The *Heller* Effect: How Expanding the Second Amendment Increased Handgun Purchasing

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Abstract

In 2008, the Supreme Court overturned a longstanding D.C. law and enshrined an individual right to keep and bear arms under the Second Amendment. This paper tests the effects of the *Heller* decision on legal handgun purchasing. Using administrative data from the National Instant Criminal Background Check System (NICS), I find that *Heller* led to an increase of 5.38 handgun sales per 100,000 residents per month, on average, in the two years following the ruling – a rate increase of approximately 1,630 percent compared to the pre-*Heller* rate of handgun NICS reports per capita. These findings highlight that the *Heller* decision led to first-order effects where we most expect to find them, perhaps leading to downstream changes in policy and political attitudes that we are unable to study with the same level of rigor.

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1 Introduction

The landmark decision in *District of Columbia v. Heller*, 554 U.S. 570 (2008) affirmed an individual right to keep and bear arms under the Second Amendment and struck down a 1975 D.C. law that sought to reduce gun violence within the District (Winkler, 2011). Prior to 2008, states had considerable leeway in restricting firearm ownership and possession. In D.C., the Firearms Control Regulations Act of 1975 effectively acted as a ban on the private ownership of handguns (Whitman, 2023). The Court's decision in *Heller* ultimately struck down the handgun ban and trigger lock provisions of the 1975 bill, ruling that these regulations were prohibitions on the use of firearms for self-defense and constituted unconstitutional restrictions on the rights enshrined in the Second Amendment (Winkler, 2011). The 1975 law, which sought to address high rates of gun violence within the D.C. community, was essentially hamstrung with the striking of the provisions related to restrictions on handgun possession. *Heller* changed the landscape for Second Amendment litigation, paving the way for future decisions that require states to justify any contemporary firearms policies by showing an analogous policy existed in the founding era, and leading the Court to hear cases that have challenged long-standing firearms law (Whitman, 2023). In summation, the decision could be thought of as having primarily impacted D.C. law and having less direct (though perhaps observable) effects on other measurable outcomes.

In contrast, multiple studies examining the effects of the *Heller* decision on public opinion on firearms, gun policy, and the Second Amendment argue that the effects of the ruling "have been rather limited... [with] generally small or non-existent impacts on gun policy... and on public attitudes toward gun regulation," (Goss and Lacombe, 2020), with other scholars going so far as to argue that "*Heller* is likely to have relatively little impact as a legal weapon against other current and future gun laws," (Henigan, 2009). This past scholarship on *Heller* focuses upon changes in gun policy across the states to assess the direct effects of the ruling. However, I argue that any effect of the *Heller* decision on state policy is more appropriately considered a "downstream" outcome. While it is not unreasonable to think that the Court's ruling may have affected public opinion more broadly, previous research designs have not measured the primary effect of the *Heller* decision: how policy *in D.C.* was changed, and, consequently, how this change affected gun ownership. While previous scholars focus on other effects of the 2008 decision, this paper is the first to examine how the Supreme Court's ruling affected handgun purchasing in the District of Columbia.

In examining the effects of *Heller* on downstream measures such as state policy, past work is complicated by the difficulties in quantifying policy change – especially for those laws that are the most directly affected by the Court's opinion. State-level policy is hard to code in an unbiased and rigorous manner. Moreover, inferring how policy changes affected the restrictiveness of a state's law – for example, by tracking the number and type of policies by state – may miss how the policies are implemented and enforced by non-legislative actors (Arkhangelsky and Imbens, 2023; Sharkey and Kang, 2023). Perceived ratings of a state's policy may over- or understate the impact of a state law by incorporating non-policy factors such as the contemporary political climate. And while the Supreme Court's ruling is binding across all the states and therefore may reasonably affect areas outside of D.C. and their respective policy regimes, the ruling ultimately struck down bans on the possession of handguns and some storage requirements as unconstitutional, but emphasized that "...nothing in [the Court's] opinion should be taken to cast doubt on longstanding prohibitions on the possession of firearms by felons and the mentally ill, or laws forbidding the carrying of firearms in sensitive places such as schools and government buildings, or laws imposing conditions and qualifications on the commercial sale of arms."[*] In other words, a wide range of firearms policies and restrictions were left unaffected by the decision. Therefore, the primary effects of the ruling should be observed *only* in places that had legislation actively banning handguns. Importantly, as of 2008, the District of Columbia was the only federal

[*] *District of Columbia v. Heller*, 554 U.S. 570 (2008), pp. 54–56.

district or state/state-equivalent in the United States with a handgun ban in effect.[†]

Court opinions have long been thought to have "first-order" effects on policymaking and politics, which might produce or incentivize changes in social or economic behavior as a response to a ruling (Rosenberg, 2008). For example, *Brown v. Board of Education of Topeka* (1954) ruled that segregation in public schools was unconstitutional, and directly led to increases in racial integration across the United States. "Second-order" effects might also come in the form of changes in political dynamics surrounding the issue in the decision (Mettler and Sorelle, 2018; Pierson, 1993). In the case of *Brown v. Board*, the backlash by white, Southern political leaders to integration is one such change in broader political dynamics. Regarding *Heller*, changes in state gun policy and public opinion on the Second Amendment are best conceptualized as "second-order" effects. In this paper, I focus on the behavioral outcome most closely tied to the decision striking down D.C.'s handgun ban: the purchase of handguns. I predict that these changes in gun policy *experienced* by D.C. residents may have affected social or economic behavior, constituting a first-order effect. Specifically, I examine changes in handgun ownership in the District of Columbia by using an administrative dataset on background checks run at the time of handgun sales by federal licensees to approximate rates of handgun purchases both before and following the Court's ruling.

In this paper, I show that this first-order outcome, individual handgun purchasing in D.C., was strongly affected by the *Heller* decision. Using a quasi-experimental interrupted time series design, I estimate the decision's effects on the District's per capita monthly rates of background checks through the FBI's National Instant Criminal Background Check System (NICS) for handgun-related transactions, which serves as a proxy for handgun purchasing.[‡] I find that background checks for hand-

[†]See, for instance, RAND's State Firearm Law Database. While some *cities* had similar policies, such as Chicago, *Heller* laid the groundwork for these laws to be challenged, as in *McDonald v. City of Chicago*, 561 U.S. 742 (2010). No other state had such a policy at the time of the ruling, so no state policy regime should presumably be as affected as D.C.'s.

[‡]Any change in purchasing uncovered through NICS must be considered as a change in the behavior of gun purchasers who are willing to go through the administrative processes that allow for regulated firearm ownership. I do not presume to argue that this decision had any effect on illegal firearm ownership or purchases through means that are not captured in the NICS data.

gun transactions increased by 5.38 checks per 100,000 people in the 24 months following the decision, on average, with similarly large effects observed over longer periods. Additionally, I show that the decision led to a trend in handgun purchasing unparalleled in other states at the time, or seen after other major changes in gun policy or Second Amendment case law.[§] Where past scholarship focuses on other effects and finds limited evidence for any caused by the *Heller* decision, I argue that gun purchasing is exactly the socioeconomic behavior that would presumably be the most responsive to the Court's decision, and my estimations confirm this. After all, private possession and purchasing of handguns was effectively banned in D.C. until, after *Heller*, it suddenly was not.

This paper also explores what we can learn from *Heller* as a case study of the link between increased rates of firearm ownership and rates of firearm-related violence. Conventional wisdom suggests that increased firearm availability is associated with higher rates of firearm-related deaths (Geier, Kern and Geier, 2017; Siegel, Ross and King III, 2013). Many scholars have sought to better understand the degree to which government action can reduce gun violence and gun crime. Studies that examine the effects of policy on outcomes narrowly tailored to the intended effect of the law find compelling results such as states with more restrictive firearms laws have lower rates of firearm suicides (Glasser et al., 2023), states that restrict firearm access among minors have associated reductions in the rates of firearm suicide deaths among these age groups (Kappelman and Fording, 2021), and restrictive licensing regimes coincide with declines in rates of firearm suicides and homicides (Loftin et al., 1991). Ultimately, I conclude that while my research design is appropriate for studying the effects of a policy change on rates of gun purchasing, it can tell us less about the downstream impacts of *Heller* on gun violence or firearm-related crime, for instance. While one plausible outcome is that governmental interventions weakening firearm restrictions cause increased gun violence (Miller and Hemenway,

[§]See Appendix Section A.1. for a discussion of the effects of other governmental interventions on gun purchasing rates. I find that (1) a subsequent Supreme Court decision that changed a city's handgun possession statutes failed to affect handgun sales, and (2) the 2004 expiration of the Federal Assault Weapons Ban did not affect the rate of long gun purchases on the state level.

2008), this research design investigates how *Heller* affected the rate of *flow* of gun stock in the District of Columbia, not the decision's long term effect on the *level* of gun stock. With this limitation in mind, the penultimate section explores how *Heller* may have affected rates of firearm-related violence and crime, given my finding that *Heller* caused a 1,630 percent increase in the monthly per capita rate of handgun purchasing in D.C, on average.

2 *Heller* and the Law

The Firearms Control Regulations Act of 1975 was passed by the city council of D.C. to prevent residents from owning handguns, automatic weapons, and other prohibited classifications of firearms, with exceptions for police officers or guns registered in the city before the laws' enactment. An additional prohibition under the statute related to the storage of firearms in the home so that they had to be kept unloaded, locked, or otherwise disassembled and not readily operational (Winkler, 2011; Whitman, 2023). On June 26, 2008, the Supreme Court's decision in *District of Columbia v. Heller*, 554 U.S. 570 overturned the firearms ban and trigger lock provisions, though laws requiring firearm registration and restrictions on assault weapons remained in place.

Heller also marked a major departure in past legal conceptualizations of the Second Amendment and enshrined the right to keep and bear arms – specifically handguns at that – as an individual right unconnected to militia service. Due to D.C.'s status as a federal enclave, the decision in *Heller* did not ultimately incorporate the Second Amendment under the Fourteenth Amendment to prevent overreach by the states, but it laid the groundwork for this to occur in the 2010 ruling of *McDonald v. City of Chicago* (Winkler, 2011). While firearms restrictions in other states may not have changed in response to the ruling (Goss and Lacombe, 2020), D.C.'s firearms policy was upended almost overnight. Ultimately, *Heller* struck down D.C.'s ban on private handgun ownership and offered residents the opportunity to purchase handguns far more readily than they had been able to for the past thirty-three years. How might this change have affected D.C. residents and their firearm-purchasing behavior?

