**Closing the Gun Show Loophole:**

**Symbolic Victory or Legitimate Change?**

**Abstract**

Research summary

On May 20, 2024, the Biden Administration implemented a rule change designed to close the so-called “gun show loophole,” which had allowed unlicensed private sellers to deal firearms without the requirement of conducting background checks on prospective buyers. The rule change broadened the definition of individuals classified as dealers “engaged in the business” of selling firearms, potentially increasing the proportion of gun sellers who will need to be federally licensed and need to conduct background checks when selling firearms. Accordingly, in this study we use a difference-in-differences design as well as an event study design to examine the effect of the rule change on the number of new Federal Firearm Licensees and the count of background checks completed in each state. Analyses fail to reveal evidence of an increase in new FFLs following the loophole rule change. For background checks, we find suggestive evidence of a delayed and short-term increase following the rule change. However, we caution against interpreting this finding as causal, as the analysis reveals violations of the parallel trends assumption likely due to recurring seasonal spikes in gun purchasing during the holiday buying season that are of differing magnitudes between treated and control states.

Policy Implications

The loophole rule change originated in the Bipartisan Safer Communities Act of 2022, with the goal of curtailing the black market of guns that are being sold by unlicensed individuals in the business of dealing firearms. However, its impact appears negligible and largely symbolic. Its apparent failure likely stems from a combination of weak enforcement, ambiguous regulatory language, and perhaps a miscalculation about the number of sellers that would be affected by the rule change. Consequently it seems unlikely that the rule change has had any meaningful impact on broader outcomes like firearm violence.

**Background**

The Brady Handgun Prevention Act, enacted by Congress in 1993 and implemented in 1994, requires federally licensed gun dealers to conduct background checks on prospective purchasers of guns. However, unlicensed sellers who are purportedly not “engaged in the business” of selling guns are not required to conduct background checks on prospective buyers. Yet in 2024, then Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) Director Steven Dettelbach asserted: “There is a large and growing black market of guns that are being sold by people who are in the business of dealing and are doing it without a license; and therefore, they are not running background checks the way the law requires. And it is fueling violence” (DOJ 2024). In support of this contention, researchers estimate that 85% of the guns used in a crime that are recovered by the police were sold at least once through a private party transaction (Wintemute, Braga, and Kennedy 2010).

To compensate for this federal regulatory gap, known as the “gun show loophole,” 21 US states and the District of Columbia require universal background checks on all or most firearm sales, including private sales.[[1]](#footnote-1) Research on prior changes in state laws related to universal background checks suggests that whereas point-of-sale background checks alone do not appear to be related to subsequent rates of firearm violence, universal background checks, when paired with permit-to-purchase laws, are associated with a significant reduction in firearm homicide (McCourt et al. 2020; Siegel 2024). Nevertheless, in the other 29 states, transactions taking place between buyers and private sellers did not require a background check if the seller was only making occasional sales. This gun show loophole contributed to the fact that an estimated 13% of recent firearm purchases in the US occurred without a background check, including 50% of private purchases made online, at gun shows, or otherwise outside of stores and pawn shops (Miller, Hepburn, and Azrael 2017). While there is debate in the academic literature as to whether gun show sales, specifically, have an effect on gun-related deaths (Duggan, Hjalmarsson, and Jacob 2011; Wintemute et al. 2010), the so-called loophole is applicable to all private party sales despite the moniker of “gun show.”

As part of the steps to implement the 2022 Bipartisan Safer Communities Act, on April 10, 2024, then U.S Attorney General Merrick Garland signed off on a rule change taken to close the gun show loophole.[[2]](#footnote-2) It sought to do so by broadening the definition—in title 27 of the Code of Federal Regulations, part 478—of who exactly is a dealer “engaged in the business” of selling firearms, potentially increasing the proportion of gun sellers who will need to be federally licensed and need to conduct background checks when legally selling firearms. Individuals who deal in firearms to “predominately earn a profit” through repetitive purchase and resale will require a federal license to do so (Federal Register 2024), with the rule change applicable to any such individuals, not just those seeking to sell at gun shows. Specifically, ATF advised sellers: “You will need a license if you are devoting time, attention, and labor to dealing in firearms as a regular course of trade or business to predominantly earn a profit through the repetitive purchase and resale of firearms…By contrast, if you make only occasional resales of firearms to enhance your personal collection or if you liquidate your personal collection (without restocking), you do not need to be licensed” (ATF 2024, p. i). Previously the rule did not include the statement on intent to “predominantly earn a profit.” The penalty for dealing in firearms without a license is up to five years of imprisonment and/or a fine up to $250,000.

The rule change took effect on May 20, 2024 (Federal Register 2024), although a federal lawsuit filed in the Northern District of Texas (case 2:24-CV-00089-Z) provided preliminary injunctive relief to the states of Texas, Louisiana, Mississippi, and Utah from the requirement to implement the rule change.[[3]](#footnote-3) This injunctive relief remained in effect throughout the first year after the rule change took effect—the period examined in this study—meaning that the rule change was not in effect in these four states during the period under investigation but was technically in effect elsewhere.

