violence by criminals, the escalation of violence in otherwise non-lethal conflicts, or, possibly, an increase in legally justified homicide that is misreported as murder or non-negligent manslaughter.[38] In order to shed light on that issue, we look at two other outcomes, both of which measure the escalation of violence by criminals in response to castle doctrine. The ratio of robberies committed with a gun measures whether criminals respond by being more likely to carry and

[38] The general possibility that disputes can escalate dramatically in environments perceived to be dangerous is discussed in O'Flaherty and Sethi (2010).

use weapons during the commission of a crime, as one might expect if they believe they will be faced with lethal force by the victim. We also look at felony-type and suspected felony-type murders, which also measure the escalation of violence by criminals. We expect to see increases in these outcomes if castle doctrine laws induce criminals to be more likely to carry and use deadly weapons during the commission of crimes.

In addition, we also ask whether the laws increase homicides that are reported to the FBI as "justifiable homicides by private citizens", which the FBI defines as "the killing of a felon during the commission of a felony" (Uniform Crime Reporting Handbook, 2004).[39] The major disadvantage of these data is that they are widely believed to be underreported; Kleck (1988) estimates that around one-fifth of legally justified homicides are reported that way to the FBI. However, note that we use these data only to look for evidence of *relative* changes in legally justified homicide. We then use those estimates, along with assumptions about the degree of underreporting, to determine if the entire increase in criminal homicides can be explained as (misreported) legally justified homicides.

The data also allow us to perform several placebo, or falsification tests. Specifically, we use data on the rate of larceny and motor vehicle theft to determine whether castle doctrine laws appear to affect those crimes.[40] In both cases we expect to find no effects so long as the

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[39] The Uniform Crime Reporting Handbook emphasizes that by definition, justifiable homicide occurs in conjunction with other offenses, and those other offenses must be reported. Additionally, the handbook gives examples of specific hypothetical events that would and would not qualify as justifiable homicide under the guidelines. An example given of an incident that would qualify as a justifiable homicide is "When a gunman entered a store and attempted to rob the proprietor, the storekeeper shot and killed the felon" (Uniform Crime Reporting Handbook, 2004). An example of what would NOT qualify as a justifiable homicide is "While playing cards, two men got into an argument. The first man attacked the second with a broken bottle. The second man pulled a gun and killed his attacker. The police arrested the shooter; he claimed self-defense" (Uniform Crime Reporting Handbook, 2004). We note that under castle doctrine, the hypothetical shooter may have been justified as acting in self-defense, though again the reporting handbook explicitly states that this would not qualify as a justifiable homicide under the guidelines.

[40] While it may be possible for castle doctrine law to deter these crimes as well, our view is that deterrence should be considerably less likely for these crimes than for burglary, robbery, and aggravated assault.

identifying assumptions of our difference-in-difference research design hold, which we discuss at length in the next section.

Finally, we have data on several time-varying control variables. Specifically, we observe the number of full-time equivalent police per 100,000 state residents (Uniform Crime Reports, 2000-2010). We also include both contemporaneous and lagged measures of the number of persons incarcerated in state prison per 100,000 residents (Bureau of Justice Statistics Bulletin, 2000-2010). These variables capture the effects of deterrence and incapacitation caused by additional policing or incarceration. In addition, we have two variables from the American Community Survey of the U.S. Census Bureau that measure local legal opportunities, including median family income and the poverty rate. We also have data on the share of white and black men in the 15-24 and 25-44 age groups for each state over time (American Community Survey, 2000-2010). Finally, we measure the generosity of public assistance in each state by measuring per capita spending on assistance and subsidies and per capita spending on public welfare (US Census, 2000 – 2010). Descriptive statistics can be found in Table III.2.

III.3 Identification

To distinguish the effect of the castle doctrine laws from confounding factors, we exploit the within-state variation induced by the fact that 21 states passed such laws between 2000 and 2010. Specifically, we use a difference-in-differences research design that asks whether outcomes change more in states that adopt castle doctrine laws than in states that do not, and focus primarily on within-region comparisons.

Formally, we estimate fixed effects ordinary least squares (OLS) panel data models, where we follow convention and use the log of the outcome per 100,000 population as the dependent variable.[41] For homicide, we also estimate negative binomial models. Ordinary least squares models are estimated with and without weighting by state population.[42] The OLS model estimated is

$$\text{Outcome}_{it} = \beta_1 CDL_{it} + \beta_1 X_{it} + c_i + u_t + \varepsilon_{it} \qquad \text{(III.1)}$$

where CDL_{it} is the treatment variable that equals the proportion of year t in which state i has an effective castle doctrine law, X_{it} is the vector of control variables, and c_i and u_t control for state and year fixed effects, respectively. In addition, in most models we also include Census region-by-year fixed effects, to allow states in different regions of the country to follow different trajectories and account for differential shocks by region over time.[43] Note that for states that enacted the law partway through a year, we set CDL equal to the proportion of the year in which the law was in effect, though estimates are almost identical when we exclude the year of adoption.[44] Robust standard errors are clustered at the state level, though we also do additional exercises in the spirit of Bertrand, Duflo, and Mullainathan (2004) to ensure standard errors are being estimated accurately, as well as to perform inference using placebo estimates from pre-castle doctrine data. This last approach of using distributions of placebo

[41] See, for example, Ayres and Levitt (1998), Duggan (2001), and Lott and Mustard (1997). An alternative specification is to use the log of homicide count as the dependent variable, and control for the log of population. Estimates from that specification that correspond to those in column 3 of Table III.5 are 0.097 and 0.0602 for weighted and unweighted OLS regressions, compared to estimates reported in Table III.5 of 0.0937 and 0.0600.

[42] Specifically, we use analytic weights where average state population over the time period is the weight. This was done using the aweight command in Stata.

[43] There are four Census Regions: West, Midwest, Northeast, and South.

[44] Specifically, when we drop observations containing the year of adoption, estimates corresponding to column 3 of Table III.5 are 0.0947, 0.0569, and 0.0895, compared to reported estimates in Table III.5 of 0.0937, 0.600, and 0.0879, respectively.

Table III.2 Descriptive Statistics

	Mean (Unweighted)	Mean (Weighted by Population)
Dependent Variables		
Homicides per 100,000 Population	4.8	5.5
	(2.5)	(1.9)
Justifiable Homicide by Private Citizens (count)	5.1	11.8
	(8.2)	(12.9)
Justifiable Homicide by Police (count)	8.0	23.4
	(16.9)	(34.3)
Robberies per 100,000 Population	107.2	143.1
	(59.6)	(47.5)
Aggravated Assault per 100,000 Population	267	296
	(131)	(114)
Burglary per 100,000 Population	710	744
	(240)	(235)
Larceny per 100,000 Population	2,334	2,328
	(533)	(532)
Motor Theft per 100,000 Population	331	381
	(178)	(174)
Proportion of Robberies in Which a Gun Was Used	0.35	0.37
	(0.13)	(0.13)
Control Variables		
Police per 100,000 residents	315	336
	(65)	(66)
Unemployment Rate (%)	5.49	5.93
	(1.99)	(2.10)
Poverty Rate (%)	12.4	12.9
	(3.0)	(2.6)
Median Household Income ($)	51,648	52,146
	(7873)	(6895)
Prisoners per 100,000 residents	439	461
	(169)	(150)
Government spending (assistance and subsidies) per capita	125	110
	(56)	(48)
Government spending (public welfare) per capita	1,319	1,344
	(391)	(409)
% Black Male Aged 15-24	2.60	0.97
	(4.61)	(2.11)
% White Male Aged 15-24	10.77	4.36
	(17.70)	(7.69)
% Black Male Aged 25-44	4.32	1.61
	(7.71)	(3.53)
% White Male Aged 25-44	21.97	8.88
	(36.40)	(15.90)

Notes: Each cell contains the mean with the standard deviation in parentheses. All variables have 550 observations except for the proportion of assaults in which a gun was used (544) and the proportion of robberies in which a gun was used (544).

estimates to do inference is similar in spirit to the permutation inference approach used in the synthetic control method by Abadie, Diamond, and Hainmueller (2010).

Since we primarily rely on specifications that include state fixed effects and region-by-year fixed effects, the identifying assumption is that in the absence of the castle doctrine laws, adopting states would have experienced changes in crime similar to non-adopting states in the same region of the country. Our data allow us to test and relax this identifying assumption in several ways. First, we look for graphical evidence of whether the two groups diverged prior to treatment. Along similar lines, we offer a formal statistical test by including an indicator in equation (1) for the two years prior to the passage of the laws. That is, we ask whether states that pass the laws diverge even *before* they pass the laws. If they do, it suggests that the identifying assumption of our research design is violated.

We also examine whether time-varying determinants of crime are orthogonal to the within-state variation in castle doctrine laws. Under our identifying assumption, factors such as economic conditions, welfare spending, and policing intensity should not change more over time in adopting states than non-adopting states, as this would suggest that crime in the two groups might have diverged even in the absence of treatment. Thus, we examine whether adding these controls changes our estimates in a meaningful way. To the extent that our difference-in-differences estimates remain unchanged, it provides some assurance that our research design is reasonable.[45]

[45] The primary concern is not that observed determinants vary systematically over time—we can control for those variables directly—but that if they do, it may suggest that unobserved determinants also change systematically over time in the treatment and control groups.

Along similar lines, we also show results from specifications that include contemporaneous motor vehicle theft and larceny as controls. While it is possible that castle doctrine laws could affect these crimes, we would expect any such effects to be second-order and at most small in magnitude. Thus, we use these crime measures as controls that pick up any differential trends in crime in adopting and non-adopting states. We also perform falsification exercises using these crimes as outcomes to explicitly test whether castle doctrine laws appear to affect crimes unrelated to self-defense. If our identifying assumption holds, we would expect to see no effects on these crimes.

Finally, we allow for state-specific linear time trends, thereby allowing each state to follow a different trend.

III.4 Results

III.4.1 Falsification Tests

One way to test the identifying assumption is to directly examine whether crimes that ought not be affected by the laws—and thus proxy for general crime trends—appear to be affected by the laws.[46] Finding effects on crimes that ought to be exogenous to castle doctrine law would invalidate our research design.

Thus, we examine whether castle doctrine laws appear to affect larceny or motor vehicle theft. While it is possible that these outcomes are affected directly by self-defense laws, we argue that such effects should be second-order, at best.

[46] Similar tests are performed by Ayres and Levitt (1998), when they look for effects of Lojack on crimes other than motor vehicle theft.

Results are shown in Table III.3, which uses a format similar to subsequent tables showing other outcomes. Columns 1 through 6 represent OLS estimates that are weighted by population, while Columns 7 through 12 are unweighted OLS estimates. The first column of each group controls for only state and year fixed effects. The second column adds region-by-year fixed effects, while the third column adds time-varying controls. The fourth column additionally includes an indicator variable for the two years before the castle doctrine law was adopted; the fifth drops the leading indicator but adds controls for contemporaneous larceny and motor vehicle theft. Finally, the last column controls for state fixed effects, region-by-year fixed effects, time-varying controls, and state-specific linear time trends.

Estimates for larceny are close to zero and statistically insignificant across all specifications. Estimates of the effect on the log of the motor vehicle theft rate are more interesting. Results in columns 1 and 7 in which only state and year fixed effects are included provide suggestive evidence of increases in motor vehicle theft of 5 to 8 percent, the latter of which is significant at the 10 percent level. However, including region-by-year fixed effects in columns 2 and 8 causes the estimate to drop to zero or even turn negative, and both are statistically insignificant. This suggests that accounting for differences in regional trends in some way may be important in assessing the impact of castle doctrine laws.

III.4.2 Deterrence

We now examine whether strengthening self-defense law deters crime. We examine three types of crime: burglary, robbery, and aggravated assault. To the extent that criminals respond to the higher actual or perceived risk that victims will use lethal force to protect themselves, we would expect these crimes to decline after the adoption of castle doctrine.

Results are shown in Table III.4, where the first 6 columns show estimates from an OLS regression weighted by state population, while the last 6 columns are from unweighted OLS regressions. Results in Column 1 in Panel A for burglary are similar to the finding for motor vehicle theft, in that estimates range from 6 to 8 percent and are statistically significant at the 5 percent level. Again, however, including region-by-year effects in columns 2 and 8 reduces the estimates considerably, and all are statistically indistinguishable from zero at the 5 percent level.

