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Essays on Firearm Sales

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy

in

Management

by

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2022

The Dissertation of Jessica Jumee Kim is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

University of California San Diego

2022

DEDICATION

I dedicate this dissertation to my loving and supportive parents.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

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Chapter 2, contains unpublished material. The dissertation author was the solo investigator and author of this material.

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FIELDS OF STUDY

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ABSTRACT OF THE DISSERTATION

Essays on Firearm Sales

by

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Doctor of Philosophy in Management

University of California San Diego, 2022

Professor Kenneth C. Wilbur, Chair

This dissertation comprises two papers examining an understudied topic in marketing: firearm sales. Firearm sales and acquisition data are not easily obtainable, but there is a pressing need for data to study firearm policy. Chapter 1 examines candidate proxy measures to measure legal prevalence when prevalence data is not readily available. Chapter 2 examines the role of loosening firearm usage restrictions on firearm sales and public health-related outcomes including suicides and crime.

Chapter 1 introduces Legal Firearm Prevalence (LFP), a direct behavioral measure based on the population of firearm licensees in Massachusetts that captures the proportion of the population who legally own or access a firearm. We argue that LFP can help evaluate firearm

sales and usage restrictions. LFP is not directly measurable in most firearm markets, so we test candidate proxies for Legal Firearm Prevalence in several common research designs, finding that firearm acquisitions is the best proxy in every research design tested. We update the classic study of guns and crime by Cook and Ludwig (2006), finding that choosing an invalid proxy can lead to false research conclusions. We recommend systematic collection and reporting of firearm acquisition data to improve firearm research and inform firearm policy.

Chapter 2 evaluates three frequent firearm usage policy changes: Concealed Carry Shall Issue (removes local authority discretion and mandates permit issuance when basic requirements are met), Permitless Concealed Carry (permit removal), and Stand Your Ground (legal protection of self-defensive use of firearms in public). I construct a unique state/month panel dataset from 2010 to 2017 and measure sales directly with sales data from an online retail platform and the federal background checks database. Using a difference-in-differences research design with staggered law adoption timing, I estimate the average policy effects on firearm sales, suicides, aggravated assaults, burglary, accidental firearm deaths, and mass shootings. The findings show that Concealed Carry Shall Issue laws increase handgun online sales and handgun background checks, while reducing that of long guns. Permitless Concealed Carry laws increase handgun background checks, firearm suicides and accidental firearm deaths. There is weak evidence of Stand Your Ground laws changing the studied outcomes.

Chapter 1

Proxies for Legal Firearm Prevalence

Governments regulate acquisition and use of potentially harmful products, including firearms, alcohol, tobacco, explosives and others. Acquisition policies define legal sales by allowing or prohibiting buyers, sellers and products. Governments also deter illegal sales and use through penalties and enforcement. For example, firearm acquisition policies include buyer age minimums, background checks and waiting periods; seller license requirements; and product bans on new machine guns, assault weapons and low-quality handguns. Usage policies include concealed carry, open carry and self-defense laws. Deterrence policies include detection, prosecution and punishment of illegal firearm acquisitions.[1]

Buyer and seller behavior interact with government policies to define legal and illegal product markets. We distinguish between three potentially measurable constructs in the firearm context. Total Firearm Prevalence (TFP) is the proportion of the population with access to a firearm. Legal Firearm Prevalence (LFP) is the proportion of the population with legal access to a firearm. Illegal Firearm Prevalence (IFP) is the proportion of the population with illegal access to a firearm. By definition, $LFP + IFP \geq TFP$.

TFP, LFP and IFP are difficult to measure in the United States. We presume that illegal firearm buyers or sellers would conceal IFP to evade detection and punishment. IFP is interesting

[1] Castillo-Carniglia et al. (2018) document qualitative differences between firearm acquisition and illegal firearm deterrence policies. Law enforcement officials in two states announced that they would not enforce new Comprehensive Background Check regulations that had been passed by state legislatures.

but we do not study it. We focus instead on LFP, which by itself is difficult to measure.[2] We are unable to find any direct measure of LFP in prior literature.

Researchers urgently need data to study firearm acquisition and usage policy effects on LFP, among other outcomes. A comprehensive research synthesis by Smart et al. (2020a) concludes that "current efforts to craft legislation related to guns are hampered by a paucity of reliable information about the effects of such policies." The stakes are high. Federal Bureau of Investigation data show that firearm murders increased faster in 2020 than any previous year on record, up 29% from 10,537 in 2019 to 13,620 in 2020. Gun Violence Archive data show that incidents with four or more gunshot victims increased 127% over six years, from 269 in 2014 to 611 in 2020. US Centers for Disease Control data show that firearm suicides increased 28% over ten years, from 18,735 in 2009 to 23,941 in 2019. Opinion surveys show that beliefs about firearm policy effects diverge along political lines: self-identified Democrats and Democratic-leaning independents favor stricter gun laws, whereas Republicans and Republican-leaning independents favor looser gun laws; both groups tend to predict their preferred policy would reduce crime (Research, 2021).

Prior research has seldom distinguished between TFP, LFP and IFP. Instead, lacking data, researchers have typically used proxies to stand in for TFP when estimating policy effects. The most frequent proxy for TFP is Firearm Suicides divided by Suicides (FSS), also known as Percent of Suicides committed with Guns (PSG).[3] FSS measurements depend on mortality data from state vital registration offices, so FSS is comparable across geography and time. Kleck (2015) found that 12 of the 19 studies of guns and crime published since 2000 used FSS as a proxy for firearm prevalence.

In this paper we introduce Legal Firearm Prevalence (LFP), a direct behavioral measure based on the population of firearm licensees in Massachusetts. We focus on LFP because firearm

[2]In fact, the Firearm Owners Protection Act of 1986 restricts the U.S. Treasury Department from "the establishment of any system of registration of firearms, firearm owners, or firearm transactions."

[3]FSS was introduced by Cook (1979) and subsequently validated by two highly cited studies (Azrael, Cook, & Miller, 2004; Kleck, 2004) as the highest cross-sectional correlate of survey measures of firearm prevalence.

acquisition policies directly influence LFP by defining legal markets for guns. Additionally, firearm use policies may motivate or demotivate legal firearm acquisitions. Valid proxies for LFP are needed to better interpret extant research and to enable policy evaluations of how firearm acquisition and usage policies affect LFP in jurisdictions where sales and prevalence data are not available.

We evaluate proxies for LFP in several common research designs, including cross-sectional and intertemporal correlations, county/year panel regressions, and county/month panel regressions. More specifically, we seek to answer the following questions:

1. How well do FSS and other suicide-based proxies explain LFP, based on Massachusetts data from 2010-2017?
2. How well do retail-based proxies explain LFP? We evaluate three candidate proxies: the population of legally registered firearm acquisitions; the population of firearm sales on a leading online platform; and the population of federally licensed firearm retailers. Guns are durable goods with an estimated 393 million firearms in circulation in the U.S. (Small Arms Survey, 2018), so it is an empirical question how strongly firearm acquisitions or other contemporaneous retail proxies would correlate with LFP.
3. How do state/month FBI background check data compare to LFP and candidate proxies?
4. How similar or different are LFP and proxy estimates in applied research? We repurpose the classic design of Cook and Ludwig (2006) and compare LFP's effect on homicide to proxy estimates.

The main findings are as follows. Firearm acquisitions are the best proxy for LFP in all research designs tested. Online sales also perform well in most designs. FSS serves as a good proxy for LFP in cross-sectional designs but not in intertemporal or panel designs. FBI data correlate highly with LFP across time at 0.41, but are not available at the county level and therefore cannot be tested in cross-county research designs. Finally, the Cook and Ludwig (2006) exercise shows that choosing the wrong proxy may cause a Type I error by underestimating parameter uncertainty.

We undertake these analyses with three specific goals. First, we hope that the results will help policymakers to evaluate the degree to which published firearm policy evidence which proxies for TFP may also apply to LFP. Our findings indicate that cross-sectional research that uses FSS likely proxies well for LFP, whereas intertemporal or panel research designs do not. Second, we hope that the results can help to inform future researchers in their selection of proxies for LFP. Third, we hope that the results can help motivate policymakers to collect and publish privacy-compliant measurements of LFP or firearm acquisitions, as that would enable more and better firearm policy-relevant evidence. We discuss several specific actions that are currently available to various policymakers.

Before proceeding, we specify five important caveats to minimize any potential misunderstandings. First, we focus on Massachusetts data because it was the only state that made the requisite LFP information available. Second, we do not claim to estimate causal effects; all findings are descriptive or correlational in nature. The focus of the current article is measurement, not identification.[4] Third, we do not claim to measure or study IFP or TFP. Those topics interest us, and certainly may be affected by firearm acquisition and usage policies, but they are not the subject of this paper. Fourth, we claim no ability to use LFP to evaluate illegal firearm deterrence policies. We suspect IFP or TFP proxies would be needed to evaluate deterrence policy effects. Fifth, we do not advocate for particular firearm acquisition or usage policies; our goal is to facilitate empirical evaluations of firearm policy effects on LFP.

The next section provides the paper's intended contributions. After that, we introduce and describe LFP. The following two sections describe the candidate retail-based and suicide-based proxy variables and compare them to LFP in six common research designs. Subsequently, we apply the research design of Cook and Ludwig (2006) to recent data to compare LFP and proxy estimates, holding data and methods constant. The paper concludes with implications for

[4]Causal effects of firearm acquisitions are interesting and important, e.g. Donohue, Aneja, and Weber (2019).

researchers and policy makers and the limitations of the exercise.

1.0.1 Relevance and Intended Contributions

We take an expansive view of the links between policy, quantitative marketing and economics. Any empirical economic analysis of regulated markets must condition on the acquisition policies that define legal transactions in those markets. Marketers operating in regulated markets can influence policy through a variety of actions, including directly via lobbying, or indirectly via demarketing, public relations or product design.

We think the findings make several contributions to quantitative marketing and economics. Evaluating durable goods' market potential requires estimation of market saturation, which in turn requires estimation of durable goods' served available market. To our knowledge, this is the first research to show that durable good acquisitions reliably indicate overall levels of a durable good's penetration. Prevalence and sales are directly related by construction, yet prevalence may also be affected by migration, divestiture or death. Additionally, collectors' purchases may increase sales without increasing overall prevalence. Surprisingly, we find that the number of product retailers does not reliably predict durable good prevalence. We further believe this is the first study of archival firearm purchase data in the marketing literature. Finally, some of the methods we employ might help to guide proxy evaluation in other marketing contexts.

More broadly, we hope to contribute to the policy evaluation literature that spans several fields, including economics, marketing, medicine, public health and public policy. We offer the first evaluation of proxies for LFP, as opposed to TFP, and introduce two new retail-based proxies for LFP. We also find that FSS is highly correlated with LFP cross-sectionally, but changes in FSS do not reliably indicate changes in LFP. This latter finding may be relevant to several dozen published studies which have used FSS as a proxy for TFP in cross-temporal research designs. Finally, we provide the first assessments of FBI background checks as a proxy for LFP.

1.1 Legal Firearm Prevalence

We first discuss firearm prevalence measures and proxies in previous studies, then introduce and describe Legal Firearm Prevalence (LFP). An important distinction is that existing firearm proxies were intended to indicate Total Firearm Prevalence (TFP), which combines legal firearm prevalence with illegal firearm prevalence.

1.1.1 Extant Firearm Prevalence Measures and Proxies

Azrael, Cook, and Miller (2004) and Kleck (2004) compared candidate proxies for TFP to surveys of firearm prevalence. The two surveys used were the General Social Survey (GSS) and the Behavioral Risk Factor Surveillance System Survey (BRFSS). GSS is a biennial national interview-based survey with a response rate of 77% (Kleck, 2004). However, the high cost of the interview format limited the national sample size to about 3,000 respondents per year (Azrael, Cook, & Miller, 2004), too few to reliably estimate firearm prevalence at the state level. BRFSS is a telephone survey conducted by state health departments with a median response rate of 67% and a median of 2,061 respondents per state per year (Azrael, Cook, & Miller, 2004; Powell, Jacklin, Nelson, & Bland, 1998).

One might ask whether survey data can measure firearm prevalence directly, without need for a proxy. Most literature has not taken that approach. Surveys often feature small samples, design inconsistencies, and insufficient frequency to estimate granular studies of firearm policy effects. Further, firearm surveys may feature nonrandom response rates, question ambiguities, and respondent misreporting, considering the political and personal sensitivity of the topic.

Prior research has used a variety of behavioral proxies for TFP. Duggan (2001a) uses *Guns & Ammo* magazine circulation data to proxy for firearm prevalence. Azrael, Cook, and Miller (2004) and Kleck (2004) evaluated a range of firearm proxies including concealed carry permits, NRA memberships, crime and arrest data, unintentional firearm deaths, outdoor magazine subscriptions and federal firearm licensees. Lang (2013), Briggs and Tabarrok (2014) and Vitt, McQuoid, Moore, and Sawyer (2018) use FBI background checks as a proxy for prevalence. Kovandzic, Schaffer, and Kleck (2011, 2013) use outdoor sports magazines subscriptions, percentage of those voting Republican in the 1988 presidential election, and numbers of military veterans as instruments for their proxy FSS.[5]

A variety of earlier papers have studied firearm license data. Krug (1967), Stolzenberg and D'alessio (2000), and Haas, Jarvis, Jefferis, and Turley (2007) interpreted concealed carry permits as a proxy for firearm prevalence. Fisher (1976) uses data on handgun licenses and registrations in Detroit to measure firearm availability. Bordua (1986) uses Firearm Owners Identification Cards in Illinois to measure firearm prevalence. Sloan et al. (1988) use concealed-weapons permits issued in Seattle and restricted-weapons permits in Vancouver in their comparison of crime data between the two regions. McDowall (1991) uses handgun purchase licenses issued in Detroit, in addition to FSS. Moody et al. (2010) and Bangalore and Messerli (2013) use gun registration rates to proxy for gun ownership across nations. Hunting licenses have been used to proxy for firearm prevalence and augment other firearm proxies (Andrés & Hempstead, 2011; Kleck, 2004; Siegel, Negussie, et al., 2014; Siegel, Ross, & King, 2014). Unfortunately, hunting license data are often limited by locally imposed quotas and "therefore may have limited use in identifying changes in firearm ownership over time." (Schell, Peterson, et al., 2020, p. 17) Hunting license quotas may be more related to local animal populations than changes in firearm prevalence. Although firearm licenses have been studied previously, we do not know of any prior work that distinguished TFP from LFP or evaluated proxies for LFP.

Kleck (2015) documents the extensive use of proxies for firearm prevalence in the literature studying links between guns and crime. He counts 19 proxies used by 41 published studies, but concludes that "none of the proxies used in prior research, including [FSS], have been shown to be valid for purposes of judging trends over time."

Schell, Peterson, et al. (2020), motivated by some of the same observations as us, estimate

[5]Recent NRA magazine subscriptions have flattened (Gilson, 2021) despite a four-fold increase in FBI background checks between 2006 and 2020, so we do not suspect that print magazines still offer a good proxy for firearm prevalence. NRA membership data is not publicly available (Ingraham, 2021).

a latent state/year index of household firearm ownership combining survey and proxy data. We view this as an important effort and a worthy goal. Yet one limitation is that measures of ground truth are needed to establish the validity of latent empirical constructs. This concerns us because the Schell, Peterson, et al. (2020) index estimates Massachusetts firearm penetration to have fallen by about one third between 2010 and 2016 (from 12.2% in 2010 down to 9% in 2016), whereas the state's license data showed that the proportion of individuals with valid firearm licenses increased by about half from 2010 to 2016, from 4% to 6%. A second concern is that the latent index uses FSS intertemporally, which has been questioned by Kovandzic et al. (2013) and Hayo, Neumeier, and Westphal (2019), among others.[6]

1.1.2 Legal Firearm Prevalence

We measure Legal Firearm Prevalence (LFP) using firearm license data from Massachusetts. All adults in Massachusetts are legally required to maintain an active license in order to purchase or possess a firearm. Firearm licenses expire after six years. New licenses cost $100, as do license renewals.

We observe all new firearm licenses and license renewals issued between January 1, 2006 and December 31, 2017. For each record, we observe the license issue date, type, status (new or renewal), licensing authority (typically a local police agency), and an anonymous licensee identifier. We use the term "license" to include all firearm licenses that enable legal firearm purchase and possession in Massachusetts. The data consist mainly of Class A Licenses (90%) and Firearm Identification Cards (9%).

We map each licensing authority into the county that contains it. We then measure LFP by counting firearm licensees with one or more active licenses in each county in each month. Counting licensees, rather than licenses, avoids double-counting people who hold multiple valid licenses concurrently. Web Appendix A.1 describes potential left-censoring of LFP in 2010 and

[6] Cook and Ludwig (2019) replied to Hayo et al. (2019) that, as the ratio between relatively rare events, FSS requires sufficient data on population and time to stabilize and limit measurement error; but they do not prove that FSS is a valid intertemporal proxy for firearm prevalence.

2011 and how we resolve the issue.

1.1.3 LFP Description

Figure 1.1 shows that the number of firearm licensees increased by 74% over an eight-year period, rising monotonically from 3.9% of the state population in January 2010 to 6.8% in December 2017. The rate of growth was nearly constant, with minor accelerations in 2013 and 2016.

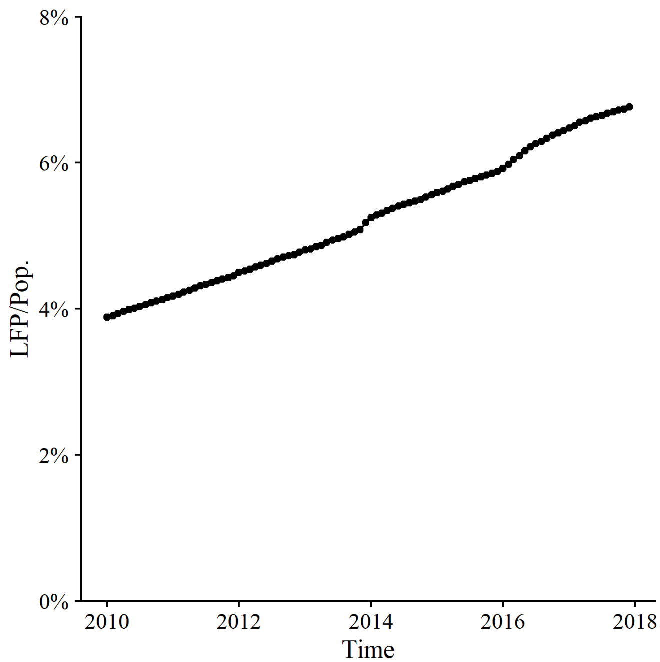


Figure 1.1. LFP

LFP indicates the number of active licensees/pop.