**Research Questions and Hypotheses**

If the Biden Administration was successful in closing, or narrowing, the gun show loophole, this should manifest in changes in the nature of gun transactions, including a decreasing number and proportion of sales through private unlicensed sellers that do not undertake background checks. Whereas much evaluation research on gun policy focuses only on violence outcomes, the central aim of this study is to comprehensively examine the more proximal outcomes of the regulatory change, which might then serve as mediators of the effect of the regulation on violence. Accordingly, in this study we examine the effect of the rule change on the number of new Federal Firearm Licensees (FFLs) and the count of background checks completed in each state. We ask:

1. Did the loophole rule change lead to an increase in the number of new Federal Firearms Licensees?
2. Did the number of National Instant Criminal Background Check System (NICS) firearm background checks per month significantly change following the enactment, for firearms sold by FFLs and also for private sales transacted through FFLs?

It could be the case that the loophole rule change was largely symbolic, in that few to no individuals previously considered private sellers would now fall within the definition of “engaged in the business” of dealing firearms to “predominantly earn a profit.” If, however, it is the case that some number of individuals now do meet the revised definition of “engaged in the business,” we anticipate three possible outcomes. These individuals could (1) knowingly or perhaps unknowingly flout the rule change and continue selling firearms without a license, (2) stop selling firearms, or (3) obtain a Federal Firearm License in order to continue selling.

As for NICS background checks, if the demand and supply of firearms remain stable but there are fewer private dealers, then we would expect the volume of background checks to increase as buyers turn to federally licensed dealers for purchasing. Under the Brady Act, federally licensed dealers are required to conduct background checks.[[4]](#footnote-4)

Per this rationale, we hypothesize the following:

1. The loophole rule change led to an increase in the number of new Type 1 Federal Firearms Licensees (i.e., dealer in firearms other than destructive devices).
2. The loophole rule change led to an increase in the average number of monthly background checks for firearm purchases.

Given that an application for a Federal Firearm License to sell firearms takes a minimum of two months for ATF to review and approve, we would expect that any changes in FFLs would not appear until July or August 2024.[[5]](#footnote-5) For background checks, any increase may be more immediate, if some private sellers leave the market and buyers turn to federally licensed dealers to purchase firearms.

**Study Design**

To answer our research questions about the effects of the policy and regulatory change, we draw upon the following data sources of state by month information: (1) listings of all *Federal Firearms Licensees*, available on the ATF’s website and (2) *National Instant Criminal Background Check System (NICS)* data, available on the FBI’s website.[[6]](#footnote-6) FFL data includes aggregate counts of active licenses per month as well as a full monthly list of active licensees. The number of new licensees, one of our outcome variables, can be determined by comparing consecutive months of active listings. NICS data lists the number of background checks of various types, including for the purchase of handguns and long guns as well as background checks conducted when an individual is seeking to obtain some form of firearm permit or a permit renewal (Smucker et al. 2022). Our second outcome variable is the number of background checks associated with the *purchase* of firearms through federal licensed dealers (i.e., we exclude background checks for permitting or other types of background checks).

To answer our research questions about the effects of the gun show loophole rule change, we will use two related analytic designs: a difference-in-differences (DiD) design and an event study. We define the treatment group as those states, 25 in total, *without* universal background check regulations as of May 20, 2024, and which had not received preliminary injunctive relief from implementing the rule change (see Figure 1). Of the remaining states and the District of Columbia, all but Minnesota (only handguns and semi-automatic weapons), Nebraska (only handguns), and Pennsylvania (only handguns) required background checks on the private sale of all firearms on the date the loophole rule change was implemented (Everytown for Gun Safety 2024). The 21 states and DC with preexisting background check laws on private gun sales constitute our control group.

**Figure 1. US States and DC by Treatment Status**

A map of the united states

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Conceptually, we seek to compare the average path of outcomes for treated states to the average path of outcomes for those states if they had not been subject to the rule change. To do so, in both designs we will compare the treated states to control states with preexisting laws mandating universal background checks. Essentially, we assume that the control states with existing background check laws will be minimally affected by the federal rule change and can be used to reveal the path of outcomes in the absence of treatment. Accordingly, a key identifying assumption of our design (i.e., the parallel trends assumption) is that in the absence of the loophole rule change, average untreated potential outcomes for our treated states and for our control group would have followed parallel paths in the post-treatment periods (i.e., the months following the rule change).

An event study allows us to examine the dynamic effect of the loophole rule change (i.e., separate coefficients for each lead month before the rule change took effect and for the lag months post rule change) whereas the canonical DiD design estimates a single coefficient representing the average treatment effect on the treated states. The canonical DiD framework has an intuitive appeal by estimating the average effect across the 12 month period after the rule change took effect.