Importantly, there is little evidence of deterrence effects in any specification for any outcome: of the 36 estimates reported, none are negative and statistically significant at the 10 percent level. The estimates are sufficiently precise as to rule out large deterrence effects. For example, in our preferred specification in column 3, the lower bounds of estimates on burglary, robbery, and aggravated assault are -2.1 percent, -1.9 percent, and -2.5 percent. Put differently, our estimates and standard errors from column 3 indicate that if we were to perform this castle doctrine policy experiment many times, we would expect that 90 percent of the time we would find deterrence effects of less than 0.7 percent, 0.4 percent, and 0.5 percent for burglary, robbery, and aggravated assault, respectively. In short, these estimates provide strong evidence against the possibility that castle doctrine laws cause economically meaningful deterrence effects. Thus, while castle doctrine law may well have benefits to those legally justified in protecting themselves in self-defense, there is no evidence that the law provides positive spillovers by deterring crime more generally.[47]

[47] It is worth noting that it is difficult to measure the benefits of these laws to actual victims of violent crime. These benefits could include fewer or less serious physical or psychological injuries, or lower legal costs. We make no attempt to measure these benefits in this paper.

Table III.3 Falsification Tests: The Effect of Castle Doctrine on Larceny and Motor Vehicle Theft

	OLS - Weighted by State Population						OLS - Unweighted					
	1	2	3	4	5	6	7	8	9	10	11	12
Panel A: Larceny	Log (Larceny Rate)						Log (Larceny Rate)					
Castle Doctrine Law	0.00300	-0.00660	-0.00910	-0.00858	-0.00401	-0.00284	0.00745	0.00145	-0.00188	0.00199	-0.00361	-0.0137
	(0.0161)	(0.0147)	(0.0139)	(0.0165)	(0.0128)	(0.0180)	(0.0227)	(0.0205)	(0.0210)	(0.0230)	(0.0201)	(0.0228)
0 to 2 years before adoption of castle doctrine law				0.00112						0.00924		
				(0.0105)						(0.0121)		
Observation	550	550	550	550	550	550	550	550	550	550	550	550
Panel B: Motor Vehicle Theft	Log (Motor Vehicle Theft Rate)						Log (Motor Vehicle Theft Rate)					
Castle Doctrine Law	0.0517	-0.0389	-0.0252	-0.0294	-0.0165	-0.00708	0.0767*	0.0138	0.00814	0.0151	0.00977	-0.00373
	(0.0563)	(0.0448)	(0.0396)	(0.0469)	(0.0354)	(0.0372)	(0.0413)	(0.0444)	(0.0407)	(0.0490)	(0.0391)	(0.0361)
0 to 2 years before adoption of castle doctrine law				-0.00896						0.0165		
				(0.0216)						(0.0278)		
Observation	550	550	550	550	550	550	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes
Controls for Larceny or Motor Theft					Yes						Yes	
State-Specific Linear Time Trends						Yes						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics.

* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table III.4 The Deterrence Effects of Castle Doctrine: Burglary, Robbery, and Aggravated Assault

	OLS - Weighted by State Population						OLS - Unweighted					
	1	2	3	4	5	6	7	8	9	10	11	12
Panel A: Burglary	Log (Burglary Rate)						Log (Burglary Rate)					
Castle Doctrine Law	0.0780***	0.0290	0.0223	0.0181	0.0327*	0.0237	0.0572**	0.00961	0.00663	0.00293	0.00683	0.0207
	(0.0255)	(0.0236)	(0.0223)	(0.0265)	(0.0165)	(0.0207)	(0.0272)	(0.0291)	(0.0268)	(0.0330)	(0.0222)	(0.0259)
0 to 2 years before adoption of castle doctrine law				-0.00906						-0.00884		
				(0.0133)						(0.0195)		
Panel B: Robbery	Log (Robbery Rate)						Log (Robbery Rate)					
Castle Doctrine Law	0.0408	0.0344	0.0262	0.0197	0.0376**	0.0515*	0.0448	0.0320	0.00839	0.000483	0.00874	0.0267
	(0.0254)	(0.0224)	(0.0229)	(0.0257)	(0.0181)	(0.0274)	(0.0331)	(0.0421)	(0.0387)	(0.0462)	(0.0339)	(0.0299)
0 to 2 years before adoption of castle doctrine law				-0.0138						-0.0189		
				(0.0153)						(0.0237)		
Panel C: Aggravated Assault	Log (Aggravated Assault Rate)						Log (Aggravated Assault Rate)					
Castle Doctrine Law	0.0434	0.0397	0.0372	0.0330	0.0424	0.0414	0.0555	0.0698	0.0343	0.0326	0.0341	0.0317
	(0.0387)	(0.0407)	(0.0319)	(0.0367)	(0.0291)	(0.0285)	(0.0604)	(0.0630)	(0.0433)	(0.0501)	(0.0405)	(0.0380)
0 to 2 years before adoption of castle doctrine law				-0.00897						-0.00391		
				(0.0147)						(0.0249)		
Observations	550	550	550	550	550	550	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes						Yes	
State-Specific Linear Time Trends						Yes						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

III.4.3 Homicide

We now turn to whether strengthening self-defense laws increases criminal homicide. Given that the laws reduce the expected costs associated with using violence, economic theory would predict that there would be more of it.

We start by showing the raw data in a set of figures. Figure III.1 shows log homicide rates over time for adopting states and non-adopting states, by year of adoption. For example, Figure III.1a shows the log homicide rate for the 2005 adopting state (Florida), relative to states that did not adopt the law from 2000 – 2010. While it is somewhat difficult to appreciate the magnitude of changes over time from the graphs and keeping in mind that the adoption year is only partially treated, two patterns emerge. The first is that with the exception of the two states adopting in 2008 (Ohio and West Virginia),[48] the homicide rates of adopting states have a similar trajectory to those of non-adopting states *prior* to the adoption of the law.[49] That is, there is little reason to believe that the homicide rates of adopting states would have increased relative to non-adopting states in the absence of treatment.

Second, Figure III.1 shows that there is a large and immediate increase in homicides for states adopting in 2005, 2006, and 2009. Similarly, while the 6 states that adopted in 2007

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[48] It is little surprise given the small sample sizes involved in this exercise that there would be some set of sets that did not track non-adopting states perfectly in trajectory for the entire period prior to treatment. In addition, we note that while homicide rates did increase in both Ohio and West Virginia from 2000/2001 to 2003, rates there tracked the rest of the country quite closely in changes from 2003 through 2007.

[49] As shown in Figure 1, adopting states have homicide rates that are about 30 percent higher than non-adopting states. However, because we are using a difference-in-differences research design that conditions on year and state fixed effects, differences in *levels* is not a concern for identification. Instead, what would worry us is if the homicide rate in adopting states increased more than in non-adopting states even before treatment, as that would suggest that the groups might have continued to diverge afterward, regardless of castle doctrine. We see no evidence of that, which suggests that the relative increase seen after 2005 is caused by castle doctrine. Moreover, note that homicide estimates remained similar even after controlling for time-varying police and incarceration rates and other controls, including region-by-year fixed effects, and allowing for state-specific linear time trends.

or 2008 did not appear to experience much of a relative increase in the year or adoption or the year afterward, they notably did not experience the relative drop in homicide rates that other states nationwide did in 2009 and 2010. Of course, given the small samples involved, it is difficult to infer much about short-term versus long-term patterns across these different sets of states, but it is clear from the raw data that castle doctrine states experienced a relative increase in homicides after adoption.

Figure III.2 shows the estimated divergence between adopting and non-adopting states over time, where t=0 is the year of treatment. Specifically, Figure III.2 graphs coefficients from a difference-in-differences model in which we control for state and region-by-year fixed effects and time-varying covariates, and then allow for divergence 3 and 4 years prior to adoption, 1 and 2 years prior to adoption, the year of adoption, the 1st and 2nd years after adoption, and 3 or more years after adoption. Estimates are relative to the average difference in log homicide rates 5 or more years prior to law adoption.

Consistent with Figure III.1, there is little evidence of divergence in the years prior to adoption. For example, there was almost no divergence in the 4 years prior to adoption using the negative binomial model, and only around 1 to 2 percent using weighted OLS. For weighted OLS, the divergence increases to 10 percent after the year of treatment, and to around 8 percent in the negative binomial model. This offers of preview of the estimated effect on homicide of around 8 percent. There is more modest evidence of divergence prior to adoption using unweighted OLS, though there still appears to be a discrete change at the year of treatment from around 2.5 percent to 7 percent. The difference between the estimated pre-adoption divergence in weighted and unweighted specifications appears to be largely due to

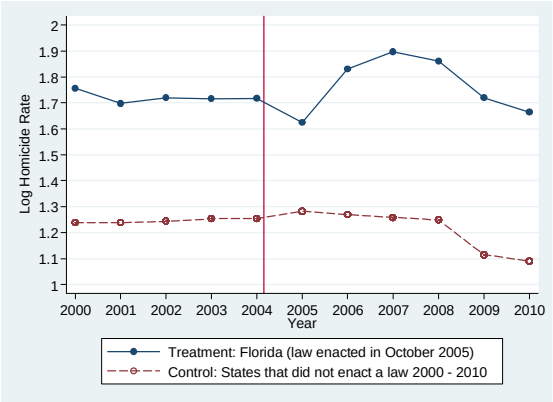


Figure III.1a: 2005 State Adopting in 2005(Florida)

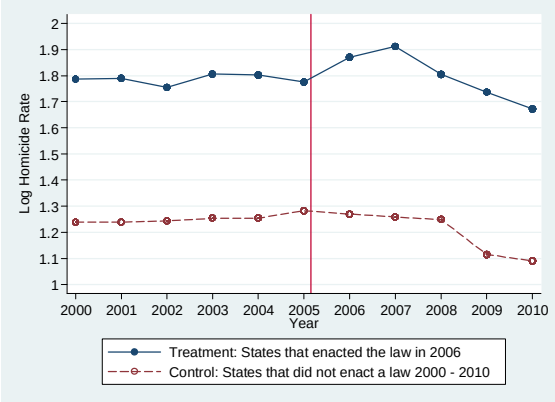


Figure III.1b: States Adopting in 2006 (Alabama, Alaska, Arizona, Georgia, Indiana, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Oklahoma, South Carolina, South Dakota)

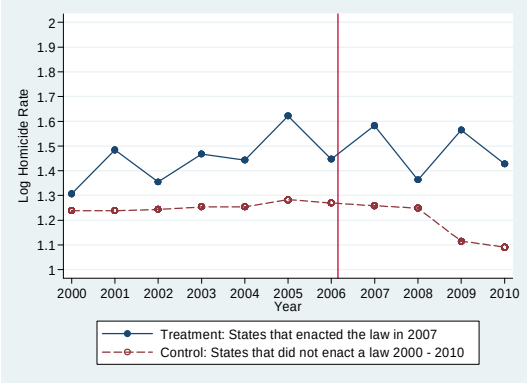


Figure III.1c: States Adopting in 2007 (Missouri, North Dakota, Tennessee, Texas)

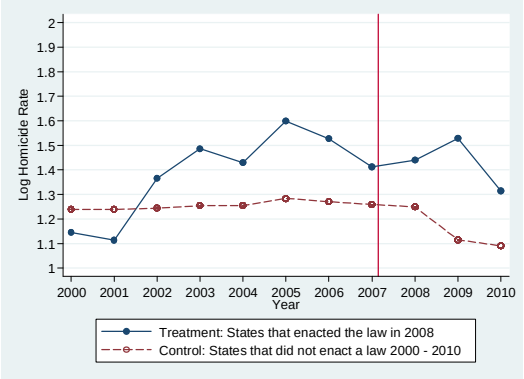


Figure III.1d: States Adopting in 2008 (Ohio, West Virginia)

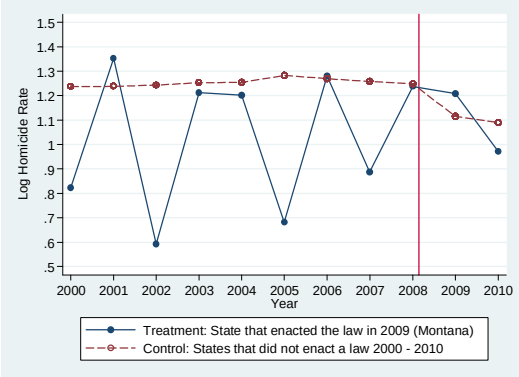


Figure III.1e: State Adopting in 2009 (Montana)

Figure III.1 Log Homicide Rates Before and After Adoption of Castle Doctrine, by Year of Adoption

the small population states of North and South Dakota.[50]

We now turn to estimating the average effect of the laws in a difference-in-differences regression framework. Results are shown in Panels A, B, and C of Table III.5, which show population-weighted OLS estimates, unweighted OLS estimates, and estimates from a negative binomial model. Estimates from the negative binomial regression are interpreted in the same way as those from a log-linear OLS model. Results from the population-weighted OLS model shown in Panel A indicate that the laws increased homicide rates by 8 to 10 percent; all 6 estimates are statistically significant at the 5 percent level, and 3 are significant at the 1 percent level. Estimates from unweighted OLS regressions shown in Panel B range from 5 to 9 percent, though all are measured imprecisely: t-statistics range from 0.6 to 1.5. Estimates in Panel C from a negative binomial model indicate castle doctrine leads to a 6 to 11 percent increase in homicide. All negative binomial estimates that include region-by-year fixed effects are significant at the 5 percent level, and that which does not (column 1) is significant at the 10 percent level.