3 Measuring Gun Sales Using Data on FBI Background Checks

To assess *Heller's* effect on handgun purchasing in the District of Columbia, I require an outcome measure of handgun purchasing. However, accessing data on firearms purchasing is complicated by the fact that no such data is collected anywhere in the United States. To approximate firearm purchasing data, I rely on the FBI's NICS reports – data collected from a background check system established by the 1993 Brady Act that was officially implemented as a national electronic background check system in 1998.[¶] The principal dataset underlying this paper contains the monthly count of NICS firearm check totals across a variety of transaction categories for each state from November 1998 to July 2018. The categories of these checks come directly from the NICS classifications, though I am primarily interested in handgun transactions due to the explicit focus on these firearms in the *Heller* decision.[‖] I rely on annual population estimates from the United States Census Bureau to calculate the monthly rate of handgun NICS reports per 100,000 people for every state and the District of Columbia. This study relies on publicly available administrative data and did not involve any human subject research.

Table 1 shows descriptive statistics for the primary dependent variable of interest

[¶] NICS reports the total number of background checks run through the FBI's system by Federal Firearm Licensees (FFLs) in each state in each month. FFLs can include gun store owners, pawn shop dealers, firearms manufacturers, importers, or others who are licensed by the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF). Because NICS data only reports background checks conducted or requested by FFLs, it could be seen as an undercount of firearms sales in the United States as it does not account for transactions between private individuals, familial transfers of firearms, or sales at certain gun shows (Brownstein, Nahari and Reis, 2020; Miller, Hepburn and Azrael, 2017; Steidley, 2019). In some states, a prospective buyer is required to undergo a NICS check before the transaction can proceed. If a person decides not to finalize the transaction after a waiting period or other delay following their initial NICS check, the report to NICS will still be seen in the data tabulated and published by the FBI.

[‖] NICS defines handgun transactions as pertaining to background checks run for "(a)... any firearm which has a short stock and is designed to be held and fired by the use of a single hand; and (b) any combination of parts from which a firearm described in paragraph (a) can be assembled."

across four cuts of the data: (1) D.C.'s rate of handgun NICS reports per 100,000 people for the ten years before and (2) ten years after the decision, and (3) the rate of handgun NICS reports per 100,000 people in all other states (excluding Hawaii and U.S. Territories) for the ten years before and (4) ten years after *Heller*. Importantly,

Table 1: Summary Statistics for Handgun NICS Report Rate per 100,000 people

	D.C. Only		*All Other States*	
	Pre-*Heller*	Post-*Heller*	Pre-*Heller*	Post-*Heller*
Mean	0.27	6.49	78.60	175.43
Median	0.18	6.49	76.05	166.05
SD	0.35	2.69	56.15	120.37
Maximum	2.63	13.26	400.35	981.07
N	116	121	6,265	6,534

Table 1 shows an increase in the handgun NICS report rate per 100,000 across all states, on average, in the years following the *Heller* ruling. This captures a general over-time increase in background checks across states that I account for in my later specifications to appropriately estimate the effect of the *Heller* decision on handgun sales.

There are two issues in using NICS data as a record of gun sales: (1) the conservative nature of NICS as a proxy measure, and (2) differences in administrative reporting practices that may bias any estimates reliant on NICS data as an outcome. To the first point, even if NICS does not precisely estimate all gun sales in the United States, it does more accurately measure the number of legal or official transactions, as NICS firearms checks are conducted when a person tries to legally buy a firearm from an FFL. Additionally, I also show in Appendix Fig. A.3 that the annual rate of handgun and long gun NICS reports combined is positively correlated with RAND Corporation estimates of a state's annual rate of gun ownership. This indicates that NICS does generally approximate rates of gun purchasing.

The second possible issue with using NICS as a proxy of legal gun sales – differences in administrative reporting practices among states – is addressed in greater detail in Appendix Section A.2. In this section, I rely only on states that have consistently used the standardized NICS reporting system across all years of study, and drop from my analysis any states that had observable breaks in the trends of NICS handgun reports with identifiable causes (for instance, in 2006, the ATF required FFLs in New York to report background checks in a different manner than they had previously). The results of this robustness check show that the effect of the *Heller* decision is large and significant even when states with changes in reporting methods were dropped from the analysis (See Appendix Fig. A.4). This investigation is evidence that differences in administrative procedures did not substantially bias my results, and it seems reasonable to conclude that NICS data is relatively reliable when limitations associated with any large administrative dataset are considered and accounted for.

4 Empirical Approach

I seek to causally estimate the effect of the *Heller* ruling on gun sales in D.C. and therefore turn to an interrupted time series (ITS) approach. Under this design, I compare the monthly rates of handgun NICS reports per 100,000 people in the period immediately before the Court's decision to the period immediately after the decision. I use a fixed-effects ordinary least squares estimation and fit regressions of the form:

$$\begin{aligned} y_t &= \alpha + \beta(\text{Post-}\mathit{Heller}_t) + \gamma(MonthsSince_t) \\ &\quad + \lambda(\text{Post-}\mathit{Heller}_t \times MonthsSince_t) + \varepsilon_t \end{aligned} \tag{1}$$

Under this specification, y_t is the monthly rate of handgun NICS reports per 100,000 population in D.C. in month t. $MonthsSince_t$ is used as a linear time trend to account for any general relationship between monthly rates of handgun NICS reports and time in D.C., so that a unit change in $MonthsSince_t$ represents a single month change. This approach allows for a more granular time variable than measuring time on an annual basis and is intended to capture more subtle and immediate changes in gun purchasing. Post-$Heller_t$ is an indicator variable taking a value of 1 for any month including and following July 2008. In Equation 1, I am primarily interested in β, the average difference between post-*Heller* monthly rates of handgun NICS reports and pre-*Heller* monthly rates. In other words, β is the effect of *Heller* on the rate of handgun NICS reports in D.C.

This research design explicitly focuses on the effect of *Heller* on D.C. due to the fact that the ruling most directly changed that city's policy regarding handgun possession. While D.C. is an obviously treated unit in this observational study, identifying a control unit is somewhat more challenging. Under the assumptions of this ITS specification, however, a control unit is not necessary for deriving a causal relationship: I assume that absent the decision, D.C. would have experienced the same trend in handgun NICS reports as it did prior to the decision. This assumption is not testable due to the absence of evidence from a counterfactual world unaffected by the *Heller* ruling, but β estimates the difference in post-*Heller* handgun NICS rates in the observed treated units – D.C.'s rate of handgun NICS reports post-*Heller* – and the unobservable control units – D.C.'s rate of handgun NICS reports post-*Heller* in a world where *Heller* never occurred. Later, I seek to validate the results of this analysis by utilizing observed control units that were reasonably unaffected by the decision. Due to the nature of the Supreme Court's decision, however, all states and territories were technically affected by the precedent established in *Heller*, but the *degree* to which they were affected allows for the identification of a control group.

One challenge in estimating the effect of the *Heller* decision on rates of handgun NICS reports arises in the periods long after or long before the decision, as these periods may be different from periods closer to the decision for a variety of unobservable (and some observable) reasons. These factors are more likely to be held constant closer to the decision's announcement, and so I use varying bandwidths of time to estimate the effect of the *Heller* decision on handgun sales in the two, five, and ten years before and after the Court's intervention. The specifications that span fewer years therefore introduce less bias but greater variance in the estimates, while the specifications that span a larger period introduce more bias but less variance.

5 Results

5.1 Graphical Evidence of *Heller's* Effect on Handgun Sales

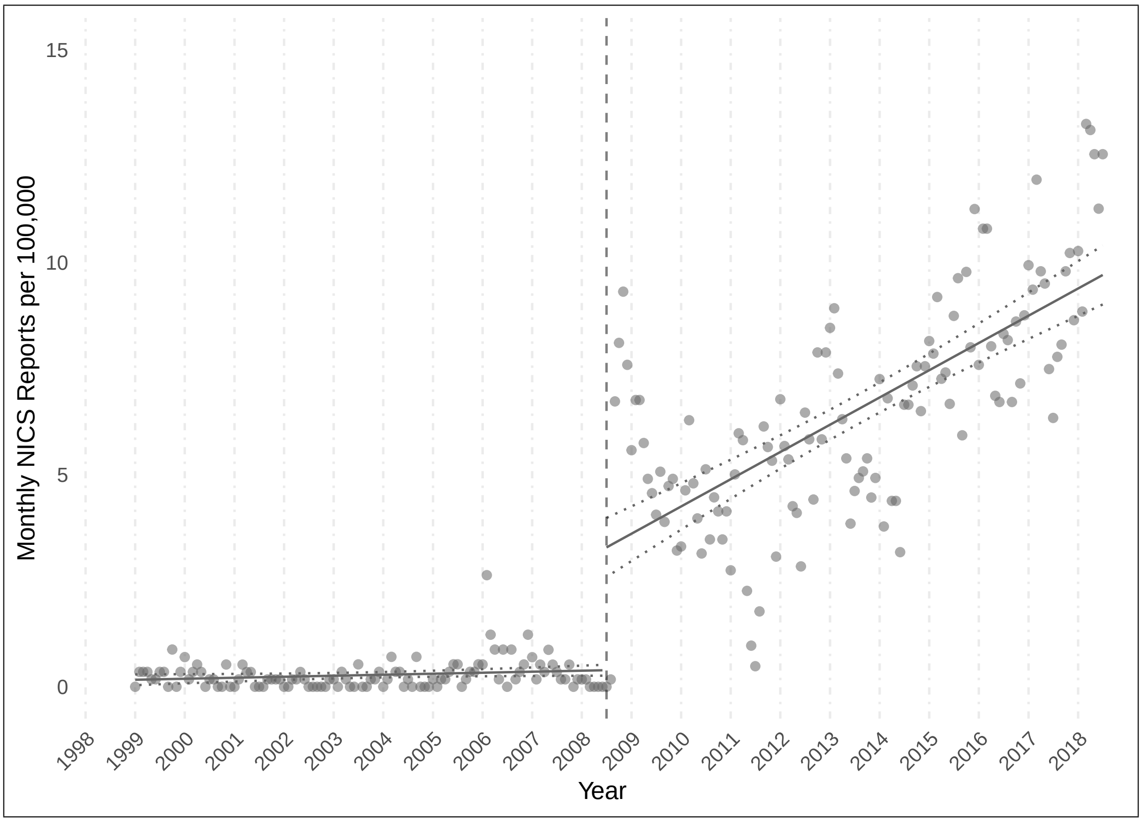

Figure 1: *Heller* Increased D.C.'s Monthly Rate of Handgun NICS Reports

Fig. 1 presents graphical evidence of *Heller's* effect on handgun sales in D.C. only. On the horizontal axis, I plot the month and year associated with each observation, from November 1998 to July 2018. The vertical axis shows the monthly rate of handgun NICS reports per 100,000 population, with a discontinuity in July 2008 to mark the beginning of the post-*Heller* period. Linear smoothers are fit to the underlying data on either side of this cutoff to represent general trends in handgun purchasing rates over time.