Figure 1.2 shows the time series of LFP per capita within each of the fourteen Massachusetts counties. Growth rates vary across counties, but downturns are remarkably rare. Franklin County had the highest average LFP, reaching 14.1% in December 2017. The counties with the highest initial LFP in 2010 grew most during the next 7 years, with the exception of Nantucket. Suffolk County had the lowest average LFP, growing from 0.9% to 2.2% over 8 years.

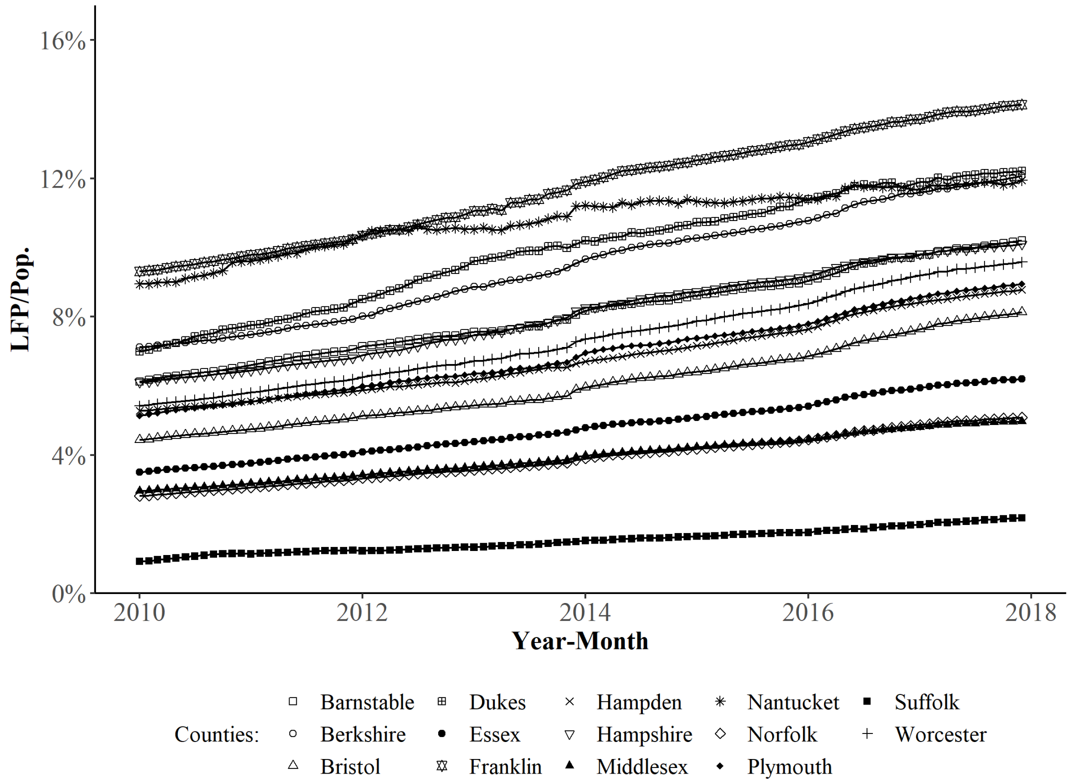


Figure 1.2. LFP by County

This figure shows the LFP trend for each MA county.

1.1.4 Strengths and Weaknesses of LFP

LFP has several strengths. Most importantly, it counts all firearm licensees based on official government records. It is a population rather than a small or self-selected sample drawn from a population. The fact that licenses must be renewed regularly at substantial cost suggests that LFP should meaningfully reflect both new acquisitions and divestitures of legal firearm access. Unlike survey data, it does not rely on firearm owners' self-reports, and therefore is not subject to sampling error, nonresponse bias, survey design problems or respondent misreporting. Finally, the measure counts distinct people, rather than individual licenses issued or individual firearms, an important property when one individual may hold multiple license types and collect multiple firearms.

The measure also has some weaknesses. Most importantly, we only have data from

Massachusetts. Although we filed similar Freedom of Information Act (FOIA) requests with three other states (California, Connecticut and Hawaii), none responded by providing comparable data, despite our repeated inquiries. Second, LFP does not measure firearm lethality, concealability, or concentration of weapons among households or individuals.

We do not claim that LFP also indicates illegal firearm prevalence; hence the term "Legal" in Legal Firearm Prevalence. One of the primary reasons to conduct firearm research is to inform firearm policy, and LFP is a direct outcome of firearm acquisition policies. Therefore, if we seek to understand the effects of policy changes on legal firearm acquisitions, we should distinguish LFP from illegal firearm prevalence. We view measurement of IFP and evaluation of IFP proxies as important topics for future research.

1.2 Candidate Firearm Proxies

We analyze two types of candidate proxies for firearm prevalence: retail-based proxies and suicide-based proxies.

1.2.1 Retail-based Proxies

Firearm retailing activity leads directly to firearm acquisitions, suggesting that retail-based measures may proxy for legal firearm prevalence. However, retail-based proxies have two limitations. First, no national database of firearm acquisitions exists, so direct, comprehensive measures of firearm retailing activity are not available for all areas. Second, any retail-based data source will exclude acquisitions that occurred prior to the data sample. Firearms are durable goods, so retail-based data sources cannot provide a fully accurate measure of firearm prevalence. Still, we have found three sources of retail-based data that offer candidate proxies for LFP.

Massachusetts is one of a few states that requires firearm acquisitions to be registered with the state. Acquisition data include Sales, Transfers and Registrations ("STR"). We received the population of Massachusetts STR along with the firearm license data by filing a FOIA request.

We also obtained data on the population of firearm sales intermediated by an anonymous online firearm retail platform (*Online Sales* or "OS"). The data report individual transactions and include buyer zip codes, which we use to assign each firearm acquisition to buyer counties.[7] OS data are less comprehensive than the STR data, as they do not report offline sales transactions through traditional firearm retailers, but they do have two advantages. First, OS data cover the entire United States, suggesting that OS data may provide a retail-based proxy with national scope. Second, some firearm sales might not be legally registered with the state, and therefore excluded from the STR data; or they might not be registered in a timely fashion.

The third retail-based proxy we analyze is the population of Federal Firearms Licensees (FFLs) that sell firearms to consumers, a retail-based proxy that was first considered by Kleck (2004). The data were downloaded from the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) website.[8] We matched each FFL address to the county that contained it using ArcGIS, a popular geocoding software. The FFL proxy counts the number of firearm retailers in each county in each month and shows substantial intertemporal variation. Licenses for FFLs expire after three years. Substantial numbers of FFLs enter and exit during the sample period.

1.2.2 Suicide-based Proxies

The proportion of firearm suicides, FSS, was validated as the highest cross-sectional correlate of TFP in survey data. We have not found any clear theoretical justification in the literature for FSS as a proxy for firearm prevalence, but FSS is the most frequent proxy for total firearm prevalence in the scientific literature, and is therefore an important proxy to evaluate. We also analyze the proxy's two components, Firearm Suicides (FS) and Suicides (S), as they are readily available and less volatile over time. We lack a compelling theoretical reason to consider S or FS valid proxies; we consider them because they each contribute to variation in FSS.

We count suicides by county and month using Multiple Causes of Death (MCOD) files from the National Center of Health Statistics (NCHS) division of the Centers for Disease Control

[7] Some zip codes span county borders. In cases where 85% of a zip code's population resides in one county, we assign transactions to that larger county. Otherwise, we drop the transaction.

[8] We only count FFL types 1, 2 and 7 as these indicate retail firearm sales.

and Prevention (CDC). MCOD files report information collected from U.S. death certificates provided by state vital registration offices.[9]

1.2.3 Sample Period

All candidate proxies described above are available monthly from January 2010 through December 2017, except for FFL data. FFL data are available from January 2014 through December 2017, with two months of data missing in September and October of 2015.[10]

1.2.4 Cumulative and Static Measures

Firearms are durable goods, so most consumers who acquire firearms retain them for substantial periods of time. However, the proxy measures listed above are counts of transitory events (e.g., suicides, firearm acquisitions). Therefore, we consider two forms of each proxy: *static* and *cumulative*. The static version is the count observed within each county and time period. The cumulative version is the accumulation of all activity observed in a county from the beginning of the sample up until a given time period.[11] For FFLs, the contemporaneous value of the proxy is the net accumulation of firearm retailers from the start of the sample until the current time period; therefore, we take the static version of the proxy as the *change* in FFLs from the previous time period to the current time period.

We see a meaningful distinction between cumulative retail-based proxies and cumulative suicide-based proxies. LFP is the totality of accumulated firearm acquisitions and divestitures, so accumulated firearm acquisitions should meaningfully reflect some variation in LFP. However, prior literature provides no similar theoretical connection between LFP and suicide data. We include FSS and its components because FSS is frequently used in existing literature, not because we have a theory that predicts suicide-based proxies will reflect LFP. We consider cumulative

[9]We define firearm suicides using standard International Statistical Classification of Diseases and Related Health Problems (ICD-10) codes X72-X74. Suicides are assigned to counties using associated Federal Information Processing Standards (FIPS) codes.

[10]ATF staff were unable to explain why those two months were missing or to provide data prior to 2014.

[11]For STRs, the sample period begins in January 2006. For OS and suicide-based proxies, the sample period begins in January 2010. For FFLs, the sample period begins in January 2014.

versions of the suicide-based proxies because we believe they have not been evaluated previously, and in fact we find they perform better than FSS in cross-temporal research designs.

1.2.5 Summary of Proxy Data Sets

Table 1.1 summarizes candidate firearm proxies, labels, data sources, sample periods, and descriptive statistics.

Table 1.1. Summary of Candidate Firearm Proxies

Proxy	Labels	Source	Sample Period	Summary
Proportion of Firearm Suicides	FSS	Multiple Causes of Death (MCOD) data from the National Center of Health Statistics (NCHS), Centers for Disease Control (CDC)	2010-2017	Mean=21.6%
Firearm Suicides	FS	MCOD data from NCHS, CDC	2010-2017	1,049 total
Suicides	S	MCOD data from NCHS, CDC	2010-2017	4,861 total
Sales, Transfers, and Registrations	STR	Massachusetts Firearms Records Bureau	2010-2017	915,196 total
Online Sales	OS	Leading Online Firearm Retail Platform	2010-2017	24,375 total
Federal Firearm Licensees	FFL	Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF)	2014-2017, excluding Sept.-Oct. 2015	615 unique FFLs

1.3 Methods and Results

Previous research has linked firearm proxies to societal outcomes using a wide variety of research designs. Outcome data vary in their geographic and temporal resolutions, such as county/state or month/year. Research designs also vary, such as cross-sectional, time series, panel, etc. We assess the empirical performance of each candidate proxy in four common research designs.[12]

1. Cross-sectional correlation analysis.

2. State/month trends analysis.

3. County/year panel regression.

[12]See, e.g., Moody and Marvell (2005), Cook and Ludwig (2006), Siegel, Ross, and King III (2013) or Depetris-Chauvin (2015), among many others.

4. County/month panel regression.

Following Azrael, Cook, and Miller (2004) and Kleck (2004), cross-sectional and intertemporal designs are evaluated with bivariate correlation coefficients. We use regressions with unit and time fixed effects to evaluate candidate proxies in panel designs.

Assessment of candidate firearms proxies in multiple settings may offer two types of insight. One is to inform researchers about the potential utility of a particular proxy within a specific research design of interest. The other is to indicate patterns in order to gain more general insights into the nature of the empirical relationships between each proxy and LFP, in hopes of finding consistent patterns that might apply in other research designs that we do not consider.

1.3.1 Cross-sectional Correlation Analysis

We report cross-sectional correlations between candidate proxies and LFP per capita, following Azrael, Cook, and Miller (2004) and Kleck (2004), using population data from the 2010 Census for the 14 counties in Massachusetts. Unlike subsequent research designs, cross-sectional analysis using the full sample does not allow for distinctions between cumulative and static versions of candidate proxies.

Table 1.2 shows the cross-sectional correlation matrix of LFP and the candidate proxies using the full sample period. All measures except FSS are per capita. All six suicide-based and retail-based proxies are significantly correlated with LFP, ranging from .56 to .86. STR is the most highly correlated with LFP at .86.

FSS is the second most strongly correlated with LFP at .74. This correlation lies within the .64-.92 range reported by Kleck (2004, Tables 1 and 3) and below the .81-.93 range reported by Azrael, Cook, and Miller (2004, Table 3).[13]

The remaining four proxies have correlations with LFP ranging from .56 (FFL) to .68 (FS). The suicide-based proxies are strongly correlated amongst themselves (.83-.95) by construction,

[13] Kleck's cross-sectional correlation ranges are from various analyses using multiple GSS constructs and different temporal and geographic units. Azrael, Cook, and Miller's ranges are from different survey measures of gun ownership.

Table 1.2. Cross-sectional Correlations between LFP and Candidate Firearm Proxies

Proxy	ID	1	2	3	4	5	6	7
LFP/Pop.	**1**	1.00						
FSS	**2**	0.74*	1.00					
FS/Pop.	**3**	0.68*	0.95*	1.00				
S/Pop.	**4**	0.66*	0.83*	0.93*	1.00			
STR/Pop.	**5**	0.86*	0.52	0.41	0.41	1.00		
OS/Pop.	**6**	0.65*	0.10	-0.02	0.01	0.68*	1.00	
FFL/Pop.†	**7**	0.56*	0.39	0.01	-0.11	0.75*	0.43	1.00

Note. *p<0.05. 14 observations (1 per county) are used to calculate each correlation.
†:Data available from 2014-2017, with Sep, Oct 2015 missing.

as Suicides include Firearm Suicides, and FSS is the ratio of the two. The retail-based proxies correlate with each other to a lesser degree (.43-.75). Correlations between the two sets of proxies are not significant and some are close to zero, suggesting that the retail-based proxies may offer information that is not available in the suicide-based proxies.

1.3.2 State/Month Trends Analysis

We rely on intertemporal correlations and visual interpretation to assess the utility of each candidate proxy for LFP in trends analysis. Recall that Figure 1.1 shows that LFP per capita increased monotonically over time with variable growth rates. In this analysis, LFP and the proxy measures are again measured per capita, except static and cumulative FSS.

Figure 1.4 shows how static and cumulative suicide-based proxies change over time at the state/month level. The three static proxies are highly variable and their time trends do not track LFP. Suicides correlates with LFP most highly at .28; FSS shows the lowest correlation with LFP at .03.

The cumulative suicide-based proxies are somewhat different. Cumulative FS and S both increase, by construction, and therefore both correlate very highly with LFP at 0.997, although their average growth rates exceed that of LFP. Cumulative FSS, in contrast, correlates negatively with LFP at -.57.

Figure 1.6 shows how static and cumulative retail-based proxies change over time at

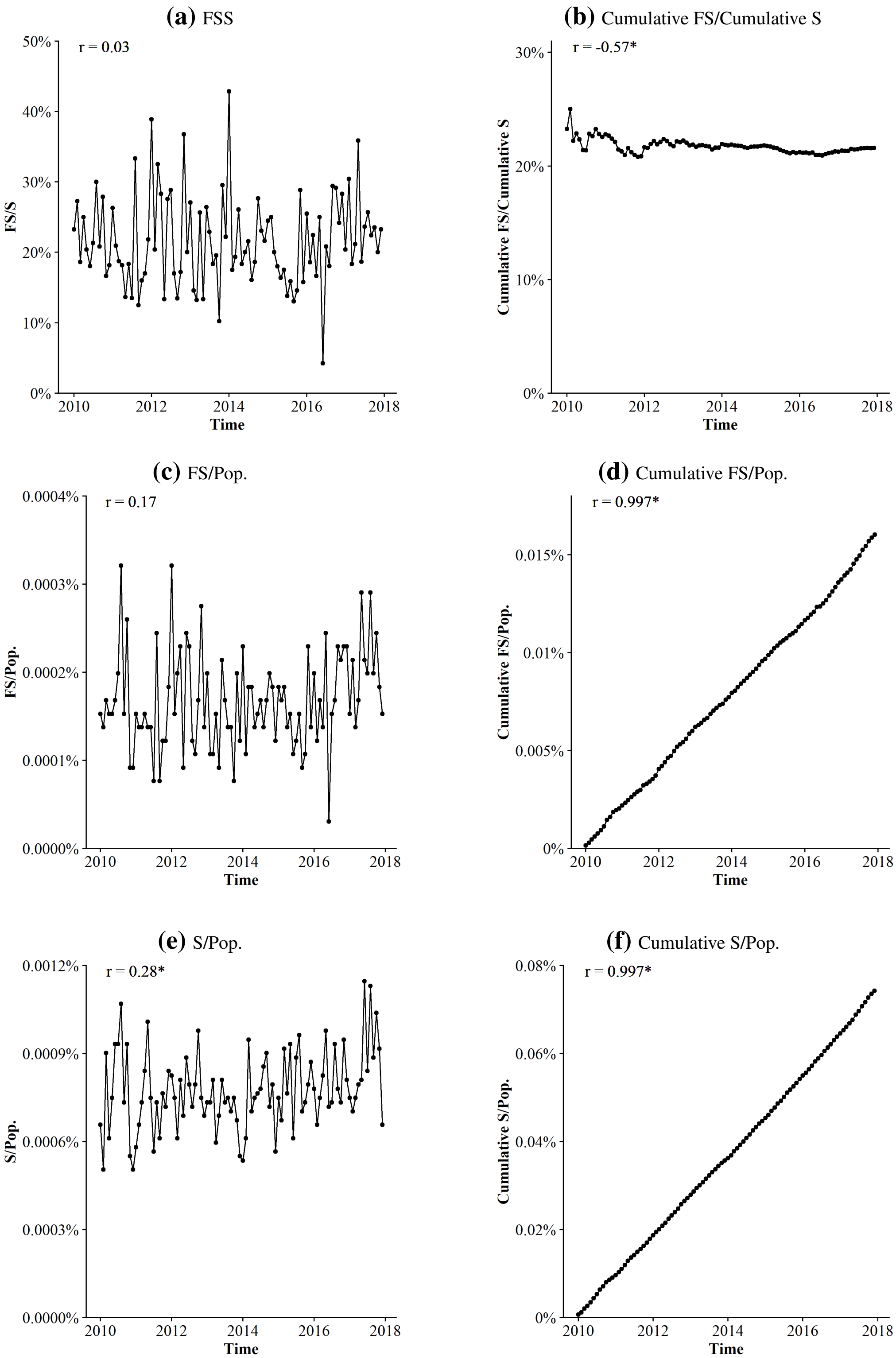


Figure 1.4. Suicide Proxies Over Time

r is the Pearson correlation score of the proxy and LFP. *p<0.05.

the state/month level. The three static proxies are again highly variable. STR correlates with LFP most highly at .76, followed by OS at .47; both are statistically significant. Change in FFL correlates negatively with LFP at -.26.