We have also done additional tests in order to ensure that we are making correct inferences about statistical significance. Toward that end, we do tests in the spirit of Bertrand et al. (2004), in which we randomly select 11-year panels from 1960 to 2004, and then randomly assign states to the treatment dates found in our data, without replacement. Thus, we assume that one state adopted castle doctrine on October 1st of the 6th year of the 11-year panel (just as Florida actually adopted in 2005, the 6th year of our panel), and that 13 more

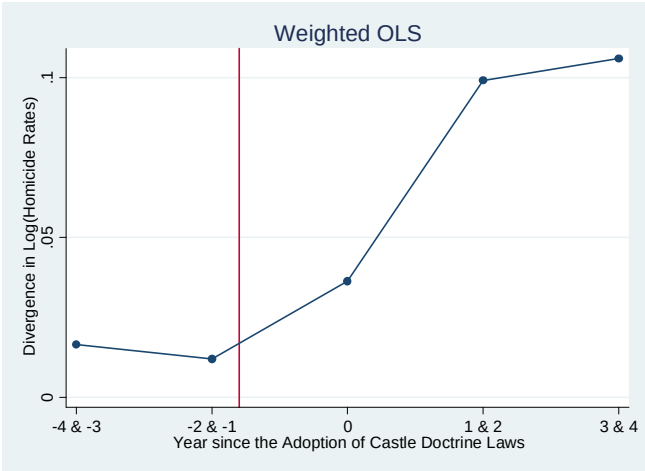
[50] In North Dakota, homicide rates per 100,000 population went from 0.87 in 2000-2002 to 1.5 in 2003-2006, prior to law adoption in 2007. Similarly, homicide rates went from 0.96 in 2000-2001 to 1.89 in 2002 – 2005 in South Dakota, who adopted the law in 2006. South Dakota averages 20 homicides per year and North Dakota averages less than 10, so we suspect the changes in the pre-adoption period were idiosyncratic.

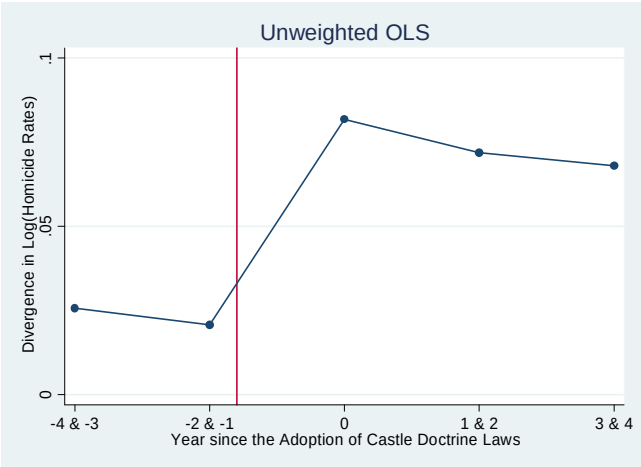
states adopted in the 7th year of the 11-year panel, etc. We generate distributions of estimates, and ask how often we reject the null hypothesis of no effect at the 5 percent level, as well as what proportion of the placebo estimates are larger than the actual estimated effect of (real) castle doctrine. The latter figure corresponds to a p-value and is similar to the method used in synthetic control methods (Abadie et al., 2010), as well as by Chetty, Looney, and Kroft (2009).

The resulting placebo distributions from 1,000 random draws are shown in Figure III.3, and correspond to Table III.5 results from column 2 of Panels A, B, and C, respectively. Results from population-weighted OLS placebo estimates suggest that robust clustered standard errors may be a bit too small: 10.0 percent of simulated estimates are significant at the 5 percent level. However, the estimate of 9.46 percent in column 2 ranks in the 95.4th percentile of placebo estimates, which means only 4.6 percent of placebo estimates are larger than it is.

Results for unweighted OLS simulation results are also interesting. On the one hand, simulations suggest that clustered standard errors from unweighted OLS regressions are accurate: 5.7 percent of the simulated estimates are significant at the 5 percent level. At the same time, however, the estimate of 8.1 percent shown in Table III.5 corresponds to the 95.1st percentile, which would give it a p-value of 4.9 percent using the Abadie et al. (2010) approach to inference. This suggests that results in Panel B of Table III.5 understate the degree of statistical significance.

Finally, simulations for the fixed effect negative binomial model corresponding to column 2 in Panel C indicate that 7.6 percent of placebo estimates are significant at the 5 percent level, while 14.1 percent are significant at the 10 percent level. As shown in Figure III.3, the estimate of 7.3 percent in Table III.5 ranks at the 95.7th percentile, as fewer than 5





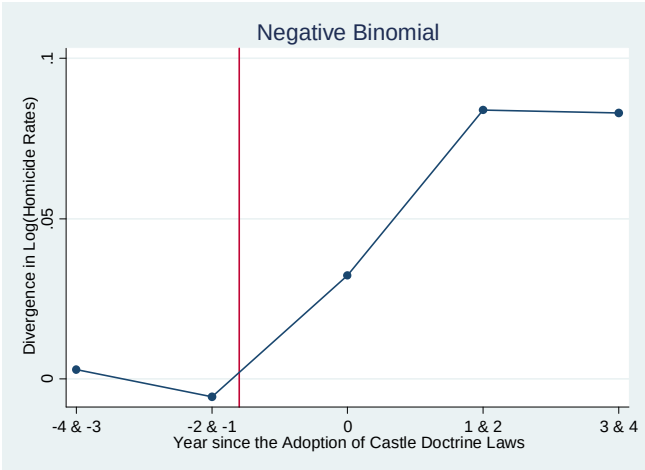


Figure III.2 Divergence in Log Homicide Rates Before and After Adoption of Castle Doctrine, Relative to the Difference 5 or More Years before Adoption

Table III.5 The Effect of Castle Doctrine on Homicide

	1	2	3	4	5	6
Panel A: Log Homicide Rate (OLS - Weighted)						
Castle Doctrine Law	0.0801**	0.0946***	0.0937***	0.0955**	0.0985***	0.100**
	(0.0342)	(0.0279)	(0.0290)	(0.0367)	(0.0299)	(0.0388)
0 to 2 years before adoption of castle doctrine law				0.00398		
				(0.0222)		
Observations	550	550	550	550	550	550
Panel B: Log Homicide Rate (OLS - Unweighted)						
Castle Doctrine Law	0.0877	0.0811	0.0600	0.0588	0.0580	0.0672
	(0.0638)	(0.0769)	(0.0684)	(0.0807)	(0.0662)	(0.0450)
0 to 2 years before adoption of castle doctrine law				-0.00298		
				(0.0350)		
Observations	550	550	550	550	550	550
Panel C: Homicide (Negative Binomial)						
Castle Doctrine Law	0.0565*	0.0734**	0.0879***	0.0854**	0.0937***	0.108***
	(0.0331)	(0.0305)	(0.0313)	(0.0385)	(0.0302)	(0.0346)
0 to 2 years before adoption of castle doctrine law				-0.00545		
				(0.0227)		
Observations	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes	

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Negative binomial estimates are interpreted in the same way as those in a log-linear OLS model. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates. Homicide data are from the published FBI Uniform Crime Reports.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

percent of placebo estimates were larger than the actual estimate in the simulations.

On the basis of these exercises, we conclude that it is unlikely that we would have obtained estimates of the magnitude and statistical significance shown in Panels A, B, and C of Table III.5 due to chance.

We have also performed simulations to see if the homicide rates of these particular 21 states *ever* diverged in the way they did after adopting castle doctrine in the late 2000s. To do so, we created 40 panel data sets, each covering separate 11-year time periods between 1960 and 2009. In each 11-year panel, we assume that Florida adopts castle doctrine on October 1st of the 6th year, and that the 13 states that adopted in 2006 adopted in the 7th year, etc. None of the 40 estimates corresponding to either the OLS population-weighted regressions or from the negative binomial regression were larger than those shown in column 2 of Table III.5. In the case of the OLS unweighted regressions, only 1 of the 40 placebo estimates was larger than the actual estimate of 8.1 percent shown in Column 2, Panel B, of Table III.5.[51] The average estimated divergence across the 40 years was -0.008, -0.004, and -0.005 across the unweighted OLS, weighted OLS, and negative binomial models.[52] Thus, there is no evidence that the homicide rates in castle doctrine states show a general tendency to increase relative to their regional counterparts: in the last 40 years they have almost never done so as much as they did immediately after castle doctrine.

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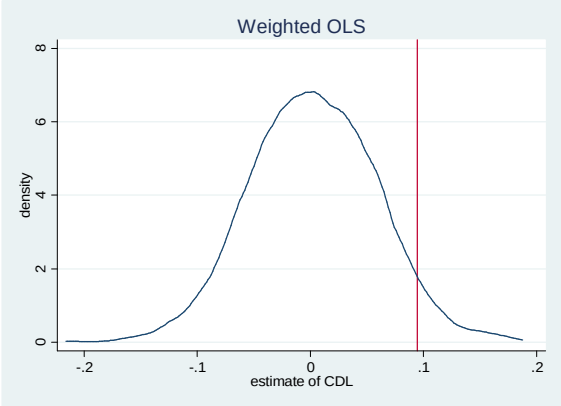
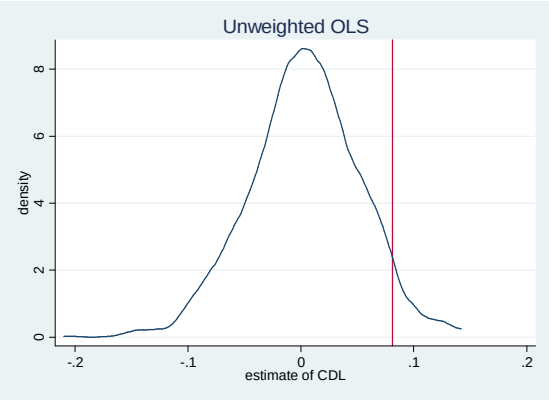
[51] The one larger estimate was 10.5 percent, and was from the 1975 to 1985 time period.

[52] Estimates for the most recent 5 panels (1995 – 2005 through 1999 – 2009) were 0.022, 0.015, 0.004, -0.027, and -0.069 for weighted OLS, 0.01247, 0.02391, 0.00826, -0.02142, and -0.04719 for unweighted OLS, and 0.004, -0.003, -0.0185, -0.0562, and -0.106 for negative binomial. In these latter panels, we exclude all state-year observations when castle doctrine was actually in effect, so as not to bias placebo estimates upward due to the real treatment effect.





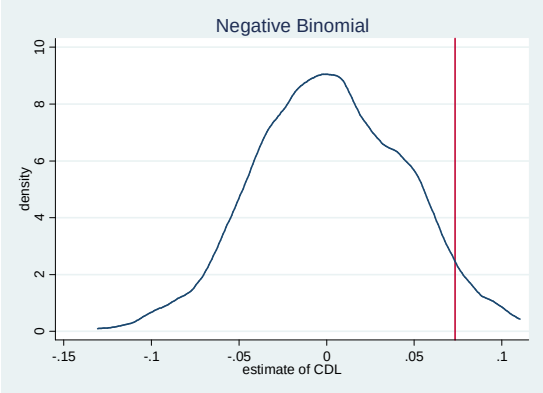


Figure III.3 Empirical Distributions of Placebo Homicide Estimates

Notes: The vertical lines represent the actual estimated effects of castle doctrine on log homicide. These estimates are 0.0946, 0.0811, and 0.0734 and correspond to population-weighted OLS, unweighted OLS, and negative binomial estimation, respectively, as shown in Column 2 of Table III.5. A total of 4.6 percent, 4.9 percent, and 4.3 percent of placebo estimates lie to the right of these estimates.