Fig. 1 clearly shows a marked increase in the monthly per capita rate of handgun

NICS reports in D.C. following *Heller*. Before the decision, D.C.'s rate of handgun NICS reports was consistently close to zero, with many months having exactly zero background checks performed. After the decision, the average rate in D.C. appears to have increased substantially, which is potentially a meaningfully large effect that merits further investigation.

5.2 Formal Estimates of *Heller's* Effect on Handgun Sales

Table 2 presents the formal estimates of the ITS regression specification outlined in Equation 1, for the years before and after the Court's decision. Column 1 reports the effect of *Heller* on D.C.'s monthly rate of handgun NICS reports in the two years after the decision. I find that *Heller* led to an increase in handgun sales immediately following the ruling by 5.38 handgun NICS reports per 100,000 residents per month, on average. This constitutes a considerably large effect: the average monthly rate of handgun background checks per 100,000 residents was 0.33 in the two years before the Court's ruling, and this finding indicates that the *Heller* decision caused a 1,630 percent increase in the monthly rate of handgun background checks in D.C. in the two years following the decision, on average. Columns 2 and 3 widen the bandwidths to five and ten years, respectively, and find similarly large effects: an increase of 4.02 checks per 100,000 people from an average of 0.36 in the five pre-*Heller* years amounts to a roughly 1,117 percent increase in the monthly rate of handgun purchases over the five post-*Heller* years, and an increase of 2.89 checks per 100,000 people from an average of 0.27 in the 10 prior years amounts to a roughly 1,070 percent increase in the monthly rate of handgun purchases over the ten years following the ruling, on average.

These results suggest that *Heller's* effect on handgun sales was considerably large and robust across varying specifications of the model in Equation 1. These findings also comport with the graphical evidence observed in Fig. 1 – β is large and positive, meaning that *Heller* led to a substantively large increase in the level of the rate of monthly handgun background checks in D.C.

Table 2: ITS Estimates of *Heller's* Effect on Washington, D.C.'s Rate of Handgun NICS Records per 100,000 Residents

	Handgun NICS Report Rate		
	(1)	(2)	(3)
Post-*Heller*	5.38	4.02	2.89
	(1.36)	(0.65)	(0.44)
No. Observations	49	121	237
Mean Rate Pre-*Heller*	0.33	0.36	0.27
Sample Period	07/2006 - 07/2010	07/2003 - 07/2013	11/1998 - 07/2018

Note: All models are estimated using ordinary least squares. Heteroskedasticity-robust standard errors are reported. For the 10-year bandwidth model, NICS only began reporting in November 1998, hence the pre-*Heller* period starting after July 1998 and not accounting for a full ten years prior to the decision.

5.3 *Heller's* Effect Not Larger in States with the Most Permissive Gun Policy

The ITS design used in the previous sections does not include observed control units due to assumptions underlying the methodological approach. In this section, I assume that absent the *Heller* decision, D.C. would have experienced the same trend in handgun NICS reports as the states I identify as control units. To identify a causal effect of *Heller* on handgun NICS reports under this design, I assume that the post-treatment change in the control units provides an estimate of the change for the treated units in a counterfactual world. In other words, states that were less directly affected by *Heller*, for reasons that will be discussed below, would have experienced the same trend in handgun NICS reports as D.C. in the absence of treatment.

Ostensibly, one would not reasonably expect the *Heller* decision to have an observable effect in states with laws that already made handguns readily available for private ownership. In states with the most permissive handgun possession laws, residents would have already been able to purchase and possess handguns with very limited restrictions, so *Heller's* expansion of the right to possess handguns likely would not affect substantially the rates of handgun NICS reports within these states to the level that we ultimately observe in D.C. Therefore, these permissive states serve as an appropriate control group for an analysis seeking to validate the results of my ITS design.

I identify control units by turning to state firearm law rankings generously provided by the Giffords Law Center (GLC).[**] These rankings assess the permissiveness and restrictiveness of a state's firearm laws in each year since 2010. I focus specifically on the states within the first quartile of the 2010 GLC scores, because these were the states with the most permissive gun laws in the country at the time. To estimate the permissiveness of a state's gun policy in 2008, I utilize data from the RAND Corporation's Gun Policy in America state policy database to confirm if the most permissive states (according to the GLC) changed their firearms policies between 2008 and 2010 in any way that would affect the 2010 GLC scores. I do not find any instances wherein a state that was ranked as extremely permissive in 2010 had any policy changes that would have led to a major change in the state's respective ranking for 2008 or 2009.

An additional assumption underlying this approach is that before the ruling, D.C. and the most permissive states would have had similar trajectories in their respective rates of handgun background checks, which I evaluate in Figure 2. Here, I find no evidence of a substantial *Heller* effect in the most permissive states, as expected. Even though there appears to be a small discontinuity in the plot, this increase is largely due to the general linear increase in background checks over time, and further captures a mechanical feature of NICS data: background checks are seasonally at their highest in the winter months. In Appendix Table A.2, I report the formal estimates of a difference-in-discontinuity specification following the work of Eubank and Fresh (Eubank and Fresh, 2022) and find that *Heller's* effect on D.C.'s rate of handgun NICS reports is large and robust even when compared to relevant control units.[††]

[**]This confidential, internal data was conditionally provided in response to a request on the part of the author. Due to the sensitive nature of this data, it can not be made available in replication materials provided by the author but can be specially requested from the GLC.

[††]I further account for variations in administrative reporting procedures by examining states

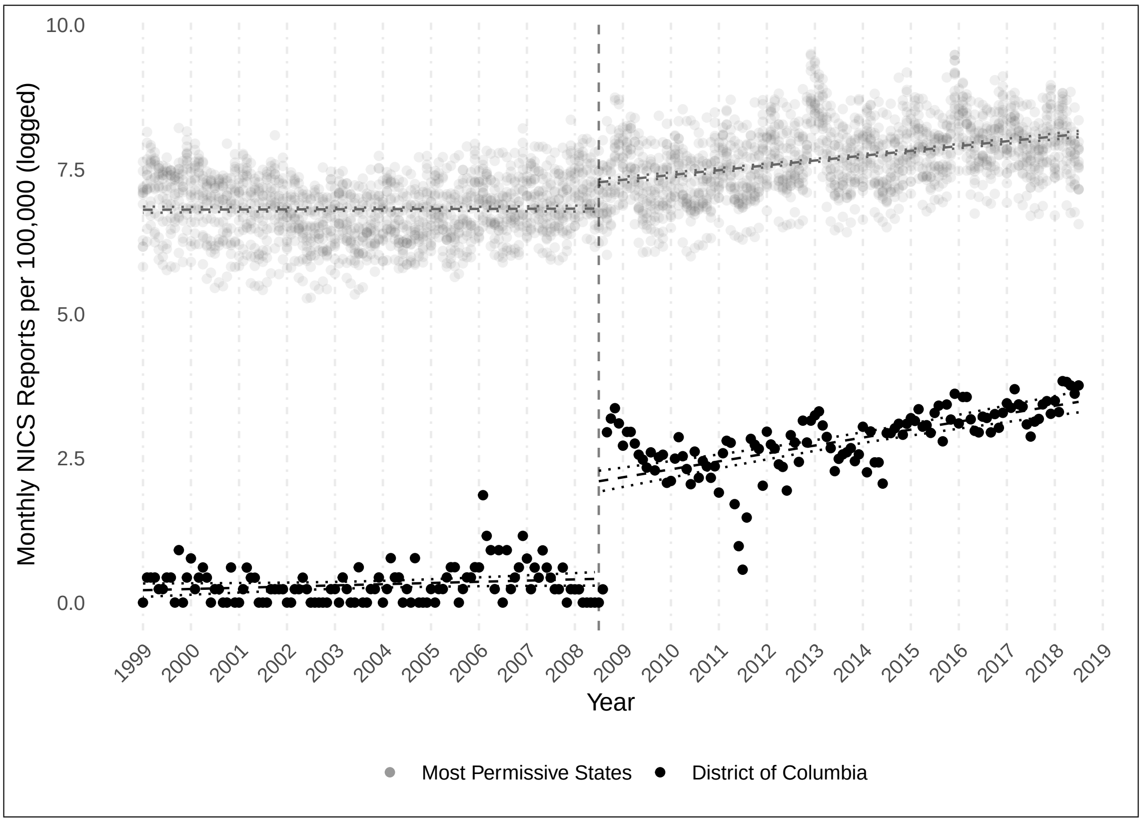

Figure 2: *Heller's* Large Effect on the Monthly Rate of Handgun NICS Reports in D.C. Compared to States with the Most Permissive Firearm Laws

Note: The vertical axis shows the monthly rate of handgun NICS reports per 100,000 population per state (y_{it}) transformed to a $log_2(y_{it} + 1)$ scale. Adding the constant of 1 to the pre-transformed rate ensures the inclusion of monthly observations wherein zero background checks were reported. The log_2 transformation accounts for heteroscedasticity in the rate across all states, and allows for the interpretation that a one-unit increase on the log scale represents a doubling in the underlying data.

5.4 Estimated Effect Not Driven by Substitution of Purchasing in Surrounding States

The question motivating this validation exercise is one of substitution: were residents of D.C. purchasing firearms in surrounding states before the Court's opinion in *Heller*, and did the ruling lead to reductions in the rates of firearms being purchased in these states after the decision? In Appendix Section A.4., I compare handgun background check rates in Maryland and Virginia to D.C., and I find that *Heller*

that may have experienced changes in reporting standards or practices, but I find that none of the states that undergo observable changes in administrative procedures are included in the list of the most permissive states for gun policy. These states are California, Colorado, Connecticut, Florida, Hawaii, Illinois, Maryland, Nevada, Nebraska, New Hampshire, New Jersey, New York, Oregon, Pennsylvania, Tennessee, Utah, Virginia, Washington, and Wisconsin.

did not affect handgun purchasing in these states.

A caveat should be made here – handgun NICS records in Maryland and Virginia would not capture purchases by an out-of-state (D.C.) resident as these types of sales were banned in Maryland and Virginia both before and following the *Heller* decision. In other words, D.C. residents could not purchase guns in Maryland and Virginia legally, and these transactions would not show up in NICS data. Additionally, since NICS data only captures sales at FFLs, data on illegal gun purchasing does not exist in a form that allows me to directly answer whether *Heller* affected illegal gun purchasing. Instead, in Appendix Section A.4., I explore administrative data (discussed in greater detail in Appendix Section A.7.) that suggests *Heller* may have affected the number of firearms being brought into the District from Maryland and Virginia and later used in crime.

5.5 *Heller's* Effect Not Larger for Long Gun Background Checks

Heller would reasonably have a larger effect on handgun sales than long gun sales considering the Court's ruling explicitly struck down handgun restrictions and left assault weapons regulations in effect. But did the ruling affect rates of long gun background checks? I evaluate this possibility in Appendix Section A.5., and find that *Heller's* effect on long gun background checks in D.C. is not greater than the effect observed for handgun NICS reports.

