A Two-Stage Analysis Considering Gun Theft & Overall Crime:
Evidence from Child Access Prevention Laws

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Abstract

Child Access Prevention Laws (CAP) came to prominence in the early 1990s in the wake of the highest recorded rate of overall and adolescent firearm deaths seen in the United States at that time, placing mandatory firearm storage requirements on adults living in a home with children. While the primary – and perhaps sole – intention behind these policies are to prevent adolescent gun death, I contend CAP laws have the added function of reducing the rate of firearms stolen from homes due to the legal incentives against improper firearm storage. In the first of a two-stage analysis, CAP laws are proven to substantially reduce the rate of household firearm theft based on the ascending stringency of different CAP law storage requirements. The scope of the study is then widened in the second stage of analysis, where I demonstrate the overall impact illicitly-obtained firearms have on predicting increased firearm homicides.

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1 Introduction

Guns are more prevalent in the United States than anywhere else in the developed world. A recent report estimates that there are 1.2 guns per citizen in the country, which is double the rate of possession than Switzerland, the nation with the second-highest estimated rate amongst OECD nations (McDougal et al., 2023). A separate study estimates there are 433.9 million firearms circulating in the country (Maschia & Brownlee, 2023). On a household level, a 2017 PEW Research report revealed that 30% of American adults report owning a gun, while 42% claim to live in a home with a gun (Parker et al., 2017). The unprecedented prevalence of guns in the US is met with some of the highest rates of firearm homicides in the world: in 2021, there were an estimated 4.5 gun-related homicides per 100,000 population in the United States, representing the seventh-highest rate globally behind six Latin American countries (Leach-Kemon, 2023). This figure dwarfs that of other OECD nations, with firearm homicide rates ranging from 0.1 to 0.6 per 100,000 population amongst countries like Australia, Germany, the UK, and Canada (McDougal et al. 2023).

The fact that the United States holds such levels of gun prevalence and gun homicide begs the question as to the causal relationship between the two. Pro-gun researchers contend legal firearm possession acts as a deterrence on crime, arguing that criminals are less likely to commit crime in areas with concentrated firearm ownership due to risk of serious injury or death. The greater body of literature suggests firearm prevalence is positively correlated with violent crime, as heightened gun ownership leads to a greater misuse of firearms by legal owners, and a higher likelihood of firearms entering the illicit market through theft and other unregulated avenues (Khalil, 2016).

With these two schools of gun research so diametrically opposed, the simple question “do more guns lead to more crime?” may overlook intricacies in both the primary and secondary gun markets that impact firearm crime rates. I argue that there must be a larger emphasis placed on isolating the illicit market for guns, for a large percentage of crimes involving firearms are done so using illegally-obtained firearms (Alper & Glaze, 2019).

Policies that effectively limit the flow of guns from the legal to illicit market are appealing, then, as they may disrupt the accessibility for criminals while not infringing on legal gun ownership. This paper makes the unique contribution of analyzing one possible mechanism behind rates of gun theft: Child Access Prevention (CAP) laws. CAP laws came into prominence in the policy sphere in the 1980s and early 1990s, when rates of overall and adolescent firearm-related deaths were at an all time high in the country, peaking at 4.87 per 100,000 youths in 1993 (DeSimone et al., 2013). The first CAP law was passed in 1981 in Missouri, followed by a wave of similar legislation in the 1980s and 1990s. According to Everytown for Gun Safety – a pro-gun control think tank – 26 states, including the District of Columbia, currently mandate some degree of storage for legal gun owners.

There are a wide range of these policies, and, as Kappelman & Fording (2020) note, their degrees of stringency must be considered when analyzing the effect on gun storage practices. The two main types of CAP laws concern "negligent storage" and "reckless provision" of firearms around children; the former is considered the more stringent of the two, imposing liability on gun owners who negligently store firearms where a minor could gain access to them (DeSimone et al., 2013). "Reckless provision" laws forbid gun owners from actively providing their child with a firearm. CAP laws also vary by a state's legal definition of a minor, ranging from 14 to 18 years. CAP laws have been found to lower rates of adolescent and overall suicide, as well as unintentional firearm injuries among minors (Webster et al., 2014; DeSimone et al., 2013).

While little research has been conducted regarding the effect of CAP policies on gun storage practices, these laws ostensibly create a higher incentive for legal gun owners to securely store their firearms. This incentive is theoretically more extreme in states with more severe repercussions for unsafe storage, thus calling for a stratified analysis of the differing levels of CAP laws. If these policies significantly improve secure household gun storage, CAP laws represent a possible mechanism against rates of gun theft, with firearms less accessible to criminals attempting to steal firearms during burglaries and break-ins. Thus, if CAP laws are presented not simply as an added precautionary measure against child access, but a legitimate mechanism against firearm theft, the 25 states without these policies may be more inclined to adopt them. Given these laws do not restrict the right to own a firearm, CAP policies may offer a more realistic opportunity[2] to lower rates of gun-related crime by specifically targeting the illicit market for firearms.

This paper presents strong evidence that CAP laws negatively impact the rates of stolen firearms in states where they are adopted. Presenting a two-stage series of analysis, I first examine the interaction between states with CAP laws and firearm prevalence and the effect of this relationship on rates of gun theft. The outcome of interest in this first stage of analysis draws on stolen property data from the FBI's National Incident Based Reporting System (NIBRS), which records reported stolen gun values from law enforcement agencies nationwide. The lack of federal or statewide registries of firearm ownership makes ascertaining the exact rates of gun prevalence impossible; thus, this study proxies for state firearm prevalence using federal background check data from the National Instant Criminal Background Check System (NICS). While the responsiveness of firearm crime to rates of gun prevalence has been extensively studied, this research makes the novel contribution of interacting CAP gun laws to measure these laws' effectiveness in reducing gun theft relative to the prevalence of firearms in the states they exist in. Ultimately, CAP laws are found to reduce rates of gun theft with respect to prevalence – especially those with more severe legal repercussions for unsafe storage – indicating this legislation has the unintentional benefit of curbing the market for illicit firearms beyond its original purpose of keeping children safe from improper gun use.

As CAP laws are found to significantly reduce the level of guns entering the illicit market through theft, a second stage of analysis is presented to demonstrate how illicitly-obtained firearms impact rates of gun homicide. The Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) produces annual gun tracing reports recording the number of guns seized by law enforcement agencies – either at crime scenes or from individuals illegally possessing firearms – throughout the country. The volume of traced crime guns represents a proxy for the overall supply of illicitly-obtained firearms in a state. Regressing this data on the number of gun-related homicides, the second stage of analysis finds a heavily significant relationship between the market for illicit firearms and gun homicide.

[2] With respect to pro-gun rights groups like the National Rifle Association, who routinely use their strong ties to public offices to block any legislation that limits legal gun ownership

Given the tendency for criminals to target handguns when stealing firearms (Cook, 2018) and their predominant use in violent crime and homicide (Alper & Glaze, 2019), this research isolates the role of handguns on both theft and violent crime.

This study offers empirical evidence for the effectiveness of improved gun storage through CAP legislation on reducing the volume of firearms entering the illegal market, where a significant majority of firearms used in homicides and other gun-related crimes derive from. By mapping this two-stage sequence of firearm flows, CAP laws represent a promising avenue for lawmakers to reduce rates of gun violence without infringing on legal gun ownership.

The rest of the paper proceeds as follows: Section I consists of a literature review, with separate discussions regarding prior research on CAP laws, gun storage practices among owners, the question of overall gun prevalence and crime (and the best means to proxy for said prevalence), and gun theft and crime-related firearms; Section II introduces the theoretical framework behind both stages of analysis; Section III describes the data; Section IV presents the results; and Section V concludes with a discussion on policy implications.

2 Literature Review:

2.1 Prior Literature on CAP Laws

While an analysis of CAP laws' effects on firearm theft is original to the gun literature space, there is limited commentary on how CAP laws impact their more intended purpose of reducing youth firearm suicides and accidental firearm discharge. Cummings et al. (1997) produced the first analysis on CAP law effectiveness after the wave of CAP law passages in the early-mid 1990s; drawing from 1979-1994 mortality data, 12 states recently adopting CAP legislation were compared with the rest of the country through incidence rate ratios. While unintentional deaths amongst adolescents (15 years or younger) were found to drop 23% with significance, the negative effects on gun-related homicide and suicides failed to find statistical significance (Cummings et al., 1997).

Lott & Whitley (2000) also provided analysis on CAP laws soon after their adoption.

In line with the “more guns, less crime” theory, Lott frames CAP legislation as a tradeoff between preventing adolescent death by firearm suicide/accidents with the costs associated with gun owners having more restricted access to their firearm.[3]He finds no evidence that CAP laws reduce accidental gun deaths and total suicides, and thus calls for their repeal to reinstill crime deterrence (Lott & Whitley, 2000). However, in a review of Lott’s findings, Pepper (2005) finds Lott’s results to be subject to model specificity, and couldn’t replicate the study using the same data and empirical specification.

Webster et al. (2004) found significance of CAP laws on youth firearm suicides where Cummings et al. (1997) did not; passage of CAP laws were associated with an 8.3% decrease in firearm suicides amongst 14-20 year olds across a 1976-2001 sample. Minimum purchase age laws – which ostensibly may also impact youth firearm suicide rates by making those under 21 unable to purchase a firearm – failed to significantly influence adolescent firearm suicide. However, a more recent study analyzing a 1981-2017 panel found that only “strict” CAP legislation had significant downward effects on youth firearm suicide, while laws imposing a minimum age of 21 to purchase and possess firearms had larger coefficient terms at a stronger significance level (Kappelman & Fording, 2020). Kappelman & Fording (2020) present one of the few empirical analyses comparing stronger & weaker CAP policies, creating an ordinal variable that divides participating CAP states based on their level of severity for improper gun storage. The study suggests that CAP laws may only significantly alter gun storage practices if the legal repercussions for improper storage are especially severe.

Kivisto et al. (2021) related rates of firearm ownership and state implementation of CAP laws on their effect towards adolescent and overall firearm suicide. The report found a high impact rate on gun prevalence towards suicide: a 10% increase in a state’s firearm ownership rate led to a 39.3% rise in firearm suicide. CAP laws themselves also were found to reduce overall firearm suicide, with larger effects on adolescents. Anderson & Sabia (2018) take an alternative approach, studying the effect of CAP laws on adolescent gun carrying, as well as the rate in which students are threatened with firearms at school. The

[3]Thus, according to the ”more guns, less crime” theory of gun prevalence, reducing the deterring effect guns have on prospective home invaders

study finds CAP laws lead to an 18.5% decrease in youth gun carrying, and a 19% reduction in instances of gun threatening in schools (Anderson & Sabia, 2018).

DeSimone et al. (2013) explored a previously overlooked relationship between CAP laws and nonfatal youth firearm injuries. Using hospital discharge data from 1988-2003, a Poisson regression including state and time fixed effects indicated that CAP laws led to a decrease in unintentional and non-self inflicted firearm injuries. Similar to Kappelman & Fording (2020), this study makes the important distinction to divide CAP laws based on their severity. The study includes several counterfactual experiments, such as CAP laws' effects on non gun self-inflicted injuries and knife assaults. In one of the few comments in the field of literature relating CAP laws to gun theft, DeSimone et al. (2013) notes that a "reduction in intentional shootings might arise from a reduction in gun thefts, as thieves would find it more difficult to steal securely stored guns and a large proportion of stolen guns are used in crime."