Given the robustness of the estimates to various specifications, it is worth considering what one would have to believe for a confounding factor to cause the observed increase in homicide rates, rather than castle doctrine. That is, one would have to believe that something else caused homicides to increase relative to non-adopting states immediately after castle doctrine was enacted, but not in the years prior to enactment. In addition, this confounder must have only caused a divergence in homicide rates in the late 2000s coincidental with the passage of castle doctrine, and not at any point in the 40 years prior. Furthermore, this confounder must cause an increase in homicides in castle doctrine states after adoption, but not cause a similar increase in states *in the same region of the country* that did not adopt castle doctrine at that time. Additionally, the confounder must cause adopting states to diverge from their own pre-adoption trend in homicide rate, coincidental with the enactment of castle doctrine. The confounder must also increase homicides in adopting states after adoption without causing proportionate increases in motor vehicle theft, larceny, robbery, burglary, or aggravated assault. Finally, the confounder must be uncorrelated with changes in the economic conditions, welfare generosity, and the rates of incarceration and policing in adopting states immediately following adoption. We are unable to think of any confounding factor that would fit this description, and thus we interpret the increase in homicides as the causal effect of castle doctrine.

III.4.4 Homicide: Interpretation

Collectively, we view these findings as compelling evidence that castle doctrine increases homicide. However, we note that one downside of the homicide measure is that it could potentially include homicides that are justified under the new self-defense law, but were improperly reported as criminal homicides rather than justifiable homicides. If all the

additional homicides were misreported as criminal homicides, the increase may not be viewed by everyone as unambiguously bad. We note, however, that the net increase cannot be driven by a one-to-one substitution of homicides of assailants for homicides of innocent victims. In contrast, in order for the entire increase in homicide to be driven by life-saving use of force, there would have to be at least some cases of multiple killed assailants by a would-be-killed victim.

To shed light on this issue, we look directly for evidence for or against the different interpretations of the increase in reported homicide. We start by examining whether the laws increase the number of homicides classified as murders. This classification available in the Return A files excludes non-negligent manslaughter classifications that one might think would be used more often in potential self-defense killings not classified as justifiable homicides. Estimates in Panel A of Table III.6 indicate a similarly sized increase in murder, which suggests that police are largely classifying these additional homicides as murders.

We then turn to assessing whether criminals appear to escalate violence in response to castle doctrine laws. For example, a rational criminal may respond to a real or perceived increase in the likelihood of encountering a victim willing to use lethal force by using a deadly weapon himself. Thus, we examine whether castle doctrine increases felony-type and suspected felony-type murders, which appeared to be committed during a felony. Results are shown in Panel B of Table III.6. The estimate from column 1, which controls only for state and year fixed effects, is 10 percent and is statistically indistinguishable from zero. Estimates from specifications including region-by-year fixed effects are more suggestive of a criminal escalation effect: estimates in columns 2 through 5 are around 20 percent and are statistically significant at the 10, 5, 1, and 5 percent levels, respectively, though we note the estimate goes

Table III.6 The Effect of Castle Doctrine on Murder, Felony-Type Homicide, Proportion of Robberies Committed Using a Gun, and Justifiable Homicide by Private Citizens

	1	2	3	4	5	6
Panel A: Murder						
(OLS - Weighted)						
Castle Doctrine Law	0.0906**	0.0955**	0.0916**	0.105**	0.0981**	0.0813
	(0.0424)	(0.0389)	(0.0382)	(0.0425)	(0.0391)	(0.0520)
				0.0277		
0 to 2 years before adoption of castle doctrine law				(0.0309)		
Observations	550	550	550	550	550	550
Panel B: Log Felony-Type and Suspected Felony Type Homicides						
(OLS - Weighted)						
Castle Doctrine Law	0.0993	0.203*	0.220**	0.284***	0.222**	0.00121
	(0.112)	(0.109)	(0.0907)	(0.103)	(0.0871)	(0.0686)
				0.143***		
0 to 2 years before adoption of castle doctrine law				(0.0516)		
Observations	539	539	539	539	539	539
Panel C: Proportion of Robberies Using Gun						
(OLS - Weighted)						
Castle Doctrine Law	0.0444***	0.0218	0.0187	0.0247	0.0183	-0.00404
	(0.0145)	(0.0186)	(0.0153)	(0.0167)	(0.0155)	(0.0133)
				0.0124		
0 to 2 years before adoption of castle doctrine law				(0.0101)		
Observations	544	544	544	544	544	544
Panel D: Justifiable Homicide by Private Citizens						
(OLS - Unweighted, Dep. Variable = Count)						
Castle Doctrine Law	4.328***	3.370**	3.200**	3.374**	3.239**	0.960
	(1.467)	(1.300)	(1.202)	(1.335)	(1.216)	(1.219)
				0.417		
0 to 2 years before adoption of castle doctrine law				(0.709)		
Observations	550	550	550	550	550	550
Panel E: Justifiable Homicide by Private Citizens						
(Negative Binomial - Unweighted)						
Castle Doctrine Law	0.573***	0.428*	0.283	0.320	0.324	NA
	(0.210)	(0.244)	(0.235)	(0.254)	(0.228)	NA
				0.0862		
0 to 2 years before adoption of castle doctrine law				(0.136)		
Observations	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes	
State-Specific Linear Time Trends						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Negative binomial estimates are interpreted in the same way as those in a log-linear OLS model. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates. NA indicates that the model did not converge. Castle doctrine states averaged 4.9 justifiable homicides in the year prior to enactment.

* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

to zero when allowing for state-specific time trends in column 6.

We also examine whether criminals are more likely to use guns during robberies.[53] Results in Panel C of Table III.6 indicate that there is little evidence of this type of escalation, at least once one compares states to others in their same region.[54] In short, while we find suggestive evidence of escalation by criminals, it is not conclusive.

Finally, we turn to evidence on whether the laws increase the reported number of justifiable homicides. The problem with these data is that justifiable homicides are believed to be underreported: Kleck (1988) estimates that only one-fifth of legally justified homicides by civilians are reported. Only 200 to 300 homicides are classified this way every year in the U.S., compared to around 14,000 total criminal homicides. However, even though the *level* of justifiable homicides may be underreported, *relative* changes in justifiable homicide may still be informative. As a result, we focus on examining the relative increase in reported justifiable homicide, and then estimate how many additional legally justified homicides there really are by scaling the pre-castle doctrine figure by estimates of underreporting.

Results are shown in Panels D and E of Table III.6. Panel D shows estimates from unweighted regressions in which the number of justifiable homicides is the dependent variable. Estimated effects range from 1 to 4.3 additional justifiable homicides, which is relative to a baseline average of 4.9 justifiable homicides per state in the year prior to castle doctrine

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[53] We also look at the proportion of assaults in which a gun was used and find no evidence of an increase, though the baseline rate is small (3 percent). We also note that examining these ratios as outcome variables could be problematic if the laws were found to reduce robbery or aggravated assault. However, as we show in Table III.4 there is no effect on robberies or aggravated assaults.

[54] It is difficult to think of how using other FBI classifications could help answer this question. For example, the FBI classifies some non-felony-type homicides as having originated in an argument. It is difficult to know, however, whether the argument would have resulted in serious injury to the killer, had that person not used lethal force, or if the argument escalated from, say, a fistfight into a homicide. Yet most would agree that the latter is more disturbing than the former.

enactment. The estimate in our preferred specification in column 3 is 3.2, is statistically significant at the 5 percent level, and represents a 65 percent increase.[55, 56]

Panel D reports estimates from a negative binomial model. Estimates range from an insignificant 22 percent increase to a significant 57 percent increase.

Using these estimates, we now turn to assessing whether the relative increases observed in Table III.6 can explain the entire increase in homicide, given estimates of the degree of underreporting of legally justified homicide. The largest estimated relative increase from a specification in Table III.6 that controls for region-by-year fixed effects is 70 percent, which is relative to a baseline total of 103 justifiable homicides across the 21 states in the year prior to castle doctrine enactment. We assume that i) police departments are not *less* likely to report an otherwise-identical homicide as justifiable after castle doctrine, and ii) the relative increase in legally justified homicide due to castle doctrine is no lower for reporting agencies than for non-reporting agencies. We view the first of these assumptions as likely to hold, and the second as reasonable, though we emphasize that they are in fact assumptions. Combining these assumptions with our estimates in Table III.5 suggests that the true castle-doctrine-induced relative increase in legally justified homicide across the 21 states should be no larger than 70 percent.

Kleck (1988) reports that approximately one-fifth of legally justified homicides are reported correctly, while the others are classified as (criminal) homicides. Given the 103

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[55] In contrast, we find no evidence of an increase in justifiable homicide by police, consistent with the identifying assumption. Results are shown in Table B2 of the appendix.

[56] Estimates from weighted OLS are broadly similar. Specifically, estimates corresponding to those in columns 1 through 5 of Table 6 were 9.6^{***}, 6.0^{**}, 4.6^{*}, 4.8, and 4.6^{*}, respectively, where asterisks denote statistical significance. The population-weighted baseline state average was 10.0 justifiable homicides per year.

reported pre-castle doctrine justifiable homicides, that suggests that the true figure is 515. A 70 percent increase means that castle doctrine causes an additional 361 legally justified homicides, of which 289 (80 percent) would be (mis)reported as homicides. Recall that estimates from Table III.5 indicate that castle doctrine causes approximately an 8 percent increase in homicide, which translates to an additional 611 homicides given the 7,632 pre-castle doctrine homicides. Thus, under these assumptions, our best estimate is that no more than half of the additional homicides caused by castle doctrine were legally justified.

Of course, different assumptions yield different conclusions. For example, assuming that only 10 percent of legally justified homicides are reported correctly, along with a 70 percent relative increase and the second assumption outlined above, would suggest that all of the additional homicides were legally justified.

To summarize our results, we find no evidence that strengthening self-defense law deters crime. On the other hand, we find that a primary consequence of castle doctrine laws is to increase homicide by a statistically and economically significant 7 to 10 percent. Relative increases in justifiable homicide along an estimate of the degree of underreporting suggest that it is unlikely, but not impossible, that the additional reported criminal homicides consist entirely of legally justified homicides. We emphasize, however, that one's conclusion on that issue depends on assumptions about the nature and degree of underreporting of legally justified homicides.

III.5 Conclusion

In recent years, more than 20 states have strengthened their self-defense laws by adopting castle doctrine laws. These statutes widen the scope for the justified use of lethal

force in self-defense by stating the circumstances under which self-defense is justified and removing the duty to retreat from a list of protected places outside the home. In addition, in some cases they establish a presumption of reasonable fear and remove civil liability. Thus, these laws could hypothetically deter crime or, alternatively, increase homicide.

Results presented indicate that castle doctrine law does not deter crime. Furthermore, our estimates are sufficiently precise as to rule out moderate-sized deterrence effects. Thus, while our view is that it is *a priori* reasonable to expect that strengthening self-defense law would deter crime, we find this is not the case.

More significantly, results indicate that castle doctrine laws increase total homicides by around 8 percent. Put differently, the laws induce an additional 600 homicides per year across the 21 states in our sample that enacted castle doctrine laws. This finding is robust to a wide set of difference-in-differences specifications, including region-by-year fixed effects, state-specific linear time trends, and controls for time-varying factors such as economic conditions, state welfare spending, and policing and incarceration rates. These findings provide evidence that lowering the expected cost of lethal force causes there to be more of it.

A critical question is whether all the additional homicides that were reported as murders or non-negligent manslaughters could have been legally justified. Based on the results of various tests and exercises performed here, our view it is that this is unlikely, albeit not impossible.

With respect to policy, our findings suggest that an informed debate over these laws will weigh the benefits of increased protections given to victims against the net increase in violent deaths that result. More broadly, our findings indicate that incentives and expected costs matter when it comes to the decision of whether to use lethal force.

CHAPTER IV

DOES SIMPLIFYING DIVORCE AND MARRIAGE REGISTRATION MATTER? REGRESSION DISCONTINUITY EVIDENCE FROM CHINA

IV.1 Introduction

Since the seminal works by Becker (1973, 1974), economists have studied how expected costs and benefits affect marital decision-making. Most of the empirical literature has focused on how changes in state law in the U.S. affect divorce decisions. For example, Friedberg (1998) and Wolfers (2006) find evidence that the no-fault unilateral divorce laws raised divorce rates by making it easier to divorce.[57] In this paper, I add to this literature by examining a case in China in which there were massive changes made to the ease of divorce and marriage. In doing so, I also contribute to a growing literature on the societal impacts of central policies in China (e.g., Li, Rosenzweig, and Zhang, 2010; Ebenstein , 2010; Edlund, Li, Yi, and Zhang, 2013).