6 How Might *Heller* have Affected Gun Violence and Crime?

There is a broad consensus among academics that increased availability of firearms within a community leads to increased risks of firearm violence, and scholars point to the *Heller* ruling as not only a Court decision that might increase the availability of firearms but could also lead to higher rates of gun violence (Miller and Hemenway, 2008). Other work finds that in D.C. specifically, more restrictive firearms policy regimes, like the 1975 law that Heller struck down, are associated with substantial reductions in the rate of firearm suicides (Loftin et al., 1991). Considering past work that estimates that for each percentage point increase in gun ownership rates, there is a 0.9 percent increase in the firearm homicide rate of a state (Siegel, Ross and King III, 2013), and states with more permissive firearms policies see an average of 4.62 more deaths per 100,000 people than states with more restrictive regulatory regimes (Glasser et al., 2023), the decision by the Court to dismantle key restrictive provisions of D.C.'s firearm policy might reasonably be expected to affect the rates of gun-related violence and death in the District under these theories.

The results of my analysis indicate that D.C. experienced a dramatic increase in handgun purchasing following the *Heller* ruling, so how might the decision have affected rates of gun violence? Under the theories of the aforementioned scholarship, gun violence increases as the gun stock in a community increases. However, a change in the flow of firearms into D.C. does not necessarily mean that firearm violence would also be affected by the *Heller* decision, as large-scale changes in the flow of firearms might only marginally affect the existing firearm stock. Unfortunately, there is no way to know how many firearms were owned in D.C. before *Heller*, largely due to presumably high rates of illegal gun ownership in the District both before and after the decision. So, the effect of *Heller* on the rate of flow of firearms into D.C. does not help us to estimate the effects of the decision on gun violence. My identification strategy assumes that gun purchasing would be immediately responsive to changes in legal regimes regarding the Second Amendment, but regardless of how many guns are bought, the stock of firearms in D.C. may have changed much more gradually compared to the change in the flow of firearms that my results capture. Furthermore, as my results seek to identify the immediate effects of the Court's decision, my analytical approach would not be appropriate for assessing how the introduction of these newly purchased firearms affected rates of violence in D.C. in the long run. My research design would therefore only work under the assumption that legally purchased guns are then immediately used to perpetuate firearm violence.[‡‡] Put simply, my methodological approach is not equipped for estimating the effects of the *Heller* decision on gun violence.

With these limitations regarding the generalizability of the results presented in this paper in mind, I can apply my findings on the effects of the *Heller* decision on gun purchasing to informally estimate the number of guns that *Heller* introduced into D.C.'s firearms stock. In Appendix Fig. A.8, I use SARIMA forecasting to estimate that approximately 4,892 more guns were bought in D.C. in the ten years following *Heller* than there would have been in the absence of the decision. However, not every background check amounts to a transaction (or the purchase of *only* one handgun), so this estimate could be positively or negatively biased. Even though there are no reliable estimates for rates of gun ownership in D.C. over time that might proxy the overall stock of firearms, this observed increase in the rate of flow is substantively large. In summation, one might expect to see an increase in the rate of firearm-related deaths as a result of the ruling, considering the decision weakened D.C.'s firearms-related restrictions (Glasser et al., 2023; Loftin et al., 1991; Miller and Hemenway, 2008). However, this long-term effect would not be captured by applying the models I previously constructed to an outcome variable capturing gun violence generally, and future work seeking to estimate the effects of the *Heller* ruling on rates of gun violence would have to account for the myriad of other factors that may affect firearm-related mortality (Fiebig, 2010; Okoro et al., 2005; Loftin et al., 1991; Fontaine, Markman and Nadeau, 2010).

7 Discussion

Prior research suggests that the *Heller* ruling had little effect on gun politics due to limited evidence of policy or political attitude change across the states in the post-

[‡‡]I test this assumption in Appendix Section A.7., using a novel, administrative dataset of firearms recovered in law enforcement operations in the District of Columbia (as reported by the Bureau of Alcohol, Tobacco, Firearms and Explosives), but limited conclusions can be drawn considering issues with the measurement of this data.

ruling period (Goss and Lacombe, 2020; Henigan, 2009). In this paper, I argue that these previously examined downstream measures do not fully capture the impact of the Court's ruling, and instead provide strong evidence that *Heller* led to large first-order effects on the measure that would reasonably be the most affected by the decision: handgun purchasing. This paper demonstrates that the *Heller* decision notably influenced gun owners' behavior due to the Court's explicit expansion of the individual right to keep and bear arms and striking of D.C.'s handgun ban, which suggests that increases in gun ownership allowed by *Heller* may have led to eventual downstream effects on gun politics in D.C., and beyond. I show that when an outcome measure that is precisely and theoretically tied to the treatment of interest is utilized, clear and strong effects are causally identifiable and further serve as evidence of the Court's ruling having a much larger impact on gun politics than has been previously considered.

In summation, the findings from this paper imply that *Heller* produced particularly strong first-order effects on politics as measured through changes in social and economic behavior (in this case, handgun ownership in D.C.) (Rosenberg, 2008; Mettler and Sorelle, 2018; Pierson, 1993). The evidence for second-order effects is perhaps more limited – I do not test specifically for changes in political dynamics surrounding the Second Amendment, as past work does, due primarily to issues with the measurement of these outcomes. I further argue that my research design, while appropriate for estimating the direct effects of *Heller*, is constrained in what it can tell us about *Heller's* downstream effects on firearm mortality or crime, for instance. Instead, by focusing on the direct impacts of *Heller*, I show that handgun purchasing patterns changed substantially in the months following the ruling, with the monthly rate of purchasing increasing by as much as 1,630 percent, on average, according to some estimations. In short, experienced policy in D.C. changed significantly as a direct response to the Court's decision.

Of course, these findings could be seen as merely proving the obvious: there was an effective ban on handgun sales in D.C. until, after the ruling, there was not.

Similarly, water typically struggles to flow freely through a dam until the floodgates are opened. An increase in handgun purchasing in D.C. is perhaps only logical, yet the sheer scale of the effect that I show in this paper speaks to the extent to which *Heller* affected gun ownership, and gun politics by extension. The floodgates opened, and the water burst through. Assessing *Heller's* impact through other measures – such as changes in policy in the states or public opinion – understates the significance of the decision, and these findings demonstrate that the effects of the ruling are considerably greater than past scholarship might lead one to believe. Water did not merely trickle out of the dam. This paper's contribution is not to show that firearm sales increase when buying a firearm becomes legal, but rather that through an appropriately specified and rigorous methodological design, the direct, causal effects of a landmark Supreme Court ruling are readily identifiable.

This case-study of *Heller* could also be seen as a demonstration of public policy *working*. The D.C. Act sought to reduce gun violence by limiting gun ownership, and legal gun purchasing through Federal Firearms Licensees was remarkably low in D.C. before *Heller*. After the decision, gun purchasing increased when the provisions of the law effectively banning handgun ownership were struck down. Illegal handgun ownership aside, it appears that aspects of D.C.'s law achieved the intended effect – reducing gun ownership – at least until the Supreme Court weighed in.

As Second Amendment litigation plays out and legal doctrines further develop, the questions of if and how court decisions affect behavior remain salient. Considering the high level of activity by the Supreme Court on this issue, and the major changes in interpretations of the Second Amendment that follow these rulings, this paper indicates that future decisions that severely alter the conventional readings of the Constitutional right to keep and bear arms may continue to have a lasting impact on gun politics and firearm ownership in the United States.

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Data Sharing Plans/Data Availability

All data utilized in this paper, excluding the confidential data shared by the Giffords Law Center (GLC), was collected from publicly available administrative datasets. All data and code necessary to replicate these findings will be made publicly available online in a data repository. The data from the GLC must be specially requested from the organization and cannot be shared by the author.

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Appendix

A.1. Have Other Governmental Interventions Produced Effects Similar to Those Observed Post-*Heller*?

The short answer is no. But in order to explore this question in greater detail, I sought to identify instances wherein governmental action (or, in one case, inaction) might have reasonably affected firearm purchasing in a similar manner to what is observed in the *Heller* case study.

A.1.1 A *McDonald* Effect?

A great deal of scholarship focuses on how *Heller* served to open the doors for subsequent Second Amendment litigation to challenge firearms restrictions (Goss and Lacombe, 2020; Henigan, 2009; Winkler, 2011; Charles, 2023). One such case was that of *McDonald v. City of Chicago*, 561 U.S. 742 (2010). While not as groundbreaking and precedent reversing as *Heller*, in *McDonald*, the Supreme Court ruled that the individual right to keep and bear arms is not only protected by the Second Amendment, but incorporated by the Fourteenth Amendment and enforceable against states. The Court reversed a Seventh Circuit ruling that had upheld a Chicago ordinance banning the possession of handguns (among other restrictions on firearm ownership), striking down a city ordinance in much the same way as it had in *Heller*. Therefore, it seems reasonable to expect that the *McDonald* decision may have an effect on handgun purchasing in Chicago that is comparable to what is observed in D.C.

Yet, analyzing the effect of *McDonald* on handgun purchasing in Chicago is complicated by the fact that NICS data is only reported on the state level. The only reason I am able to analyze a city-level effect of the *Heller* decision is because of D.C.'s unique status as a federal district – which NICS treats as a state or territory equivalent. So, I can only examine *McDonald's* effect on handgun purchasing in *Illinois* – which is certainly an imprecise unit of analysis, but, unfortunately, the only option available.

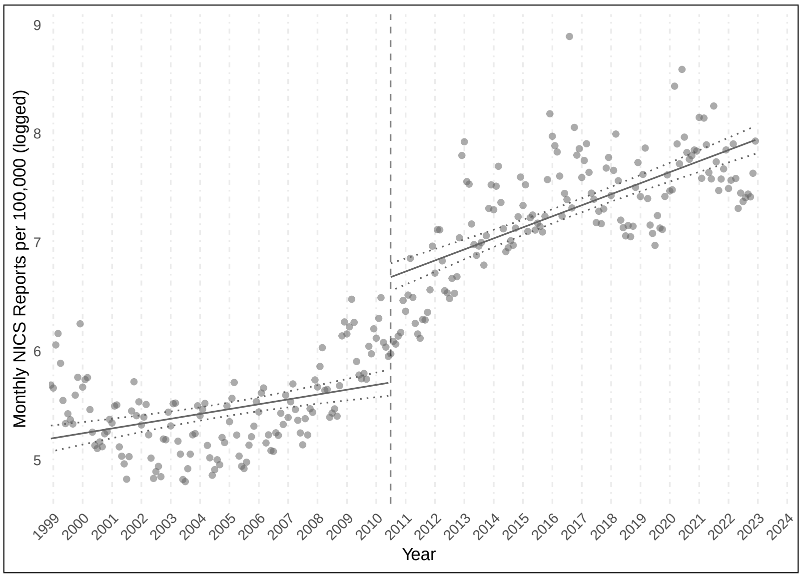


Figure A.1: *McDonald's* Effect on Illinois' Monthly Rate of Handgun NICS Reports (transformed to a $log_2(y+1)$ scale)

Figure A.1 shows no evidence of any effect from the *McDonald* decision on handgun purchasing in Illinois. This is perhaps unsurprising, as the expectation that a Supreme Court decision striking down a regulatory regime confined to a single city would thus affect gun purchasing across the entire state seems somewhat unreasonable. Perhaps the lack of a *McDonald* effect is evidence of *Heller's* unique importance. Or, perhaps the nature of *McDonald* as a case that effectively clarified some confusion stemming from the *Heller* opinion's text – and not upending longstanding precedent as *Heller* did – effectively tempered a response among gun buyers in Illinois to a level that is not comparable to what was observed post-*Heller*. Future work ought to consider how other types of court cases affect gun purchasing behavior – from lower court decisions to more contemporary Supreme Court decisions that I cannot study due to the limited post-treatment period data (such as the Court's decision in *Bruen*) (Charles, 2023). Perhaps the effect observed post-*Heller* is unique due to circumstances that my analysis has not uncovered, but I leave that question to future work.