2.2 Gun Storage Practices Among Owners

In order to understand the potential for CAP laws to influence rates of gun theft and thereby crime, it's important to first recognize how gun owners in America store their firearms. If every American gun owner kept their guns in a secure, locked space, then an investigation into the effect of CAP laws on gun theft would be unnecessary. However, such is not the case, and a body of literature shows gun owners habitually leave their firearms in unsecure locations. Household gun storage practices were scrutinized as early as the 1980s; as DeSimone et al. (2013) note, surging rates of firearm homicides, as well as adolescent gun-related deaths, in the mid-eighties prompted serious investigation into the mechanisms behind this trend. Unsecure firearm storage within households offer increased opportunity for both accidental firearm deaths, as well as firearm suicides & and other forms of unforeseen firearm discharge. Since the 1990s, several attempts at large scale, survey-based data have been performed to target firearm storage practices among gun owners.

Hemenway (1995) conducted one of the first analyses on household gun practices. A random telephone survey was administered to 800 gun owners in the country, finding 21% (170 participants) kept at least 1 firearm kept loaded and unlocked. 50% of gun owners reported keeping an unloaded gun unlocked in the house. Of note, formal training in firearm handling was not significant on safe or unsafe storage practices; at the time, the NRA advocated that a gun purchased for protection is “always in use,” and promoted loaded firearm storage (Hemenway, 1995). While the organization now promotes keeping firearms unloaded, it has no explicit advice for gun owners on storage techniques (National Rifle Association).

Grossman et al. (2005) utilizes a sample of survey participants to identify the effects of gun storage practices on adolescent firearm deaths, comparing 106 case gun-owning households in which a shooting occurred (comprising 82 suicide attempts and 24 unintentional shootings), and 480 control gun-owning households with no instances of firearm discharge. While aggregated data suggests a higher rate of secure gun storage relative to other surveys, guns in case households (where shootings occurred) were less likely to be securely stored. Moreover, these households were also less likely to store firearms separately from ammunition, indicating that a combination of safe storage practices reduces the likelihood of gun discharge.

Azrael et al. (2018) conducted a 2015 national survey of 2072 gun owners with children. Amongst participants, 21% of households reported storing at least 1 firearm loaded and unlocked, while less than a third of participants stored their guns unloaded and locked. The remaining 50% of participants stored their firearms to some intermediate degree of safety between these two extremes, indicating a wide range of storage practices (Azrael et al, 2018). In 2016, Crifasi et al. (2018) surveyed 1444 participants on their household gun practices. Their results find similar levels of unlocked, loaded firearms, yet slightly higher secure gun storage practices at the expense of the intermediate degree of storage which comprised a near-majority of the Azrael et al. (2018) survey (Crifasi et al., 2018).

Anestis et al. (2022) offers the most recent survey data on gun storage practices. The survey is limited to respondents of 5 different states with a relatively large sample of 2152 participants. This survey delineates between locked and unlocked, but also ”hidden” and ”unhidden” firearms. This specification yields alarming results, suggesting 58.3% of participants kept firearms “unlocked and hidden.” 17.9% of participants kept a firearm "unlocked and unhidden" (Anestis et al., 2022). While this data may potentially overrepresent hazardous gun storage practices on account of sampling characteristics, the phrasing of the survey questions indicates firearms are often stored within sight and reach of children, guests, criminals, or anyone else entering a gun owner's home.

It is difficult to draw temporal inferences across an array of survey data; each analysis likely reflects a level of sampling error, and none of the surveys are constructed to capture panel data on how storage practice shifts with time on an individual level. That said, despite some deviation throughout these surveys, it is clear that firearms are routinely left in accessible areas around the household, opening the door for a mechanism behind gun theft and subsequent flows into the illicit market. While there is little empirical evidence measuring how firearm storage is directly affected by the implementation of child access prevention laws, the wide body of evidence pointing towards reduced youth suicide and accidental gun death rates suggests these laws may have a substantive impact on safer firearm storage. A synthesized overview of these survey estimates are seen in Table 1.

Table 1: Gun Storage Practices Across Historical Surveys

Source	N Participants	Questioned Storage Practice	% True
Hemenway (1999)	800	Store firearm loaded and unlocked at home	21
		Store firearm unlocked at home	50
Grossman et al. (2005)	586	Gun unloaded	84
		Gun locked	53
		Ammunition locked	42
Azrael et al. (2018)	2072	Store all guns securely	21
		Loaded and locked or unloaded and unlocked	50
		Unloaded and locked	29
Crifasi et al. (2018)	1444	Store all guns securely	46
		Store some guns unlocked	24
		Store all guns unlocked	21
Anestis et al. (2022)	2152	Store gun unlocked, hidden	58
		Store gun unlocked, unhidden	18

2.3 Gun Prevalence on Crime & the Question of Prevalence Proxying

As mentioned, a longstanding dispute in the field of gun literature concerns the role of gun prevalence on overall crime outcomes: the "more guns, more/less crime debate. Lott & Mustard (1997) prompted the discussion, arguing "shall issue," or right-to-carry laws, are associated with lower rates of violent crime. The study finds that if non shall-issue states had adopted the policy over the sampling period between 1977-1992, "1,570 murders; 4,177 rapes; and over 60,000 aggravate assaults would have been avoided yearly" (Lott & Mustard, 1997). Lott stands at the forefront of the "more guns, less crime" hypothesis, arguing higher rates of gun prevalence deter criminals from violent crime given the added risks of encountering firearms.

There is an acute difficulty, however, in estimating rates of gun prevalence themselves, especially at a state or local level. The 1986 Firearm Owners Protection Act explicitly outlaws any federal registry of firearm owners, and few states have adopted their own measures, meaning controlling for localized gun prevalence is not a straightforward task in gun research (Kim & Wilbur, 2021). Proxies are necessary, then, to estimate geographic firearm prevalence. An inappropriate proxy device that over or under represents local gun prevalence likely leads to incorrect findings on the relationship between gun flows and a given outcome, namely violent crime.

To test the validity of firearm prevalence proxies, researchers typically assess their correlation with state or national survey level data. The General Social Survey (GSS) is often considered the "gold standard" for assessing national and regional gun ownership rates (Azrael et al., 2014). Conveniently for firearm research, the survey – administered to roughly 3000 participants biannually[4] across 9 census region divisions – has questioned participants on overall firearm, handgun, & long gun ownership since its inception in 1972[5]. Lott & Mustard (1997) used voter exit polls to estimate state-level gun ownership rates

[4]The survey was administered annually from 1972-1993, where from 1994-onwards the survey has been performed biannually.

[5]As Kim & Wilbur (2021) note, the limited sampling size, which can often lead to fewer than 100 responses in a census region division, makes the GSS (& other surveys like it) insufficient in representing firearm prevalence on a state level.

that led to their "extraordinarily large" elasticity of gun homicides with respect to ownership (Cook & Ludwig, 2006). The polling data indicated a significant increase in gun ownership rates from 27.4% to 37.0% across 1986-1998, while GSS results remained level over the same period. Thus, in this case, Lott's incorrect proxy specification led to a reduced causal relationship between firearms and violent crime in response to increased gun prevalence, when really no shift in prevalence existed at all (Kleck, 2004).

Several proxy considerations emerged in the wake of Lott's findings. Directly responding to Lott, Duggan's (2001) *More Guns, More Crime*[6] found a positive elasticity of homicide with respect to gun ownership. In a comprehensive study, Kleck (2004) assessed GSS correlation on Guns & Ammo magazine subscriptions[7], National Rifle Association membership rates, weapons arrests on a per-police officer basis, percentage of homicides using firearms, and percentage of suicides via committed via firearm[8], amongst others.

First introduced by Cook (1979), the proportion of gun suicides to overall suicide deaths (FSS) has emerged as the predominant proxy for state and local gun ownership in the field of gun literature[9]. Cook & Ludwig (2006) brought the proxy to public attention, and it has since been used extensively in gun policy research (Siegel et al., 2013; Siegel et al., 2014; Siegel & Ross, 2015; Miller et al., 2012; Rosenfeld et al., 2007; Geier et al., 2017; Reeping et al., 2019). The proxy has been found to strongly correlate with GSS ownership estimates cross-sectionally; in the most expansive assessment of FSS/GSS correlation, Kang & Rosich (2023) found a strong and stable cross-sectional intercorrelation between the two metrics in excess of 0.75 from 1975-2023, barring an exception in 2010.

The FSS proxy has received criticism, however, largely due to its shortcomings when used in intertemporal data. Kleck (2004) denies its validity in a non cross-sectional setting, concluding that the proxy is not significant over time with GSS on a census division level – even finding negative correlation coefficients for some divisions. He thus advises against its use in longitudinal research with panel or time series data. Revisiting an analysis of prevalence proxies a decade later, Kleck concludes that out of 41 used prevalence proxies –

[6] Lott published a book in 1997 entitled *More Guns, Less Crime*, a more general assessment of firearm prevalence beyond shall-issue laws.

[7] Kleck (2004) largely dismisses the *Guns & Ammo* proxy, finding a weak correlation with GSS estimates.

[8] Fraction of firearm suicides/total suicides

[9] FSS data is available at the county level, allowing for granular localized analysis.

including FSS – none are valid for testing trends over time (Kleck, 2015). Kim & Wilbur (2020) corroborate the shortcomings of FSS, concluding FSS proxies well for legal firearm prevalence within cross-sectional analyses, while the opposite is true in intertemporal research. Gomez et al. (2020) finds a significant positive skew in bias across the proxy and a subpar correlation coefficient (0.60) between FSS and GSS. Analyzing Massachusetts data – one of the few states requiring individual firearm purchases to be registered – FSS proves insignificant as a proxy for gun ownership on a panel-level basis (Kim & Wilbur, 2020). The study ultimately suggests gun researchers shift away from the frequented FSS proxy to an alternative measure of gun prevalence more suited for intertemporal research.

To model for legal firearm prevalence, Kim & Wilbur (2020) contend that gauges of legal firearm acquisitions best capture these flows. Intuitively, firearm purchases likely have high rates of correlation to homeowner gun prevalence, as these metrics chart new firearm flows into the legal market directly through consumers. FBI background checks (BGCs) have been used to measure new firearm flows on a state-year basis (Vitt et al., 2018, Lang, 2013; Lang, 2016, Briggs & Tabarrok (2013); Kim & Wilbur, 2020; Koenig et al., 2023)[10]. Uses of this proxy measurement are more recent across gun literature, largely due to the dominance FSS has held. The Brady Handgun Act of 1993 required individuals – for the first time – to purchase firearms through an official Federal Firearm License (FFL) holder and submit a BGC through the FBI before purchasing a firearm. The FFL will communicate with the FBI through the National Instant Criminal Background Check System (NICS), and a search is conducted to see whether the prospective buyer is ineligible to purchase a firearm[11]. These checks do not necessarily indicate the purchase of a firearm, as prospective buyers aren't required to purchase firearms after submitting a BGC (Lang, 2013). That said, check counts do measure a level of consumer intent to purchase firearms, and offer a compelling alternative to the FSS proxy for gun ownership prevalence. More information about the NICS system itself, and various other background check considerations, are made in the data section.

[10]Koenig et al. (2023) contribute substantially to the identification of irregular and problematic data within the NICS database, and will be referenced to a greater extent in the discussion of the dataset itself.

[11]Restrictions on gun ownership vary across states, yet most exclude convicted felons, those with serious mental conditions, domestic violence convicts, and those with demonstrated substance abuse issues from purchasing owning firearms.