Specifically, I study the short-term impact of a major policy change in China that simplified the registration process of divorce and marriage. In 2003, China adopted the Regulations on Marriage Registration (referred to as the 2003 Regulations hereafter), which replaced the old policy and made the whole registration process much more convenient. For example, under the new regulations, when a couple agree to divorce, they no longer need to request letters from someone else to prove that their marriage is truly broken. At the same time they are also free from the "lengthy and tedious" societal mediation (Zeng, 2009) in which

[57] Other examples include Gruber (2004), Stevenson and Wolfers (2006), and Stevenson (2007). See Stevenson and Wolfers (2007) for a review.

mediators from local communities strive to save broken marriages. Similarly, requesting approval letters for marriage is no longer required as well. The 2003 Regulations hence granted easier access to both divorce and marriage.

To distinguish the impact of reducing barriers to divorce and marriage, I use a modified regression discontinuity design (RDD). That is, while the typical RDD focuses on the change in the *level* of the outcome, I ask whether there was an abrupt change in the *change* in divorce or marriage rates at the cutoff year of 2003. The corresponding identifying assumption requires that annual changes in other determinants of divorce and marriage rates vary continuously around 2003 so that any discontinuity in the annual change in divorce and marriage rates in 2003 should be properly interpreted as the causal effect of the simplified divorce and marriage registration. I find this assumption to be quite plausible. First, there was little scope for couples to postpone their marriages and divorces from 2002 to 2003 for the simple reason that the 2003 Regulations were announced in early August of 2003 and became effective two months later. Consistent with this institutional background, there is significant empirical evidence supporting the identifying assumption. Graphical analysis does not show any evidence of discontinuities in a wide range of time-varying covariates, including average wage, private sector employment rate, percent of populations with at least college degree, illiteracy rate, and demographics. Along similar lines, RDD estimates are robust to controlling for these covariates. As a result, there is little reason to believe there would have been discontinuities in the growth rates of marriage or divorce in the absence of the 2003 Regulations.

Results indicate that the 2003 Regulations immediately caused divorce and marriages rates to grow at a faster rate, compared to just before 2003. RDD estimates indicate that the annual change in the divorce rate increased by one divorce per 1,000 married females and the

annual change in the marriage rate increased by nine marriages per 1,000 single females shortly after the registration process was simplified. I show that these results are robust to different choices of bandwidths and functional forms. In addition, in a falsification test I show that international divorces and marriages are largely unaffected by the 2003 Regulations. This is expected because these regulations were aimed at simplifying the registration process mainly for couples where both parties were Mainland China citizens.

Collectively, these results show that marital decision-making is very responsive to changes in expected costs and benefits. In addition, they demonstrate that central social policy in China has the scope to induce significant changes in the behavior and demographics of China.

IV.2 Background

IV.2.1 Divorce and Marriage Registration before 2003

Historically, the Chinese government closely regulated the marriage market to help maintain high levels of social harmony and stability. One important channel was by regulating the divorce and marriage registration processes through local civil affairs offices.[58] Legally, this was performed by promulgating regulations on marriage registration in accordance with China's Marriage Law.[59] Civil affairs offices issued divorce and marriage certificates provided

[58] Civil affairs offices help implement social affairs policies developed by the Ministry of Civil Affairs of People's Republic of China, which include: formulate development plan and policies for civil affairs, responsible for registration and monitor of social groups, funds, private non-enterprise units; develop disaster response policies, organize and coordinate relief efforts; development of social assistance plans and its policies and standards; responsible for works related to subsistence for urban and rural residents, medical and temporary assistance; formulation of social welfare development planning, policies and standards; developing policies for marriage, funeral and interment management and guiding related work.

[59] According to Article 1 of both the 2003 Regulations and the 1994 Regulations (Regulations on Control of Marriage Registration), the regulations are formulated "for the purposes of standardizing marriage registration, ensuring implementation of the marriage system of free choice of partners, monogamy and equality between man and woman, and protecting the lawful rights and interests of the parties to a marriage."

there were necessary documents and mutual consent of the parties. To get those documents, however, was usually not an easy or enjoyable experience, because in addition to the identity documents like residence booklets and identity cards, so-called introduction or approval letters from applicants' employers or local community committees were also required. The purpose of doing so was slightly different for divorce and marriage registration: introduction letters for divorce were required to ensure the wish to divorce was genuine, while letters for marriage were mainly to guarantee that neither party was currently married. Since requesting these letters involved outsiders in private marital decision-making, it was widely regarded as an "embarrassment" (Zhu, 2003).

Furthermore, in requesting approval letters to apply for divorce, the long tradition of "mediation" in Chinese society that aimed at resolving disputes made it even harder to divorce. "Mediation", which is synonymous with "conciliation" from a Western perspective and stems from Confucianism that honors harmony, can be traced back to the Ming dynasty (1368-1644) when village leaders and elders were encouraged to solve petty disputes within and between families (Cohen, 1966; Wall and Blum, 1991). After China formalized its mediation system in the mid-1950s, community or societal mediation service was provided in local communities (e.g., "communes" in rural areas or "streets" in the cities) by the governing unit (Wall and Blum, 1991). According to the survey study by Wall and Blum (1991), Chinese mediators were usually not neutral (e.g. often they were friends or relatives of the disputants) and their major goals were eliminating the dispute and keeping the anger down rather than using logic and emotional appeasement to help disputants reach a mutually acceptable resolution. More importantly, mediators regarded finding conflict resolutions as the responsibility for both them and the local communities so that it would feel like a loss of face if the dispute could not be

settled or moved into the court system. Specifically, Wall and Blum (1991) find that in couple disputes community mediators told disputants much more often to consider the feeling of others, to cherish harmony in their families or community, and not to undermine the respect of the neighbors or the reputation of the family. Therefore, many couples filing for divorce were persuaded and even pressured to give up on divorce ultimately.

If no party decided to withdraw the initial divorce request, the civil affairs offices should grant the divorce certificates without due delay. However, under two circumstances the divorce cases would move into the court system. First, if the local community mediation was not successful, the divorce case was referred to the local people's court as a contested divorce. Second, the case was also transferred to courts if no agreement was reached over property division and child parenting even if a divorce was granted by the local civil affairs authority. According to data used in this paper, from 1995 to 2011 courts handled 46% of the divorce cases compare to 54% by civil affairs offices.

IV.2.2. The 2003 Regulations on Marriage Registration

In response to complaints and criticism over the divorce and marriage registration requirements, Regulations on Marriage Registration (*hunyin dengji tiaoli*), the 5th version of its kind, was enacted on October 1 2003. These changes substantially streamlined the registration procedures specified in the 1994 version, Regulations on Control of Marriage Registration (*hunyin dengji guanli tiaoli*). As evidenced by removing the term "control" from the name, the civil affairs offices were hence reshaped to work more in the spirit of serving the people.

Table IV.1 lists the major changes made by the 2003 Regulations on divorce and marriage, respectively.[60] For divorce, the new regulations did not require applicants to request introduction letters from each other's working units, villagers committees, or residents committees. Only residence booklets, identity cards, marriage certificates, and a divorce agreement jointly signed by both parties were necessary. Moreover, the marriage registration office was required to issue divorce certificates on the spot after examining these certificates and materials, confirming that the desired divorce is voluntary, and finding no further disputes over children rearing, property and debt management. By comparison, the 1994 Regulations required an examination period of up to 30 days. In short, spouses who both desired to divorce no longer needed to request reference letters and were able to complete the divorce registration without delay.

Similarly, the process of marriage registration was made easier by the 2003 Regulations. First, couples only needed their residence booklets and identity cards to receive their marriage certificates, after signing a statement confirming that neither individual in the couple had a spouse and that the couple did not consist of close kin unsuitable for marriage. In contrast, the 1994 Regulations additionally required marital status certification issued by each applicant's working unit, villagers committee, or residents committee. Second, under the new regulations, pre-marital health check-ups were not mandatory anymore. As a result, couples no longer had to have check-ups in designated medical care centers and submit the check-up reports to the marriage registration authorities.

[60] Other minor changes include, for example, consolidating marriage registration authorities.

Table IV.1 Major Comparisons between the 2003 Regulations and the 1994 Regulations

	2003 Regulations	1994 Regulations
Divorce		
Need introduction letters?	No. (Article 11)	Yes. Introduction letters issued by each applicant's working unit, villagers committee, or residents committee are required, in addition to residence booklet, identity card, marriage certificate, and divorce agreement from both parties, (Article 14)
Registration completed on the spot?	Yes. The marriage registration office shall examine the certificates and certifying materials presented by the parties concerned for divorce registration and inquire about the relevant information such as whether the parties concerned desire divorce on a voluntary basis and have reached a consensus on such issues as children rearing, property and debt management. Registration shall be granted on the spot and the divorce certificates be issued. (Article 13)	No. The marriage registration authority shall examine the divorce application submitted by the parties and shall, within one month from the date of accepting the application, register the divorce, issue them a divorcecertificate and revoke their marriage certificates where the parties conformto the divorce conditions. (Article 16)
Marriage		
Need marital status certification?	No. (Article 5)	Yes. Marital status certification issued by each applicant's working unit, villagers committee or residents committee are required, in addition to residence booklet and identicy card from both parties, (Article 9)
Need mandatory pre-marital health check-up?	No. (Article 5)	Yes. In places practising pre-marital health check-ups, parties applying for marriage registration must go to the designated medical care institution for pre-marital health check-ups and present the pre-marital health check-up report to the marriage registration authority. (Article 9)

IV.3 Identification Strategy

In order to estimate the causal effect of the simplified registration process on divorce and marriage, I apply a regression discontinuity design (RDD) to exploit the abrupt change in the expected costs of getting divorced and married in 2003.[61] However, rather than estimating an RDD using the levels of marriage and divorce rates, I instead use the differences, or year-to-year changes, as the outcome variables. I do this because there are several reasons to believe that these policies would primarily affect the growth rates in marriage and divorce, rather than an immediate change in the rate levels. First, both marriage and divorce decisions depend in large part on the investments each party made previously. Those investments could not change in the very short term in response to the 2003 Regulations, though they likely will change over the longer term. This suggests that looking for a change in the growth rates of marriage and divorce is appropriate. There is also little reason to expect that all parties would become immediately aware of the policy change, or that the role of social norms associated with marriage and divorce would change immediately. Both of these factors also suggest that the long-run effect on rate levels would not be achieved immediately, and rather would take some time to achieve via higher growth in both marriage and divorce rates.

I use RDD to intuitively compare the rate of divorce or marriage changes just after relative to just before the regulation changes that took effect in 2003. Since this is essentially comparing the slope changes in divorce or marriage trajectory, this strategy is in spirit similar to the regression kink design (Card, Lee, and Pei, 2009) that focuses on the changes in marginal

[61] For detailed description of regression discontinuity design, see Imbens and Lemieux (2008) and Lee and Lemieux (2010).

effect. Moreover, as the 2003 Regulations were mandatory in all 31 provinces, the identification strategy is a sharp RD design.

The identifying assumption of this design is that annual changes in all other determinants of divorce and marriage rates vary smoothly around year 2003. Under that assumption, any discontinuity in the changes in divorce and marriage rates in 2003 is properly interpreted as the causal impact of the 2003 Regulations. This assumption appears to be reasonable in this context. For example, it was not possible for couples who would have married or divorced in 2002 to wait until 2003 since the 2003 Regulations were announced in August of 2003, and enacted two months later.[62] It is also difficult to think of why other determinants of marriage and divorce would have suddenly followed a different trajectory in 2003. Empirical evidence is also consistent with the identifying assumption: graphical evidence shows that there is no discontinuity in the annual changes in various time-varying covariates in 2003. Furthermore, I find that that the estimated effects of the 2003 Regulations are unaffected after simultaneously controlling for these covariates in a RD framework, consistent with the identifying assumption of RDD.

[62] There is evidence from various media reports showing that couples deliberately postponed their divorces and marriages after the announcement of the 2003 Regulations and *within* year 2003. Namely, couples waited to marry or divorce in the last quarter in 2003, after the State Council announced the effective date of October 1st in August, which does not bias my estimates. For example, see this media coverage on how young couples in Zhejiang Province postponed the marriage registration in August 2003 at http://www.china.com.cn/chinese/law/388452.htm.

Formally, to estimate the discontinuities in the annual changes in divorce and marriage rates at year 2003, I estimate the following equation using OLS:

$$\Delta Outcome_{it} = b_0 + b_1 2003Regulations_{it} + b_2 f(Year - 2003)_{it} + e_{it} \qquad \text{(IV.1)}$$

where *i* indexes the province and *t* indexes the year. $\Delta Outcome_{it}$ is the change in divorce or marriage rates between year *t* and year *t-1* for province *i*. $2003Regulations_{it}$ is a dummy variable that is equal to 1 if the 2003 Regulations are effective in year *t* for province *i* and 0 otherwise. $f(Year - 2003)_{it}$ is a flexible parametric or nonparametric function of the difference between *Year* (the actual year) and 2003 (the effective year of the 2003 Regulations) and is allowed to take different functional forms on either side of the cutoff. e_{it} is the idiosyncratic term. b_1 measures the effect of the 2003 Regulations on trend changes in the divorce or marriage rate, which is expected to be positive if simplified registration induces divorces or marriages to grow faster immediately.