The cumulative retail-based proxies are again somewhat different. Cumulative STR and Cumulative OS both increase monotonically, by construction, and both correlate with LFP at 0.998, although their aggregate growth rates are again larger than LFP. FFL, in contrast, increases slowly but then levels off, and correlates with LFP at 0.73.

Overall, we find that 4 out of 12 candidate proxies correlate very highly with LFP intertemporally, above .997. Among the static proxies, STR and OS are the most promising with correlations of .76 and .47. Static FSS correlates with LFP at just .03, and cumulative FSS is negatively correlated with LFP at -.57.

Web Appendix A.2 reconciles the high cross-sectional correlation and low cross-temporal correlation between FSS and LFP. It repeats the cross-sectional correlation and graphical analyses across four successive two-year subsamples. It shows that FSS is the most volatile of the candidate proxies, thereby explaining its low intertemporal correlation with LFP. Kovandzic et al. (2013) and Cook and Ludwig (2019) treat similar topics in greater depth.

1.3.3 County/Year Panel Regressions

Cross-sectional and intertemporal correlations are elegant and valid ways to estimate empirical relationships, but aggregated data may conceal unmeasured confounds that influence both firearm prevalence and firearm proxies. Next, we consider county/year panel regressions, both with and without county fixed effects and year fixed effects to control for unobservable county-specific and time-specific variables.[14]

We do not use per capita measures in the panel regressions, because county populations are only measured directly in the decennial census. The Census provides interpolated population

[14]Three county/year observations exhibit zero suicides, leading to undefinable static FSS, so we drop those three cases when estimating the models. FFL proxy models lose observations for periods in which FFL data were not available from ATF.

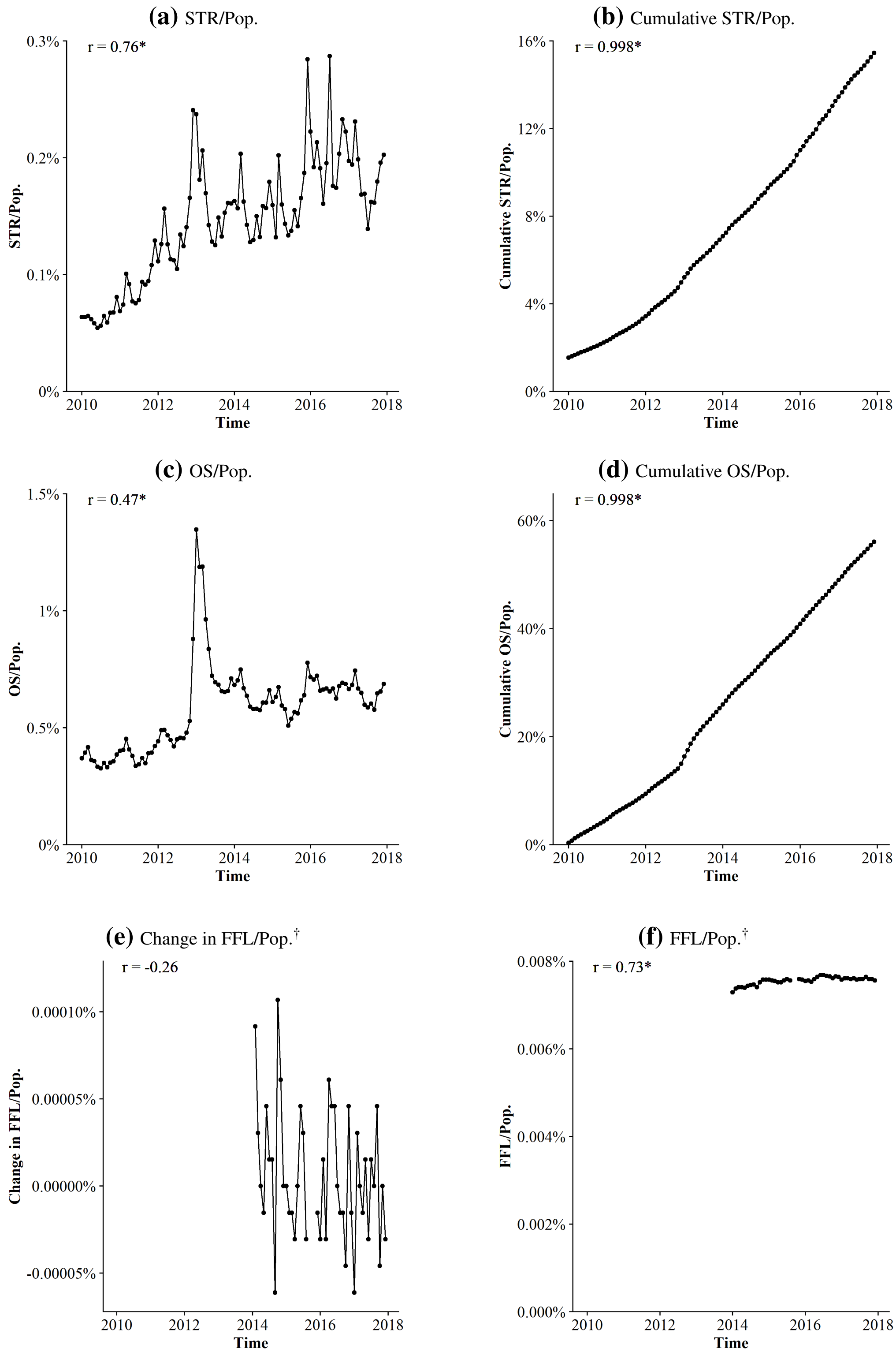


Figure 1.6. Retail Proxies Over Time

r is the Pearson correlation score of the proxy and LFP. *p<0.05.†:Data available from 2014-2017; Sep, Oct 2015 missing.

estimates, but those figures are projections rather than direct measurements, and therefore subject to forecasting and interpolation errors. Further, scaling both dependent and independent variables in a regression by common factors can induce spurious correlation (Hayo et al., 2019; Kronmal, 1993). Instead, we follow Azrael, Cook, and Miller (2004) in using county populations to weight observations and control for heteroskedasticity. We also cluster standard errors at the county level, following the experimental design rationale described by Abadie, Athey, Imbens, and Wooldridge (2017).

We specify and estimate two models:

$$LFP_{ct} = \beta_0 + \beta_1 x_{ct} + \varepsilon_{ct} \tag{1.1}$$

$$LFP_{ct} = \beta_0 + \beta_1 x_{ct} + \alpha_c + \lambda_t + \varepsilon_{ct} \tag{1.2}$$

The Proxy-only Model in Equation 1.1 is a regression of LFP in county c in year t on an intercept and one candidate proxy denoted x_{ct}. The Model with Proxy and Controls in Equation 1.2 augments that simple specification with county- and year-specific fixed effects. We investigate both models because some proxies are more highly correlated with the control variables than others. Models with control variables offer more stringent tests of candidate proxies, as unit and time fixed effects typically explain large fractions of variation in panel data sets.

There is no widely agreed-upon set of criteria to establish proxy validity in panel settings. In the context of firearm prevalence, Azrael, Cook, and Miller (2004) and Kleck (2004) focus on size and statistical significance of correlations between candidate proxies and firearm prevalence estimates. Kovandzic, Schaffer, and Kleck (2013, p. 484) further emphasize that any valid proxy should also explain a large portion of the variance in firearm prevalence. We evaluate the utility of each proxy in each research design using the following criteria:

1. F-test to test proxy inclusion in the model specification.

2. The squared partial correlation coefficient (Partial R^2) to indicate the proportion of variance

of LFP that is explained solely by the proxy after partialing out the controls.[15]

3. Estimated proxy parameter sign, magnitude and precision. A perfect proxy would have an effect size equal to one, meaning LFP and the proxy show a 1:1 relationship.

Later in the paper, we also pay attention to the consistency of proxy performance across research designs, as such patterns may suggest proxy utility in untested designs.

Table 1.3 summarizes the results of both the Proxy-only Model and the Model with Proxy and Controls. Each row of Table 1.3 reports results from two distinct regressions pertaining to that particular row's candidate proxy. The entire table summarizes 24 distinct regressions.

Table 1.3. County/year Regressions

		Proxy-only Model				Model with Proxy and Controls			
x	Obs.	β_1	t-stat	R^2	F-test p-value	β_1	t-stat	Partial R^2	F-test p-value
FSS	109	-1349.3	-0.02	.000	.986	-12684.5	-1.32	.013	.209
Cum. FS/Cum. S	112	32665.6	0.30	.009	.770	-4916.1	-0.21	.012	.836
FS	112	2018.4	13.50	.480	.000	-178.7	-2.15	.001	.051
Cumulative FS	112	356.6	14.27	.620	.000	189.8	8.45	.912	.000
S	112	449.4	14.93	.559	.000	125.2	2.24	.178	.043
Cumulative S	112	64.1	12.27	.507	.000	31.0	8.20	.842	.000
STR	112	2.5	23.96	.899	.000	1.4	21.21	.839	.000
Cumulative STR	112	0.4	10.59	.738	.000	0.2	12.41	.971	.000
OS	112	83.4	10.79	.785	.000	21.7	4.21	.510	.001
Cumulative OS	112	13.9	8.11	.646	.000	6.1	7.69	.904	.000
Change in FFL†	42	-553.7	-0.74	.002	.471	136.2	0.55	.002	.592
FFL†	56	659.2	14.04	.866	.000	-18.4	-0.08	.001	.937

Note. Each row is a regression of Firearms Licensees (y_{ct}) on Proxy (x_{ct}) weighted by 2010 Census Population with/without County Fixed Effects (α_c) and Year Fixed Effects (λ_t) where c $\in$ MA Counties and t $\in$ Year := $\{2010,...,2017\}$. Standard errors are clustered by county. F-stat p-values test the null hypothesis that the model without the proxy fits the same as the model with the proxy. †:Data available from 2014-2017, with Sep, Oct 2015 missing.

In the Proxy-only Models, 9 of the 12 candidate proxies are statistically significant predictors of LFP. These 9 proxies explain between 48-90% of the variation in LFP; all have

[15]It would be easy to misread the Partial R-square statistics reported below, as a high Partial R-square between a cumulative proxy and LFP might be incorrectly interpreted as following directly from both variables' cumulative nature and mutual positive trends over time. In fact, the panel models with controls include time period fixed effects which are completely differenced out before the Partial R-square statistics are calculated. Therefore each Partial R-square statistic solely reflects panel covariation between LFP and proxy remaining *after* unit and time period fixed effects are estimated.

positive relationships with LFP. Three proxies explain less than 1% of the variation in LFP: static FSS, cumulative FSS, and static FFL.

Next we focus on the more stringent Models with Proxy and Controls. 6 candidate proxies each explain more than 50% of the residual variation in LFP. Those 6 include two suicide-based proxies—Cumulative Firearm Suicides and Cumulative Suicides—and four retail-based proxies: Static and Cumulative STR, and Static and Cumulative OS. The t-stat and F-test p-values for these 6 proxies also are significant, indicating the proxy adds important information beyond that provided by the county and year fixed effects.

The remaining 6 proxies explain 0.1-17.8% of the residual variation in LFP after partialing out the controls. Static and Cumulative FSS each explain about 1% of the residual variation in LFP. This echoes the results of Kovandzic, Schaffer, and Kleck (2013), who also find near-zero intertemporal correlations between FSS and survey measures of firearm prevalence.

The Static Suicides proxy is statistically significant and explains 17.8% of the residual variation in LFP. It performs substantially better than FSS, but also substantially less well than the 6 candidate proxies with Partial R^2 statistics exceeding 0.5.

Three candidate retail-based proxies seem especially promising, with Partial R^2 statistics ranging from .839 to .971, and effect sizes falling within an order of magnitude of 1: STR (1.4), Cumulative STR (0.2) and Cumulative OS (6.1).

1.3.4 County/Month Panel Regressions

More granular temporal resolutions allow for better controls for unmeasured confounds while reducing potential aggregation biases. Many societal outcomes (e.g., crime) are measured monthly and therefore can be studied in higher-resolution data that enables more extensive controls. Therefore we test the Models with Proxy and Controls again using county/month panel data.

We specify and estimate the following models:

$$LFP_{ct} = \beta_0 + \beta_1 x_{ct} + \alpha_c + \lambda_t + \varepsilon_{ct} \tag{1.3}$$

$$LFP_{ct} = \beta_0 + \beta_1 x_{ct-1} + \alpha_c + \lambda_t + \varepsilon_{ct} \tag{1.4}$$

The models regress LFP in county c in year/month t on an intercept (β_0), one candidate proxy (x_{ct}), a county fixed effect (α_c) and a year/month fixed effect (λ_t). In the second model, we replace the contemporaneous value of the candidate proxy x_{ct} with its first lag, x_{ct-1}. We evaluate first lags of candidate proxies because many longitudinal analyses in long panels (e.g. Cook & Ludwig, 2006; Duggan, 2001b; Khalil, 2017) have used lagged proxies as informal checks for reverse causation or simultaneity.

Table 1.4 shows the county/month panel regression results. Each row in the table reports two unique regressions pertaining to that particular row's proxy: one for the contemporaneous value and one for the first lagged value. All regressions included county and year/month fixed effects.

The qualitative conclusions in Table 1.4 are remarkably similar to the Proxy and Controls models estimated using county/year data. The qualitative results are nearly identical whether using contemporaneous or lagged values of the candidate firearms proxies.

Among the suicide-based proxies, only Cumulative Firearm Suicides and Cumulative Suicides explain large amounts of residual variance in LFP after partialing out the unit and time controls. The remaining contemporaneous and lagged suicide-based proxies explain 0.0-2.2% of the residual variance in LFP. The only major quantitative difference between county/month results and county/year results is that the residual variance in LFP explained by Static Suicides falls from 17.8% in annual data to about 2.0% in monthly data.

Among the retail-based proxies, Static STR, Cumulative STR, and Cumulative OS perform particularly well, with high Partial R^2 statistics, statistically significant beta coefficients,

Table 1.4. County/month Regressions

	Model with Proxy and Controls					Model with Lagged Proxy and Controls				
x	Obs.	β_1	t-stat	Partial R^2	F-test p-value	Obs.	β_1	t-stat	Partial R^2	F-test p-value
FSS	1075	-1131.7	-1.92	.000	.078	1065	-1059.6	-1.90	.000	.080
Cum. FS/Cum. S	1326	-3444.1	-0.34	.003	.739	1312	-3153.8	-0.32	.003	.757
FS	1344	-135.9	-1.97	.000	.070	1330	-117.9	-1.73	.000	.108
Cumulative FS	1344	176.6	9.60	.909	.000	1330	176.9	9.67	.909	.000
S	1344	143.6	2.58	.020	.023	1330	143.3	2.73	.022	.017
Cumulative S	1344	30.0	9.10	.844	.000	1330	30.2	9.05	.843	.000
STR	1344	9.7	20.92	.523	.000	1330	9.7	20.80	.524	.000
Cumulative STR	1344	0.2	12.93	.982	.000	1330	0.2	12.80	.981	.000
OS	1344	79.4	3.56	.191	.003	1330	77.5	3.54	.187	.004
Cumulative OS	1344	6.2	8.84	.919	.000	1330	6.3	8.81	.919	.000
Change in FFL†	616	-174.9	-1.62	.003	.129	602	-179.1	-1.70	.003	.112
FFL†	644	12.8	0.09	.004	.928	630	21.4	0.14	.005	.889

Note. Each row shows two unique regressions of Firearms Licensees (LFP_{ct}) on either Proxy (x_{ct}) or Lagged Proxy (x_{ct-1}) pertaining to that particular row's proxy. Controls include County Fixed Effects (α_c) and Year/Month Fixed Effects (λ_t) where c $\in$ MA Counties and t $\in$ Year/Month := {2010-Jan,...,2017-Dec}. The regressions are weighted by population and standard errors are clustered by county. F-stat p-values test the null hypothesis that the model without the proxy fits the same as the model with the proxy. †:Data available from 2014-2017, with Sep, Oct 2015 missing.

and effect sizes within one order of magnitude of unity. Cumulative STR again explains the most variation in LFP (98.2%). The two largest quantitative differences between county/month results and county/year results is that the Partial R^2 of Static STR falls from 83.9% in annual data to 52.3% in monthly data, and the Partial R^2 of Static OS falls from 51% in annual data to 19.1% in monthly data. These changes in variance explained align with the theory that monthly data allow better controls for unmeasured confounds than annual data.

1.3.5 FBI Background Checks

This section seeks to quantify associations between LFP, candidate proxies, and a frequent state/year-month proxy for firearm prevalence, FBI Background Checks. We treat FBI Background Checks separately from other candidate proxies for two reasons. First, Lang (2013) showed recently that FBI background checks correlate highly with GSS survey measures of firearm prevalence in a panel of census divisions and years. Second, FBI Background Checks are only available at the state/month level, and therefore inevaluable in most of the research designs considered previously.

The Brady Handgun Violence Prevention Act of 1993 required that any person who wants to buy a gun from a federally licensed firearms retailer must submit for a background check conducted by the Federal Bureau of Investigation (FBI). The FBI publishes the number of background checks conducted for firearms purchasers in each state in each month. Most purchasers complete their purchase shortly after the FBI background check, suggesting that FBI background check data reliably indicate firearm purchase intention. Consequently, recent literature has used FBI background checks to proxy for firearm prevalence (e.g., Briggs & Tabarrok, 2014; Lang, 2013; Vitt et al., 2018).

Still, FBI background check data have some weaknesses. First, like STR, FBI background checks are a flow variable rather than a stock variable. Second, the FBI only publishes background check data at the state/month level, meaning it is not possible for external researchers to access more granular variation in FBI background check data. Third, the FBI publishes the total count of background checks sought rather than the number of background checks passed or the number of people who seek background checks. That means that a single person could account for multiple FBI background checks in a single state/month, and that FBI data count checks that do not lead to firearm purchases alongside those that do lead to firearm purchases (FBI, 2017b). Fourth, private party sellers and transactions at gun shows do not require FBI background checks in all states (Lang, 2013). Finally, there may be temporal differences between when a background check is conducted and when a weapon is purchased. 10.7% of FBI background checks in 2017 were delayed by incomplete criminal records (FBI, 2017a).

Table 1.5 presents state/month correlations between FBI background checks, LFP and the other candidate proxies at the state/month level for 2010-2017. The most important entry in the table is the correlation between FBI Background Checks and LFP at 0.41. This is larger than any of the suicide-based proxy correlations over the same period, as those range from 0.03-0.28. However, it also falls short of the STR correlation with LFP of 0.76 over the same time range.

Table 1.5. State-month Correlations between Background Checks, LFP, and Candidate Firearm Proxies.