Recent studies using FBI BGC counts appear to represent reliable proxy for both new firearm flows as well as overall gun prevalence (Kim & Wilbur, 2020; Lang, 2013). Kim & Wilbur (2020) test state-month correlations between both NICS background checks and FSS on known gun purchases in Massachusetts. Whereas checks are significantly correlated at the 5% level with Massachusetts gun purchases, FSS report no correlation with either Massachusetts gun purchases or BGCs themselves. If Massachusetts is indicative of the country writ-large, BGCs represent a suitable means of proxying for firearm flows into the legal market using panel data. Given the nature of firearms as durable goods, new firearm flows through BGCs may not directly represent total gun ownership, as guns can last years in possession before their expiration. However, when such a maturation occurs, previous gun owners may quickly purchase a new firearm, engage with the NICS system, and thus re-engage with the gun-owning population.

Lang (2013) offers a two-way fixed effect model to capture how changes in BGC rates influence the change in population gun ownership levels through GSS. Checks were significantly and positively related with changes in GSS division level firearm ownership, with Lang (2013) noting "a one standard deviation increase in the 2-year BGC rate[12] increases the fraction of the population with a firearm by approximately 2%". These results are significant to a 1% level with a strong goodness of fit. Thus, BGC counts likely serve as a valid proxy measurement for both new gun entrances into the legal consumer market, as well as overall gun prevalence. Their empirical strength when regressed on panel-level data shows a marketable improvement from FSS metrics.

2.4 Role of Theft Guns in Crime

While a wide body of aforementioned literature concerns overall gun prevalence measured through legal ownership and its effects on rates of firearm crime, the market for illicit firearms is far less understood. The role of illicitly-obtained firearms in crime, however, is made more clear by surveys administered to prison inmates. The Survey of Inmates in State and Federal Correctional Facilities is a nationally-representative data on

[12] Given the biannual collection of GSS surveys, Lang (2013) created a two year average of BGC counts to apply to each specific GSS observation.

the behavior of thousands of inmates at the time of their crime. The survey is not regularly-administered, and has been performed in the years 1974, 1979, 1986, 1991, and 1997, 2004, and 2016. Alper & Glaze (2019) provide a comprehensive assessment of the most recently-administered survey in 2016, and report that 1 in 5 (21%) of all state and federal prisoners possessed or carried a firearm when they committed the offense for which they were serving time in prison. However, fewer than 1 in 50 (less than 2%) of all prisoners had obtained their firearm from a retail source, suggesting guns eventually involved in crime may have long life cycles from their original legal purchase. 6% of inmates report they obtained their firearm directly through theft themselves, with 43% obtaining it off the street or from the underground market where the previous owner is more likely to have acquired the firearm through theft. Of the 21% of respondents who reported possessing a gun at the time of their arrest, 86% reported possession of a handgun as opposed to long guns or other firearms (Alper & Glaze, 2019).

Two studies – Cook (2018) and Braga et al. (2021) provide city-level analysis of the role of illegal firearms on crime. Cook (2018) addresses the role of theft in supplying firearms used in robbery, assaults, and homicides. Unlike national survey estimates, Cook finds an insignificant amount of firearms used in crime are derived from stolen guns. Braga et al. (2021) identifies the illegal firearm flows in and out of Brooklyn that were involved in crime, and also finds that guns involved in crime were less likely to be directly obtained through theft. However, as both reports note, the life cycle of a firearm as a durable good is a long one; firearms may enter the illicit market through theft and change hands several times before their eventual involvement in crime. Cook (2018) notes that handguns, especially in urban environments, are far more ideal for use in crime due to their concealability and ease of use. This corroborates with data obtained by inmate surveys, indicating handguns are both the preferred firearm of theft for criminals, and the predominant firearm used in violent crime (Cook, 2018).

Robinson et al. (2024) recently performed an analysis of nearly 7,000,000 firearms recovered in crime in the state of California. California is one of 15 states that require citizens to report when their firearm has been stolen, allowing the researchers to distinguish whether guns involved in crime were ever reported stolen. The report finds that the likelihood for a gun to be involved in crime was 8.93 times larger if the gun was reported stolen in the past, than if the crime gun had never been reported stolen (Robinson et al., 2024). This strongly suggests an ultimate path from gun theft to crime, regardless of whether it was the criminal themself who stole the firearm used in crime. With data captured from 1996-2021, the study allows visibility into the durability of a firearm and the path from theft to crime perhaps more than Cook (2018) & Braga et al. (2021).

Hemenway et al. (2017) takes the reverse approach, analyzing the epidemiology of gun theft victims and their characteristics that may increase the risk of theft. Although a relatively small and cross-sectional sample, the study revealed key risk factors associated with firearm theft: owning 6 or more guns, owning guns for protection, carrying a gun in the past month, storing guns unsafely, and living in the Southern United States. Of course, their finding of unsafe gun storage as a mechanism towards theft is of key interest to this research (Hemenway et al., 2017). Using the results of their survey, Hemenway et al. (2017) estimate that approximately 380,000 guns were stolen per year, with a significant concentration of guns stolen from the South region of the US.

To that end, there is credible evidence as to geographic irregularities of crime gun flows. Knight (2013) establishes the externalities imposed by differing gun regulations across states, and charts a significant flow of gun-dissemination between weak-regulation states and strong-regulation states using ATF gun-tracing data. Knight (2013) theorizes these externalities also affect illicit flows of guns, with black market gun dealers likely to take guns from weakly regulated states to more strict, nearby states and jurisdictions. The extent of Knight's findings must be viewed as descriptive given its cross-sectional nature, but Andrade et al. (2020) confronts the same question with 3 years of panel data. Their report finds a highly significant and negative relationship between the number of firearm laws in a state, and the number of guns traced elsewhere originating in said state (Andrade et al., 2020). These findings suggest a flow of illegally-obtained firearms from states with weaker gun reform than others. This may help explain how few crime-guns found in Chicago and Brooklyn – two of the most stringent gun-reform jurisdictions in the country – were directly precipitated by theft, as they may have been stolen earlier in their life cycle in a relaxed gun-control state.

In the study most closely resembling this thesis, Khalil (2016) attempts to define illegal firearm flows and its impact on violent crime. To do so, he uses the outcome of interest in this study – stolen gun property from the NIBRS system – as a proxy for illegal firearm flows. It is unclear how Knight (2016) claims to capture a specific volume of illegal firearms, as the NIBRS system records reported property values of stolen goods, but not their count. Khalil (2016) notes the simultaneity concerns with regressing stolen gun statistics against crime rates, as stolen gun flows likely precipitate violent crime, especially if criminals are "forward looking" and coordinate gun theft prior to a criminal act perceived to require a firearm. To account for this, Knight (2016) includes quarterly lags – up to 4 quarters – on firearm theft, and eventually concludes that lagged stolen gun flows have strong positive impacts on firearm aggravated assaults and homicides up to a half-year of lags.

3 Empirical Specifications

3.1 Stage One Empirical Specification

With almost all CAP-related legislation enacted before this analysis's sampling period, a standard difference-in-difference method to measure the effect of these policies on gun theft is not feasible[13]. Instead, I measure the influence of CAP laws on gun theft through a novel technique in the field of gun literature: interacting the presence of CAP policies with state handgun prevalence, as proxied through NICS handgun BGC data. While both sides of the "more guns, more/less crime" debate argue over the effects of increased firearm prevalence, little has been done to test how the interaction of prevalence and specific policies affect crime. With the inclusion of this term, I am able to isolate the significance of CAP policies on gun theft with respect to the fluctuating handgun prevalence in an observed state. If CAP laws do truly present a mechanism against theft, then the interaction of CAP laws and handgun prevalence would see reduced rates of stolen

[13]Throughout the entire sampling period of 1999-2021, there were only four instances of newly-introduced CAP legislation that had not existed prior to 1999. California escalated their CAP policies from CAP18 to include CAPliability in 2013; Colorado became a CAPliability state in 2021, as did Nevada; Oregon also enacted CAP legislation in 2021, but only to the degree of CAP14.

firearms in comparison to non-CAP adopting states.

Given the nature of the panel data used in the study, a two-way state-time fixed effects regression presents the strongest empirical mode of analysis for testing this interaction. State fixed effects control for unobserved heterogeneity at the state level, accounting for the time-invariant or gradually-shifting characteristics such as cultural sentiment towards firearms, law enforcement practices, geographic features, and other unconsidered factors that may affect the rate at which guns are stolen. Time fixed effects control for nationwide characteristics that change over time, but are consistent throughout states. These may include shifting socioeconomic conditions throughout the country, aggregate shifts in firearm sentiment, and other unobserved factors.

This research attempts to observe the effects of CAP policies on gun theft, but on a stratified basis. That said, the first stage of analysis includes three two-way fixed effects models of the same construction that correspond to the three CAP law classifications with different levels of legal stringency. The general model used for each policy is included below followed by further discussion of the empirical strategy.

$$\ln(\mathrm{T}_{it}) = \beta_0 + \beta_1 \mathrm{C}^*\ln(\mathrm{B})_{it-1} + \beta_2 \ln(\mathrm{B})_{it-1} + \beta_3 \mathrm{C}_{it-1} + \beta_4 X_{it} + \alpha_i + \mathrm{Year}_t + \epsilon_{it} \qquad (1)$$

Here, the natural logarithm of the value of stolen firearms – denoted as T – for a given state i in year t is a function of several regressors within the two-way fixed effects model. The key interaction term of this study: the binary indicator for a given CAP policy, denoted as C, and the natural logarithm of NICS handgun background checks, denoted as B, in a given state-year is the first explanatory variable, with a coefficient value of β_1. The coefficient β_2 represents an elasticity of the value of stolen firearms with respect to the natural logarithm of handgun checks. Given the inclusion of the logged CAP-BGC interaction term, β_2 gives the elasticity of stolen gun value with respect to BGCs for states where there are no CAP storage laws. A 10% increase in the number of BGCs can be interpreted as a (10 x β_2)% shift in the value of stolen firearms in non-CAP law states. In this sense, the original β_1 coefficient gives the difference in elasticity from the β_2 value in states where CAP laws are present.

The inclusion of β_3 is not meant to be taken with any serious interpretation. The standalone CAP coefficient represents the elasticity of stolen firearm values with respect to the presence of CAP policies but – given this coefficient holds the other logged covariates constant – its interpretation is based on a condition where the number of background checks in a state-year is equal to 1.[14] This, of course, is an unreal scenario, meaning its coefficient value and corresponding significance offer no insight on any effect on stolen firearm value. However, given the four states changing their CAP policies over the sample period, β_3 is included for control purposes.

β_4 represents the coefficient value for the vector of included demographic and socioeconomic controls used in the model.[15] Four general demographic controls are included to help reduce the degree of heterogeneity in state profiles not sufficiently captured by the state fixed effects term, all of which are logged to allow for an elasticity interpretation between themselves and logged value of stolen firearms. These controls are: the proportion of a state's population identifying as female, white, and black, as well as a total logged population term. To help resolve the issue of incomplete participation in the NIBRS program, a logged term for the state population covered by agencies participating in NIBRS is included as an additional control. Due to the logarithmic design of the model, this coefficient value is centered around 1. To account for socioeconomic state characteristics, the unemployment rate and the employment population ratio – a ratio of the civilian labor force to overall population – are also included as controls. As these values already represent rates, they are the only covariates in the stage one analysis left un-logged. Following control covariates, state and year fixed effects – denoted as α_i and $Year_i$ – and the error term ϵ_{it} control for time and state invariant trends not already captured by demographic controls.