A.1.2 The Federal Assault Weapons Ban

What about governmental interventions that might affect purchases of other types of firearms? To answer this, I turn to an analysis of patterns of long gun purchasing following the expiration of the Public Safety and Recreational Firearms Use Protection Act of 1994 – more commonly known as the Federal Assault Weapons Ban.

Unlike *Heller* and *McDonald*, the expiration of the Assault Weapons Ban (AWB) was not the result of a Supreme Court Decision. Instead, the AWB had been passed with a sunset provision which set an expiration date of September 13, 2004. While there were attempts by Congress to renew the AWB, none succeeded and the prohibitions on the manufacture of certain semi-automatic firearms for civilian use were allowed to lapse.

Analyzing the effect of the expiration of the AWB is complicated by the fact that assault weapons (at least as defined by the AWB itself) are not a separate category of firearms reported by NICS. Instead, I rely on the NICS reports for long guns – a category of firearms defined by NICS as: "...a weapon ... intended to be fired from the shoulder, and ... use the energy of the explosive in (a) a fixed metallic cartridge to fire a single projectile through a rifled bore for each single pull of the trigger; or (b) a fixed shotgun shell to fire through a smooth bore either a number of ball shot or a single projectile for each single pull of the trigger." Notably, the long gun classification in NICS includes shotguns – an expansive category of firearms that were not prohibited under the AWB. Hence, the monthly long gun NICS reports are an overcount and thus imprecise measure of assault weapons purchasing, but it is the most appropriate proxy measure currently available to researchers.

This analysis is also complicated by the difficulty of determining which states were reasonably "treated" by the AWB's expiration. In order to identify treatment states, I utilized the RAND state firearm policy database to determine which states had passed restrictions on assault weapons in conjunction with the federal policy. My reasoning here is straightforward: these states had separate assault weapons ban provisions prior to and following the AWB's expiration, and therefore would presumably be less directly treated by the 2004 sunset of the AWB, as the state policy would still be in effect. These states include: California (ban implemented 1990), Connecticut (ban implemented 1993), Massachusetts (ban implemented 1998), New Jersey (ban implemented 1990), and New York (ban implemented 2000). I do not consider Maryland as treated, as even though Maryland has an assault weapons ban in place, the ban was implemented in 2013, and therefore the state would have been treated by the 2004 AWB expiration.

As with my analysis in the main text, I employ a quasi-experimental interrupted time series approach and report the descriptive findings in Figure A.2.

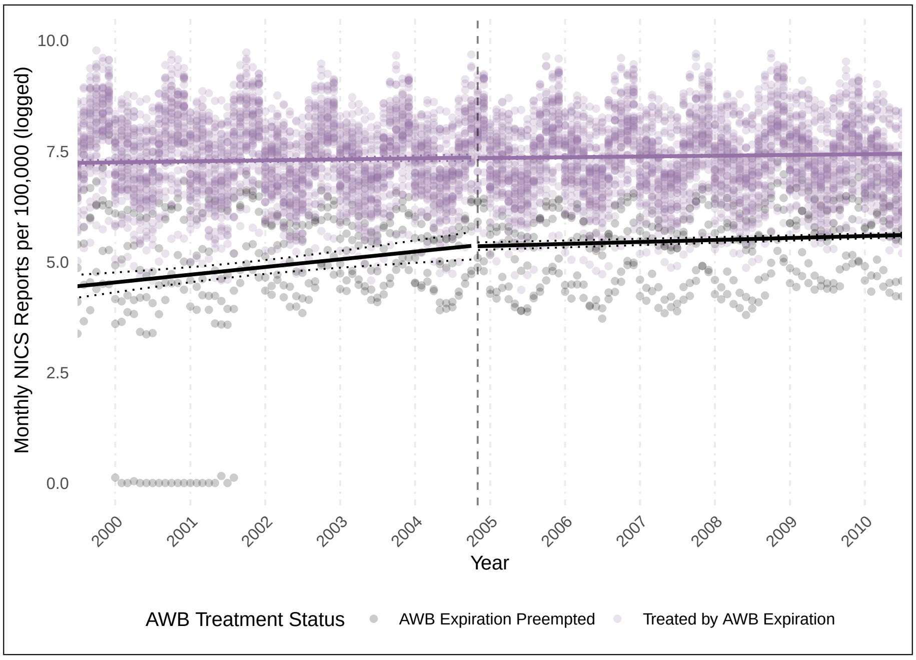


Figure A.2: Effect of the Assault Weapons Ban Expiration on Monthly Long Gun NICS Reports per State and 100,000 (transformed to a $log_2(y + 1)$ scale)

It is not immediately clear from Figure A.2 that the AWB expiration had any effect on long gun purchasing in states where the expiration was not preempted. To formally estimate the effect of the AWB's expiration on long gun purchasing in treated states, I employ a linear-in-time difference-in-discontinuity model similar to

Equation 3 and estimate

$$
\begin{aligned}
y_{it} = &\alpha_i + \tau MonthsSince_t + \eta \text{Post-AWB}_{it} + \beta(\text{Treat}_{it} \times \text{Post-AWB}_{it}) \\
&+ \theta(\text{Treat}_{it} \times MonthsSince_t) + v(\text{Post-AWB}_{it} \times MonthsSince_t) \\
&+ \omega(\text{Post-AWB}_{it} \times \text{Treat}_{it} \times MonthsSince_t) + \psi X_{it} + \varepsilon_{it}
\end{aligned} \tag{2}
$$

where y_{it} is the main outcome of interest: the rate of long gun NICS reports per 100,000 people. Due to the high variance in this measure, I transform y_{it} onto a $log_2(y_{it} + 1)$ scale for interpretability reasons, and add a constant of 1 to y_{it} to allow for the inclusion of observations for months in which no long guns were purchased. The justification for this transformation (and the best manner in which the estimates can be interpreted) will be discussed in additional detail in Appendix Section A.3. I include state-level fixed effects, α_i, to account for observable and non-observable time-invariant differences between treatment and control states. The variable Post-AWB$_i$ is an indicator for months after the AWB's expiration (e.g., any month including and following September 2004). Treat$_i$ is an indicator for whether the unit of analysis is a state reasonably treated by the AWB's expiration, or a state where the AWB's expiration was preempted. $MonthsSince_t$ is an annual linear time trend (which I set to $MonthsSince_t = 0$ in September 2004) that effectively captures any general relationship between monthly rates of handgun NICS reports and time. I account for the effects of a state's population (X_{it}) and cluster standard errors by state. The results from this analysis are presented in Table A.1.

As the results in Table A.1 show, there is no statistically significant effect of the AWB expiration on long gun NICS reports in presumptively treated states. This finding certainly comports with the descriptive finding in Figure A.2, and taken together, these results indicate that the expiration of the AWB had little meaningful effect on long gun purchasing in treated versus preempted states.

Along with the findings from the *McDonald* analysis, the findings from the AWB analysis indicate that the effect observed in the main text is unique to the *Heller* decision. Why *Heller* seems to be unique in producing effects on gun purchasing

Table A.1: Difference-in-Discontinuity Estimates of the Effect of the AWB Expiration on Treated v. Preempted States

	Long Gun NICS Report Rate	
	(1)	(2)
Post-AWB * Treat	−0.02	0.25
	(0.07)	(0.10)
Post-AWB * Treat * MonthsSince	−0.00	0.01
	(0.00)	(0.01)
Treat * MonthsSince	0.01	−0.01
	(0.01)	(0.01)
Post-AWB	0.14	−0.35
	(0.06)	(0.10)
MonthsSince * Post-AWB	0.01	−0.01
	(0.00)	(0.01)
MonthsSince	−0.01	0.02
	(0.00)	(0.01)
No. Observations	2,401	9,457
Sample Period	11/2002 - 11/2006	11/1998 - 11/2014
Adjusted R^2	0.77	0.66

Note: All models are estimated using ordinary least squares. Errors are clustered by state with state fixed effects included in the estimation. All regressions include a control for the estimated annual population of a state. Hawaii was dropped from the analysis due to differences in data reporting through NICS. MonthsSince is a linear time trend (set to 0 in 11/2004) that captures any general relationship between time and NICS long gun reports. For the 10-year bandwidth model, NICS only began reporting in November 1998, hence the pre-AWB period starting after July 1998 and not accounting for a full ten years prior to the decision. The outcome variable is transformed to a $log_2(y_{it}+1)$ *scale for interpretability reasons, where* y_{it} *is the monthly rate of long gun NICS reports per 100,000 people in state* i *and year* t.

when compared to other interventions in gun policy deserves more investigation.

A.2. Validating NICS as an Administrative Dataset

NICS has been repeatedly used by academics as a proxy measure for gun sales, but always with the same caveat mentioned by the researchers: NICS is a conservative estimate of firearm sales. Considering the prevalence of this qualification in the literature, I seek to establish in this section that NICS is, in fact, a useful source of administrative data that, when handled appropriately, can be reasonably used as a proxy for handgun purchasing in the United States.

A.2.1. NICS as a proxy of Gun Ownership

NICS data is by no means meant to represent or even approximate the full stock of firearms in private ownership in the United States. However, a useful exercise in validating NICS as a proxy of gun purchasing would be to estimate the correlation between a state's annual rate of NICS reports and a state's estimated rate of gun ownership. Considering the fact that no official measure of gun ownership exists in the United States, I turn to the RAND Corporation's State-Level Estimates of Household Firearm Ownership and correlate this estimated annual gun ownership rate with each state's annual rate of all handgun and long gun background checks per 100,000 residents. The results of this correlation can be seen in Fig. A.3, which shows that NICS handgun and long gun report rates positively correlate with RAND estimates of state gun ownership rates. Though this relationship is positive, it is not a perfect correlation, which speaks to the imprecision present in this measure. However, the general positive correlation does show that NICS is useful as a proxy – albeit a conservative and imperfect one – of gun purchasing in the United States.