Proxy	ID	1	2	3	4	5	6	7	8
LFP/Pop.	1	1.00							
FBI Background Checks/Pop.	2	0.41*	1.00						
FSS	3	0.03	0.05	1.00					
FS/Pop.	4	0.17	0.04	0.84*	1.00				
S/Pop.	5	0.28*	0.00	-0.05	0.49*	1.00			
STR/Pop.	6	0.76*	0.80*	0.03	0.07	0.10	1.00		
OS/Pop.	7	0.47*	0.80*	-0.02	-0.04	-0.02	0.77*	1.00	
FFL/Pop.†	8	0.73*	0.26	-0.13	0.12	0.27	0.42*	0.03	1.00

Note. *p<0.05. †:Data available from 2014-2017, with Sep, Oct 2015 missing.

FBI background checks correlate with STR at 0.80 and with OS at 0.80. Figure 1.7 illustrates the relationship between FBI background checks and STR, showing that they trend similarly with corresponding spikes and dips, but a difference in levels during the first four years of the sample seems to decrease during the second half of the sample.

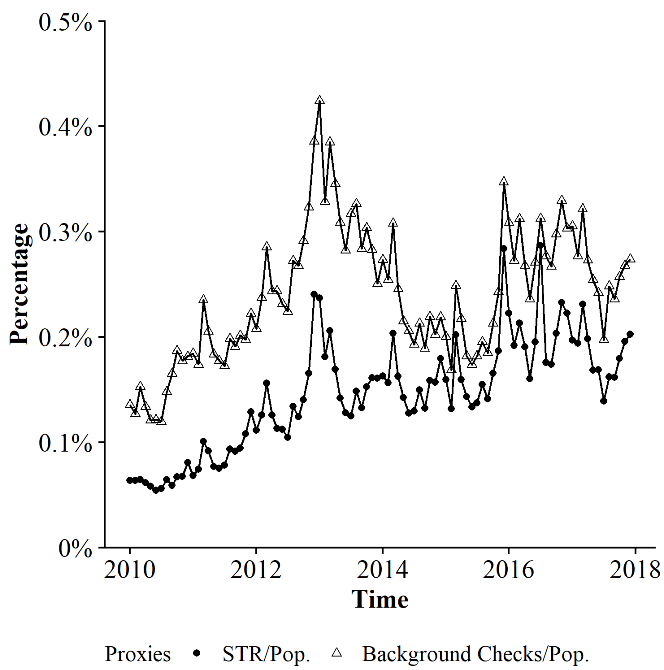


Figure 1.7. FBI Background Checks vs. STR

This figure shows the monthly trend for each FBI background checks and STR.

Overall, the state/month analysis shows that FBI background checks offer a better proxy for LFP than suicide-based proxies, but it is not as strong as other retail-based proxies.

1.4 LFP and Proxy Effects in Homicide Regressions

In this section we explore the role of proxy choice in a classic research design, the study of guns and homicide by Cook and Ludwig (2006; hereafter, "CL"). We estimate the association between LFP and homicide and then compare it with proxy estimates, holding data and methods constant.

CL analyzed annual National Center of Health Statistics (NCHS) and Uniform Crime Reports (UCR) data from 1980-1999 in the 200 most populous US counties. We focus on the model CL reported in Table 2, Column 2, in which homicide was regressed on lagged firearm prevalence, robberies, burglaries, year fixed effects and county fixed effects. The model is:

$$ln(Hom_{ct}) = \beta_0 + \beta_1 ln(Firearms_{ct-1}) + \beta_2 ln(Rob_{ct}) + \beta_3 ln(Burg_{ct}) + \alpha_c + \lambda_t + \varepsilon_{ct} \quad (1.5)$$

Errors are population-weighted and clustered by county. No data source measures total firearm prevalence directly, so CL used FSS to proxy for TFP, finding that $\hat{\beta}_1 = 0.107$, with a 95% confidence interval of $(0.034, 0.180)$.

We apply the CL research design with two key differences. First, we compare the LFP estimate to candidate proxies' estimates, rather than seeking to proxy for TFP. Second, we focus on monthly data for the 8 Massachusetts counties that were in the CL sample.[16] Our sample is more recent, has better temporal resolution, and measures LFP directly, whereas the original CL sample contained more counties and years.[17]

Table 1.6 shows the effect of LFP on homicide in the CL empirical framework, as well as estimates for the five best or most frequently used candidate proxies. The comparisons hold data

[16]We have also estimated the CL model using FSS, OS and cumulative OS proxies in annual data among the 171 most populous counties with continuous UCR reporting from 2010-2017. The association between FSS and Homicide in this larger sample was 0.114 with a confidence interval of $(0.007, 0.221)$, which is statistically significant and quantitatively similar to CL's finding.

[17]We add one to variables that contain zero values to avoid taking the log of zero. Four observations of FSS are undefined due to zero suicides, so we drop them from all regressions reported.

and methods constant; only the measure or proxy for LFP changes across columns. The estimated effect of LFP on homicide is -0.461, with a 95% confidence interval of $(-2.321, 1.400)$. The confidence interval for the estimated LFP effect on homicide is wide, so the sign of the effect is uncertain given considerable estimation error.

Table 1.6 also reports associations of homicide with five proxies for LFP: STR, Cumulative STR, OS, Cumulative OS, and FSS. Among these candidate proxies, the point estimate of Cumulative STR is -0.501, again most similar to the LFP effect, with a larger standard error of 1.289 leading to a 95% confidence interval of $(-3.548, 2.546)$. Therefore, Cumulative STR would be an accurate but conservative proxy for LFP in this setting.

The results show that other candidate proxies yield overly precise estimates. The 95% confidence intervals for STR, OS, and Cumulative OS are all too narrow, at $(-0.727, 0.540)$, $(-0.084, 0.168)$, and $(-0.396, 0.264)$, respectively. Therefore, these proxies' confidence intervals partially overlap with LFP's confidence interval. They are suboptimal given their overly narrow bounds but they accurately refrain from yielding a statistically significant association.

Surprisingly, the static FSS proxy produces a negative, significant association between LFP and homicide, with a 95% confidence interval of $(-0.520, -0.007)$. County/month data is not ideal for FSS measurement, as granularity exacerbates the measure's volatility (Cook & Ludwig, 2019; Hayo et al., 2019). Still, it is interesting that the proxy would lead to a Type I error, and also that its negative sign opposes CL's original significant, positive finding.

Table 1.7 reports estimated associations between the remaining proxies and LFP.[18] Five of these proxies accurately produce non-findings, but the Cumulative FFL proxy produces a positive, significant association with homicide, again due to underestimation of true parameter uncertainty. This result shows that use of an invalid proxy may produce either a positive or a negative spurious finding.

These findings pertain to a single empirical setting, but they indicate that selection of a

[18]The static version of FFL, the change in number of FFLs, is not included in the analysis because static FFL includes negative values and therefore cannot be logged.

suboptimal proxy in a classic research design can underestimate parameter uncertainty and even cause a Type I error. They underscore the need for caution in proxy validation and selection. Ideally, we could systematically compile and publish valid firearm acquisition data in order to accurately estimate firearm policy effects on legal firearm acquisitions.

1.5 Discussion and Implications

We introduce the first measure of Legal Firearm Prevalence. We introduce several new retail-based candidate proxies for LFP. We offer the first empirical evaluation of candidate proxies for LFP. We find that cumulative firearm acquisitions are the best proxy for LFP in every research design tested. Online sales of firearms can also proxy well for LFP, despite a small overall share of the market. Like prior work on TFP, we find that FSS is a good cross-sectional proxy and a poor cross-temporal proxy for LFP. We showed that suboptimal proxies may lead to mistaken findings in applied research.

Next, we discuss implications for proxy selection, evaluation of firearm research, feasible policies to increase firearm sales data availability and limitations.

1.5.1 Implications for LFP Proxy Selection

This paper offers clear guidance for researchers who wish to study LFP as a predictor or outcome variable. Table 1.8 summarizes the results of proxy evaluations across research designs, using our preferred criteria for proxy validity: Partial R^2 greater than .5, a statistically significant parameter estimate, and positive association between proxy and LFP.

One implication for selecting a proxy for LFP should be uncontroversial: If possible, choose legal firearm acquisitions as a proxy. It was an empirical question whether firearm sales would reflect the large served available market of legal firearms prevalence, but the data indicated STR as the best available proxy in every research design tested. Multiple states maintain firearm transaction registries; see, e.g., Sorenson and Berk (2001) or Berk (2021).

Most states do not register firearm transactions. Therefore, the finding that online firearm

Table 1.6. LFP and Proxy Effect Estimates on Homicide in County/Month CL Regressions

	Homicide					
	(1)	(2)	(3)	(4)	(5)	(6)
LFP	−0.461					
	(0.787)					
STR		−0.093				
		(0.268)				
Cumulative STR			−0.501			
			(1.289)			
Online Sales (OS)				0.042		
				(0.053)		
Cumulative OS					−0.066	
					(0.140)	
FSS						−0.264**
						(0.108)
Robbery	0.060	0.060	0.064	0.064	0.061	0.053
	(0.093)	(0.091)	(0.097)	(0.094)	(0.089)	(0.098)
Burglary	−0.047	−0.049	−0.036	−0.057	−0.042	−0.064
	(0.161)	(0.179)	(0.136)	(0.172)	(0.158)	(0.170)
Constant	5.359	1.300	5.302	0.639	0.849	0.878
	(8.698)	(1.578)	(12.509)	(1.121)	(1.212)	(1.04)
Time Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	756	756	756	756	756	756
R^2	0.549	0.549	0.549	0.550	0.549	0.552
Adjusted R^2	0.477	0.477	0.477	0.477	0.477	0.480

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

All variables are logged. Each proxy indicates the first lag. The sample is the 8 MA counties that are part of the top 171 largest US counties that reported UCR data continuously.

Table 1.7. More Proxy Effect Estimates on Homicide in County/Month CL Regressions

	Homicide					
	(1)	(2)	(3)	(4)	(5)	(6)
Cumulative FSS	−0.663					
	(0.902)					
Suicides (S)		0.048				
		(0.054)				
Cumulative S			0.245			
			(0.279)			
Firearm Suicides (FS)				−0.045		
				(0.034)		
Cumulative FS					−0.053	
					(0.269)	
Cumulative FFL						0.685***
						(0.077)
Robbery	0.052	0.060	0.053	0.055	0.058	0.055
	(0.091)	(0.092)	(0.091)	(0.094)	(0.091)	(0.082)
Burglary	−0.056	−0.054	−0.056	−0.061	−0.053	−0.007
	(0.174)	(0.178)	(0.175)	(0.167)	(0.175)	(0.218)
Constant	0.908	0.676	0.473	0.839	0.779	−2.387
	(0.957)	(1.024)	(0.840)	(1.004)	(0.975)	(1.374)
Time Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	756	756	756	756	756	360
R^2	0.550	0.550	0.550	0.550	0.549	0.556
Adjusted R^2	0.478	0.478	0.478	0.478	0.477	0.477

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

All variables are logged. Each proxy indicates the first lag. The sample is the 8 MA counties that are part of the top 171 largest US counties that reported UCR data continuously.

Table 1.8. Proxy Summary Across Common Designs

Proxy	Cross-section Correlation	State/Month Intertemporal Correlation	County/Year Panel (No Controls)	County/Year Panel (with Controls)	County/Month Panel Contemporaneous proxy	County/Month Panel Lagged proxy
FSS	✓					
Cum. FS/ Cum. S	✓					
FS	✓					
Cumulative FS	✓	✓	✓	✓	✓	✓
S	✓	✓	✓			
Cumulative S	✓	✓	✓	✓	✓	✓
STR	✓	✓	✓	✓	✓	✓
Cumulative STR	✓	✓	✓	✓	✓	✓
OS	✓	✓	✓	✓		
Cumulative OS	✓	✓	✓	✓	✓	✓
Change in FFL	✓					
FFL	✓	✓	✓			

Note. Checkmark indicates the p-value of the correlation coefficient is less than .05 for cross-sectional and time-series designs. In panel designs, the checkmark indicates that the proxy exhibits a Partial R^2 greater than .5, a statistically significant parameter effect and a positive association with LFP.

sales serve as a valid proxy for LFP in many research designs, despite having just a 2.7% overall market share, is quite promising. We anticipate that new sources of firearm sales may become available in the future. Possible sources include systematic collection of online firearm sales listings, large consumer expenditure panels, credit card transaction data or retailer parking capacity utilization. All of those data sources have been analyzed in other academic research, though not yet within the context of firearms. We recommend inclusion of firearm retail measures in future assessments of firearm data; see e.g., NORC (2021).

We also found that FBI background checks correlate highly, at 0.41, with LFP. FBI background checks also correlate with STR and OS, each at 0.80, respectively, showing that background checks are a very good indicator of overall firearm sales. However, the analysis also illustrates limitations of the FBI background checks, since data are only available by state and month. Therefore, they cannot be used in more granular county-level, daily or weekly analyses.

Finally, the suicide-based proxy results provide several clear guidelines. FSS is only a valid proxy for LFP in cross-sectional research designs. Its selection could lead to mistaken conclusions in cross-temporal designs by overcontrolling for LFP. Surprisingly, despite the non-validation of FSS as an intertemporal proxy, the results show that accumulations of the two components of FSS – namely, Firearm Suicides and Suicides – both perform well in all research designs tested. It seems logical that if FSS contains valid but noisy signals about firearm prevalence, the accumulation of those signals may offer helpful information about local changes in firearm prevalence. However, we reach this conclusion with two strong caveats. First, the finding emerged *ex post*; we did not expect to find this *ex ante*. Second, we do not know any previous analysis that made the same point or found the same result, despite numerous papers that sought to study firearm prevalence in longitudinal designs. Therefore we would encourage further research on this topic to deepen our collective understanding.

1.5.2 Evaluation of Published Firearm Research

FSS is the most common cross-sectional proxy for firearm prevalence, as validated by comparisons to survey measures of TFP. We found that FSS also correlates cross-sectionally with LFP. Therefore, cross-sectional analyses that use FSS to proxy for TFP could additionally be interpreted as applying to LFP.

We also found that static FSS is nearly uncorrelated with LFP over time, and FSS was not indicated as a valid proxy in any cross-temporal design tested. Further, the analysis in Section 5 found that FSS estimates were misleadingly precise compared to LFP. Therefore, published results that used FSS in intertemporal designs may be unrepresentative of LFP.

1.5.3 Feasible policies to increase firearm sales data availability

Evaluations of firearm acquisition policies' effects on legal firearm acquisitions require a measure or proxy for LFP. LFP also may be relevant as a moderating variable between policy enactment and other potential policy-relevant outcomes, such as firearm suicides, assaults, defensive gun uses, mass shootings or other variables. A recent report found that "the current firearms data environment is disordered and highly segmented" (2021 2021).

The following actions are feasible and could improve the quality and coverage of existing firearm data.

- The Federal Bureau of Investigation could publish background check data at more granular levels, such as county, city, zip code, week and date.
- States that collect firearm acquisition data, such as California and Massachusetts, could publish granular counts of firearm transactions.
- States, counties or cities that do not collect firearm acquisition data could collect and publish such data.

- Firearm retailers, retail chains or retailer associations could publish aggregate sales data by place and time.

- Digital platforms, advocacy groups or researchers could scrape, track and report online firearm sales.

We would advise caution in designing procedures for firearm acquisitions data collection, reporting and distribution. Firearm market participants do not always comply with restrictive policies (Balakrishna & Wilbur, 2021), so we would recommend transparently safeguarding individual privacy. We would also advise that governments and retailers work to develop a coherent data reporting approach in order to maximize comparability of firearm acquisitions data across geography and time. We are confident that such concerns can be resolved, as they have already been considered and addressed in other sensitive contexts, such as the CDC's mortality data.

We speculate that political leadership and advocacy will be required before such actions become policy. Firearm data production and research has been controversial in the past. A Congressional amendment in 1996 stipulated that "none of the funds made available for injury prevention and control at the Centers for Disease Control and Prevention (CDC) may be used to advocate or promote gun control." The law effectively froze firearm research, to the later regret of the amendment's author (Inskeep, 2015).

We believe that collecting and publishing privacy-compliant firearm acquisition data would be a helpful step in evaluating firearm acquisition and use policies. It could also help reveal the mechanisms between policy changes and criminal or health outcomes. As an example, it could show how concealed-carry, open-carry or stand-your-ground policies affect legal firearm acquisitions, and then how changes in legal firearm prevalence affect crime, violence or health. However, such analyses may remain incomplete without IFP or TFP data.

1.5.4 Limitations

The current article has several important limitations. We were only able to collect LFP and STR data for Massachusetts. Massachusetts has some of the strictest firearm laws and lowest firearm ownership rates in the nation, so it remains to be seen whether the results will generalize to other jurisdictions.

We do not have measures of IFP, so we are not able to make empirical statements about TFP or IFP. We expect that IFP will be more strongly related to illegal firearm uses than LFP. We also suspect that LFP and IFP may be related, as increasing LFP may increase supply in illegal firearm markets.

A smaller concern is that, although LFP could have decreased during the sample period, it rose nearly uniformly in every county throughout the observation window. Cumulative proxy variables may perform worse if firearm prevalence sometimes decreases. However, firearms are durable goods and we do not know of any periods of sustained firearm divestitures in recent U.S. history, therefore we are not certain how important this limitation may be. It certainly could become important if, for example, jurisdictions bought back or outlawed particular types of firearms. However, if such policies were enacted, public records might provide firearm divestitures data which could then be used to adjust LFP measures or proxies.

1.5.5 Conclusion

Firearm acquisition and usage policy evaluations require reliable, systematically collected, longitudinally valid proxies for legal firearm prevalence. We hope the present paper can advance firearm research by evaluating proxies for LFP. We hope that collaborative efforts between policy makers, researchers and data providers will enhance scientific knowledge and provide the evidence needed to help evaluate and inform firearm policy.

Chapter 1, in full, is a reprint of the material in *Quantitative Marketing and Economics*, 2022, Kim, Jessica Jumee & Wilbur, Kenneth C. The dissertation author was the primary investigator and author of this paper. This chapter has received a dissertation award from the National Collaborative on Gun Violence Research.

Chapter 2

The Impact of Loosening Firearm Usage Restrictions on Firearm Sales and Public Health

2.1 Introduction

Firearm sales and deaths are on the rise. FBI background checks increased by 95% from 2010 to 2019 (14.3 million to 28 million) and reached an all-time high of 39.7 million in 2020 (Economist, 2021). The U.S. gun death rate increased by 17% from 2010 to 2019, with suicides accounting for 60% of all gun deaths in 2019 and homicides accounting for 36% (CDC, 2021). Amidst this continuing rise of gun violence, how states should regulate firearm usage is a key debate.

Opinions are polarized on gun control. Gun control advocates believe that stricter laws will prevent criminals from acquiring firearms and reduce violence (Everytown, 2021). Gun rights advocates argue that stricter laws will only take guns away from lawful owners who have a right to self-defend and deter crime on their own (Lund, 2014). Both beliefs need to be empirically tested and current evidence is limited and inconclusive.