Given new BGCs represent a simultaneous entrance of new firearms into the market, a one-year lag is applied to background check terms; while analyzing on an annual level, some guns may be purchased and enter the market at much earlier points in the year than others. Thus, a better reflection for the stock of household firearms that may be stolen in a

[14]Given $\log(1) = 0$

[15]Although shortened to a vector form in this model, each demographic control and its corresponding coefficient are included in their entirety in the results section.

given year is a delayed count on BGCs. Furthermore, Kleck (2015) notes the possibility for crime rates themselves to influence the prevalence of firearms, for current rises in crime may incentivize individuals to purchase firearms in response as a protective measure. To avoid such a problem with reverse causality, a one-year lag is justified. While, once again, the interpretation of the standalone CAP policy indicator is meaningless, this covariate is also lagged to remain consistent with the lagged CAP-BGC interaction term.

The sign of the standalone prevalence proxy is theoretically ambiguous. Researchers like John Lott contend that increased legal firearm ownership leads to a reduction in firearm and overall property theft due to the perceived deterrence criminals feel from robbing a home that is increasingly likely to hold a firearm. Those in the "more guns, more crime" camp would predict increased BGCs to lead to higher rates of crime due to legally owned firearms entering the illicit market – where they are much more likely to be used in crime – as well as increased firearm misuse through gun owners.

An important falsification test – f-tests across the three CAP policy regressions between the CAP-BGC interaction term and the standalone BGC term – is included following the main findings of the first stage regression. If these f-tests are significant, there is likely a strong degree of multicollinearity between the two terms, meaning the terms reflecting non-CAP and CAP state relationships with stolen gun values are highly correlated. This would call into question any significant coefficient results found in either covariate. Insignificant f-tests, however, help support the empirical model as reliably capturing the differences in gun theft between CAP and non-CAP states.

An additional validity test is applied to at the end of the first stage of analysis to test an already-documented feature of CAP laws. CAP laws have been found to lower rates of overall firearm suicides through both reduced access to adolescents seeking to inflict self harm as well as a less-spontaenous process for gun-owning adults to access their firearms (Webster et al., 2014). So, the same fixed effects model is applied, exchanging the outcome of interest from logged value of stolen firearms, to the logged count of firearm suicides. A well-performing model featuring CAP laws would likely witness these same findings.

3.2 Stage Two Empirical Specification

The second stage of this two-part analysis shifts the focus from the relationship between CAP storage laws and firearm theft towards a more broad discussion regarding the role the illicit market for firearms plays in determining rates of gun violence. The findings of Alper & Glaze (2019) indicate that the vast majority of prison inmates possessing a gun at the time of their arrest obtained their firearms through means other than a retail source; a large percentage of these inmates reported acquiring their firearms off the street – where guns may have entered the unregulated market through theft originally[16] – or through theft of the gun themselves.

Wilbur & Kim (2020) note that almost all proxies used in gun research solely seek to capture the prevalence of legal firearms as a measure of overall gun ownership. This is not without reason; while legal firearm prevalence is already a difficult metric to capture – considering more than 20 years of debate over the ideal choice of gun prevalence proxy – the nature of the illicit firearm market is an even vaguer entity. Yet, if the illicit market for firearms plays a significant role in violent crime as inmate survey data would suggest, attempts to predict gun violence proxied through legal prevalence may lead to skewed results if the legal and illicit firearm flows are uncorrelated.

Thus, this research attempts to proxy for the prevalence of illicit-obtained firearms using ATF gun tracing data. While ATF traces, when used in research, are typically measured as an outcome variable as a means of capturing gun crime, I argue the database offers the best method of quantifying illicitly-obtained firearms available to the field of gun literature. Of the roughly 500,000 guns processed through the ATF system in 2022, illegal possession of firearms was by far the most frequent category associated with gun traces, at over 110,000 firearm traces (ATF, "Summary of Firearms Trace Data – 2022"). These are, by definition, illicitly-held guns, as their owners at the time of seizure were prohibited from carrying firearms.[17] While ATF tracing data can be used as means to capture firearm

[16]Consider the earlier discussion of Robinson et al.'s (2024) findings on guns involved in crime in California: during their lengthy sample period of analysis, the report found that the likelihood for a gun to be involved in crime was 8.93 times larger if the gun was reported stolen in the past, than if the crime gun had never been reported stolen.

[17]Refer to footnote 8 regarding personnel types of those prohibited from owning and carrying firearms.

crime outputs, it also offers a since-overlooked means for proxying for the volume of illicitly-obtained firearms in a given state-year. While studies such as Knight (2013) and Andrade et al.(2020) analyze the flow of ATF-traced firearms from state-to-state, this study utilizes the static measure of traced guns in a given state-year to measure the volume of illicitly-held firearms, providing a stepping-stone from the first stage's discussion of storage practices on gun theft, to the second stage's analysis of illicitly-obtained firearms on gun homicides. ATF trace counts present the most accurate account of a state's illicit gun pool in any given year, regardless of whether said guns originated from out of state.

As with the first, this stage of analysis also employs a two-way fixed effect model to examine the effect of illegally-held firearms on gun homicide levels. The model used to capture this relationship is included below followed by further discussion:

$$\ln(\mathrm{H}_{it}) = \beta_0 + \beta_1 \ln(\mathrm{A})_{it-1} + \beta_2 \ln(\mathrm{B})_{it-1} + \beta_3 \mathrm{X}_{it} + \alpha_i + \mathrm{Year}_t + \epsilon_{it} \tag{2}$$

Here, the natural logarithm of the number of homicides committed by firearms – denoted by H – for a given state i in year t is a function first of β_1, which represents the coefficient for natural logarithm of traced handguns processed through ATF, denoted as A. Given the possibility of correlation between the illicit market for guns and legal firearm prevalence and the latter's influence on rates of gun homicide, logged NICS handgun checks, again denoted as B, is included as the second covariate. The coefficients for both of these variables represent a direct elasticity of gun homicides with respect to them each. A 10% increase in either ATF handgun traces or NICS handgun checks represents a (10 x β_1)% or (10 x β_2)% in the number of gun homicides, respectively. X_{it} represents the set of demographic and socioeconomic controls replicated from the stage one analysis. Given this stage of analysis does not contend with the NIBRS database, the control for the covered population in that system is dropped. State and year fixed effects designed to capture the same time and state invariant characteristics are included alongside the error term. Once more, standard errors are clustered at the state level as an additional robustness check.

Making the strong assumption that both ATF trace guns and NICS BGCs perfectly capture their respective realm of firearm prevalence, this regression offers insight as to the comparative strength of the illicit and legal market for firearms in predicting rates of firearm homicide. While the model does not include lags past one year, future analysis may benefit from observing far-reaching lags on this expression to identify whether a longer ”time to crime” exists for firearms representative of legal ownership. ATF gun traces, alternatively, provide a set of firearms already having entered the illicit market, and may appear sooner and more frequently in gun-related crime as a result.

Logged ATF traces are treated with a one-year lag; while traced guns represent instantaneous moments of illegal gun involvement, their application as a proxy for the prevalence of illicitly-obtained firearms intuits a sequential path of firearm acquisition to crime. If a significant relationship exists between firearm homicide and the prevalence of illicitly-held firearms, it is likely that homicides are precipitated by the rising rates of illicit firearms.

As a key validity test in the second stage of analysis, I test the same two-way fixed effects model on logged non-gun homicides. A significant relationship between either legal firearm and illicit firearm prevalence should have little to no effects on the rate of homicides committed without using a gun. Results are especially concerning if the rate of non-gun homicides rise in conjunction with increased rates of ATF trace guns or BGCs, as a greater access to firearms would likely impose a substitution effect away from the use of non-gun homicides.

4 Data Overview

4.1 CAP Law Data: State Firearm Laws Database, 1991–2020

To control for the presence of CAP laws in a given state, I draw policy data from the “State Firearm Laws Database, 1991–2020” database, developed by Michael Siegel at the Boston University School of Public Health.[18] Used extensively in gun research, the database offers a comprehensive guide to 133 different pieces of firearm legislation across 13 different categories, with binary coding indicating the presence of the law in each state-year

[18] Given my analysis spans from 1999-2021, firearm policy data for 2021 was manually drawn from individual research into each state’s 2020-2021 legislative session.

observation. Two primary sources were used in the database's collection: the Thomson Reuters Westlaw database was used for most firearm policies, with 33 supplemental policies derived from the Everytown for Gun Safety database. Conveniently, one category within the database is "child access prevention," with twelve unique policies within the category. For the purposes of this research, three such policy codes are used for empirical analysis: "CAPliability," "CAP18," and "CAP14."

23 states are found with CAP provisions with at least one observation in "CAPliability," "CAP18," or "CAP14." It should be noted that the three states and one district excluded from regression analysis due to gaps in reporting data – Illinois, Hawaii, New Jersey, and the District of Columbia – all have some degree of CAP laws. Thus, a total of 19 states are compared in analysis to 28 non-CAP states. "CAPliability" represents the most stringent of the used CAP laws, representative of the "negligent storage" laws that impose criminal liability to parents or guardians regardless of whether the child gains access to the firearm. States with "reckless endangerment" policies are coded 0 for "CAPliability," but are found in "CAP4" or both "CAP14" & "CAP18," depending on the state's age cutoff for a minor. Once again, the "reckless endangerment" CAP policy incurs criminal liability for parents and guardians actively providing children with firearms, and, for most states, only punishes households when the firearm is used. Descriptions drawn verbatim from the Siegel Law Database for each of the policies are included in Table 2 below.[19]

[19]Given the varying stringency of CAP laws and the need to test separately their empirical strength (Kappelman & Fording, 2020), it is essential that the choice of policy database reflects these varying levels of severity. Siegel et al. (2017) ensures this explicitly when describing the ability for his dataset to reflect stratifications amidst one umbrella of policy type: "Laws are not always dichotomous, as there may be graded variations in their strength. We attempted to account for this by separately coding the operative terms of each law. For example, "child access prevention" statutes—which make parents liable for negligent storage of firearms—have previously been coded as present or absent. However, the strength of these laws varies according to the type of event that triggers parental liability. Some states make a parent liable for negligent storage regardless of whether the child actually accesses the gun, others impose liability only if the child gains access to the gun, and still others impose liability only if a child actually uses the gun. Because we coded each of these variations separately, our dichotomous coding can easily be converted into a scale that reflects levels of increasing stringency of these laws" (Siegel et al., 2017).

Table 2: CAP Law Provisions Used in Analysis

Provision Code	Provision Description	Additional Notes	States with Provision
CAPliability	Criminal liability for negligent storage of guns	Owner of gun is criminally liable if a gun is not stored properly, regardless of whether a child actually gains access to the gun.	5
CAP18	Liability applies to access by children less than 18 years old	Owner of gun is criminally liable if child under age 18 has access to the gun.	6
CAP14	Liability applies to access by children less than 14 years old	Owner of gun is criminally liable if child under age 14 has access to the gun.	14

Source: State Firearms Law Database, 1991-2020

4.2 Proxying for Gun Prevalence: National Instant Criminal Background Check System (NICS)

While the General Social Survey provides what is considered to be the most reliable data on national and census-division gun prevalence levels, it cannot accurately represent overall firearm prevalence on a state-year level given its drawbacks: the survey is administered biannually, features a relatively low number of average respondents, and is aggregated to the census division level. While the fraction of firearm suicides has been predominantly used in the field of gun literature as a panel, it too, the criticisms of Kleck (2004) and others concerning its legitimacy in the use of panel data warrants an alternative approach to proxying for gun prevalence.