A.2.2. NICS and Changes in Administrative Reporting Procedures

While few would argue that the true volume of all firearm sales would *not* be correlated with the observed volume of NICS reports, there are discrepancies in reporting procedures among states. These discrepancies are identified by NICS from the outset: 31 states, five U.S. territories, and the District of Columbia require FFLs to

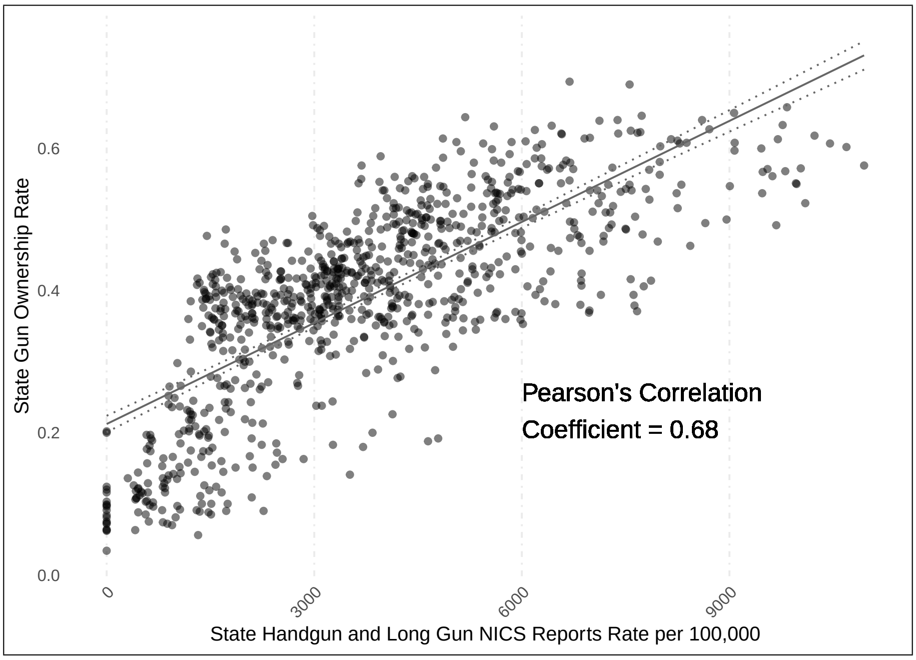


Figure A.3: NICS Report Rates Positively Correlate with Estimates of State Gun Ownership Rates

utilize the services of the NICS Section of the FBI for all firearm background checks. Of the remaining 19 states: Nebraska requires FFLs to contact the FBI for long gun background checks, but a state-issued handgun permit is the only identity verification required for handgun purchases; Washington, Wisconsin, New Hampshire and Maryland require FFLs to contact state agencies for handgun background checks, and the FBI for long gun background checks; and 14 states require FFLs to contact state agencies for all firearm background checks.* In instances where states require FFLs to contact state agencies, these agencies report their respective background check totals to NICS, which then tabulates and reports these totals in the data made publicly available.

Notably, 26 states have state specific background check policies that vary individually and sometimes allow for other types of previously state-issued firearms permits to stand in for an otherwise required background check. This obviously might affect the interpretation of NICS data, as FFL sales to individuals who already have a qualifying permit would not have their sale affiliated with a background check re-

*These states include: California, Hawaii, Oregon, Nevada, Utah, Colorado, Illinois, Tennessee, Florida, Virginia, Pennsylvania, New York, Connecticut, and New Jersey.

port in NICS. For an exhaustive overview of how state-level variations in firearms policy might affect any research relying on NICS data, I would highly recommend all researchers to consult the RAND Corporation's 2022 report, "Using National Instant Criminal Background Check Data for Gun Policy Analysis: A Discussion of Available Data and Their Limitations" by Smucker and colleagues.

I utilized plots of NICS handgun data for each of the states, territories, and federal districts that had observable interruptions in patterns of reporting to identify cases where changes in administrative policies may have affected the rate of NICS reports in a given locality. After identifying these states, I cross-referenced the RAND state gun policy database to validate whether these states had policy changes that may have been responsible for the interruptions that I observed, and ATF records to determine if administrative procedures had been adjusted in some manner. The following list indicates the states that I identified as having an observable interruption, and the identified reason for the change:

- Wisconsin has interruption in 2012, likely the result of the passage of a Castle Doctrine law that same year.
- Pennsylvania has interruption starting in mid-2011 and larger jump in 2014, and in 8/2014, PA passed a Castle Doctrine law that reasonably could have increased gun purchasing.
- North Carolina's NICS reporting rate drops in 2002, and bumps in 2012 – likely related to *Bateman v. Perdue*, 881 F. Supp. 2d 709 (E.D.N.C. 2012).
- Nebraska jumps in 2010, likely due to a state law overturning and preempting a large number of local regulations on firearms purchasing.
- Missouri repealed a restrictive policy in 2007 that may be responsible for an observed interruption.
- Maryland experienced an interruption in 2005 due to undetermined reasons.
- Michigan experienced an interruption in 2005 due to undetermined reasons. Perhaps a change in the shall-issue concealed carry permitting regime?
- Indiana experienced an interruption in 2005 due to undetermined reasons.
- Connecticut has an interruption in the early 2000s due to a temporary change in how background checks were reported to NICS.
- New York has an interruption in 2006 due to an ATF requirement to change reporting procedures.

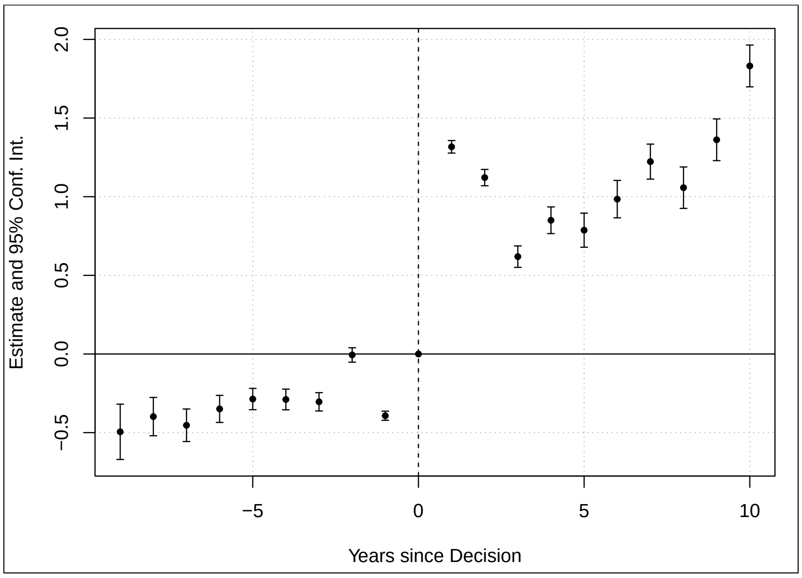


Figure A.4: Effect of Time from *Heller's* Decision on the Monthly Rate of Handgun NICS Reports (transformed to a $log_2(y+1)$ scale) in D.C. Compared to States with Consistent NICS Reporting Methods.

Note: As observations in 1998 were limited to only November and December, which are conveniently the months that typically see the highest levels of firearms sales due to seasonal discounts and holiday promotions, I drop observations from this year and only observe annual effects from 1999 to 2018. Coefficients are from ordinary least squares regression with state and year-fixed effects. States other than D.C. are included and they only contribute to the estimation of the year fixed effects. N = 6,720 state-years.

With this list of states in mind, I exclude them from my analysis in order to produce Figure A.4, and still find a large effect of the *Heller* decision on D.C.'s rate of handgun NICS reports. With the removal of outlier states that may have experienced possible changes in administrative procedures that could have reasonably biased the results of other analyses reported in the main text, this approach validates the robustness of the results previously reported in the main text.

A.3. *Heller's* Effect Compared to the Most Permissive States and All States

In Section 5.3 of the main text, I show graphical evidence that *Heller's* effect on handgun sales was considerably larger in D.C. than it was in the states with the most permissive firearms laws at the time of the decision. To formally estimate the effect of *Heller* in D.C. versus other states that I specify as control units, I estimate difference-in-discontinuity regressions of the form:

$$\begin{aligned} y_{it} = &\alpha_i + \tau MonthsSince_t + \eta \text{Post-}Heller_t + \beta(\text{D.C.}_i \times \text{Post-}Heller_t) \\ &+ \theta(\text{D.C.}_i \times MonthsSince_t) + \upsilon(\text{Post-}Heller_t \times MonthsSince_t) \\ &+ \omega(\text{Post-}Heller_t \times \text{D.C.}_i \times MonthsSince_t) + \psi X_{it} + \varepsilon_{it} \end{aligned} \tag{3}$$

where y_{it} is the main outcome of interest: a state's rate of handgun NICS reports per 100,000 residents. Due to the high variance in this measure, I transform y_{it} onto a $log_2(y_{it}+1)$ scale for interpretability reasons, and add a constant of 1 to y_{it} to allow for the inclusion of observations for months in which no handguns were purchased. I include state-level fixed effects, α_i, to account for observable and non-observable time-invariant differences between states and D.C. The variable $Post - Heller_t$ is an indicator for months after the *Heller* decision (e.g., any month including and following July 2008). D.C.$_i$ is an indicator for whether the unit of analysis is D.C. or *either* all states or permissive states. $MonthsSince_t$ is a linear time trend (which I set to $T = 0$ in July 2008) that effectively captures any general relationship between monthly rates of handgun NICS reports and time. I account for the effects of a state's population (X_{it}) and cluster standard errors by state.

Due to my expectation that D.C. will experience an immediate level shift as well as a generally increasing rate of background checks, I am primarily interested in β and ω in Equation 3. β measures the differential shift in the *level* of the rate of monthly NICS handgun reports (the immediate jump seen in past Figures 1 and 2), and ω measures the average change in the difference in the *linear trend* of monthly NICS handgun reports after the *Heller* decision (the slope of the line in the respective figures). Under my expectations, I assume that β and ω will be positive and that β will be larger than ω due to the shock of the treatment. Adding these coefficients together allows for the computation of the entire causal effect of the *Heller* decision on NICS handgun records, accounting for linear-in-time changes.

Table A.2 presents the formal estimates of the effect of the *Heller* decision on the monthly rates of handgun NICS reports, using the ITS design from Equation 1 and the linear-in-time difference-in-discontinuity (DiD) estimation from Equation 3 with control units that were discussed previously. Columns 1 and 2 utilize Equation 1 to estimate the effect of *Heller* on only D.C. in the two years and ten years prior to and following the decision, respectively (these results are the full results of what is otherwise seen in Table 2) – and the outcome measure here is the unadjusted rate of handgun background checks per 100,000 population in D.C.