The pending U.S. Supreme Court case, *New York State Rifle Pistol Association Inc. ("NYSRPA") v. Bruen*, heightens the need for more research on how firearm usage laws affect public safety and firearm demand. The pending case deals with the issue of whether New York's concealed carry law allowing local authority discretion in issuing permits based on an applicant's need for self-defensive gun use is within constitutional limits (Cornell, 2021). The pending Supreme Court's decision could have a major impact on gun regulation in many states.

Reducing firearm deaths is an uncontroversial policy goal; the debate is how to achieve it. We do not know how loosening or tightening firearm usage restrictions affect firearm ownership and public health.

States have been loosening firearm usage restrictions. 25 states adopted Stand Your Ground (allows legal protection for self-defensive use of guns in public) between 2000 and 2020 (Cherney, Morral, Schell, & Smucker, 2020). There were 27 changes to the Concealed Carry Weapon (CCW) law across 24 states and the District of Columbia between 2000 and 2020: 9 states and DC changed policy to CCW Shall Issue, 13 states changed policy to CCW Permitless Carry, and 2 states loosened restrictions twice, once to CCW Shall Issue then to CCW Permitless Carry (Cherney et al., 2020). CCW Shall Issue removes local authority discretion on concealed carry permit issuance by mandating a permit when basic requirements are met, and CCW Permitless Carry is the most permissive policy with no permit required. Therefore I focus on these questions:

- How does CCW Shall Issue, CCW Permitless Carry, and Stand Your Ground each impact firearm sales? This research uses sales data from an anonymous online retail platform and FBI background checks data to measure sales.

- What impact does each policy have on public health outcomes related to safety? I test this through evaluating the effect of each policy on the following relevant outcomes: total suicides, firearm suicides, non-firearm suicides, total aggravated assaults, aggravated assaults using firearms, aggravated assaults not involving firearms, mass shootings, burglary, and accidental firearm deaths.

I construct a unique, comprehensive state-year-month panel data set from 2010 to 2017 from six data sources. I use two-way fixed effects panel regressions within a difference-in-differences research framework to estimate the average policy effects of each CCW Shall Issue, CCW Permitless Carry, and Stand Your Ground on each outcome of interest. I control for state fixed effects and year-month fixed effects alongside state-specific linear time trends to separate the policy effects from unobserved confounds. The identifying assumption is that conditional on these observables, the timing of policy adoption is not decided by unobserved factors that drive outcome variables. The timing of firearm policy adoption typically depends on the state legislative calendar rather than unobserved changes in firearm demand or usage.

The results show that CCW Shall Issue laws increase total online firearm sales (+6.8%, 95% CI: 2.8% to 11.1%), increasing handgun online sales (+17.9%; 95% CI: 11.1% to 25.4%), while reducing that of long guns (-5.7%; 95% CI: -10.3% to -0.9%). CCW Shall Issue laws increase non-gun aggravated assaults by 5.5% (95% CI: 2.8% to 8.4%). The results do not show evidence of notable change in gun-related assaults, suicide, or accidental firearm deaths upon CCW Shall Issue law adoption. There is no strong evidence of impact on mass shootings (+0.03%; 95% CI: -0.1% to 0.2%).

CCW Permitless Carry laws increase handgun background checks by 9.2% (95% CI: 2.5% to 16.3%), but do not seem to precisely change total online firearm sales (-0.7%; 95% CI: -5.9% to 4.7%). CCW Permitless Carry laws increase firearm suicide (+1.7%; 95% CI: 0.3% to 3.0%) and accidental firearm deaths (+0.9%; 95% CI: 0.1% to 1.7%). Again, there is no strong evidence of an effect on mass shootings (-0.1%; 95% CI: -0.3% to 0.1%).

Stand Your Ground effects on total online firearm sales (-2.0%; 95% CI: -5.1% to 1.2%), firearm suicides (-0.2%; 95% CI: -1.4% to 1.1%), accidental firearm deaths (+0.4%, 95% CI: -0.7% to 1.5%), and gun aggravated assaults (+3.0%; 95% CI: -3.0% to 9.3%) are small with 95% confidence intervals containing zero, providing inconclusive evidence of any notable impact on each of the outcomes. I do not find strong evidence of an effect of Stand Your Ground on mass shootings (-0.2%; 95% CI: -0.5% to 0.0%). The findings are robust to models including covariates that are common in the firearm literature.

The structure of this paper is as follows: Section 2 presents the literature review, predictions, and intended contribution, Section 3 provides the empirical context, outlining the firearm usage restrictions studied. Section 4 describes the data and Section 5 explains the analysis method. Section 6 shows the regression results and Section 7 concludes.

2.2 Literature Review, Predictions, & Intended Contribution

This paper contributes to the literature at the intersection of marketing and policy that studies the effects of regulation on market demand and various externalities. Seiler, Tuchman, and Yao (2021) find a reduction in soda demand in places where a tax on sweetened beverages are imposed. Rao and Wang (2017) find a significant reduction in revenue of four separate food products after the Federal Trade Commission issued public consent orders to terminate false health claims in ads for those products. Tuchman (2019) finds a positive effect of e-cigarette advertising on the demand for traditional cigarette and smoking cessation products and calibrates a structural model to predict how a ban on e-cigarette advertising may not necessarily reduce smoking behavior. Wang, Lewis, and Singh (2016) analyze how anti-smoking strategies (e.g. excise taxes, anti-smoking advertising campaigns) reduce cigarette sales, but find that taxes have unintended consequences of causing consumers to switch to higher nicotine containing cigarettes. The firearm market has not been studied in marketing, and this paper contributes by studying the effect of regulation change on firearm sales and public safety.

Outside of marketing, this paper adds to the firearm literature that studies firearm usage restrictions effects on firearm acquisition. Evidence is scarce and inconclusive, in part due to the difficulty in measuring firearm ownership. Duggan (2001b) does not find any evidence that Concealed Carry Shall Issue adoption leads to change in firearm ownership, as measured by *Guns & Ammo* magazine sales data. Steidley and Kosla (2018) do not find a relationship between permissive concealed carry laws and firearm demand, as measured by the annual number of FBI Background checks per 100,000 using data from 1999-2010. Wallace (2014) finds a positive association between Stand Your Ground and FBI background checks, and a negative association of Stand Your Ground with the proportion of firearm suicides (a popular proxy for firearm ownership).[1] My paper contributes to the literature by analyzing more recent direct sales through unique data available for all states from an online firearm retail platform. I have not found any other paper that studies how firearm policy impacts firearm acquisitions using multiple measures and extensive policy variation.

The authors of the aforementioned studies had in-going predictions that firearm acquisition would increase if firearm carrying and self-defense protections make firearm ownership more beneficial or useful (Duggan, 2001b; Wallace, 2014). The extent of increase is unclear because of differing perceptions of public safety after loosening firearm usage restrictions. For example, if all consumers felt more unsafe, then one could expect a huge surge in sales. On the other extreme, if some concealed carrying is perceived as a crime deterrent, then non-gun owners may free-ride on the existing carriers, which would lead to smaller increases in sales after the law change.[2]

This paper also adds to the vast firearm literature studying the causal effects of firearm policies on various public health outcomes (Donohue et al., 2019; Luca, Malhotra, & Poliquin, 2017). Schell, Cefalu, Griffin, Smart, and Morral (2020) studied the effects of child access prevention laws, concealed carry laws (which they refer to as Right-to-Carry "RTC" laws), and Stand Your Ground laws on total firearm deaths, and for each firearm homicides, and firearm suicides. Their primary outcome of interest is the effect of each of the laws six years after implementation. They analyze annual state level data from 1970 to 2016 across 50 states.

For concealed carry laws, the authors specifically code both Shall Issue and Permitless Carry laws as RTC laws instead of separating them. The majority of the states in their analysis

[1]Though the author refers to laws studied as the Castle Doctrine, the author analyzes Stand Your Ground effects based on the definitions in the paper.

[2]This is based on a strong reliance on community policing and so may be less applicable to rural areas. Dr. Jennifer Carlson, author of Citizen Protectors, summarized it well: "It's not just the idea of if I conceal carry then I'm safer. It's the idea that if I just imagine there's people out there who are conceal carrying then the world is safer" (Burnett, 2016).

shifted from May Issue to Shall Issue laws. They find a 0.87 probability of an increase in firearm deaths after 6 years of the adoption of permissive concealed carry laws. They find a 0.90 probability of an increase in firearm suicides and a 0.77 probability of increase in firearm homicides at year 6 post law implementation. The authors do not find conclusive evidence of an effect of Stand Your Ground on overall firearm deaths (0.77 probability of an increase in firearm deaths after 6 years of law implementation). They do not find an effect of Stand Your Ground on suicides, but find that Stand Your Ground laws increase homicides in the first year (probability 0.99), but the effect wanes by year 6.

The authors conducted research in parallel with my research. I also focus on concealed carry laws and Stand Your Ground effects. My work uses state-year-month data from 2010 to 2017 and in addition to suicides, I focus on several outcomes that the authors do not study such as aggravated assaults (split by total, with guns and without guns), accidental firearm deaths, mass shootings, and burglary. I also focus on the effects of these laws on firearm sales and background checks. Another notable difference is that I analyze the effects of Concealed Carry Shall Issue laws and Permitless Concealed Carry separately. I believe my work adds to this research by analyzing a more expansive set of outcome variables.

With fewer restrictions on gun usage, more people could easily self-defend themselves and deter crime, but this may also increase the likelihood of gun-related accidents and self-harm. How criminals may respond to loosening firearm usage restrictions is also unclear. Criminals may be less motivated to commit crime knowing that potential victims may be armed or can self-defend (Cook & Goss, 2020). Alternatively, more concealed carrying could escalate conflict and increase crime. I do not expect these laws to affect burglary since the laws are related to firearm usage in public. Therefore, I run the analysis on burglary as a placebo test to check the credibility of the model specification.

Increased concealed carrying could create deterrence of mass shootings in large public spaces where there may be a higher likelihood of more armed citizens. However, mass shooter motives vary, with some motives being gang-related while others relate to domestic violence or mental health (Krouse & Richardson, 2015). For these cases, increased public concealed carrying may not necessarily reduce or impact mass shootings.

Smart et al. (2020b) provides an in-depth synthesis of the current literature and concludes that there is inconclusive evidence of each Concealed Carry and Stand Your Ground law effects on assaults, mass shootings, suicides, and accidental firearm deaths. This is due to varied set of methods and data used. Therefore, my work is timely and important to understand the social impact of loosening firearm usage restrictions.

2.3 Empirical Context

2.3.1 Firearm Usage Restrictions

This study focuses on Concealed Carry laws and the Stand Your Ground law because they directly regulate one's ability to conceal carry and to use a gun for self-defense.

Concealed Carry laws

Concealed Carry laws apply to handguns and are regulated differently across states.

- **CCW Complete Prohibition**: Restricts all private citizens from concealed carry.
- **CCW May Issue**: Grants permits at the discretion of local law authorities who evaluate the specific need for firearm use (Blocher, 2014).
- **CCW Shall Issue**: Grants permits if all objective state requirements (e.g. no criminal record, proof of residency, no mental illness, background check) are met (HG.org, 2021).[3] Removes local authority discretion in permit issuance.
- **CCW Permitless Carry**: Allows people to carry guns in a concealed manner without a permit.

[3] Permit fees range from as low as $20 to approximately $150 (Csere, 2013).

Prior to the 1980s, most of the U.S. prohibited or highly regulated concealed carry laws to keep social order and prevent firearms from getting into the wrong hands (Cook & Goss, 2020). Carrying pistols was seen as behavior largely limited to felons, and therefore was more restricted (Spitzer, 2017). The National Rifle Association (NRA) supported the more restrictive laws at the time, but changed stance when more citizens wanted to arm themselves for protection amidst surging violence and rioting during the late 1960s and 1970s (Elving, 2017). From the 1980s through 1990s, some states moved from May Issue laws to Shall Issue laws, with the NRA actively lobbying for the changes. Now the NRA and other gun rights movements push for the adoption of Permitless Carry (Vasilogambros, 2021).

Stand Your Ground

Stand Your Ground (also referred to as No Duty to Retreat) is meant to give potential victims more legal protection when using guns for self-defense to reduce victimization (RAND, 2020). Stand Your Ground allows a person who perceives himself to be in imminent danger to use lethal force without trying to first retreat (Light, 2017). The law grants immunity and a valid self-defense claim to this person when charged with criminal homicide (Ward, 2014).

- **Castle Doctrine**: A person has no duty to retreat within one's home. This is the status quo. Some states loosely define "home" to include one's car or other private property.
- **Stand Your Ground**: Expands Castle Doctrine to anywhere in public (Cherney et al., 2020).

The Castle Doctrine was set in place in 1895 by the Supreme Court (*Beard v. United States*), and the doctrine's definitions of home and level of intrusion vary across states (Catalfamo, 2006). Florida was the first to adopt Stand Your Ground in 2005 expanding the legal protection to anywhere someone has a legal right to be beyond the home (Catalfamo, 2006; Wallace, 2014). Since then, more states have adopted Stand Your Ground (Cherney et al., 2020; Light, 2017).

2.3.2 Firearm Usage Restrictions Variation

A total of 17 states and the District of Columbia have loosened firearm usage restrictions between 2010 and 2017. Figure 2.1 summarizes the changes. There are no states that tighten firearm usage restrictions during this time period.

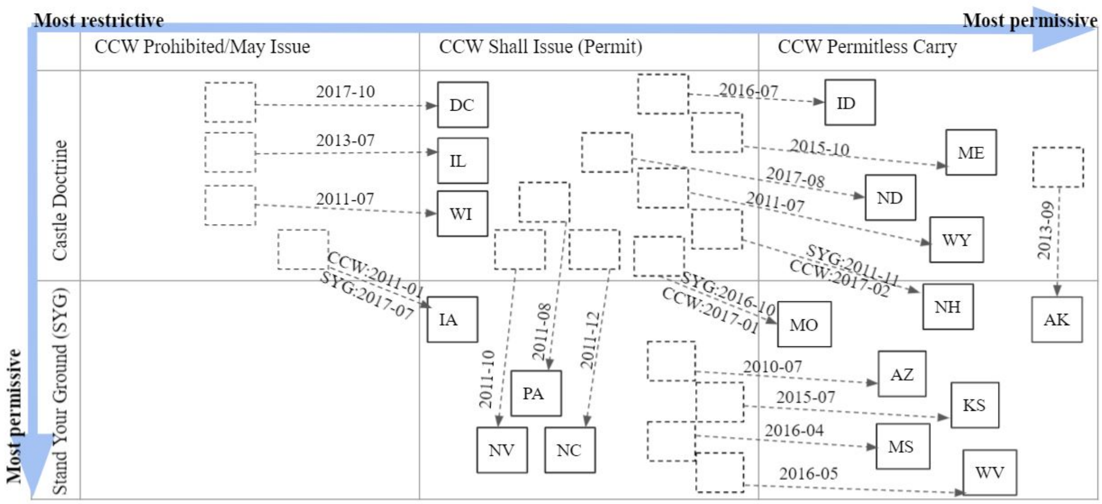


Figure 2.1. Policy Changes Summary

This figure shows which states change Concealed Carry policy or adopt Stand Your Ground. States are shown in boxes. Dotted line boxes show which policy the state previously had, and the arrow to the solid box shows the change in direction. The law adoption year-month is written for each law on the arrow. The Concealed Carry policy becomes more permissive from left to right. The vertical axis becomes more permissive from top to bottom as the law changes from Castle Doctrine to Stand Your Ground.

2.4 Data

I construct a monthly panel data set of policy measures, firearm sales measures, aggravated assaults, suicides, burglary, accidental firearm deaths, and mass shootings across 50 states and the District of Columbia between 2010 and 2017. Unlike previous literature in which analysis is limited to annual data, I analyze monthly data to allow for better controls than annual data for unmeasured confounds that may change at more granular temporal levels and reduce potential aggregation bias.

2.4.1 Policy

The policy data is from the RAND State Firearm Law Database. This is a comprehensive longitudinal data set that records which firearm laws were implemented across different states between 1979 and 2020. The database provides detailed information on 75 laws including each law's description, effective date, and type of law change (repeal, implementation, or modification). The database uses three different secondary sources, each compiled by think tanks. The database creators reviewed and added any missing information through using LexisNexis, Westlaw, and other law libraries (Cherney et al., 2020). There were no other local policies that varied extensively other than the three policies I study during the sample period (2010 to 2017).

2.4.2 Online Firearm Sales

Sales measures are from the population of firearm sales data from an anonymous online firearm retail platform. The data report individual transactions with information on the gun purchased, purchase date, sales amount, and the buyer state and zip code. I measure sales through the number of handgun transactions and long gun transactions for each state-year-month. This direct sales measure does not account for offline sales so it underrepresents total sales, but it accounts for both new and used firearms that are sold online by the retail platform, enabling me to drill down into sales features like price and buyer characteristics.

I also measure the total dollar revenue generated by the platform for each state-year-month. This is calculated using the dollar amount observed for each transaction in the data. Revenue is a valuable measure that can inform how a policy could impact average price.

2.4.3 Background Checks

FBI background checks are conducted through the National Instant Criminal Background Check System (NICS) and data are available on the FBI website. Background checks serve as a reliable measure to indicate sales. Kim and Wilbur (2021) find that background checks correlate highly at 0.8 with legal sales in Massachusetts. Background checks also correlate highly with survey measures of firearm prevalence (Lang, 2013). Recent literature has used FBI background checks as a proxy for firearm acquisition (Briggs & Tabarrok, 2014; Lang, 2013; Steidley & Kosla, 2018; Vitt et al., 2018). Background checks account for both offline and online sales. All federally licensed firearm retailers are required to run a background check before selling a firearm. However, background checks are not required for private sales in many states. Not all background checks are linked to a sale, which is why there is not a perfect correlation between background checks and sales in Kim and Wilbur (2021).

For the regressions on background checks, I remove District of Columbia, Pennsylvania and Hawaii due to NICS reporting irregularities. See Figure B.1 in the Appendix to see the exact distributions.

2.4.4 Public Health Outcomes

I collect as many public health outcome measures as possible to provide a comprehensive set of results. I evaluate suicides, aggravated assaults, mass shootings, burglary and accidental firearm deaths as separate outcomes.

Suicide data and accidental firearm deaths data are from the Multiple Causes of Death (MCOD) files from the National Center of Health Statistics (NCHS) division of the Centers for Disease Control and Prevention (CDC). MCOD files report information collected from U.S. death certificates provided by state vital registration offices.[4] Aggravated assaults and burglary data are from the FBI Uniform Crime Reports and Florida is removed from the regressions for these crime outcomes due to reporting irregularities.[5]

Mass shootings data are from the Gun Violence Archive. Mass shootings are defined as incidents where there are four or more victims aside from the shooter(s) that were shot or killed by a firearm. There were a total of 840 mass shooting incidents from January 2012 to

[4]I define firearm suicides using standard International Statistical Classification of Diseases and Related Health Problems (ICD-10) codes X72-X74.