In addition, in this regression framework I will additionally control for province fixed effects and a vector of changes in time-varying covariates, which include wage, private sector employment rate, college education, illiteracy rate, and demographics. If the identifying assumption is valid in that no determinants other than the ease of registration process experienced a trajectory change in 2003, including these control variables should not affect the estimate of b_1.

IV.4 Data

This paper uses aggregate data from 31 provincial-level divisions (provinces hereafter) to study the effect of the 2003 Regulations on divorce and marriage rates in China between 1996 and 2011.[63] Table IV.2 presents the summary statistics of outcomes and control variables.

The outcomes include annual changes in divorce and marriage rate measures. To precisely define the divorce and marriage rates, I use the refined definition rather than the crude definition because the latter simply calculates the rate as the count divided by the overall population (Friedberg, 1998).[64] Specifically, divorce rate is defined to be the number of divorces per 1,000 married females. Marriage rate is defined to be the number of marriages per 1,000 single females aged above 15. Divorce and marriage data are from *China Civil Affairs' Statistical Yearbook* (1996-2012). I obtain data on normalizing factors, the number of married and single females, from *China Statistical Yearbook* (1996-2012).[65] These yearbooks also provide information regarding domestic (both parties are Mainland China citizens) and international (only one party is Mainland China citizen) divorces/marriages. International divorces and marriages were largely not targeted by the 2003 Regulations, and thus provide the opportunity for a falsification test.

In order to test the validity of the identifying assumption of the RD design, I collect data from the National Bureau of Statistics of China on a variety of time-varying covariates

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[63] Provincial-level divisions are the highest-level administrative divisions in the People's Republic of China, which are classified into three categories: provinces (e.g., Sichuan and Guangdong), municipalities (e.g., Beijing and Shanghai), autonomous regions (e.g. Tibet and Inner Mongolia), and special administrative regions (e.g. Hong Kong and Macao). In this study, Hong Kong, Macao, and Taiwan are not included. The observations of Sichuan and Chongqing in 1996 are also excluded because Chongqing was separated from Sichuan province as a municipality in 1997 and data for these two divisions before and after 1997 are not comparable.

[64] Results are qualitatively and statistically similar to those presented in this paper when using crude definitions.

[65] Specifically, there data are computed from the population survey results and have been available since 1996 The sampling weights are generally close to 0.1% (e.g. the weight is 0.0982% for year 2003) for non-Census years and 10% for Census years (2000 and 2010).

that have been shown important in determining divorce and marriage rates. I use these covariates to examine whether the estimated discontinuities for divorce and marriage rates remain similar after controlling for these factors, which is what one would expect if the research design were valid. In particular, these covariates capture the impact of the labor market (inflation-adjusted average wage of staff and workers and employment rate in the private sector), education (percentage of population with at least college degree and illiteracy rate), and demographics (male-female ratio among populations aged above 15, percentage of populations aged between 15 and 64 among populations aged above 15, birth rate, and death rate).

IV.5 Results

IV.5.1 Divorce and Marriage Discontinuities

In this section I first examine if simplifying the divorce registration altered the trend of divorce starting in 2003. Panel A of Figure IV.1 plots the annual change in the divorce rate from 1997 to 2011, with flexible linear lines fitted on either side of the cutoff year 2003. The graphical evidence indicates a sudden discrete jump in the change in the divorce rate right after the 2003 Regulations took effect.[66] Corresponding RD estimates are shown in the first four columns of Table IV.3, using a 5-year bandwidth with both parametric and nonparametric fits. In Panel 1, I fit the data on both sides of the cutoff year with flexible linear functions. Estimates from unweighted OLS are presented in Columns 1 and 2, with the latter column additionally

[66] Figure A1 in the Appendix plots the level graphs using raw data on divorce and marriage rates, corresponding to Figure 1. For example, the discrete jump in Panel A of Figure 1 corresponds to the trajectory change in the divorce rate in Panel A of Figure A1.

Table IV.2 Descriptive Statistics

Variable	Mean	
	level	annual change
Outcome Varialbes		
divorces (per 1,000 married females)	4.867	0.228
	(2.654)	(0.577)
domestic divorces at civil affairs offices	2.854	0.269
	(2.133)	(0.453)
international divorces at civil affairs offices	0.014	0.001
	(0.031)	(0.035)
marriages (per 1,000 single females)	72.437	0.344
	(19.068)	(9.437)
domestic marriages	71.954	0.354
	(19.123)	(9.420)
international marriages	0.484	-0.010
	(0.718)	(0.248)
controls		
average wage (￥)	21,523	2,281
	(12933)	(1523)
population with at least college education (per 100,000 persons)	5,871	504
	(4793)	(1054)
employment rate in private sector (%)	0.083	0.006
	(0.051)	(0.012)
birth rate (%)	12.317	-0.290
	(3.696)	(0.810)
death rate (%)	6.093	-0.050
	(0.710)	(0.376)
male-female ratio	101.156	0.174
	(3.605)	(2.613)
illiteracy rate (%)	12.082	-0.899
	(9.225)	(2.743)
% population aged15-64	89.846	-0.085
	(2.036)	(0.900)

Notes: Each cell contains the mean with the standard deviation in the parentheses. Variables (annual change) used in the main analysis contain 465 observations, with some missing values for Chongqing, Sichuan, and Tibet.

controlling for province state fixed effects and time-varying covariates, including wage, private sector employment rate, college education, illiteracy rate, and demographics. It is important and comforting to notice that the two significant estimates, 0.894 and 0.893, are almost identical in terms of both magnitude and statistical significance, which is consistent with the identifying assumption of RDD. That finding, combined with the graphical evidence shown in Figure IV.2, suggests that as expected, there is no evidence of discontinuities in other important determinants of divorce rates.[67] In addition, when regressions are weighed using provincial population, results are still robust, as shown in Columns 3 and 4. In Panel 2 I find similarly significant results when using quadratic fits. To allow the fitting to be more flexible, I fit the data using nonparametric kernel functions and present results in Panel 3.[68] These estimates are all significant at the 1% level and lie between estimates in Panel 1 and Panel 2. Combined with graphical evidence, these results show that by making divorce registration more convenient, the 2003 Regulations caused an immediate and significant increase in the annual change in divorce rates.[69] The average of the 16 estimates in Columns 1 through 4 is about 1, indicating that immediately after the 2003 Regulations became effective the annual change in the divorce rate increased by one more divorce for every 1,000 married females relative to similar change in the previous year. This implies that the average annual increase

[67] None of the discontinuities of time-varying covariates in Figure 2 are statistically significant except for wage. But this discontinuity is negligible compared to the discontinuity in the annual change in the divorce rate in 2003, as evidenced by the fact that the estimated coefficient of wage in the specification corresponding to Column 2 of Table 3 is only -0.0000112 (s.e.= 0.0000250).

[68] I use the estimating procedure "rd" for Stata by Austin (2011), with the choice of a triangle kernel.

[69] Estimates are almost identical when including province-specific linear time trends. For example, the corresponding estimate for 0.894 (s.e. = 0.117) in Column 1 of Panel 1 in Table 3 is 0.895 (s.e. = 0.123).

in the number of divorces after 2003 was 300,000 to 400,000 higher than it would have been in the absence of the policy change.[70]

Now I move to the effect on marriages. Panel B of Figure IV.1 shows a similarly positive discontinuity in the change in the marriage rate at year 2003, which is confirmed by the positive and significant RD estimates in the last four columns in Table IV.3. The average of these estimates is 8.75, suggesting that the 2003 Regulations caused the annual change in the number of marriages to be around one million more in 2003 relative to 2002.

These findings provide compelling evidence that reducing the cost of divorce and marriage through simplified registration process immediately resulted in faster growth in divorce and marriage rates. It is also important to emphasize that these regression discontinuity estimates represent the instantaneous change, rather than the long-run effect. Thus, while we would expect divorce and marriage rates to eventually reach their new steady states, we would not necessarily expect the new equilibria to be achieved in the short time period examined here.[71]

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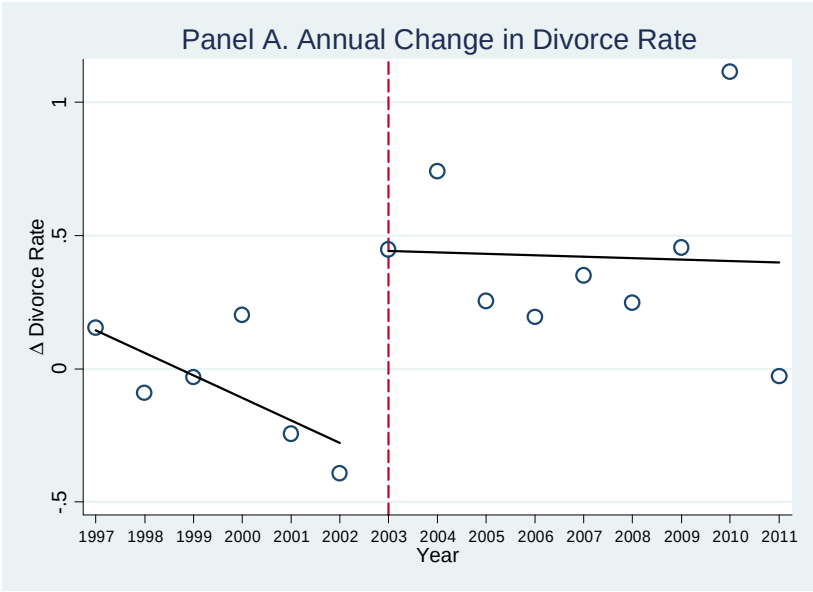
[70] Ideally I would be able to determine if the increase in the growth rate was due to an increase in the dissolution of previously unhappy marriages, or to an increase in divorce among those who married after those. Unfortunately, I was unable to acquire the individual-level data required to answer this question definitively in China.

[71] It is also interesting to understand the effect of the 2003 Regulations in the medium run. One possible way to credibly do so is to make use of cross-province variations in exposure to the policy, like what Friedberg (1998) and Wolfers (2006) did to compare differences in divorce rates between states that pass unilateral divorce laws and states that do not. However, it is difficult in this case because the 2003 Regulations are effective nationwide and also because it is not easy to measure how provinces are differentially exposed to this new policy.



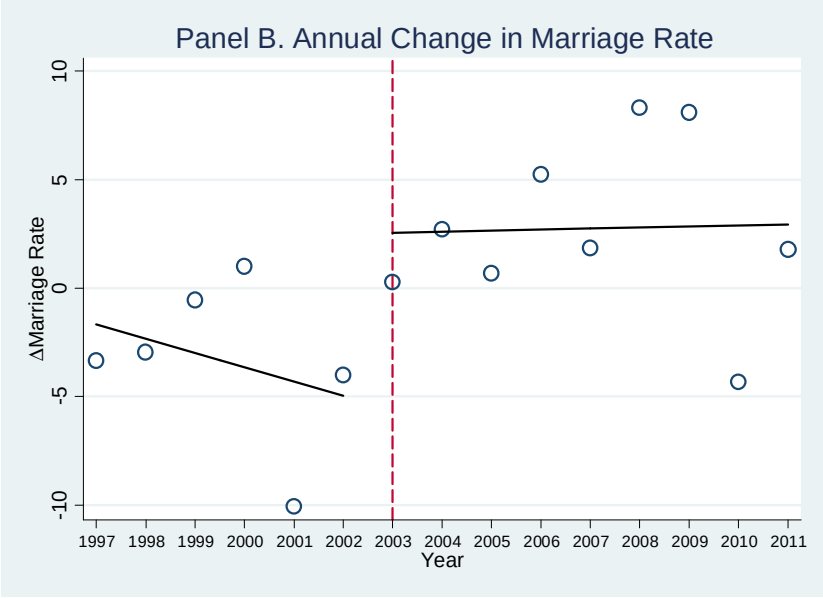


Figure IV.1 Regression Discontinuity Estimates of the 2003 Regulations on Divorce and Marriage Rates

Table IV.3 Regression Discontinuity Estimates of the 2003 Regulations on Annual Changes in Divorce and Marriage Rates