Representing a more viable proxy for state gun prevalence, federal background checks (FBCs) are used in this analysis, with data captured through the FBI's National Instant Criminal Background Check System (NICS). BGCs have been proven to correlate more significantly with GSS prevalence levels on a panel-level basis, and provide a reasonable account for new firearm flows into the legal domestic market. The NICS system began operating in November, 1998. With the other databases used in this research spanning before this, 1999 represents the lower bound of panel data in this analysis.

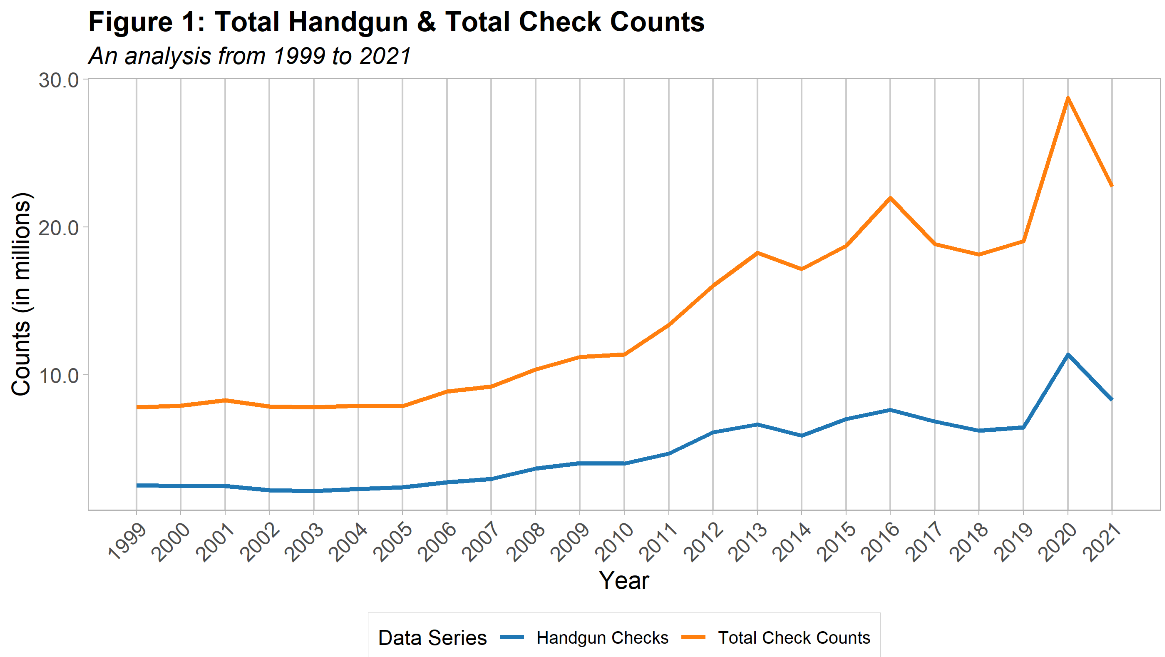
However, NICS is an intricate system with important caveats regarding its use as a prevalence proxy. Media outlets often liken total NICS BGC counts to overall gun sales (Gutowski, 2023; Gabriele, 2023; de Vise, 2023), when the FBI directly advises against this.[20] Once again, an initiated BGC does not necessitate a gun sale, only that a check has been initialized for a purchase. Furthermore, as only one check is required for a purchase from an FFL, multiple guns may be purchased under one check.

A deeper concern, though, resides in the inconsistent reporting methods to the NICS system. While the 1993 Brady Handgun Act mandated FFLs screen all firearm sales to prevent unauthorized individuals from purchasing firearms, it also allows for valid concealed handgun permit licenses to be used in lieu of a background check in certain states approved by the ATF. NICS provides full service to FFLs in 31 states and the District of Columbia, meaning 19 states allow for some form of concealed permit license to be used in lieu of an official NICS background check when purchasing a handgun. The NICS database includes "permit check" and "permit recheck" counts that represent individuals applying for a concealed carry permit in a given year, and when added to handgun background check totals, they provide an estimate for total handgun prevalence. However, some states have different ways of facilitating concealed carry permit checks, and would disrupt the data if not treated. For instance, in July, 2006, Kentucky began running monthly checks on concealed carry permit holders to ensure their continued validity to hold a firearm. This has led to a disproportionate number of "checks" in Kentucky, and would significantly skew data if not treated.[21]

In a similar manner, Utah administered quarterly permit checks on concealed carry permit holders in 2011 (Koenig, 2018). To contend with this irregularity, permit check counts were treated with a 0.25 multiple for Utah's 2011 observation. While I could not

[20]The FBI issues this disclaimer across every year of NICS data: "These statistics represent the number of firearm background checks initiated through the NICS. They do not represent the number of firearms sold. Based on varying state laws and purchase scenarios, a one-to-one correlation cannot be made between a firearm background check and a firearm sale" (FBI, "About NICS").

[21]For instance, in 2023, a reported 3,391,543 permit checks were performed in KY, a clear overestimation of the number of handguns purchased in the state.



identify a specific state statute to corroborate this, North Carolina likely instituted a similar policy in 2014, as permit check counts went from 300,000 in 2013, to 900,000 in 2014, before returning to 300,000 checks in following years.[22] To rectify this, North Carolina permit counts in 2014 were treated with a 0.3 multiple. Koenig (2018) also notes policies instituted in Maryland, Wisconsin, and Iowa that relaxed the concealed carry application, leading to significant upticks in check rates. While Koenig excluded these states in his analysis, they are included here. Illinois presents likely-problematic permit check levels (and is excluded in Koenig's (2018) analysis) yet due to issues in other data sets, the state is excluded from analysis already. Ultimately, more work is necessary to assess the validity of the NICS system and their combined permit and BGC counts to ensure the most accurate representation of new handgun flows into the legal market.

These disclaimers aside, NICS data still offers a promising means of measuring handgun prevalence. Figure 1 illustrates both total check[23] and handgun check counts

[22]Values are approximated from original NICS data.

[23]The "total check" count in NICS sums every background check observed for each state and year. This includes handgun & long gun checks, permit checks, as well as certain miscellaneous checks such as pawn shop and gun rental sales, for which the Brady Act also mandates FFLs perform BGCs. Treatments to

across 1999-2021. There are noticeable trends in the data. First, check counts see a gradual rise following 2008. Depetris-Chauvin (2015) observed an "Obama effect" on demand for firearms; following Barack Obama's election in 2008, there was a significant rise in gun demand corresponding to a fear of gun reform under the Obama administration (Depetris-Chauvin, 2015). While not an empirical analysis, Greenwood (2017) remarks on the rise in firearm demand in the leadup to the 2016 election; Hillary Clinton campaigned on strong gun reform, and consumers likely responded to the risk of imposed firearm restrictions by purchasing more during the election cycle (Greenwood, 2017). There is a noticeable decline in BGC rates following the election of Donald Trump. Finally, the onset of the Covid-19 pandemic brought with it an unprecedented acceleration of gun demand; an estimated 7.5 million Americans became new gun owners from January, 2019, to April, 2021. Stockpiling effects seen across consumer goods during the pandemic were likely exacerbated in the firearms market due to stirring events such as the January 6th Capitol Insurrection, and the murder of George Floyd (Miller et al., 2022).

Assessing BGCs on a more granular, state level, Table 3 illustrates the average handgun and total check rate from 1999-2021 for 16 states with the highest and lowest degree of handgun ownership on a per capita basis. With the exception of Utah – a rural state where long guns are far more prevalent than national averages[24] – the states with the lowest rates of handgun checks tend to have more stringent firearm legislation.

4.3 Stolen Gun Value Data: National Incident Based Reporting System (NIBRS)

For the outcome of interest in the first stage of analysis, state-level yearly stolen property data is derived from the FBI Uniform National Incident Based Reporting System (NIBRS) database, which provides a range of crime data from 1960-2022. While the FBI publishes these data publicly through the UCR "Supplement to Return A" file, data is

Kentucky, North Carolina, & Utah check counts are also applied to these totals.

[24]This is explained by Utah's significantly higher rates of per capita total gun checks. While Utah represents a less populated and more rural population – which is associated with higher rates of long gun usage through recreational/hunting means – its overall check rate may point to further data treatment issues not currently accounted for in the NICS database.

Table 3: States with the Ten Highest and Lowest Handgun BGC Rate

State	Handgun BGC Rate	Total BGC Rate
New York	0.0057	0.0143
Rhode Island	0.0085	0.0178
Maryland	0.0111	0.0219
Utah	0.0145	0.1160
Delaware	0.0174	0.0350
Massachusetts	0.0183	0.0232
California	0.0189	0.0288
Nebraska	0.0190	0.0352
Connecticut	0.0394	0.0495
Wyoming	0.0396	0.0943
New Hampshire	0.0449	0.0713
Tennessee	0.0451	0.0724
Minnesota	0.0500	0.0770
Pennsylvania	0.0503	0.0626
Alabama	0.0524	0.0923
Indiana	0.0688	0.0844

Note: Values represent average BGC counts over sample period measured on a per capita basis.

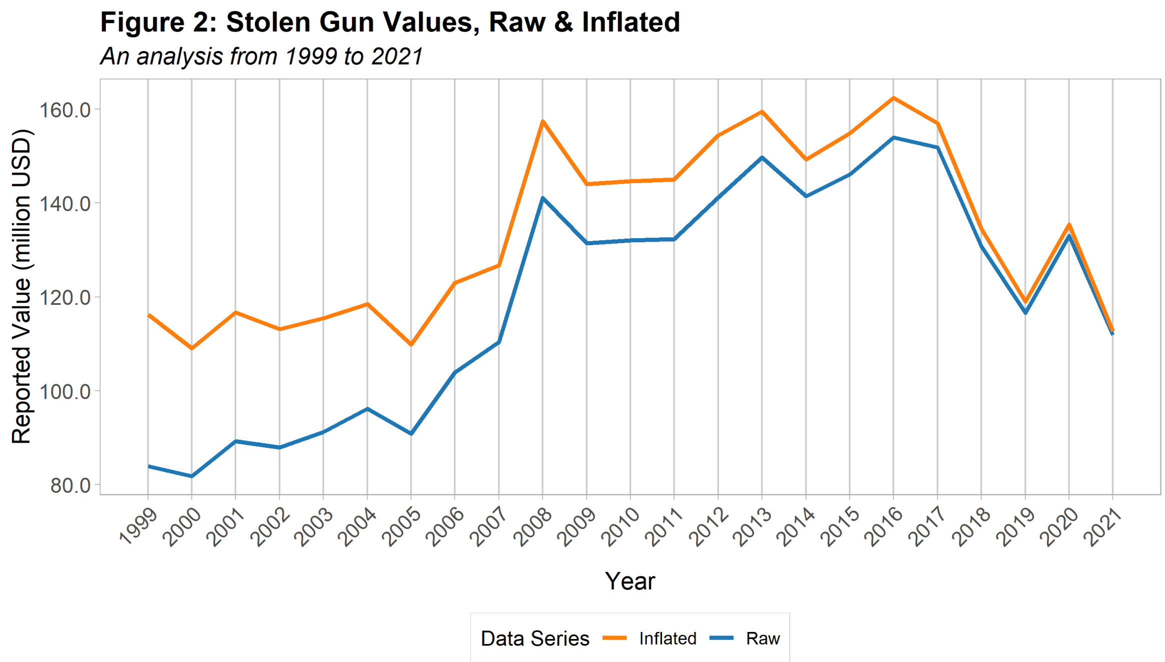
derived from Jacob Kaplan's treated and concatenated version of the file. UCR data draws monthly crime reporting statistics directly from local police agencies, allowing researchers to capture panel data on the agency-month basis. Most collected data relates to property theft, but crimes included are murder, rape, robbery, burglary, theft/larceny, and motor vehicle theft. Instances of stolen property are subdivided into 11 asset types, and – conveniently – one such division is firearms. However, firearms are not further broken down into handgun and long gun values, meaning data is representative of both long gun and handgun theft. Heterogeneity does exist across states in terms of their handgun-to-long gun ratio based on demographic and environmental characteristics, allowing for the potential for results to be slightly skewed as a result when attempting to isolate the impact of handgun prevalence on NIBRS data.