[5]Crime is reported at the police agency level, and Florida is missing in the panel data because I only report agencies that fully report for all twelve months for all years in the sample.

December 2017 across the nation. The Gun Violence Archive collects this data from more than 7,500 sources in law enforcement, government, media and commercial sources.

2.4.5 Covariates

The models in the robustness checks include time-varying covariates commonly accounted for in the firearm literature (Donohue, Aneja, & Weber, 2017; Luca et al., 2017). The goal is to observe how consistent the policy effects are conditional on the set of more robust controls. The models including these covariates are included in the Appendix.

2.4.6 Descriptive Analysis

Figure 2.3 compares the sales rate trend between each Shall Issue adopting state and the average sales rate of non-adopting states for each handguns and long guns.[6] The graphs show that there are many changes to the firearm sales rate that correspond to various events such as the 2012 election and mass shootings.[7] This shows the importance to control for time trends. The graphs also show that the law-adopting states and non-adopting states' outcome follow parallel trends prior to the law adoption date. Three out of the four graphs show weak visual evidence of a lift in handgun sales after law adoption. I do not see a similar pattern for long gun sales rate trends, which vary by state post law adoption.

The dynamic patterns of the sales rate in the raw data graphs confirm the need to control for time effects and state-specific time trends in the regression model. Time effects can control for time shocks such as elections and mass shootings. Controlling for these existing secular trends would distinguish the policy impacts more accurately.

[6] Due to the large number of graphs generated for each treated state for each law for each outcome, I only show Shall Issue adopting states sales rate trends in the paper.

[7] The huge sales surge in 2013 in all graphs coincide with the timing of the Sandy Hook mass shooting.

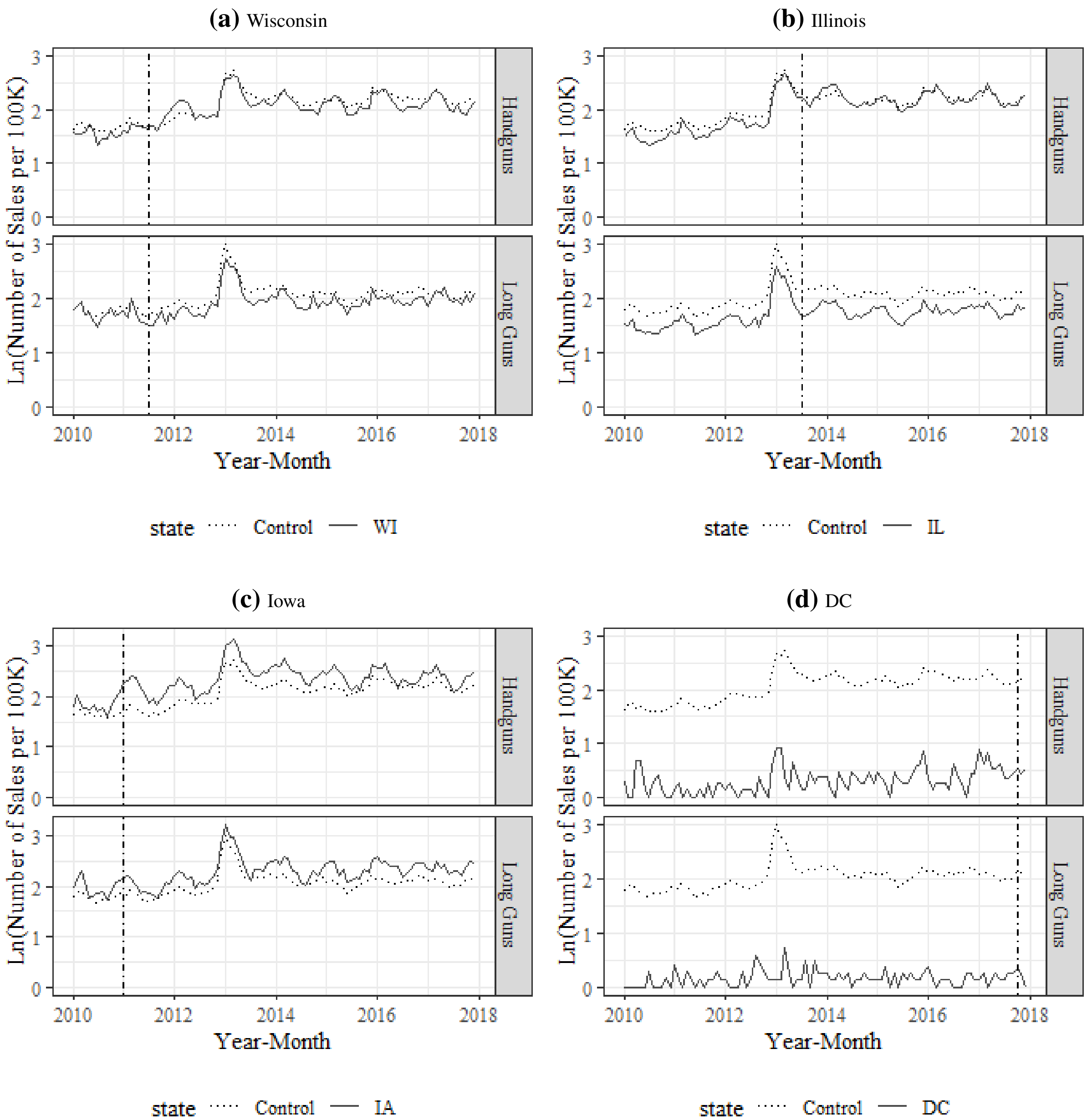


Figure 2.3. CCW Shall Issue Adoption Impact on Sales Comparison

Each panel (a), (b), (c), and (d) compares the log of number of sales transactions per 100K of an individual treated state (shown in solid line) with the average log number of sales per 100K of the control states (shown in dotted line). Within each panel, the top graph shows handgun sales, while the bottom graph shows long gun sales. The CCW Shall Issue law adoption timing is shown by a vertical dashed black line.

2.5 Method

2.5.1 Identification

I observe variation in timing of the firearm usage law adoptions and identify the policy effects from unobserved causes of the outcome variables. The identifying assumption is that conditional on observables, policy adoption timing is not determined by unobserved factors that may motivate firearm purchase. I assume that the law-adopting states and non-adopting states' outcome follow parallel paths in the absence of the law-adoption.

Conversations regarding a need for a certain policy may be inspired by a mass shooting event or a spike in sales, but the precise timing of the policy adoption is more likely to depend on where the bill is on the state legislative calendar. Not all bills on the calendar may be considered on the floor hearing. The timing of when a bill appears on the calendar and how fast a bill moves through floor hearings in the House and Senate are dependent on the level of debate, support, or urgency of the bill (congress.gov, 2021). When Maine switched to a Permitless Concealed Carry law in 2015, the bill's sponsor Senator Brakey highlighted how long the effort took to pass the law. "This legislation has been a goal for Maine supporters of the Second Amendment for nearly two decades, and it is wonderful to see this commonsense measure finally enshrined in state law" (McCrea, 2015). In this way, the specific timing of policy adoption seems to not be driven by the unobservables that drive firearm acquisitions and public health outcomes, so I find the identifying assumption reasonable.

To further support the causal interpretation of the results, I look at the policy effect in periods prior to the actual policy adoption date to check that the law-adopting and non-adopting states are comparable. I would expect coefficients of the policy effect in periods before actual law adoption to be close to zero (with the confidence intervals containing zero). I find this to be the case when doing this analysis. See Figure B.3 in the Appendix for this robustness check analysis. This gives more credibility that the permissive usage law adoption is indeed the cause of any change in outcome.

2.5.2 Model

This research uses the difference-in-differences design to identify each policy effect. This enables estimating the difference in change in outcome before and after law adoption in law-adopting states versus the contemporaneous change in non-adopting states. I specify a two-way fixed effects model to effectively separate the policy effect from unobserved confounds. I estimate the policy effect on the average time period after policy adoption.

The main regression model for each outcome is as follows:

$$ln(y_{it}) = \beta_1 permitlesscarry_{it} + \beta_2 ccwshall_{it} + \beta_3 standyourground_{it} + \alpha_i + \lambda_t + \alpha_i t + \varepsilon_{ct}$$

y_{it} indicates the outcome variable. I transform all outcome variables to per 100,000 of population and take the natural log. The model includes treatment dummies for each CCW Permitless Carry, CCW Shall Issue, and Stand Your Ground laws. The model includes fixed effects α_i and λ_t to control for time-invariant and state-invariant unobserved confounds. State-specific linear time trends ($\alpha_i t$) control for unobserved secular trends in the given outcome variable to be able to separate the policy effects from these unobserved state/time factors that might correlate with the policy. Including state-specific linear time trends into the model is a conservative econometric specification. I prefer to over-control for time-varying unobservables rather than to risk spurious findings. The model weights the observations by population and clusters the standard errors by state.

2.6 Results

This section shows the empirical model results for the policy effects on each sales (split by total, handgun, and long gun sales), suicides, aggravated assaults, burglary, accidental firearm deaths, and mass shootings. I analyze suicides and aggravated assaults by total, those involving guns, and those not involving guns.

2.6.1 Policy Effects on Firearm Sales

Regression (1) in Table 2.1 shows that adopting CCW Shall Issue leads to an average 6.8% (95% CI: 2.8% to 11.1%) increase in total online sales. Regression (2) shows that handgun sales increase by 17.9% (95% CI: 11.1% to 25.4%), which aligns with how the Concealed Carry law applies to handguns. The 5.7% (95% CI: -10.3% to -0.9%) reduction in long gun sales in Regression (3) suggests a potential substitution or shift in product interest from long guns to handguns with the law change. CCW Permitless Carry effects are small and imprecise. Stand Your Ground law effects are also small and close to zero. Table B.1 in the Appendix compares the main base model to the model including all covariates for each total sales, handgun sales, and long gun sales. The CCW Shall Issue policy effects are robust to adding other covariates in the model.

Table 2.1. Policy Impact on Sales

	Dependent variable:		
	Ln(Total Sales)	Ln(Handgun Sales)	Ln(Long gun Sales)
	(1)	(2)	(3)
CCW Permitless Carry	−0.007	−0.016	−0.017
	(0.027)	(0.034)	(0.023)
CCW Shall Issue	0.066***	0.165***	−0.059**
	(0.019)	(0.030)	(0.025)
Stand Your Ground	−0.020	−0.011	−0.013
	(0.016)	(0.019)	(0.030)
Observations	4,896	4,896	4,896
R^2	0.978	0.975	0.964
Adjusted R^2	0.977	0.974	0.963

**p<0.05; ***p<0.01

Note: Coefficients represent the policy effects on the natural logarithm of sales per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The observations are weighted by population and the standard errors are clustered by state.

Table 2.2 shows the main models with Revenue as the outcome variable. The results align with the suggested handgun market expansion and product substitution from long guns to handguns shown from the sales transactions results earlier. The CCW Shall Issue adoption leads to a 12.4% (95% CI: 2.6% to 23.0%) increase in handgun revenue and a 11.1% decrease (95% CI: -17.5% to -4.1%) in long gun revenue. The handguns purchased are cheaper on average, since the average price of handguns decreases by 6.0% (95% CI: -8.2% to -3.8%) upon CCW Shall Issue adoption. The results are robust to adding other covariates in the model as shown in Table B.2 in the Appendix. CCW Permitless Carry and Stand Your Ground point estimates are small and close to zero.

Table 2.2. Policy Impact on Revenue

	Dependent variable:		
	Ln(Total Revenue)	Ln(Handgun Revenue)	Ln(Long Gun Revenue)
	(1)	(2)	(3)
CCW Permitless Carry	0.035	0.056	0.008
	(0.028)	(0.032)	(0.030)
CCW Shall Issue	0.011	0.117^{**}	-0.117^{***}
	(0.028)	(0.045)	(0.037)
Stand Your Ground	-0.022	-0.013	-0.024
	(0.018)	(0.027)	(0.037)
Observations	4,896	4,896	4,896
R^2	0.963	0.957	0.937
Adjusted R^2	0.961	0.955	0.935

$^{**}p<0.05$; $^{***}p<0.01$

Note: Coefficients represent the policy effects on the natural logarithm of revenue per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The observations are weighted by population and the standard errors are clustered by state.

I also investigate whether using background checks as a proxy for sales shows a similar pattern to the direct sales increase shown previously. Background checks conducted for firearm transactions are split by the type of firearm the purchase was for. Similar to previous results, Table 2.3 shows that CCW Shall Issue increases handgun checks by 24.5% (95% CI: 1.8% to 52.3%) and reduces long gun checks by 8.1% (95% CI: -14.8% to -1.0%). CCW Permitless Carry increases handgun checks by 9.2% (95% CI: 2.5% to 16.3%). These results remain robust

Table 2.3. Policy Impact on FBI Background Checks

	Dependent variable:		
	Ln(Total Checks)	Ln(Handgun Checks)	Ln(Long Gun Checks)
	(1)	(2)	(3)
CCW Permitless Carry	−0.028	0.088***	0.018
	(0.043)	(0.031)	(0.025)
CCW Shall Issue	0.097	0.219**	−0.085**
	(0.135)	(0.100)	(0.037)
Stand Your Ground	0.093	−0.064**	0.039
	(0.071)	(0.027)	(0.027)
Observations	4,608	4,608	4,608
R^2	0.940	0.967	0.956
Adjusted R^2	0.937	0.966	0.954

**p<0.05; ***p<0.01

Note: Coefficients represent the policy effects on the natural logarithm of the number of background checks per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The observations are weighted by population and the standard errors are clustered by state. Hawaii, Pennsylvania, and District of Columbia are excluded from data.

when adding covariates as shown in Table B.3 in the Appendix.

The Shall Issue adoption's effect on handgun checks has a much larger magnitude than that of CCW Permitless Carry (24.5% vs. 9.2%). This could be that the shift to Shall Issue already made concealed carry easy enough to pursue. Since CCW Permitless Carry does not necessarily have legal requirements that differ from CCW Shall Issue (other than the existence of a permit), it makes sense that there is only a small increase. There are no states that directly shift from CCW May Issue to CCW Permitless Carry. The findings show that Stand Your Ground reduces handgun checks by 6.2% (95% CI: -11.1% to -1.0%). Yet, Stand Your Ground does not meaningfully change any other sales measure.

2.6.2 Policy Effects on Public Health

The loosened usage restrictions make it easier for the public to use firearms. The firearm sales, revenue, and background check results suggest that loosening concealed carry restrictions increase handgun sales. Opinion is split on whether loosening firearm usage restrictions make the public safer or more dangerous. In this section, I explore several different outcomes related to public safety: suicides (split by total, handguns and long guns), aggravated assaults (split by total, handguns and long guns), burglary, mass shootings, and accidental firearm deaths.

Suicides

Nearly three fourths of firearm suicides are committed by handguns (Hanlon, Barber, Azrael, & Miller, 2019). Table 2.4 shows that CCW Permitless Carry laws increase firearm suicides by 1.7% (95% CI: 0.3% to 3.0%). CCW Permitless Carry's effect on total suicides and non-firearm suicides are small with the confidence interval containing zero. The effects of CCW Shall Issue and Stand Your Ground are all small and close to zero, with 95% confidence intervals containing zero. These results are robust to adding other covariates in the model (See Table B.4 in the Appendix).

Table 2.4. Policy Impact on Suicides & Accidental Firearm Deaths

	Dependent variable:			
	Ln(Total Suicides)	Ln(Firearm Suicides)	Ln(Non-Firearm Suicides)	Ln(Accidental Firearm Deaths)
	(1)	(2)	(3)	(4)
CCW Permitless Carry	0.030	0.017**	0.006	0.009**
	(0.025)	(0.007)	(0.017)	(0.004)
CCW Shall Issue	−0.004	−0.008	0.001	−0.001
	(0.037)	(0.009)	(0.017)	(0.001)
Stand Your Ground	0.005	−0.002	0.004	0.004
	(0.012)	(0.006)	(0.011)	(0.005)
Observations	4,896	4,896	4,896	4,896
R^2	0.818	0.865	0.590	0.323
Adjusted R^2	0.810	0.860	0.573	0.294

$^{**}p<0.05$; $^{***}p<0.01$

Note: Coefficients represent the policy effects on the natural logarithm of suicide rate per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The model is weighted by population and standard errors are clustered by state.

Accidental Firearm Deaths

While many people carry firearms for self-defense with no ill intentions, firearms can be dangerous and accidental deaths may occur. Regression (4) results in Table 2.4 show that CCW Permitless Carry laws increase accidental firearm deaths by 0.9% (95% CI: 0.1% to 1.7%). Shall Issue and Stand Your Ground effects are very small and the confidence intervals include zero. These results are robust to adding other covariates in the model (See Regressions (1) and (2) in Table B.6 in the Appendix).

Aggravated Assaults

Loosening usage restrictions could either reduce assaults if it creates crime deterrence or increase assaults by escalating conflict. The first three regressions in Table 2.5 show the effects of the policies on total aggravated assaults, aggravated assaults involving a gun, and those not involving a gun. Effects on total assaults and firearm assaults by each of the three policies are small and the 95% confidence interval contain zero.

Non-gun aggravated assaults increase by 5.5% (95% CI: 2.8% to 8.4%) after CCW Shall Issue adoption. It could be that carrying guns escalates conflict as people are less fearful to encounter conflict while carrying a gun. The gun carrying could serve as an insurance in case of a threat, so non-gun assaults increase rather than gun assaults. Stand Your Ground effects on total, gun, and non-gun assaults are small and the confidence intervals contain zero. These results are robust to adding other covariates in the model (See Table B.5 in the Appendix).

I run the regression model on burglary as a placebo test to check the credibility of the model specification. Concealed carry laws and Stand Your Ground are related to firearm usage in public, therefore I do not expect these laws to affect burglary, which is breaking into homes to steal. I do not find a meaningful effect as shown in Regression (5) of Table 2.5.

Table 2.5. Policy Impact on Various Crime

	Dependent variable:				
	Ln(Agg. Assaults)	Ln(Gun Agg. Assaults)	Ln(Non-gun Agg. Assaults)	Ln(Mass Shootings)	Ln(Burglary)
	(1)	(2)	(3)	(4)	(5)
CCW Permitless Carry	−0.008	0.047	−0.033	−0.001	0.002
	(0.018)	(0.024)	(0.021)	(0.001)	(0.024)
CCW Shall Issue	0.004	0.015	0.054***	0.0003	−0.060
	(0.036)	(0.011)	(0.013)	(0.001)	(0.045)
Stand Your Ground	0.023	0.030	0.009	−0.002	0.043
	(0.014)	(0.030)	(0.016)	(0.001)	(0.041)
Observations	4,800	4,800	4,800	3,672	4,800
R^2	0.987	0.970	0.990	0.159	0.984
Adjusted R^2	0.987	0.968	0.989	0.116	0.983

**p<0.05; ***p<0.01

Note: Coefficients represent the policy effects on the natural logarithm of crime rate per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The model is weighted by population and standard errors are clustered by state. Assaults and Burglary data excludes Florida due to missing crime data. Mass Shootings data is from 2012-2017.