Columns 3-6 utilize the linear-in-time DiD design from Equation 3, and, unlike Columns 1 and 2, the outcome measure here is a log-base-2 transformation of the rate of handgun background checks per 100,000 people so that y_{it} is really $log_2(y_{it} + 1)$. Therefore, the coefficients allow for a cursory interpretation wherein a one-unit increase in the log effectively represents a doubling in the underlying data (the monthly rate of handgun background checks per 100,000 people). In other words, Column 3 reports a coefficient of 2.03 for Post-*Heller**D.C., and this can be best conceptualized as an increase in the rate of handgun background checks per 100,000 people of $2^{2.03} - 1$, approximately 3.08 handgun background checks per 100,000 people in D.C., compared to all other states. In Columns 3 and 4, I again find a remarkably large effect of *Heller* on D.C. (an increase in the rate of handgun NICS reports per 100,000 people by between roughly 3.08 (3) and 1.50 (4), on average – a roughly 933 and 556 percent increase in the rate of handgun NICS reports in D.C. post-*Heller*, respectively), but also discern suggestive evidence for an increase in handgun NICS report rates in presumably less affected states in the years following the decision (Post-*Heller* is small but positive and statistically significant). However, as the estimates for *MonthsSince* * Post-*Heller* and *MonthsSince* demonstrate, some of this increase across all states is attributable to a general increase in the rates of NICS handgun checks over time. This comports with the earlier prediction that D.C. experienced a relatively immediate shift in the rate of handguns being purchased, whereas other states did not see nearly as responsive adjustments in purchasing behavior, but instead increased more stably over time.

Columns 5 and 6 present the two-year and ten-year bandwidth DiD estimations,

Table A.2: ITS and DiD Estimates of *Heller's* Effect on Washington, D.C., All States, and Most Permissive States by GLC Rank

	Handgun NICS Report Rate					
	D.C. Only		*D.C. v. All States*		*D.C. v. Permissive States*	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-*Heller* * D.C.	5.38	2.89	2.03	1.32	2.03	1.29
	(1.36)	(0.44)	(0.02)	(0.09)	(0.03)	(0.06)
Post-*Heller* * D.C. * MonthsSince	−0.01	0.05	0.06	0.00	0.06	0.00
	(0.08)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)
D.C. * MonthsSince	−0.02	0.00	−0.04	−0.00	−0.04	−0.00
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Post-*Heller*			0.16	0.36	0.16	0.40
			(0.02)	(0.09)	(0.03)	(0.06)
MonthsSince * Post-*Heller*			−0.02	0.01	−0.02	0.00
			(0.00)	(0.00)	(0.01)	(0.00)
MonthsSince			0.02	0.01	0.02	0.00
			(0.00)	(0.00)	(0.01)	(0.00)
No. Observations	49	237	2,450	11,850	686	3,318
State FE			Yes	Yes	Yes	Yes
Sample Period	07/2006 - 07/2010	11/1998 - 07/2018	07/2006 - 07/2010	11/1998 - 07/2018	07/2006 - 07/2010	11/1998 - 07/2018
Clusters			50	50	14	14
Adjusted R^2	0.69	0.85	0.97	0.85	0.96	0.92

*Note: All models are estimated using ordinary least squares. Heteroscedastic standard errors are reported for Columns 1 and 2. All regressions for Columns 3-6 include a control for the estimated annual population of a state. Hawaii and other Territories were dropped from the analysis due to differences in data reporting through NICS. MonthsSince is a linear annual time trend (set to 0 in 07/2008) that captures any general relationship between time (measured in months) and NICS handgun reports. For the 10-year bandwidth models, NICS only began reporting in November 1998, hence the pre-*Heller *period starting after July 1998 and not accounting for a full ten years prior to the decision. For Columns 3-6, the outcome variable is transformed to a* $log_2(y_{it}+1)$ *scale for interpretability reasons and high variance in the raw rate measure. Here,* y_{it} *is the monthly rate of handgun NICS reports per 100,000 people in state i and year t.*

respectively, for D.C. versus the most permissive states (according to the GLC state law rankings). Again, the outcome measure here is a log-base-2 transformation of the rate of handgun background checks per 100,000 people: $log_2(y_{it}+1)$. Here, I find the most compelling evidence for a causal effect from the *Heller* decision on the rate of handgun NICS reports in D.C. When compared to states that were presumably the *least affected* by the decision, D.C.'s monthly rate of handgun NICS reports increased by $2^{2.03}-1$ on average, or roughly 3.08 handgun NICS checks per 100,000 people in the two years following the decision – which amounts to roughly a 933 percent increase compared to the average pre-*Heller* rate. The ten-year bandwidth estimates that D.C.'s rate of handgun NICS reports increased by $2^{1.29} - 1$, which is roughly 1.45 handgun NICS reports per 100,000 people, on average – approximately a 537 percent increase in the ten years following the decision. Comparatively, the most permissive states saw a relatively small effect in the two and ten years after *Heller* (as seen in the coefficients on Post-*Heller* in Columns 5 and 6) – an effect that is, in part, driven by the linear increase that term $MonthsSince_t$ accounts for. While this is evidence for the *Heller* decision affecting D.C. considerably more than other localities, as predicted, it is also suggestive that the Court's decision in *Heller* had wider-ranging effects on handgun purchasing across the country – even in states where one might expect to see a much smaller effect, if any. Across all models, these estimates constitute substantially large effects, and the results are robust under model specifications that identify quasi-experimental control units.

A.4. Maryland and Virginia

In Section 5.4 of the main text, I investigate whether the *Heller* decision affected handgun purchasing rates in Maryland and Virginia. The graphical results of this analysis can be seen in Figure A.5. I find null results – considering that only Maryland and Virginia residents could purchase firearms in Maryland and Virginia, respectively, it is not reasonable to assume that the *Heller* decision would have led to D.C. residents purchasing firearms in D.C. as opposed to surrounding states before the decision. The evidence against a substitution theory confirms that substitution was not feasible before *Heller*. Furthermore, while it may appear from Figure A.5 that Maryland saw a reduction in the rate of handgun background checks following the ruling, this is more likely due to changes in administrative reporting of NICS data that Maryland underwent in 2005, as discussed in Appendix Section A.2.2.

However, there is some suggestive evidence that purchases of firearms outside of the District of Columbia still affected the overall stock of firearms in D.C. post-*Heller*. As shown in Figure A.6, Annual ATF Firearms Trace Data in D.C. from 2006 to 2019 shows a general increase in the percentage of firearms traced to a point

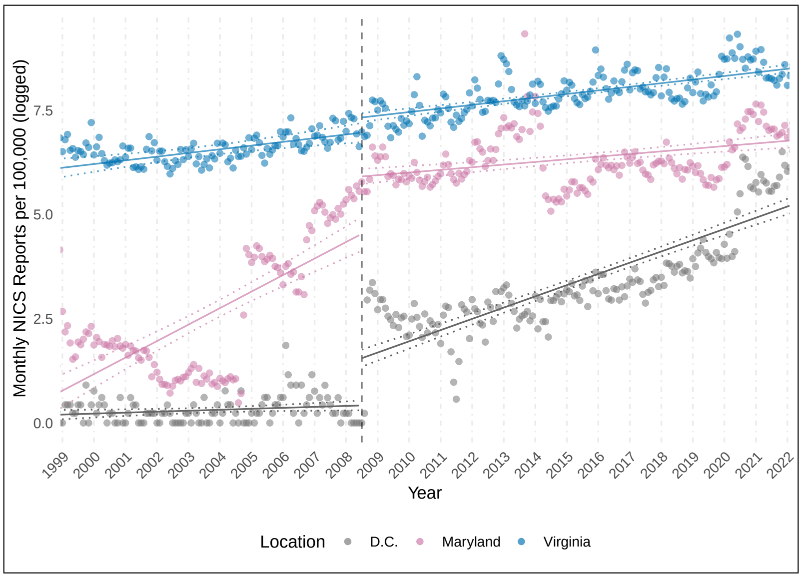


Figure A.5: No Effect of the *Heller* Decision on Handgun NICS Reports in Maryland and Virginia (transformed to a $log_2(y+1)$ scale)

of origin in Virginia and D.C. over time. The nature of this administrative ATF data, which is discussed in greater detail in Appendix Section A.7., prevents one from making any particularly strong claims about the firearms being used in criminal activity in D.C., though this figure does suggest that private sales of firearms were still occurring long after sales through FFLs were deemed to be constitutionally protected in D.C. As individual residents of D.C. could not purchase firearms legally in Virginia from FFLs, this observed increase in the percentage of firearms recovered in D.C. that originated in Virginia suggests that D.C. residents may have been buying firearms from private sellers who acquired the firearms from Virginia-registered FFLs. However, as our conclusions are hampered by issues with the underlying administrative data, this finding should not be taken as direct evidence of a substitution effect driving the main result of the paper.

A.5. *Heller's* Effect on Long Gun Background Checks

Figure A.7 presents graphical evidence of Heller's effect on long gun sales in D.C. only. On the horizontal axis, I plot the month and year associated with each observation, from November 1998 to December 2018. The vertical axis shows the logged monthly rate of long gun NICS reports per 100,000 population. A discontinuity is

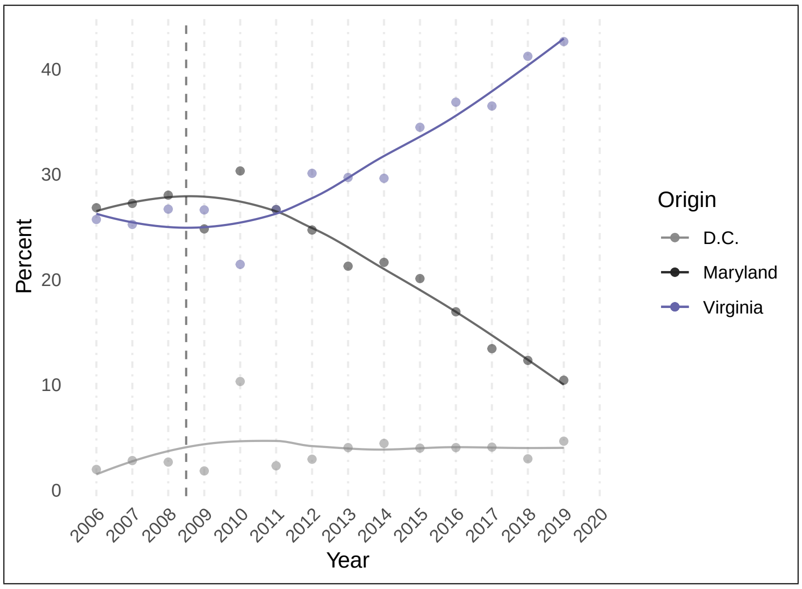


Figure A.6: Percentage of Firearms Originating in Virginia and D.C. Among All Annual ATF Traces in D.C. Suggests Progressive Increase Over Time

present in July 2008 to mark the beginning of the post-Heller period, and linear smoothers are fit to the underlying data on either side of this cutoff to represent general trends in handgun purchasing rates over time. I find no evidence that *Heller* affected the rate of long gun background checks in D.C. While a general linear increase does appear to begin years after the decision, this is likely due to other changes in D.C. firearms policy and not a product of the *Heller* decision.