Despite Khalil's (2016) study referring to distinct numbers of guns reported stolen, the Supplement to Return A file reflects the nationwide law enforcement practice of assessing the value – not the counts – of stolen firearms. As the NIBRS system attempts to standardize property valuation practices nationwide, it offers instructions for agencies to follow regarding property valuation. Among them, agencies are advised to "use the victim's valuation (in most instances) of items such as jewelry, watches, and other similar goods that decrease in value slightly or not at all with use or age" (FBI, 2021). A further instruction suggests agencies "should use common sense and good judgment to determine the value reported for the stolen items... when the victim obviously exaggerates the value of stolen/destroyed/damaged property for insurance or other purposes" (Ibid). In private correspondence with the Durham, NC Police Department, these practices were confirmed almost verbatim. Thus, while the outcome of interest in the first stage of this study is left to the common sense of both property theft victims and police officers, there are at least some standards in place for data collection. Furthermore, it is unlikely that there exists a profound degree of heterogeneity amongst states in terms of their populations' tendency to value stolen property.

Participation by local law enforcement agencies is not universal, and agencies have joined the program at different points in time. However, 21 states have fully transitioned to the NIBRS system, with most non-fully participating states covering at least 80% of their population. The exceptions are New York, Florida, Pennsylvania, and California, serving 23%, 32%, 42%, and 55% of their populations, respectively (FBI, "About NIBRS"). In any case, the Supplement to Return A file includes a population covered variable, allowing for rates of participation to be controlled for.

For the purposes of this study, data is collapsed from the agency-month to state-year level. For some state-month observations, massively unrealistic values of firearms stolen are recorded; in response, stolen firearm values in excess of the 99.95th percentile are excluded, which is a total of 309 state-month observations. Among the data, four states present flawed data, and are excluded from analysis: Illinois, New Jersey, Hawaii, and the District of Columbia. All four, for unknown reasons, have large gaps in yearly reporting participation, with either 0 reported stolen firearm value, or extremely small values. Furthermore, there are eight one-year gaps in NIBRS reporting across seven states, producing a slightly unbalanced sample given these states' inclusion.[25] No other data set

[25] These state-year observations are: 1999, 2005 Florida; 1999 Alaska; 1999 Kansas; 1999 Montana; 2005 West Virginia; 2005 Virginia; and 2005 Vermont.



has similar issues in reporting, so results can be estimated for 47 states from 1999-2021, producing an overall sample size of 1073 observations.

In order to control for inflation in reported property values, data has been treated by the Bureau of Labor Statistics's (BJS) Small Arms[26] Producer Price Index (PPI), which is available on a monthly level. PPIs are calculated using a Laspeyres index, which essentially compares the base period price of production for a commodity to the current period price of the same commodity. BJS employs economists to work with firms in a given commodity sector and establish a strictly confidential reporting relationship where firms report price levels to a secure website monthly. While there is no available consumer price index for firearms, a small arms PPI is likely the strongest representation of an index of the cost of small arms for consumers, and thus reflective of gun owner's base evaluation for the worth of their firearm. In this analysis, 2022 is considered the base year estimate; this means previous years are treated with a PPI inflator – rather than deflator – to reflect changing rates of inflation (BLS, 2024).

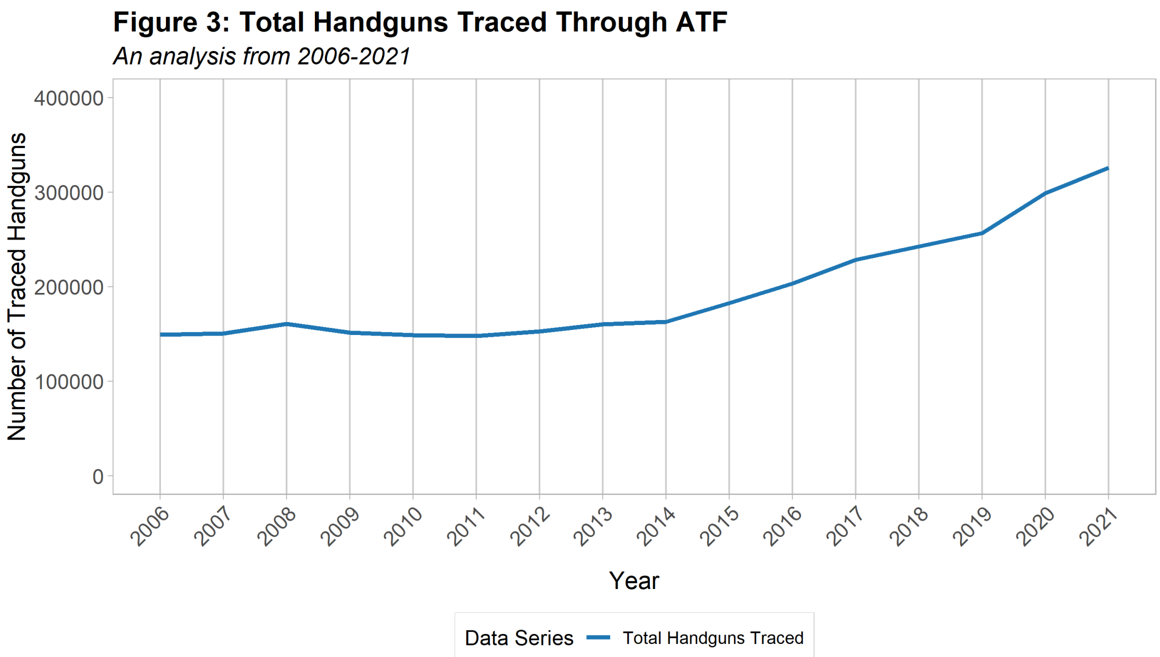
[26] "Small arms" include all weapons designed for personal use like rifles, shotguns, etc. – not just handguns. This serves as a better treatment for the value of stolen firearms, as this metric includes long gun theft.

Figure 2 illustrates reported stolen gun values, both raw and inflated. Trends in these values bear a resemblance to trends seen in BGC rates, with similar upticks in stolen gun values in the buildup to the 2008 and 2016 elections, before a significant downturn following the election of Donald Trump. Also similarly, gun theft values rose during the onset of the Covid-19 pandemic, but fell in 2021. At least descriptively, BGC counts seems to correlate with trends in gun theft, suggesting its efficacy as a gun proxy if one subscribes to the "more guns, more crime" philosophy.

4.4 ATF Gun Tracing Statistics

To represent the illicit firearms market, this research draws on Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) gun tracing data, which has tracked the flow of crime-related firearms since 2006. When law enforcement officials seize a gun in the course of their operations, they typically will send its identifying serial number to the ATF's National Tracing Center for a tracing analysis. A successful trace on the firearm allows ATF officials to report back to the originating law enforcement agency which specific FFL the gun was purchased through. A distinct drawback of ATF data is that – although the number of unsuccessful traces are not recorded – serial numbers are often obliterated from firearms involved in crime, or are otherwise unable to be traced back to their original FFL (Braga et al., 2012). If a gun is successfully traced, it is included in a tracing report produced by the ATF for each state annually. These trace reports illustrate the type of firearm recovered, the category of crime associated with the seized firearm, the "time to crime" from the original gun sale to its seizure, and the origin state for the firearm. Tracing data is also subdivided by type of firearm, which is convenient in this study's analysis focusing on the specific role of handguns. There are three classifications of handguns reflected in tracing data: pistols, revolvers, and derringers; I sum these counts to produce a total count of handguns traced through the ATF system, grouping by state and year.

Figure 3 illustrates the total count of handguns reported through ATF's National Tracing Center from 2006-2021. Illegal handgun flows as measured through these counts appear relatively stable until 2014, whereupon rates of traced handguns rise steadily through 2021.



4.5 Demographic Controls and Gun Homicide Data: National Vital Statistics System (NVSS)

Demographic data used for control purposes are drawn from the CDC's National Vital Statistics System (NVSS). The NVSS dataset captures demographic and mortality data as far back as 1968. For this research, state-year level demographic data on overall, female, black, and white populations are gathered. The socioeconomic controls of unemployment rate and employment population ratio are also recorded. Additionally, data on suicides – subdivided between firearm and non-firearm related suicides – are included. Finally, as the outcome of interest in the second stage of analysis, counts of firearm and non-firearm homicides are recorded through this database.

4.6 Summary Statistics

Summary statistics are provided below for the key variables outlined throughout the data section. Rows 1-2 describe BGC values used as the gun prevalence proxy on the explanatory side of the first stage of regressing, with row 2 denoting the first stage outcome of interest, PPI-treated stolen gun values, along with row 3, which provides the population covered by the NIBRS system.[27] Row 5 denotes the proxy for the illicit pool of guns used as the explanatory term in the second stage of regressing, given by ATF traced handguns. Row 6 captures the outcome of interest in the second stage: gun homicides. Row 7 and 8 are used in each stage as a validity & falsification test, respectively. Rows 9-14 capture the demographic controls used in each stage.

Table 4: Summary Statistics

	Variable	Mean	Std. dev.	Min	Max
1	Handgun BGCs	168403.1	225479.8	4010	1673824
2	Total BGCs	298273.4	316668.3	9414	2377167
3	VSGppi	2857844	3941078	28733.39	3.83e+07
4	UCR PopCov	5395818	6541708	106360.6	3.94e+07
5	ATF Handguns	4159.611	5331.068	54	42357
6	Gun Hom	250.1836	309.8914	0	1942
7	Nongun Hom	104.973	123.9091	0	733
8	Gun Suicides	408.9515	378.5457	12	2528
9	Pop	6042743	6905215	491780	3.96e+07
10	Pop Female	3069145	3493921	244518	1.99e+07
11	Pop Black	811766.5	989857	3535	3984144
12	Pop White	4824587	5323636	460782	2.94e+07
13	UR	5.458807	1.985681	2.1	13.8
14	EPR	61.8562	4.767335	49.5	72.8

Note: N = 1,073 observations for all observed variables except ATF handgun tracing counts (N = 752), due to ATF data limited to 2006 onwards.