Mass Shootings

Next, I also examine the policy effects on mass shootings. Whether more armed citizens would deter potential mass shootings is uncertain. As shown in Regression (4) of Table 2.5, the magnitude of the effects are very small and close to zero. The results show that the policies do not have any meaningful impact on mass shooting incidents, and this is also the case in the model with additional covariates (See Regression (4) in Table B.6 in the Appendix).

2.7 Discussion and Conclusion

This paper estimates the average impact of loosening firearm usage restrictions on firearm sales and key public health outcomes including suicides and aggravated assaults (each split by total, those involving firearms and those that do not), burglary, accidental firearm deaths, and mass shootings.

CCW Shall Issue laws increase total online firearm sales by 6.8% (95% CI: 2.8% to

11.1%) and increase handgun demand with large increases in both online handgun sales (+17.9%; 95% CI: 11.1% to 25.4%) and handgun background checks (+24.5%; 95% CI: 1.8% to 52.3%). CCW Shall Issue laws decrease long gun demand, with a decrease of online long gun sales by 5.7% (95% CI: -10.3% to -0.9%) and a decrease of long gun background checks by 8.1% (95% CI: -14.8% to -1.0%). This product substitution from long guns to handguns implies that loosening concealed carry restrictions does not mean a gain for all manufacturers - lobbying for a push for loosening usage restrictions may not necessarily be in the best interest of long gun manufacturers.

CCW Shall Issue laws increase non-gun aggravated assaults by 5.5% (95% CI: 2.8% to 8.4%), suggesting an escalation of conflict. CCW Shall Issue effects on the other crime studied, suicides, mass shootings, and accidental firearm deaths are small with confidence intervals containing zero. Suppose the Supreme Court were to decide in the current case, or in any future case, that CCW May Issue is inconsistent with the Second Amendment and makes CCW Shall Issue federal law, then approximately 6,903 more non-gun assaults would occur if the 8 non-adopting restrictive states all adopted CCW Shall Issue (2017 baseline: 413K).

CCW Permitless Carry adoption effects show a different pattern from CCW Shall Issue. There are small, imprecise changes to online sales. Yet, CCW Permitless Carry laws increase handgun background checks by 9.2% (95% CI: 2.5% to 16.3%), which is an effect size much smaller than that shown after CCW Shall Issue adoption. Shifts to CCW Shall Issue may already make concealed carry easy to pursue, and Permitless Carry legal requirements do not differ much from CCW Shall Issue other than the existence of a permit. CCW Permitless Carry laws increase firearm suicides by 1.7% (95% CI: 0.3% to 3.0%) and accidental firearm deaths by 0.9% (95% CI: 0.1% to 1.7%). This suggests that CCW Permitless Carry may attract firearm owners that are less careful than the average owner. If Permitless Carry were adopted as federal law shifting all non-adopting states to adopt the law, then there would be about 351 more firearm suicides (2017 baseline: 24K) and about 4 more accidental firearm deaths (2017 baseline: 488) than the status quo. Given these adverse consequences, the firearm industry could increase messaging to new firearm owners about the importance of firearm safety and training.

Stand Your Ground's effect on online sales is small with confidence intervals containing zero. I do not find notable evidence of Stand Your Ground effects on the public health outcomes studied. Stand Your Ground may still have an effect on other various crimes not dealt with in this paper.

There is no evidence of an effect on mass shootings for all three policies studied. Future studies can look further into why certain laws increase sales and public health outcomes versus others.

A limitation of this study is that it assumes that unobserved confounds are in the form of linear trends and thus can be controlled for through linear time trends. If they follow non-linear trends, then the model does not fully account for the potential biases.

With firearm usage regulation remaining to be a controversial topic, more attention and continued research on firearm usage regulation is needed. This study so far shows that policy can affect the type of guns sought and the possible degree of misuse. Continued research will help paint a clearer picture of the complexities of firearm usage motivations and firearm policy effects.

Chapter 2, contains unpublished material. The dissertation author was the solo investigator and author of this material.

Appendix A for Chapter 1

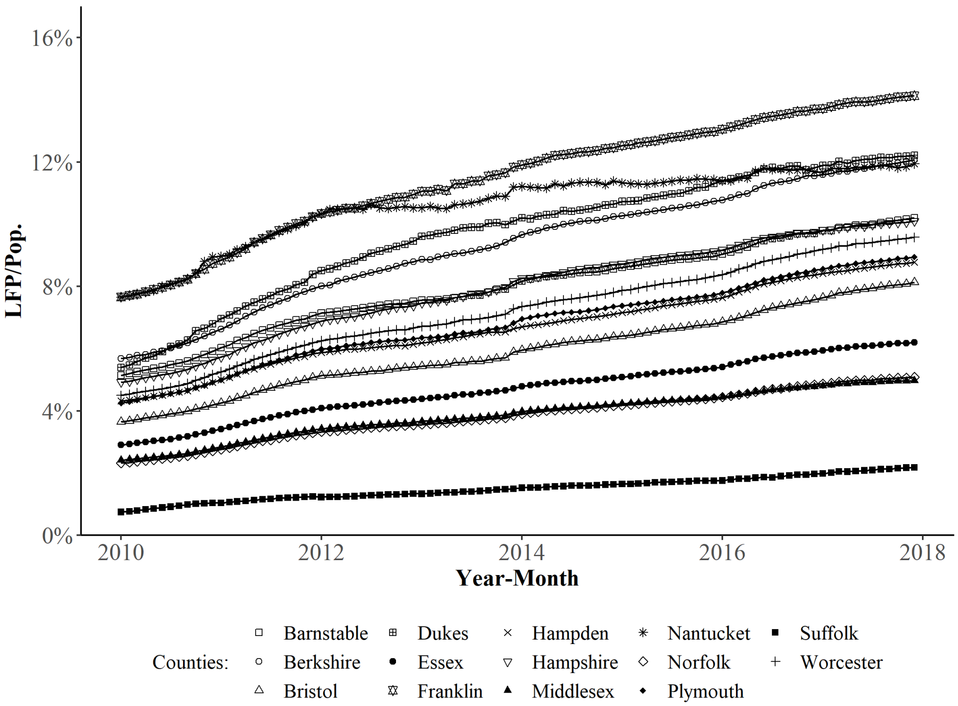
A.1 Backfilling License Data

Massachusetts' firearm license records date back to January 1, 2006. Therefore, the data enable direct counts of licensees after January 1, 2012, as licenses remain valid for 6 years after issuance. However, the 2010 and 2011 data undercount the true numbers of firearm licensees, as they exclude valid licenses issued in 2004 and 2005. This manifests in the data as a misleadingly steep upward trend in 2010-11, as depicted in panel (a) in Figure A.1.

To address left censoring in 2010 and 2011, we first checked how often firearms licenses are renewed, expecting a high renewal rate. The data confirm that expectation: among all license renewals observed after 2012, 94% of licensees are observed to hold at least one valid license between 2006 and 2012. We therefore resolve the left-censoring issue by "backfilling" all resident license renewals observed in 2010 and 2011. For example, a licensee who is issued a license renewal in February 2011 is also counted as an active licensee from January 2010 until January 2011. After backfilling the LFP trend looks much smoother as shown in panel (b) in Figure A.1.

We have replicated the analysis in section 4 without any backfilling, and with stochastic backfilling of 94% of licenses. The empirical results are relatively insensitive to which method of backfilling is used.

(a) No Backfill



(b) 100% Backfill

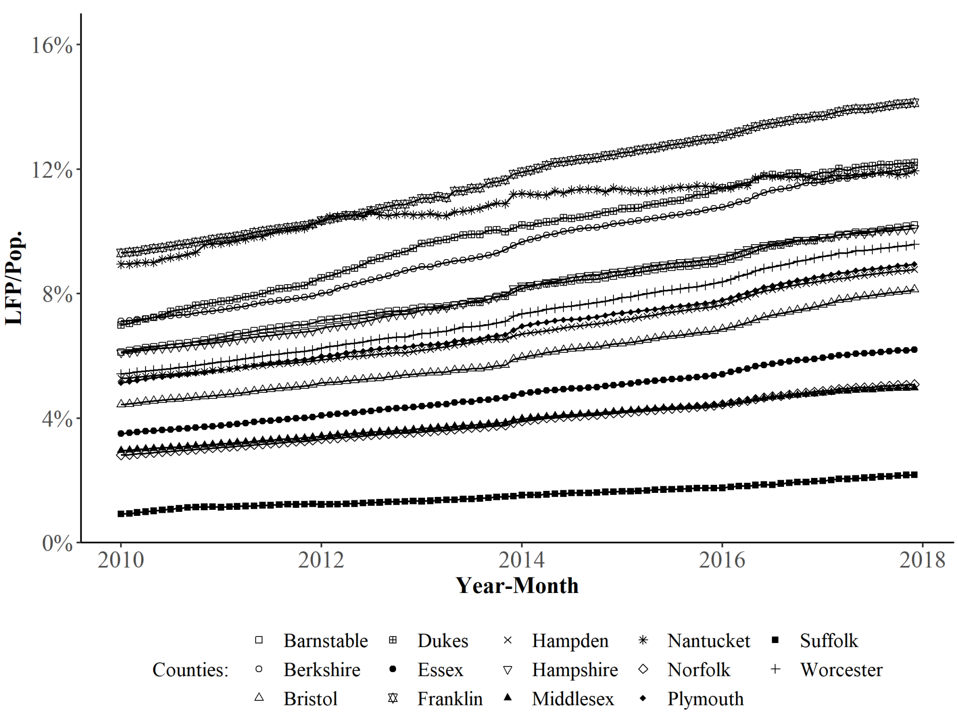


Figure A.1. 100% Backfill vs. No Backfill: LFP trend by county

Panel (a) shows LFP over time for each MA county using the raw data, while Panel (b) shows that using backfilled data.

A.2 Stability of Cross-Sectional Correlations

Here we examine the stability of cross-sectional correlations across successive time periods. The goal is to illustrate why FSS can be both a good cross-sectional proxy for LFP and a poor cross-temporal proxy for LFP.

We report the following exercise: Suppose that we only had two years of data available; how much would the cross-sectional correlations depend on which two years we analyze? We partition the sample into four distinct two-year periods and calculate cross-sectional correlations within each.

Table A.1 shows that STR is not only the best cross-sectional proxy for LFP overall, it is also the most stable across subsamples. The four cross-sectional correlations between STR and LFP range from .82 to .86, a range that contains the overall eight-year correlation of .86. OS is less stable, with correlations ranging from .46 to .64. FFL can only be measured in two partitions, correlating with LFP at .55 and .59.

Firearm Suicides is the most stable suicide-based proxy, with two-year correlations ranging from .39 to .58. Correlations between LFP, FSS and Suicides are substantially more volatile. FSS correlates with LFP at .26 in 2010-11 and at .66 in 2016-17. Remarkably, all four two-year correlations between FSS and LFP are smaller than the eight-year correlation of .74, and only one of the four is statistically significant. The correlation between Suicides and LFP also ranges widely from .36 to .79.

Figure A.2 graphs LFP and FSS for the fourteen counties within each two-year partition. The relationships are influenced by outliers, including three observations of zero firearm suicides in one county, and one observation of FSS equal to one. FSS appears to be particularly prone to outliers (e.g., 1) or indeterminacy (i.e. 0/0) when measured in more granular samples than the other candidate proxies, confirming limitations on the geographic and temporal resolutions in which it may be applied.

Table A.1. Biennial Cross-sectional Correlations between LFP and Candidate Firearm Proxies

Proxy	ID	1	2	3	4	5	6	7
Panel 1: 2010-2011								
Proxy	**ID**	**1**	**2**	**3**	**4**	**5**	**6**	**7**
LFP/Pop.	**1**	1.00						
FSS	**2**	0.26	1.00					
FS/Pop.	**3**	0.39	0.95*	1.00				
S/Pop.	**4**	0.48	0.07	0.36	1.00			
STR/Pop.	**5**	0.86*	0.29	0.41	0.42	1.00		
OS/Pop.	**6**	0.46	0.54*	0.64*	0.29	0.59*	1.00	
FFL/Pop.†	**7**	NA	NA	NA	NA	NA	NA	1.00
Panel 2: 2012-2013								
Proxy	**ID**	**1**	**2**	**3**	**4**	**5**	**6**	**7**
LFP/Pop.	**1**	1.00						
FSS	**2**	0.38	1.00					
FS/Pop.	**3**	0.49	0.96*	1.00				
S/Pop.	**4**	0.43	0.27	0.49	1.00			
STR/Pop.	**5**	0.82*	0.20	0.33	0.50	1.00		
OS/Pop.	**6**	0.64*	-0.43	-0.32	0.14	0.60*	1.00	
FFL/Pop.†	**7**	NA	NA	NA	NA	NA	NA	1.00
Panel 3: 2014-2015								
Proxy	**ID**	**1**	**2**	**3**	**4**	**5**	**6**	**7**
LFP/Pop.	**1**	1.00						
FSS	**2**	0.38	1.00					
FS/Pop.	**3**	0.58*	0.94*	1.00				
S/Pop.	**4**	0.79*	0.48	0.74*	1.00			
STR/Pop.	**5**	0.82*	0.37	0.47	0.58*	1.00		
OS/Pop.	**6**	0.64*	-0.09	0.11	0.47	0.62*	1.00	
FFL/Pop.†	**7**	0.55*	0.52	0.55*	0.43	0.71*	0.50	1.00
Panel 4: 2016-2017								
Proxy	**ID**	**1**	**2**	**3**	**4**	**5**	**6**	**7**
LFP/Pop.	**1**	1.00						
FSS	**2**	0.66*	1.00					
FS/Pop.	**3**	0.41	0.83*	1.00				
S/Pop.	**4**	0.36	0.76*	0.99*	1.00			
STR/Pop.	**5**	0.85*	0.39	0.05	-0.01	1.00		
OS/Pop.	**6**	0.59*	-0.05	-0.20	-0.19	0.65*	1.00	
FFL/Pop.†	**7**	0.59*	0.17	-0.26	-0.33	0.79*	0.36	1.00

Note. *$p<0.05$. 14 observations (1 per county) are used to calculate each correlation. †:Data available from 2014-2017, with Sep, Oct 2015 missing.

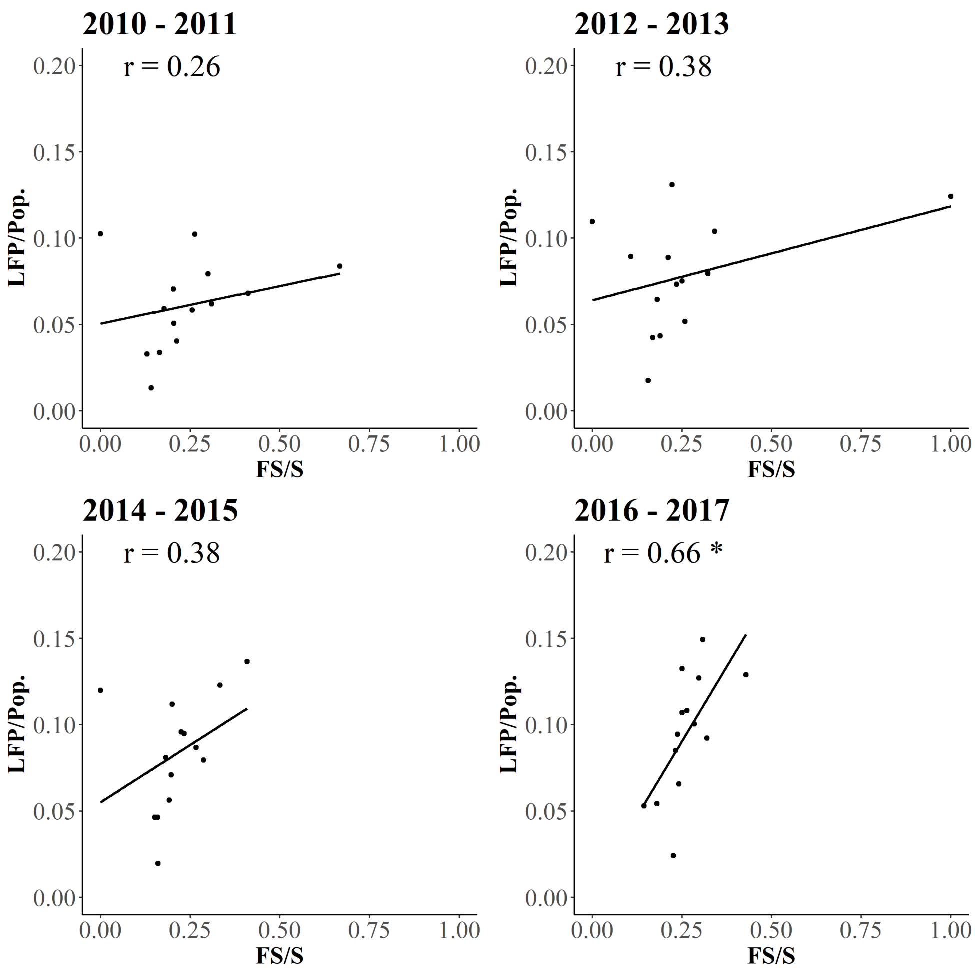


Figure A.2. Scatterplots of FSS and LFP - Biennial Comparisons

Each point represents a county. r is the cross-sectional Pearson correlation score between FSS and LFP/Pop. *p<0.05.