	Divorce				Marriage			
	Unweighted OLS		Weighted OLS		Unweighted OLS		Weighted OLS	
	1	2	3	4	5	6	7	8
Panel 1. Linear Fit								
Discontinuity at Year 2003	0.894***	0.893***	0.733***	0.742***	7.008***	7.372***	7.213***	7.494***
	(0.117)	(0.120)	(0.0990)	(0.108)	(1.721)	(1.766)	(2.189)	(2.143)
Observations	338	338	338	338	339	339	339	339
Panel 2. Quadratic Fit								
Discontinuity at Year 2003	1.473***	1.387***	1.194***	1.033***	10.43***	11.34***	8.885**	10.70**
	(0.223)	(0.239)	(0.189)	(0.207)	(2.984)	(3.401)	(3.841)	(4.486)
Observations	338	338	338	338	339	339	339	339
Panel 3. Nonparametric Fit								
Discontinuity at Year 2003	1.130***	1.086***	0.919***	0.812***	8.573***	9.728***	8.190***	9.799***
	(0.148)	(0.164)	(0.126)	(0.144)	(1.854)	(1.978)	(2.385)	(2.654)
Observations	459	459	459	459	461	461	461	461
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table reports regression discontinuity estimates using a 5-year bandwidth. Each column in each panel represents a separate regression. The unit of observation is province-year. Weighted OLS uses the number of married females as the weight in Columns 3 and 4, and uses the number of single females as the weight in Columns 7 and 8 as the weight. Controls contain province fixed effects and time-varying covariations, including wage, private sector employment rate, college education, illiteracy rate, and demographics.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

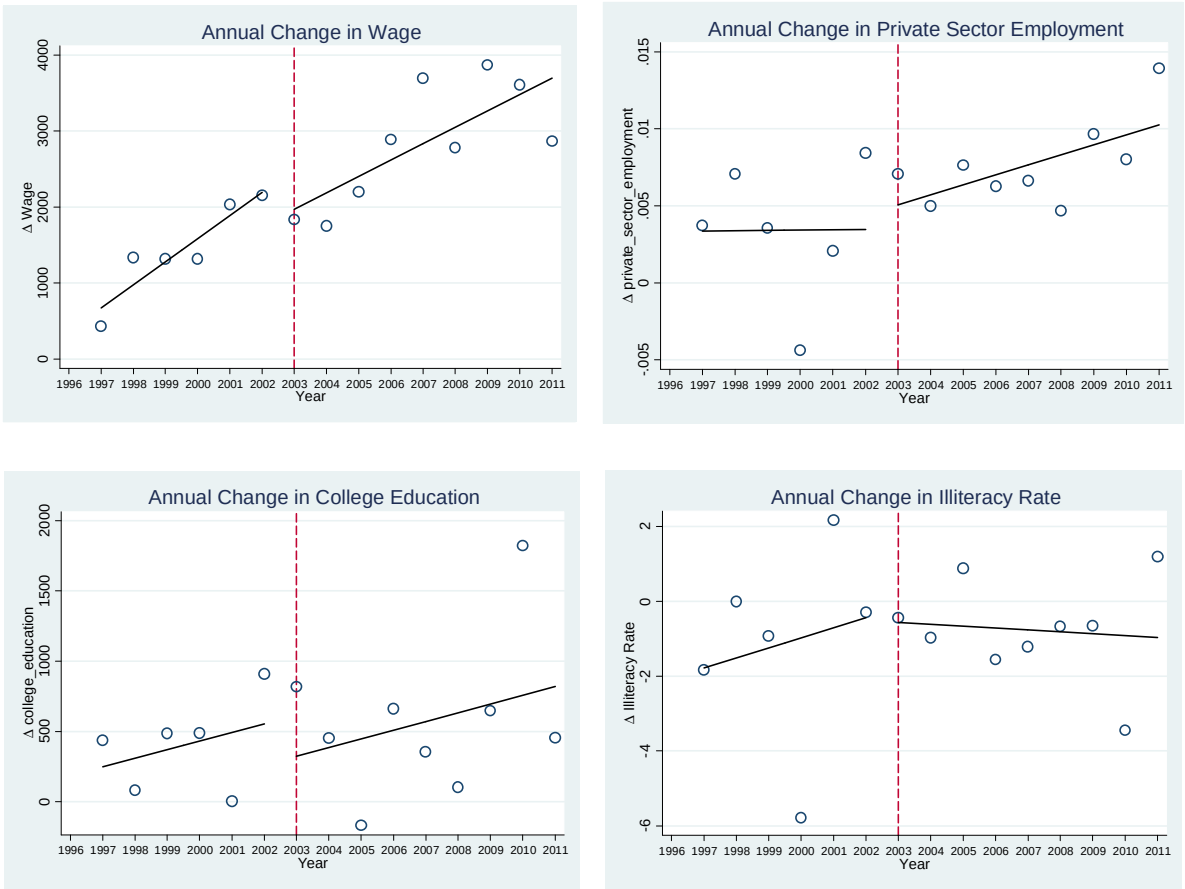


Figure IV.2 Annual Change in Time-varying Covariates

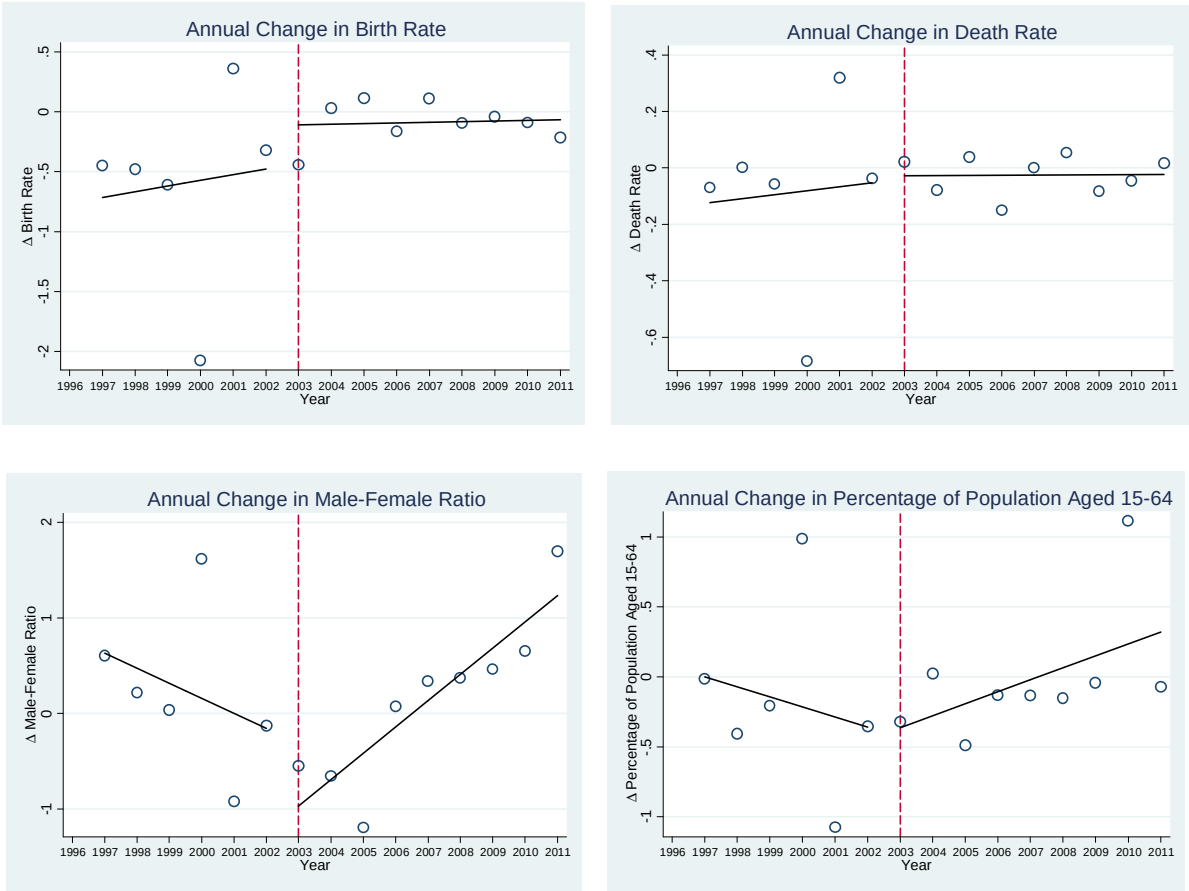


Figure IV.2 Continued

IV.5.2 Additional Checks

In this section I provide more evidence that my results are robust and represent the causal impact of the 2003 Regulations on marriage and divorce. First, I test the robustness of the main results to different bandwidths, different functional forms, and to controls. Results using unweighted OLS are presented in Table IV.4; weighted results are similar and not reported. Columns 1 through 8 progressively use bandwidths of 4, 6, 7, and 8 years, in which even-numbered columns include controls (province fixed effects and time-varying covariates) and odd-numbered columns do not. As before, I apply both parametric and nonparametric specifications to fit the data. Estimates in Table IV.4 show that the estimated effects of the 2003 Regulations on divorce and marriage are robust as they are generally similar to those in Table 3 in both magnitude and statistical significance.

Next, I perform a falsification test by comparing the effects of the 2003 Regulations on domestic and international divorce/marriage rates.[72] Since the registration reform mainly targeted divorces and marriages in which both parties were residents in Mainland China, I expect to see more meaningful effects on domestic divorce and marriage outcomes. Though the 2003 Regulations also streamlined the registration process for international divorces and marriages by consolidating related registration authorities, these changes were not comparable to the massive changes to domestic divorces and marriages described earlier. Therefore, the effects on international divorces and marriages, if any, should be at most secondary. Estimates in Table IV.5 confirm this hypothesis: while estimates regarding *domestic* divorces and

[72] An international divorce/marriage means that it involves a Mainland China citizen and a foreigner, or resident from Hong Kong, Macao, and Taiwan.

Table IV.4 Regression Discontinuity Estimates for Different Bandwidths and Specifications

Bandwidth	4 years		6 years		7 years		8 years	
	1	2	3	4	5	6	7	8
				Annual Change in Divorce Rate				
Panel 1. Linear Fit								
Discontinuity at Year 2003	1.044***	1.005***	0.843***	0.844***	0.700***	0.718***	0.807***	0.818***
	(0.134)	(0.143)	(0.104)	(0.105)	(0.106)	(0.111)	(0.102)	(0.110)
Observations	279	279	397	397	428	428	459	459
Panel 2. Quadratic Fit								
Discontinuity at Year 2003	1.548***	1.466***	1.222***	1.167***	1.294***	1.269***	1.117***	1.120***
	(0.279)	(0.300)	(0.187)	(0.195)	(0.188)	(0.207)	(0.185)	(0.207)
Observations	279	279	397	397	428	428	459	459
Panel 3. Nonparametric Fit								
Discontinuity at Year 2003	1.252***	1.208***	1.017***	0.979***	0.950***	0.924***	0.877***	0.854***
	(0.179)	(0.220)	(0.130)	(0.135)	(0.116)	(0.119)	(0.112)	(0.115)
Observations	459	459	459	459	459	459	459	459
				Annual Change in Marriage Rate				
Panel 4. Linear Fit								
Discontinuity at Year 2003	9.765***	10.41***	5.706***	6.385***	7.929***	8.087***	8.215***	8.390***
	(1.858)	(1.959)	(1.617)	(1.651)	(1.654)	(1.737)	(1.622)	(1.669)
Observations	279	279	399	399	430	430	461	461
Panel 5. Quadratic Fit								
Discontinuity at Year 2003	3.398	6.246	10.71***	11.54***	8.801***	10.20***	9.477***	9.911***
	(4.035)	(4.729)	(2.480)	(2.579)	(2.491)	(2.682)	(2.472)	(2.668)
Observations	279	279	399	399	430	430	461	461
Panel 6. Nonparametric Fit								
Discontinuity at Year 2003	7.002***	8.567***	7.868***	8.423***	6.994***	7.512***	7.314***	7.822***
	(2.252)	(2.736)	(1.684)	(1.702)	(1.580)	(1.595)	(1.564)	(1.597)
Observations	461	461	461	461	461	461	461	461
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table reports regression discontinuity estimates with different bandwidths. Each column in each panel represents a separate regression. The unit of observation is province-year. Controls contain province fixed effects and time-varying covariations, including wage, private sector employment rate, college education, illiteracy rate, and demographics.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table IV. 5 Effects of the 2003 Regulations on Annual Changes in Domestic and International Divorce and Marriage Rates

	Domestic		International	
	Divorce (civil affairs offices)	Marriage	Divorce (civil affairs offices)	Marriage
	1	2	3	4
Panel 1. Linear Fit				
Discontinuity at Year 2003	0.543***	7.339***	-0.00415	-0.0248
	(0.0741)	(1.742)	(0.00478)	(0.0622)
Observations	341	339	341	339
Panel 2. Quadratic Fit				
Discontinuity at Year 2003	0.488***	10.88***	-0.00653	0.274***
	(0.120)	(3.378)	(0.00642)	(0.0904)
Observations	341	339	341	339
Panel 3. Nonparametric Fit				
Discontinuity at Year 2003	0.517***	9.326***	-0.00521	0.0989
	(0.0861)	(1.939)	(0.00450)	(0.0613)
Observations	463	461	463	461
Controls	Yes	Yes	Yes	Yes

Notes: This table reports regression discontinuity estimates using a 5-year bandwidth. Each column in each panel represents a separate regression. The unit of observation is province-year. Controls contain province fixed effects and time-varying covariations, including wage, private sector employment rate, college education, illiteracy rate, and demographics.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

marriages are all positive and significant at the 1% level; estimates related to *international* divorces and marriages are generally close to 0 and insignificant.[73] This suggests that the discontinuous change in the growth rates of domestic marriage and divorce was in fact caused by the 2003 Regulations, rather than by some other factor that affected all marital decisions more generally.