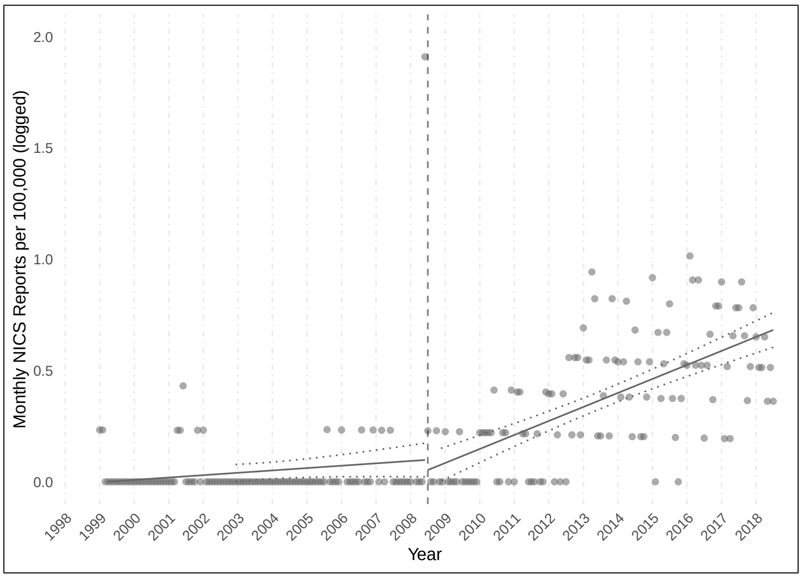
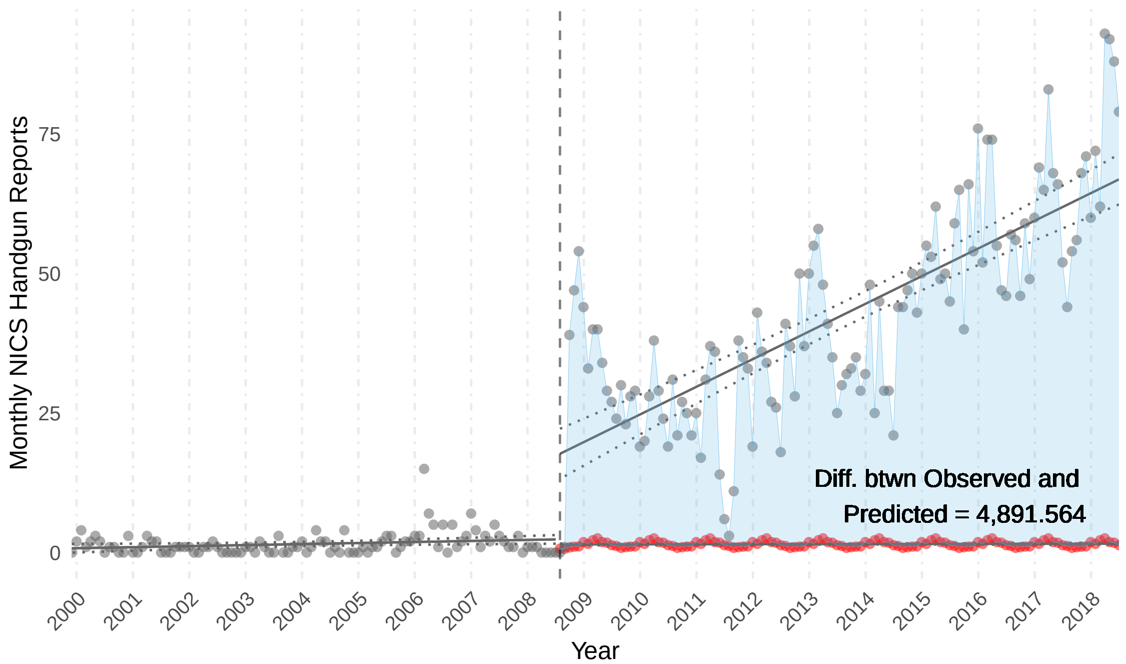


Figure A.7: No Effect of the *Heller* Decision on Long Gun NICS Reports in D.C. (transformed to a $log_2(y+1)$ scale)

A.6. Using SARIMA Forecasting to Estimate the Number of Handgun NICS Reports Due to the *Heller* Decision

In Section 6 of the main text, I attempt to estimate the number of handguns sold due to the *Heller* decision. In this section, I use SARIMA forecasting to predict the number of NICS handgun background checks run in D.C. in the 10 years following the Court's decision had the *Heller* ruling not occurred.



Note: Figure plots the raw number of NICS Handgun Background Checks for D.C. in each month. Observed raw data is in grey, with predicted values from a SARIMA model in red. The shaded blue region represents the difference between the observed values and the predicted values. The model was optimized to a SARIMA (1,0,0)(0,0,1,12), denoting the order of autoregression, differencing, and moving average components for both the non−seasonal and seasonal parts of the model.

Figure A.8: Difference Between Observed Handgun NICS Reports and SARIMA Forecasted Handgun NICS Reports in D.C.

Figure A.8 shows the raw, unadjusted counts of monthly NICS handgun background checks in D.C. in grey, pre- and post-*Heller* ruling. To estimate the number of handgun NICS reports in D.C. had *Heller* not occurred, I use a SARIMA model (1,0,0)(0,0,1,12) to estimate D.C.'s number of handgun NICS reports run in the 120 months following the release of the *Heller* decision. These estimates are plotted in red. I then subtract these predicted values from the corresponding month's observed number of handgun NICS reports, and this difference is shown as the shaded blue area. I find that based on these SARIMA estimations, the *Heller* decision was responsible for roughly 4,892 *more* NICS handgun reports in the 10 years following the Court's ruling. This is perhaps a more precise estimation of the number of handguns sold because of the *Heller* ruling, but it is also of a similar magnitude to the number I estimate by simply holding the pre-*Heller* rate constant in the post-*Heller* period, as I do in the main text.

A.7. Using ATF Firearms Trace Data to Assess the Effects of the *Heller* Decision on Gun-Related Crime

To assess *Heller's* effect on gun-related crime, I turn to descriptive evidence provided by the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) in response to a Freedom of Information Act (FOIA) request I submitted for Firearms Trace Data in D.C. ATF Firearms Trace Data generally reports the number of firearms recovered by law enforcement authorities in investigations that were then sent to the ATF to be tracked to an initial retail seller, though (1) not all firearms used in crime are traced by the ATF, (2) not all firearms with an associated trace were used in crime, and (3) firearms that were traced should not be thought of as a random sample of firearms recovered in a given area. According to a 2006 report by the Congressional Research Service, firearms trace data may be biased and should not be used to "...test for statistical significance between firearm traces in general and the wider population of firearms available to criminal or the wider American public." In this section, I simply use ATF Firearms Trace Data in D.C. to report descriptive evidence that suggests *Heller* may have had a downstream effect on the number and type of firearms recovered by law enforcement in D.C. and traced by the ATF.

Ultimately, this ATF data allows me to observe the annual number of firearms recovered in D.C. that were reported to the ATF for a trace and the location of the initial retail sale for those firearms that were successfully traced by the Bureau. Additionally, I can observe the number of handguns recovered each year as a proportion of all firearms reported to the ATF. As Fig. A.9 shows, there was a general

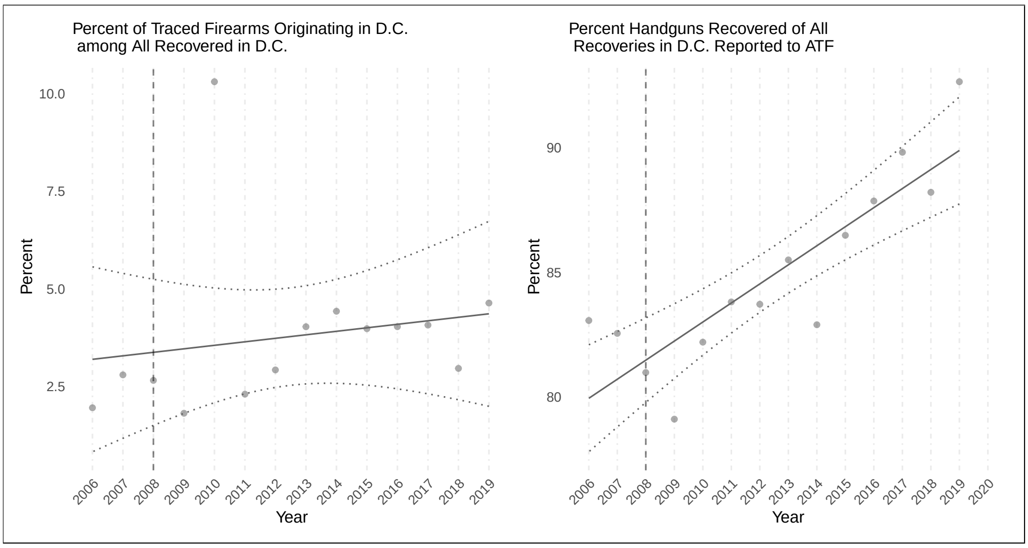


Figure A.9: ATF Traces of Firearms Originating in D.C. and Reported Recoveries of Handguns Increased Post-*Heller*

increase in not only the percentage of recovered and traced firearms that originated in D.C. out of all firearms recovered in the District in the years following the ruling (first panel) but also an increase in the percentage of recovered handguns out of all firearms reported to the ATF following the Court's decision (second panel). The conclusions that can be drawn from this figure are impeded by various issues associated with the administrative data, as discussed earlier. However, these data suggest that the significant surge in handgun stock flow following the *Heller* decision may have resulted in subsequent and enduring increases in the number of handguns recovered by law enforcement in D.C. that were originally purchased within the District. The fact that these increases are observed in years relatively distant from the ruling is not surprising, given that firearms that appear in crime generally take many years to do so. (According to ATF data, the average time from initial purchase to use in crime for firearms recovered in D.C. from 2006 to 2019 was 11.15 years, while the national average was 10.25 years.) These results hardly demonstrate that *Heller* led to increases in firearm-related crime in D.C. in the years after the ruling, but do suggest that the decision had lasting downstream effects on firearm ownership in the District, and perhaps some effect on guns used in criminal activity or recovered through law enforcement operations and investigations.