Appendix B for Chapter 2

B.1 FBI Background Checks Distribution

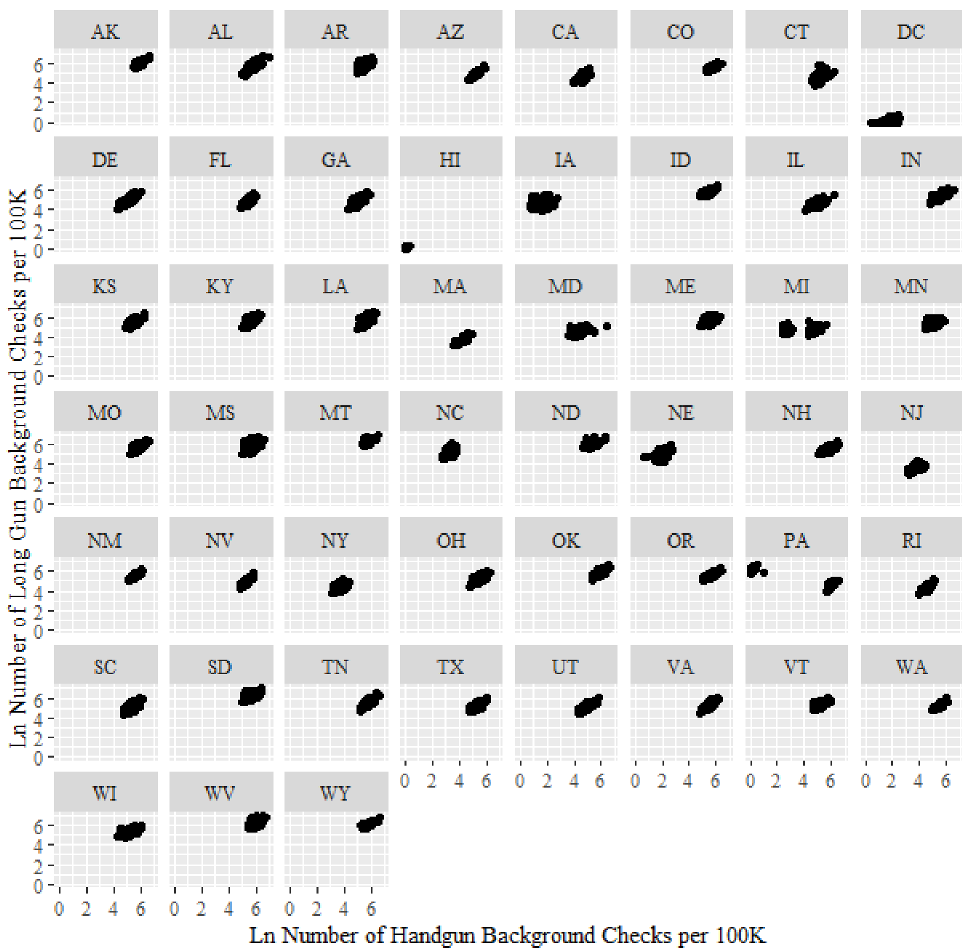


Figure B.1. Distribution of Handgun and Long Gun Background Checks 100K by State

This figure shows the distribution of the natural log number of background checks by state. The x-axis shows the log number of handgun background checks per 100K, while the y-axis shows the log number of long gun background checks per 100K. It is unclear why there are long periods of zero checks in these Hawaii, Pennsylvania, and DC.

B.2 Robustness Check: Event study plot for CCW Shall Issue on sales

To further support the causal interpretation of the results, I look at the policy effect in periods prior to the actual policy adoption date to check that the law-adopting and non-adopting states are comparable. I would expect coefficients of the policy effect in periods before actual law adoption to be close to zero (with the confidence intervals containing zero). This gives more credibility that the permissive usage law adoption is indeed the cause of any change in outcome. I expect to see the coefficients of the pre-treatment dummies to be close to zero and for there to be a lift in sales post law adoption for handguns since the concealed carry law pertains to handguns.

I regress sales on year-month dummy variables for the 12 months prior to the CCW Shall Issue adoption and 12 months after the adoption to show the dynamics of the policy effect on each total, handgun and long gun sales. I retain the CCW Permitless Carry and Stand Your Ground policy dummy variables, the year-month and state fixed effects and state-specific linear time trends in the model. The observations are weighted by population and the standard errors are clustered by state.

Figure B.3 graphs the estimates for each dummy variable for the year-months before and after the CCW Shall Issue adoption. The bars indicate the 95% confidence interval for each estimate. As expected, the pre-law adoption dummies are close to zero with most confidence intervals containing zero. Significant positive diversion from zero appear after the CCW Shall Issue adoption for total sales and handgun sales. Long gun sales start to trend downward post-law adoption but the confidence intervals are large.

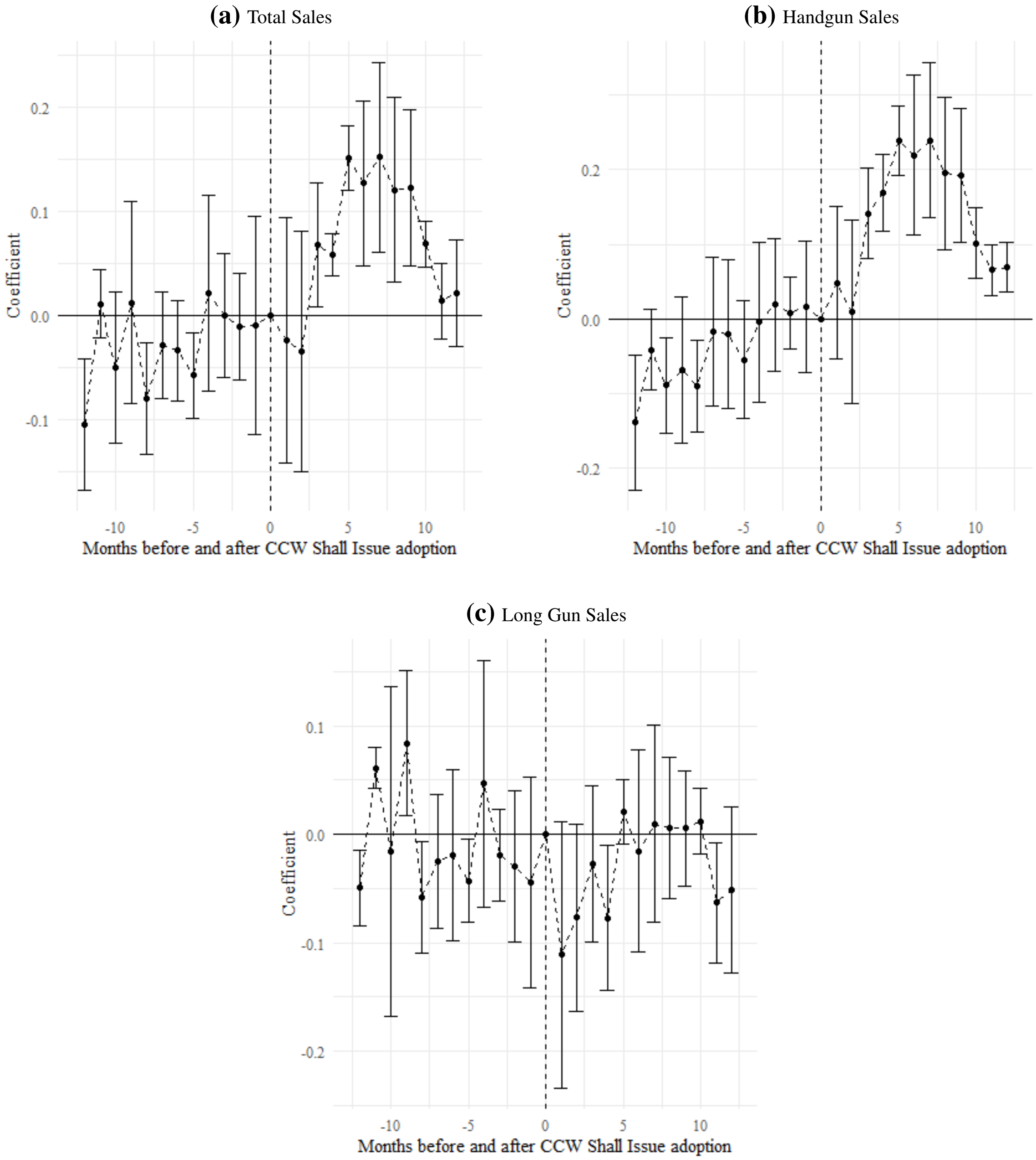


Figure B.3. Dynamic Effects of CCW Shall Issue Adoption on Sales

Each panel (a), (b), (c) shows the dynamic effects of CCW Shall Issue adoption on each total sales, handgun sales, long gun sales per 100,000, showing the estimates of the year-month dummy variables for 12 months prior to the CCW Shall Issue law adoption and post 12 months. The model retains the policy dummy variables for CCW Permitless Carry and Stand Your Ground, state and year-month fixed effects, and state-specific linear time trends. The regression model weights the observations by population and clusters the standard errors by state. The graphs display the 95 percent confidence interval for each estimate. '0' on the x-axis (dashed vertical line) is the year-month of the law adoption, which is the reference period.

B.3 Robustness Check: Results for model with more covariates

The set of covariates included in the regressions on sales, revenue and background checks are incarceration rate, police employment rate, unemployment rate, log of real per capita personal income, alcohol consumption measured by ethanol gallons per capita, percent of population under poverty line, population density, the log of aggravated assaults per 100,000, and the log of total suicides per 100,000. Florida is removed from the regressions with covariates due to reporting irregularities in the aggravated assaults data.

One caveat is that the covariates are available only at the annual level with the exception of aggravated assaults, suicides, and unemployment, which are available monthly. Despite this limitation, I include these covariates in the model.

I modify the covariates that go in the regressions on suicides, aggravated assaults, burglary, mass shootings and accidental firearm deaths. The covariates include incarceration rate, police employment rate, unemployment rate, log of real per capita personal income, alcohol consumption measured by ethanol gallons per capita, percent of population under poverty line, population density, the log of sales per 100,000, and the log of number of background checks per 100,000. Hawaii, Pennsylvania, and the District of Columbia are excluded in the model with covariates due to reporting irregularities in the background check data. For aggravated assaults and burglary regressions, Florida is removed due to reporting irregularities in the crime data.

Table B.1. Robustness Check for Sales

	Dependent variable:					
	Ln(Total Sales)		Ln(Handgun Sales)		Ln(Long gun Sales)	
	Base	Covariates	Base	Covariates	Base	Covariates
	(1)	(2)	(3)	(4)	(5)	(6)
CCW Permitless Carry	−0.007	−0.009	−0.016	−0.014	−0.017	−0.016
	(0.027)	(0.023)	(0.034)	(0.029)	(0.023)	(0.020)
CCW Shall Issue	0.066***	0.050***	0.165***	0.149***	−0.059**	−0.077***
	(0.019)	(0.015)	(0.030)	(0.035)	(0.025)	(0.015)
Stand Your Ground	−0.020	−0.008	−0.011	−0.003	−0.013	−0.002
	(0.016)	(0.016)	(0.019)	(0.020)	(0.030)	(0.028)
Observations	4,896	4,692	4,896	4,692	4,896	4,692
R^2	0.978	0.980	0.975	0.977	0.964	0.966
Adjusted R^2	0.977	0.979	0.974	0.976	0.963	0.965

**p<0.05; ***p<0.01

Note: Coefficients represent the policy effects on the natural logarithm of sales per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The model is weighted by population and standard errors are clustered by state. The covariates model includes unemployment rate, population density, log of real per capita personal income, alcohol consumption, % under poverty, incarceration rate, police employment rate, log number of aggravated assaults per 100K, log number of suicides per 100K. Florida data is not available for crime, so the model including covariates has a smaller base size.

Table B.2. Robustness Check for Revenue

	Dependent variable:					
	Ln(Total Revenue)		Ln(Handgun Revenue)		Ln(Long gun Revenue)	
	Base	Covariates	Base	Covariates	Base	Covariates
	(1)	(2)	(3)	(4)	(5)	(6)
CCW Permitless Carry	0.035	0.025	0.056	0.040	0.008	−0.001
	(0.028)	(0.020)	(0.032)	(0.024)	(0.030)	(0.023)
CCW Shall Issue	0.011	0.001	0.117^{**}	0.108^{**}	-0.117^{***}	-0.116^{***}
	(0.028)	(0.020)	(0.045)	(0.051)	(0.037)	(0.020)
Stand Your Ground	−0.022	0.001	−0.013	0.012	−0.024	−0.001
	(0.018)	(0.018)	(0.027)	(0.031)	(0.037)	(0.032)
Observations	4,896	4,692	4,896	4,692	4,896	4,692
R^2	0.963	0.972	0.957	0.970	0.937	0.955
Adjusted R^2	0.961	0.971	0.955	0.969	0.935	0.953

$^{**}p<0.05$; $^{***}p<0.01$

Note: Coefficients represent the policy effects on the natural logarithm of sales per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The model is weighted by population and standard errors are clustered by state. The covariates model includes unemployment rate, population density, log of real per capita personal income, alcohol consumption, % under poverty, incarceration rate, police employment rate, log number of aggravated assaults per 100K, log number of suicides per 100K. Florida data is not available for crime, so the model including covariates has a smaller base size.

Table B.3. Robustness Check for Background Checks

	Dependent variable:					
	Ln(Total Checks)		Ln(Handgun Checks)		Ln(Long gun Checks)	
	Base	Covariates	Base	Covariates	Base	Covariates
	(1)	(2)	(3)	(4)	(5)	(6)
CCW Permitless Carry	−0.028	−0.049	0.088***	0.109**	0.018	−0.002
	(0.043)	(0.048)	(0.031)	(0.049)	(0.025)	(0.027)
CCW Shall Issue	0.097	0.101	0.219**	0.201	−0.085**	−0.089***
	(0.135)	(0.150)	(0.100)	(0.104)	(0.037)	(0.019)
Stand Your Ground	0.093	0.128	−0.064**	−0.046**	0.039	0.064***
	(0.071)	(0.085)	(0.027)	(0.022)	(0.027)	(0.023)
Observations	4,608	4,500	4,608	4,500	4,608	4,500
R^2	0.940	0.941	0.967	0.967	0.956	0.957
Adjusted R^2	0.937	0.938	0.966	0.966	0.954	0.955

**p<0.05; ***p<0.01

Note: Coefficients represent the policy effects on the natural logarithm of the number of background checks per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The observations are weighted by population and the standard errors are clustered by state. Hawaii, Pennsylvania, and District of Columbia are excluded from data. The covariates model includes unemployment rate, population density, log of real per capita personal income, alcohol consumption, % under poverty, incarceration rate, police employment rate, log number of aggravated assaults per 100K, log number of suicides per 100K. Florida data is not available for crime, so the model including covariates has a smaller base size.

Table B.4. Robustness Check: Policy Impact on Suicides

	Dependent variable:					
	Ln(Total Suicides)		Ln(Firearm Suicides)		Ln(Non-Firearm Suicides)	
	Base	Covariates	Base	Covariates	Base	Covariates
	(1)	(2)	(3)	(4)	(5)	(6)
CCW Permitless Carry	0.030	0.031	0.017**	0.014**	0.006	0.009
	(0.025)	(0.026)	(0.007)	(0.007)	(0.017)	(0.016)
CCW Shall Issue	−0.004	−0.003	−0.008	−0.006	0.001	−0.001
	(0.037)	(0.040)	(0.009)	(0.010)	(0.017)	(0.018)
Stand Your Ground	0.005	0.010	−0.002	−0.003	0.004	0.008
	(0.012)	(0.016)	(0.006)	(0.010)	(0.011)	(0.015)
Observations	4,896	4,596	4,896	4,596	4,896	4,596
R^2	0.818	0.827	0.865	0.868	0.590	0.591
Adjusted R^2	0.810	0.819	0.860	0.862	0.573	0.572

**p<0.05; ***p<0.01

Note: Coefficients represent the policy effects on the natural logarithm of suicide rate per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The model is weighted by population and standard errors are clustered by state. The covariates model includes unemployment rate, population density, log of real per capita personal income, alcohol consumption, % under poverty, incarceration rate, police employment rate, log number of sales per 100K, log number of background checks per 100K. Hawaii, Pennsylvania, and the District of Columbia are excluded in the model with covariates due to the removal of those states for background check data.

Table B.5. Robustness Check: Policy Impact on Aggravated Assaults

	Dependent variable:					
	Ln(Total Agg. Assaults)		Ln(Agg. Gun Assaults)		Ln(Non-Gun Agg. Assaults)	
	Base	Covariates	Base	Covariates	Base	Covariates
	(1)	(2)	(3)	(4)	(5)	(6)
CCW Permitless Carry	−0.008	−0.007	0.047	0.053**	−0.033	−0.038
	(0.018)	(0.018)	(0.024)	(0.023)	(0.021)	(0.023)
CCW Shall Issue	0.004	0.008	0.015	0.015	0.054***	0.055***
	(0.036)	(0.034)	(0.011)	(0.014)	(0.013)	(0.016)
Stand Your Ground	0.023	0.017	0.030	−0.0002	0.009	0.023
	(0.014)	(0.018)	(0.030)	(0.024)	(0.016)	(0.023)
Observations	4,800	4,500	4,800	4,500	4,800	4,500
R^2	0.987	0.988	0.970	0.971	0.990	0.990
Adjusted R^2	0.987	0.987	0.968	0.970	0.989	0.990

**p<0.05; ***p<0.01

Note: Coefficients represent the policy effects on the natural logarithm of assaults rate per 100,000. Standard errors are shown in parentheses. All models include state and year-month fixed effects and state-specific linear time trends. The model is weighted by population and standard errors are clustered by state. The covariates model includes unemployment rate, population density, log of real per capita personal income, alcohol consumption, % under poverty, incarceration rate, police employment rate, log number of sales per 100K, log number of background checks per 100K. Hawaii, Pennsylvania, and the District of Columbia are excluded in the model with covariates due to the removal of those states for background check data. Florida is removed due to missing crime data.

Table B.6. Robustness Check: Policy Impact on Accidental Firearm Deaths & Mass Shootings & Burglary

	Dependent variable:					
	Ln(Accidental Firearm Deaths)		Ln(Mass Shootings)		Ln(Burglary)	
	Base	Covariates	Base	Covariates	Base	Covariates
	(1)	(2)	(3)	(4)	(5)	(6)
CCW Permitless Carry	0.009**	0.010**	−0.001	−0.001	0.002	−0.001
	(0.004)	(0.004)	(0.001)	(0.001)	(0.024)	(0.021)
CCW Shall Issue	−0.001	−0.001	0.0003	−0.0002	−0.060	−0.036
	(0.001)	(0.001)	(0.001)	(0.001)	(0.045)	(0.032)
Stand Your Ground	0.004	−0.002	−0.002	−0.002	0.043	−0.006
	(0.005)	(0.005)	(0.001)	(0.002)	(0.041)	(0.035)
Observations	4,896	4,596	3,672	3,444	4,800	4,500
R^2	0.323	0.333	0.159	0.168	0.984	0.985
Adjusted R^2	0.294	0.302	0.116	0.123	0.983	0.984
Note:						$^{**}p<0.05$; $^{***}p<0.01$

Note: Coefficients represent the policy effects on the natural logarithm of the outcome rate per 100,000. Standard errors are shown in parentheses. Coefficients represent the policy effects on the natural logarithm of assaults rate per 100,000. All models include state and year-month fixed effects and state-specific linear time trends. The model is weighted by population and standard errors are clustered by state. The covariates model includes unemployment rate, population density, log of real per capita personal income, alcohol consumption, % under poverty, incarceration rate, police employment rate, log number of sales per 100K, log number of background checks per 100K. Hawaii, Pennsylvania, and the District of Columbia are excluded in the model with covariates due to the removal of those states for background check data. Mass shootings data is from 2012 to 2017. Florida is unavailable for Burglary.

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