IV.6 Conclusion

This paper studies the effect of easier access to divorce and marriage on marital decision-making by examining a major policy change in China's marriage market where the divorce and marriage registration procedures were greatly simplified. I use a regression discontinuity design to credibly estimate the short run effect on the growth of divorce and marriage rates. I find that the simplified registration process immediately triggered divorce and marriage rates to grow faster. These findings provide evidence that reducing the cost of divorce and marriage leads to more of both, and more generally demonstrate the responsiveness of individuals' marital decisions to economic costs and benefits.

[73] The domestic-international breakdown for divorce is only available for divorce at civil affair offices, but not for overall divorce.

CHAPTER V

CONCLUSIONS

This dissertation evaluates the impacts of three public policies by empirically examining corresponding behavioral responses. The overall results provide strong evidence that these policies significantly alter behavior by changing associated expected costs.

In analyzing the effects of cell phone bans on driver behavior, I find drivers' cell phone use is very responsive: texting bans reduce visible texting while driving by around 60% and handheld bans reduce the probability of talking on handheld cell phones while driving by around 50%. In addition, cell phone bans seem to have a larger effect on adult drivers and drivers accompanied by passengers. I also replicate findings from previous literature and confirm that cell phone bans have no meaningful impacts on traffic accidents, which is also true for subgroups whose behavior is mostly affected by these bans. These seemingly counterintuitive findings lead me to discuss two possible behavioral responses that could explain the ineffectiveness of cell phone bans on accidents and casualties: one is compensating behavior that induces safer driving while using cell phones; the other is offsetting behavior that increases accidents and casualties through more hidden cell phone usage. This suggests improving the effectiveness of cell phone bans in reducing accidents is considerably more complex than merely improving enforcement. More importantly, the results have important social welfare implications: cell phone bans impose significant social costs on drivers by preventing drivers from using cell phones while driving but do not yield the intended social benefits in terms of reductions in traffic accidents and casualties.

In the second paper, I find castle doctrine laws increase homicides by 8 percent through lowering the expected legal cost associated with using lethal force in self-defense situations. This is equivalent to about 600 additional homicides per year in the 21 states that adopted castle doctrine laws. Meanwhile, I do not find these laws deter burglary, robbery, or aggravated assault, though these laws also increase the expected cost of using lethal force and could potentially have a deterrence effect. Collectively, these findings suggest the primary downside of castle doctrine laws is the increased homicides, while there is no hidden spillover effect to society at large in terms of deterring other violent crimes. This paper also suggests that an informed debate over these laws will weigh the benefits of increased protections given to victims against the net increase in violent deaths that result.

The third paper examines the short-run impact on marital outcomes of China's new marriage registration regulations, which greatly simplifies the divorce and marriage registration process. I find discontinuous increase in annual changes in divorce and marriage rates in 2003, which indicate the simplified process immediately triggered divorce and marriage rates to grow faster. Importantly, I do not find similar discontinuities in changes in other important determinants of divorce and marriage rates. This study also sheds light on understanding government intervention in private markets.

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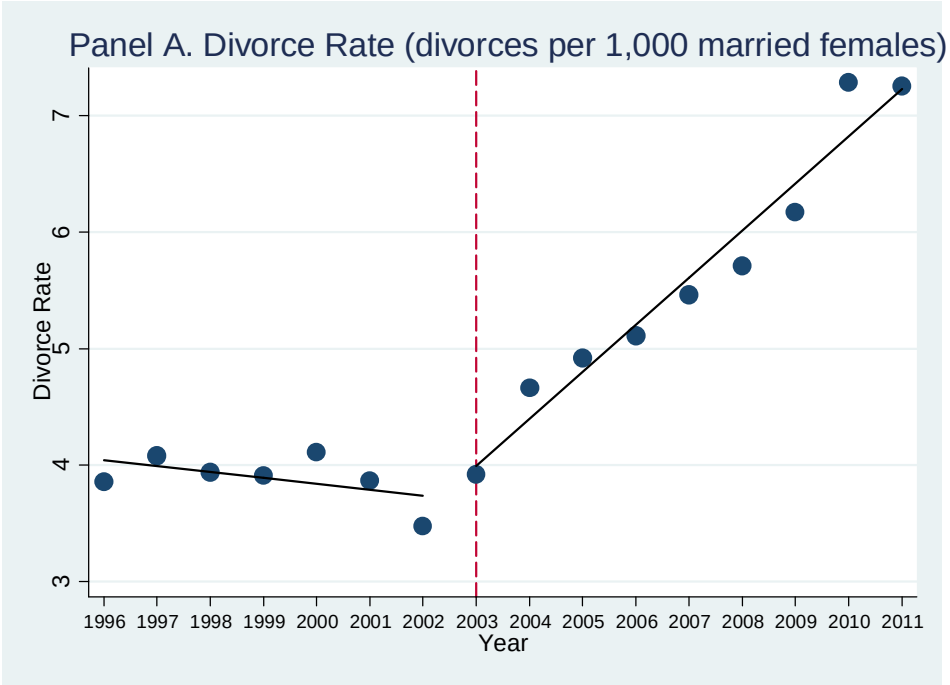
APPENDIX A

FIGURES



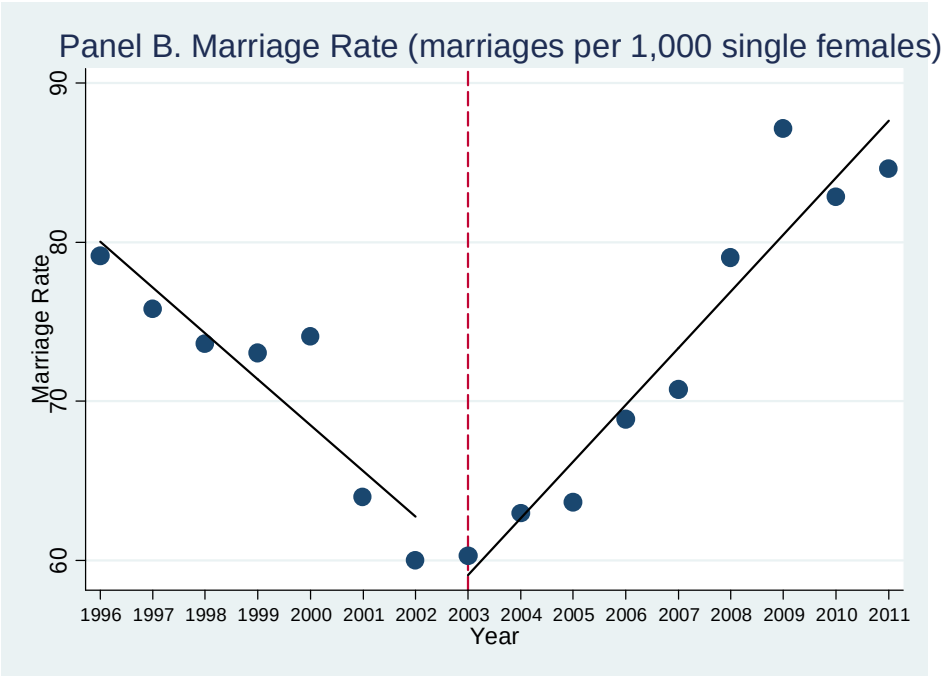


Figure A1 Divorce Rate and Marriage Rate in China (1996-2011)

APPENDIX B

TABLES

Table B1 Differential Effects of Castle Doctrine Law by Treatment of Duty to Retreat and Civil Liability

Panel A: Differential Effects by Previous Treatment of Duty to Retreat in Case Law	Log Burglary Rate	Log Robbery Rate	Log Aggravated Assault Rate	Log Homicide Rate	Proportion of Robberies with a Gun	# Justifiable Homicide by Private Citizens
Castle doctrine law in states with case law classified as requiring duty to retreat	0.0195	0.0173	0.0315	0.117***	0.0196	7.976***
	(0.0265)	(0.0256)	(0.0358)	(0.0355)	(0.0156)	(2.898)
Castle doctrine law in states with case law classified as not requiring duty to retreat	0.0271	0.0416	0.0471	0.0529	0.0173	-1.457
	(0.0305)	(0.0323)	(0.0423)	(0.0352)	(0.0259)	(3.368)
Observations	550	550	550	550	544	550
Panel B: Effect of Castle Doctrine Law That Extends to Any Place One Has a Legal Right to Be	Log Burglary Rate	Log Robbery Rate	Log Aggravated Assault Rate	Log Homicide Rate	Proportion of Robberies with a Gun	# Justifiable Homicide by Private Citizens
Castle doctrine law that removes duty to retreat in any place one has a legal right to be	0.0189	0.0219	0.0293	0.0814***	0.0212	5.597**
	(0.0252)	(0.0264)	(0.0330)	(0.0269)	(0.0174)	(2.736)
Observations	506	506	506	506	500	506
Panel C: Differential Effects by Whether the Law Includes a Presumption of Reasonable Fear	Log Burglary Rate	Log Robbery Rate	Log Aggravated Assault Rate	Log Homicide Rate	Proportion of Robberies with a Gun	# Justifiable Homicide by Private Citizens
Castle doctrine law that includes a presumption of reasonable fear	-0.00335	0.0169	0.0228	0.0959***	0.0192	6.970**
	(0.0278)	(0.0269)	(0.0322)	(0.0288)	(0.0137)	(3.208)
Other castle doctrine law	0.0646***	0.0415	0.0610	0.0900*	0.0178	0.651
	(0.0215)	(0.0272)	(0.0495)	(0.0524)	(0.0272)	(2.750)
Observations	550	550	550	550	544	550
Panel D: Effect of Castle Doctrine Law, Excluding States That Did Not Also Remove Civil Liability	Log Burglary Rate	Log Robbery Rate	Log Aggravated Assault Rate	Log Homicide Rate	Proportion of Robberies with a Gun	# Justifiable Homicide by Private Citizens
Castle doctrine law that removes civil liability	0.0218	0.0266	0.0339	0.0844***	0.0203	4.686*
	(0.0223)	(0.0235)	(0.0315)	(0.0290)	(0.0152)	(2.658)
Observations	517	517	517	517	511	517
State and Region-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column in each panel represents a regression, each of which is weighted by state population. Robust standard errors are clustered at the state level. The unit of observation is state-year. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. States classified by Koons (2006) as previously having case law or statute requiring duty to retreat in at least some circumstances include Alabama, Alaska, Florida, Louisiana, Michigan, Missouri, North Dakota, Ohio, South Carolina, South Dakota, and Texas.

* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table B2 Justifiable Homicide by Police

Panel A: OLS - Weighted, Dep. Variable = Count						
Castle Doctrine Law	8.963*	2.770	1.252	-0.0162	1.182	1.129
	(4.501)	(2.829)	(2.600)	(2.834)	(2.643)	(2.878)
0 to 2 years before adoption of castle doctrine law				-2.692***		
				(0.785)		
Observations	550	550	550	550	550	550
Panel B: OLS - Unweighted, Dep. Variable = Count						
Castle Doctrine Law	1.726	-0.244	-0.415	-0.858	-0.380	-0.352
	(1.836)	(1.423)	(1.372)	(1.458)	(1.374)	(1.628)
0 to 2 years before adoption of castle doctrine law				-1.065		
				(0.695)		
Observations	550	550	550	550	550	550
Panel C: Negative Binomial - Unweighted						
Castle Doctrine Law	0.0328	-0.204**	-0.208*	-0.296***	-0.193*	-0.0751
	(0.164)	(0.101)	(0.107)	(0.113)	(0.104)	(0.144)
0 to 2 years before adoption of castle doctrine law				-0.204**		
				(0.0834)		
Observations	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes	
State-Specific Linear Time Trends						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level