5 Results

5.1 First Stage Results

The first stage of regression results, applying the model outlined in expression (1) across the three levels of CAP policies, are found in tables 5-7 below. Regression results from this stage of the analysis illustrate a significant and negative effect of CAP laws on rates of stolen gun property, yet the strength of this relationship declines as the stringency

[27] Note how, even on the aggregate level, UCR PopCov diverges significantly from total US populations given by the Pop metric.

of CAP legislation falls from CAPliability to CAP14. A promising element from this set of regressions lies in the performance of the logged handgun BGC counts used to proxy for state handgun prevalence; the coefficient β_2 remains relatively consistent across all levels of the CAP models with significance at either the 5% or 10% level, predicting a positive elasticity of firearm theft with respect to prevalence (See Figure 4). Again, the construction of the model means that β_2 represents the stolen firearm elasticity with respect to prevalence only in states without CAP firearm laws.

Table 5: First Stage Analysis: ln(Value of Stolen Firearms) on CAPliability

	Coefficient	Std. err.	t-value	p-value
CAPliability x ln(hgBGC)$_{t-1}$	-0.2057**	0.0922	-2.23	0.031
ln(hgBGC)$_{t-1}$	0.1132**	0.0438	2.59	0.013
CAPliability$_{t-1}$	2.6869**	1.1975	2.24	0.030
ln(p_popf)	2.4296	11.0147	0.22	0.826
ln(p_popb)	0.1849	0.3977	0.46	0.644
ln(p_popw)	5.3675	4.7444	1.13	0.264
EPR	-0.0306*	0.0175	-1.75	0.087
UR	-0.0229	0.0182	-1.25	0.216
ln(popcov)	0.9793***	0.1374	7.13	0.000
ln(pop)	0.3412	0.5792	0.59	0.559
Observations: 1023 Overall R^2: 0.4861				

Table 6: First Stage Analysis: ln(Value of Stolen Firearms) on CAP18

	Coefficient	Std. err.	t-value	p-value
CAP18 x ln(hgBGC)$_{t-1}$	-0.1462*	0.0785	-1.86	0.069
ln(hgBGC)$_{t-1}$	0.1017**	0.0445	2.29	0.027
CAP18$_{t-1}$	1.8871**	0.8262	2.28	0.027
ln(p_popf)	3.8947	10.7666	0.36	0.719
ln(p_popb)	0.1715	0.3729	0.46	0.648
ln(p_popw)	6.4351	4.7289	1.36	0.180
EPR	-0.0340*	0.0192	-1.77	0.083
UR	-0.0210	0.0194	-1.09	0.283
ln(popcov)	0.9838***	0.1458	6.75	0.000
ln(pop)	0.4263	0.5540	0.77	0.445
Observations: 1,023 Overall R^2: 0.4472				

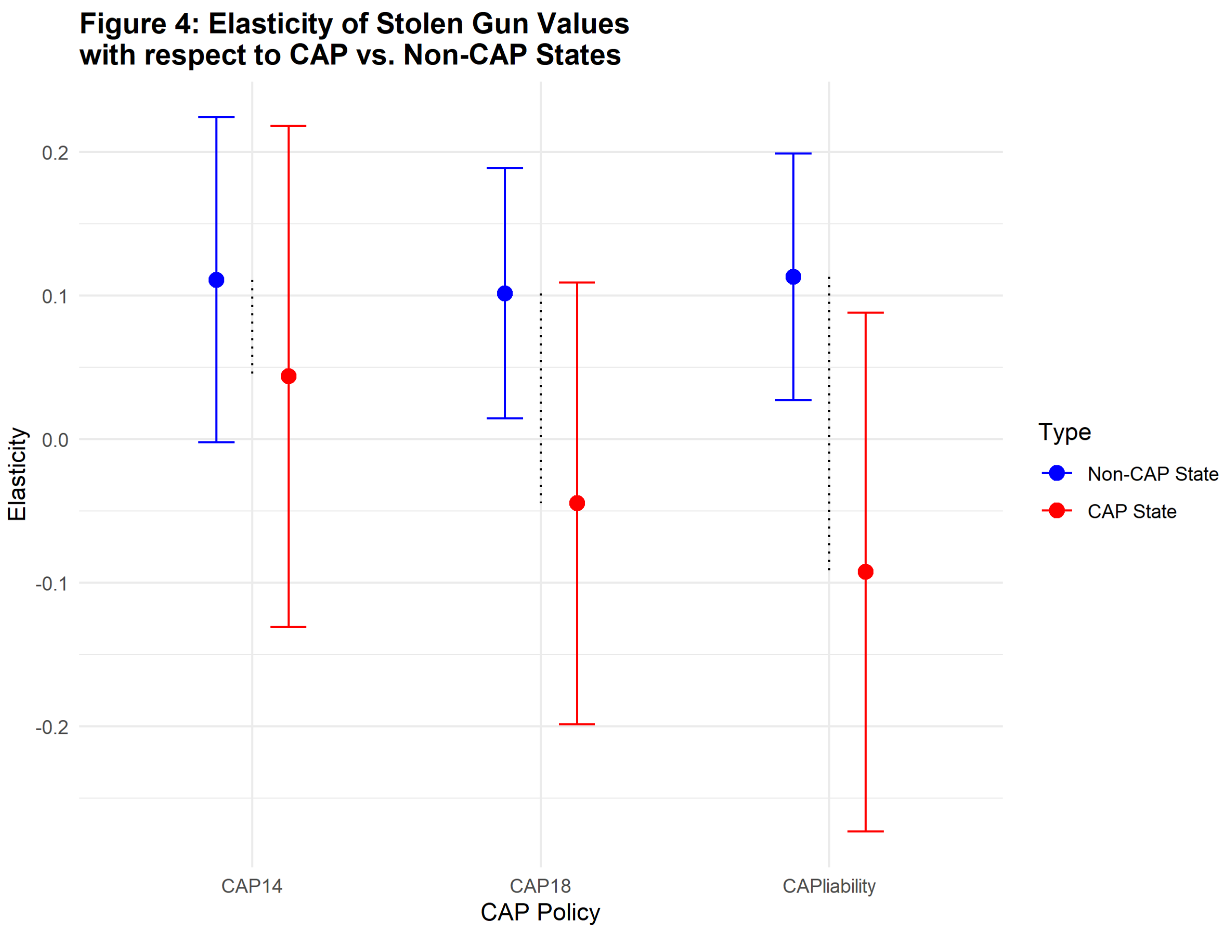
Table 7: First Stage Analysis: ln(Value of Stolen Firearms) on CAP14

	Coefficient	Std. err.	t-value	p-value
CAP14 x ln(hgBGC)$_{t-1}$	-0.0673	0.0890	-0.76	0.454
ln(hgBGC)$_{t-1}$	0.1111*	0.0578	1.92	0.061
CAP14$_{t-1}$	0.9108	0.9824	0.93	0.359
ln(p_popf)	5.3484	10.3634	0.52	0.608
ln(p_popb)	0.1962	0.3673	0.53	0.596
ln(p_popw)	7.2693	4.4919	1.62	0.112
EPR	-0.0264	0.0190	-1.39	0.170
UR	-0.0121	0.0205	-0.59	0.560
ln(popcov)	0.9902***	0.1449	6.83	0.000
ln(pop)	0.4768	0.5340	0.89	0.377
Observations: 1,023 Overall R^2: 0.4081				

Note: For Tables 5-7, Models include demographic covariates for logged proportion of female, black, white, overall, and UCR-covered population, as well as rates of unemployment and employment population ratio. State and year fixed effects are included. Year dummy variables are included, but not shown. Results are weighted based on the average state population over the sample period. Standard errors are clustered by state. Overall R^2 is calculated as the weighted average of the within and between R^2 values in the fixed effect format. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

The novel interaction term captures the elasticity of stolen firearm values with respect to handgun prevalence when accounting for the β_1 coefficient value; the true elasticity of the value of stolen guns with respect to CAP-holding states, then, is 0.1132 β_1 + (-0.2057) = an elasticity estimate of -0.0925. Thus, a 10% increase in lagged BGC counts is associated, roughly, with a 1% drop in stolen gun values. This indicates a very strong mechanism of CAP laws against theft, and the coefficient itself is significant at the 5% level. However, this estimate may be too high in value, given it predicts a decrease in the rate of gun theft to correspond with an increase in the rate of gun prevalence.

These elasticity measures of gun theft values with respect to CAP policies is broken down more explicitly in Figure 4. Figure 4 includes the gun theft elasticity with respect to firearm prevalence & CAP laws for each CAP policy type, as well as a 95% confidence interval computed from each β_1 & β_2 value and its standard error. The standalone prevalence proxy and its consistent performance across each form of CAP policy is also visualized here, as this term remains positive, significant, and around 0.11 across regressions for each CAP policy. While its standard error expands slightly when moving from CAP18 to CAP14, the general performance of β_2 serves as strong evidence for its use as a proxy for



state firearm prevalence in further research. Given its statistical strength in predicting a positive elasticity of gun theft – itself a form of crime – with respect to firearm prevalence, this thesis works in supporting the "more guns, more crime" hypothesis, as increased firearm prevalence in the form of increased background checks, lagged for one year, predicts a positive response in crime levels. A negative β_2 coefficient would support John Lott & the "more guns, less crime" hypothesis, as increased firearm prevalence would theoretically lead to reduced crime. Given the separate interaction term separating CAP states, the prevalence proxy and its performance is measured only through non-CAP law states. More analysis may be required to confirm the validity of the BGC proxy on a country-wide level.

While the non-CAP state gun prevalence proxy performs consistently across the first stage of analysis, the three different β_1 coefficient values ascribed to each degree of CAP policy diverge significantly. Once again, CAPliability, the CAP law associated with the harshest legal penalties for improper gun storage, is found to lead to the largest and most statistically significant divergence from non-CAP law states in terms of gun theft, with its aforementioned gun theft elasticity of -.0925. The significance of the β_1 value at a 5% level demonstrates the likely strong relationship between strict CAP laws and reduced rates of household firearm theft. As mentioned, CAPliability's strong negative coefficient, despite its significance, may represent an inflated value. For, the -0.0925 elasticity implies that, in CAPliability states, increased firearm prevalence (as proxied through BGC counts) leads to a reduced level crime (or, at least, reduced levels of gun theft). This conflicts with the "more guns, more crime" theory, as increased prevalence in CAP states ostensibly lowers gun theft levels. A more reasonable expectation may be that the CAPliability policy leads to a reduced – yet still positive – β_1 coefficient, as this figure would imply lower rates of gun theft compared to non-CAP states that is still in line with the "more guns, more crime" theory of gun crime elasticity with respect to prevalence.

In any case, the same effects and significance found in CAPliability cannot be said for the two lower-level CAP laws. While CAP18 is found to lead to similarly-reduced levels of gun theft when compared with non-CAP law states, its coefficient value maintains only week significance at the 10% level. While the coefficient of CAP14 also suggests reduced levels of gun theft in response to the policy, it fails to satisfy any degree of statistical significance, with a p-value of 0.454. This suggests that more relaxed CAP laws have a reduced, and, in this case, insignificant effect on gun theft when compared to more stringently regulated states.

Across the model, the only control covariate with any demonstrated level of significance is the employment population ratio, suggesting that perhaps the economic conditions may affect the rate at which firearms are stolen. Due to the likely inflated standard errors across the first stage of analysis, it is another sign that more socioeconomic features unaccounted for in both control covariates and state & year fixed effects explain some portion of the model's variation not yet considered.

The explanatory power of the first stage model, as measured through overall R^2 values, does, at face value, give pause to the model's validity. In many cases, R^2 values from 0.4-0.5 may be deemed to low to imply causal relationships between explanatory and response variables, regardless of the strength of the regression itself. However, within the context of CAP laws, relatively low R^2 values coupled with high strength in significance (at least, for CAPliability) make more sense. Namely, most American households don't even have children in the home, and thus are unaffected by CAP policies: The 2022 Current Population Survey Annual Social and Economic Supplement published by the US Census Bureau revealed children under 18 live in 40% of American households, declining from 43% in 2012. Thus, the influence of CAP laws on firearm storage practices reaches only a subset of American households, affecting similarly a subset of gun owners. Moreover, while stringent CAP laws are demonstrated to significantly lower gun theft, they are by no means the only determinant of a state's rate of gun theft. Demographic, criminogenic, and law enforcement[28] features of a state not currently accounted for in the model, such as its urban-rural breakdown, may significantly alter rates of gun theft. Including these metrics in further analysis would likely reduce the standard errors across each policy's β_1 value. These figures currently higher than expected, especially when compared to the stable β_2 standalone prevalence coefficient. So, while relatively low R^2 values are a source of concern in the first stage of analysis, I argue they are not cause enough to overlook the model's significant findings.

In all, while some concerns exist regarding the size of the β_1 coefficient associated with CAPliability & its associated standard error, the first stage of analysis shows a significant relationship between more stringent CAP policies and the rate of firearm theft. As corroborated by the likes of Kappelman & Fording (2020) and DeSimone et al. (2013), these results suggest only the strictest CAP laws significantly alter household gun storage practices, and thus rates of gun theft.

[28]Such as a state's number of police officers per capita or number of yearly arrests per capita

Table 8: Validity Test: F-test Results for Stage 1 Analysis

Linear Combination	F-statistic	Prob > F
CAPliability x $\ln(\text{hgBGC})_{t-1} + \ln(\text{hgBGC})_{t-1} = 0$	0.02	0.8791
CAP18 x $\text{hg}(\text{BGC})_{t-1} + \ln(\text{hgBGC}_{t-1} = 0$	0.00	0.9744
CAP14 x $\ln(\text{hgBGC})_{t-1} + \ln(\text{hgBGC})_{t-1} = 0$	1.61	0.2114

F-tests examining the joint significance between non-CAP state and CAP state terms – shown below in Table 8 – provide reassurance as to the model structure. Their insignificance confirms that the variability within CAP states does not explain variability within non-CAP states, and vice versa. In other words, there is no underlying feature within the model that explains variation between CAP and non-CAP states other than the policies themselves.

Table 9: Validity Test: Overall Firearm Suicides on CAPliability

	Coefficient	Std. err.	t-value	p-value
CAPliability x $\ln(\text{hgBGC})_{t-1}$	-0.0415**	0.0173	-2.40	0.020
$\ln(\text{hgBGC})_{t-1}$	0.0523***	0.0153	3.42	0.001
$\text{CAPliability}_{t-1}$	0.4469*	0.2225	2.01	0.050
ln(p_popf)	2.5394	2.1254	1.19	0.238
ln(p_popb)	0.2904***	0.0624	4.65	0.000
ln(p_popw)	2.1184**	0.8047	2.63	0.011
EPR	-0.0023	0.0044	-0.52	0.606
UR	-0.0074	0.0051	-1.45	0.154
ln(pop)	0.6231***	0.1077	5.79	0.000
Observations: 1,073				
Overall R^2: 0.7447				

Note: Model includes demographic covariates for logged proportion of female, black, white, overall, and UCR-covered population, as well as rates of unemployment and employment population ratio. State and year fixed effects are included. Year dummy variables are included, but not shown. Results are weighted based on the average state population over the sample period. Standard errors are clustered by state. Overall R^2 is calculated as the weighted average of the within and between R^2 values in the fixed effect format. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Table 9, shown above, examines the relationship between CAP laws and firearm suicide. As CAP laws have already been found to have a negative relationship with overall rates of firearm suicides (Webster et al., 2004), replicated findings here serve as further confirmation for the model's validity. Using CAPliability – the most stringent of the CAP regulations – CAP laws are shown to have significant impact on the rate of firearm suicide at the 2% level. Whereas the coefficient estimate may have been slightly overstated for the CAPliability interaction coefficient, β_1 behaves more conceivably here, as the true elasticity in firearm suicides is calculated as 0.0523+(-0.0415) = 0.0108, meaning a 10% increase in lagged BGCs in CAPliability states is associated with a 1% increase in firearm suicides, as opposed to a 5% increase in non-CAP law states. This is a more expected result, as it captures the effect of increased firearm prevalence on firearm suicide, but finds that CAP law states see a much less significant response to it. In any case, observing the same strong negative relationship between CAP laws and overall firearm suicide as found in related literature bolsters the empirical credibility of the model.

5.2 Second Stage Results

Having established a mechanism behind reduced rates of gun theft through stringent CAP law policies, the second stage of analysis proceeds to a discussion on how illicitly-obtained firearms impacts rates of gun homicides. Table 10, as well as a corresponding falsification test in Table 11, are presented below, and represent the two way fixed effects model described in the expression (2).

Table 10 shows a strong degree of statistical significance at the 1% level between illicitly-obtained firearms and gun homicides, with a β_1 coefficient representing the firearm homicide elasticity with respect to lagged ATF counts. Given the potential for influxes in the legal gun market to influence rates of firearm crime, the BGC prevalence proxy used in the first stage of analysis is included here, largely for control. Given how the ATF β_1 coefficient value, while of equal statistical significance to the BGC β_2 coefficient, is more than double its magnitude, it may suggest that illegally-obtained firearms have a higher rate of affecting firearm homicides in a given state. Put more explicitly, a 10% increase in a state's lagged pool of illicitly-held firearms leads to a 2.2% increase (β_1) in firearm homicides, compared to a 1.1% increase in firearm homicides associated with a 10% increase in legal firearm flows (β_2). This corroborates with the findings of Robinson et al. (2024), where reported stolen guns had a higher predictive power to end up as used in crime than guns that were never stolen. Thus, the second stage of analysis shows the significant effect of illegally-obtained firearms on firearm homicides, making the anti-gun theft mechanism established in stage one a more significant policy implication.

The second stage regression features a high explanatory power, with an R^2 value of 0.8826, meaning the model is able to explain a high degree of variation in firearm homicides. Furthermore, the the β_1 estimate features a low standard error with respect to its mean, another signal of its predictive power on rates of gun homicide. Whereas the nature of CAP laws as single avenue amongst many for legally-held firearms to enter the illicit market helps explain the lower R^2 observed in the first stage, the use of the ATF proxy as a more complete representation for the pool of illicitly-held firearms – coupled with shifts in legal firearm flows through the BGC proxy – likely leads to the second stage's much higher R^2 value. In this sense, the second stage model and its inclusion for both the illicit and legal market for guns is able to capture almost all variation in firearm homicide across states.

While there are strong observed effects in the second stage regression, Table 11 provides the same model, except with the outcome of interest measured in non-gun homicides. Shifts in illicit firearms shouldn't have, in theory, a measurable effect on the rate of non-gun homicides. If anything, rates of non-gun homicides should decrease in response to higher rates of illicit firearms, due to a substitution effect as firearms are more accessible to criminals. As seen in Table 11, neither the ATF measure for illicit firearms, nor the BGC control, significantly predict non-gun homicides. These results signify a justified predictive relationship between illicitly-held firearms and homicides used with firearms, and thus serve as a sign of the second stage model's validity.

Table 10: Second Stage Analysis: ln(Firearm Homicides) on ln(hgATF)

	Coefficient	Std. err.	t-value	p-value
$\ln(\text{hgATF})_{t-1}$	0.2244***	0.0554	4.05	0.000
$\ln(\text{hgBGC})_{t-1}$	0.1129***	0.0377	3.00	0.004
ln(p_popf)	1.0424	6.2562	0.17	0.868
ln(p_popb)	0.7224**	0.3194	2.26	0.028
ln(p_popw)	3.5417	2.4252	1.46	0.151
EPR	-0.0088	0.0135	-0.65	0.521
UR	-0.0092	0.0154	-0.60	0.552
ln(pop)	0.5968**	0.2896	2.06	0.045
Observations: 703				
Overall R^2: 0.8826				

Table 11: Falsification Test: ln(Nongun Homicids) on ln(hgATF)

	Coefficient	Std. err.	t-value	p-value
$\ln(\text{hgATF})_{t-1}$	0.0366	0.0290	1.26	0.213
$\ln(\text{hgBGC})_{t-1}$	-0.0064	0.0307	-0.21	0.837
ln(p_popf)	-4.9400	3.7129	-1.33	0.190
ln(p_popb)	0.3804**	0.1455	2.61	0.012
ln(p_popw)	-1.5277	1.2596	-1.21	0.231
EPR	0.0122	0.0094	1.30	0.200
UR	0.0080	0.0093	0.86	0.392
ln(pop)	0.0568	0.1680	0.34	0.737
Observations	703			
Overall R^2	0.5112			

Note: Models 10-11 include demographic covariates for logged proportion of female, black, white, overall, and UCR-covered population, as well as rates of unemployment and employment population ratio. State and year fixed effects are included. Year dummy variables are included, but not shown. Results are weighted based on the average state population over the sample period. Standard errors are clustered by state. Overall R^2 is calculated as the weighted average of the within and between R^2 values in the fixed effect format. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

6 Conclusion & Policy Discussion

While CAP laws were designed with the main purpose of reducing children's access to firearms, I find these policies likely reduce thieves' access to household firearms as well; the first stage of this analysis suggests credible evidence that the presence of CAP laws make measurable impacts on the rate at which guns are stolen from the pool of legal gun ownership. While the most stringent of these policies makes a significant impact on the rate of firearm theft, CAP laws holding parents liable for improper firearm storage only after their children have accessed and used the firearm prove insignificant in altering gun storage practices to the point of a measurable impact on gun theft. The second stage of analysis proceeds to prove the strong predictive power between the number of illicitly-held firearms in a state and its rate of firearm-related homicides in the following year. With this established, any mechanism reducing the rate at which guns transition from the legal to illicit market – where they are more routinely used in violent crime – offers a credible opportunity to lower firearm homicide. Given stage one's findings, stringent CAP laws present a viable means to reduce gun theft and the subsequent market entrance of legal firearms to the illicit market.

That said, if a state were to increase the stringency of their CAP laws – or enact stringent CAP policies with no prior legislation – the already-observed benefits of decreased accidental gun deaths would likely be met with a reduction in homicides committed via firearm. Importantly, CAP policies do not altogether restrict the right to own a firearm, and may serve as a more viable piece of legislation to pro-gun advocacy groups such as the NRA, who routinely block gun control legislation. The work of this thesis to outline the pathway from CAP laws to reduced firearm homicide will hopefully make such policies more presentable to these organizations, as well.

On an empirical level, this study's novel use of an interaction term between the presence of a particular gun policy and the geographic prevalence of firearms themselves offers a credible means of gun policy analysis left unexplored to this point. While standard difference-in-difference approaches attempt to isolate the effects of a specific gun policy, the inclusion of firearm prevalence in this research presents a more accurate depiction as to the effects of CAP policies, taking into account a state's non-static firearm prevalence. While more work needs to be done to assess the validity of the NICS background check proxy for firearm prevalence, it seems to offer a viable alternative to other measures, especially on a panel level basis.

That said, this research supports the notion that if states with less stringent CAP measures – or no CAP measures at all – were to adopt such policies, state gun theft would likely be reduced alongside the added and well-documented benefits towards public health through increased firearm safety.

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