One advantage of the event study design is the ability to assess the parallel trends assumption core to both designs. If the lead (i.e., pre- rule change) coefficients are close to zero and not statistically significant, it lends credibility to the assumption that treated and control groups were trending similarly before the rule change. In the absence of parallel trends in the pre-treatment period, it may be uncertain whether any differences between treated and control states post-treatment are due to the treatment (i.e., rule change) or due to some confounding influence.

Additional advantages of the event study design relate to examining the timing of any effect. For instance, event studies are useful for detecting if an outcome variable changed before the event officially occurred, suggesting an anticipatory effect. This may be particularly important in this study because the rule change was announced roughly six weeks before it took effect (i.e., April 10, 2024 versus May 20, 2024). Moreover, while the rule change may have created pressure or incentive for some individuals to apply for a Federal Firearm License, as noted it can take a minimum of two months to become a licensed dealer. Event studies are useful for detecting the potential delayed effect of the rule change under this type of circumstance.

**DiD Estimator**

To implement our DiD design, we use a two-way fixed effects estimator (TWFE). Since the rule change took effect in all treated states on the same date, our design is not a staggered setup that necessitates use of one of the recently developed estimators applicable to staggered policy rollouts (e.g., Callaway and Sant’Anna 2021; Roth et al. 2023).

As both of our outcomes are counts, we use the ppmlhdfe command in Stata 18 to estimate a Poisson Pseudo-Maximum Likelihood (PPML) model with fixed effects for both state and month-year:

(1)

where *s* indexes state (N = 47, including Washington, DC), *t* indexes month (N = 25 months, 12 pre-treatment, the treatment month, and 12 post-treatment), is a state fixed effect, is a month-year fixed effect, and is a treatment dummy variable. is the parameter of interest. It is the average difference in the outcome for the treated states in the pre-treatment period before versus 12 months after the loophole rule change, minus the average difference for the control group over the same time period. We cluster standard errors at the state-level.

Figures 2 and 3 display the raw trends in the two outcomes, FFLs and background checks, over the observation period. In both graphs there is evidence of seasonality. In Figure 2, we see that new FFLs are more likely to be issued a license in the spring and summer versus winter. The timing reflects, in part, when applications for a license are submitted, as it takes approximately two months to process a properly completed application.

The seasonality in average background checks depicted in Figure 3 likely reflects seasonal variation in firearm purchasing. For instance, sales typically increase around the November and December holidays. Given seasonality in the issuance of FFLs as well as gun purchasing, it is important to include the month-year fixed effect,, in our models. Doing so helps us account for the seasonality as well as common shocks to all states such as national elections or an economic shock like a tariff war. The month-year fixed effect accounts for seasonal effects that are common across all states, although would not account for any differential seasonality patterns between treated and control states. We return to this point later, as the gap in average background checks between treated and control states shown in Figure 3 appears to become an order of magnitude larger during the holiday buying season.

**Figure 2. Raw Counts of New FFLs by Treatment Status**

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**Figure 3. Raws Count of NICS Background Checks by Treatment Status**

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**Event Study Estimator**

To implement our event study design, we similarly use a two-way fixed effects estimator (TWFE), again estimated with Poisson Pseudo-Maximum Likelihood. As noted, with this design we can examine how any treatment effect evolves with time and assess whether pre-treatment trends differ between treated and control states. Our model includes leads-and-lags of the treatment variable, 12 months pre- and 12 months post treatment (where treatment occurs at *t* = 0), as follows:

(2)

where *k* indexes the event time in months relative to enactment of the loophole rule change. = 0 in all time periods in control states, and = 1 for the *kth* month relative to rule change enactment in treated states, and 0 otherwise. Accordingly, in the pre-intervention period is interpreted, for the *kth* month, as the average difference in the outcome between the control and treated states, and in the post-intervention period it is interpreted as the average treatment effect for the *kth* month after the loophole rule change. All coefficient estimates will be reported relative to the difference between treated and control states in the month before the rule change was enacted (i.e., April 2024). We cluster standard errors at the state-level.

**Sensitivity Test**

To assess the sensitivity of inferences to our estimation strategy and design decisions, we undertake several robustness checks. First, whereas our main analyses use a Poisson Pseudo-Maximum Likelihood model, for robustness we estimate linear models using an inverse hyperbolic sine (IHS) transformation of the outcome: . In contrast to a log-linear specification, an IHS transformation allows us to retain zero values with the dependent variable.

Second, an advantage of a linear model is that we can draw upon available tools to assess the sensitivity of our results to possible violations of the parallel trends assumption. One such tool is Rambachan and Roth’s (2023) *HonestDiD* sensitivity test. The idea behind this test is that if researchers are faced with potential violations of parallel trends in a pre-intervention period, a sensitivity analysis can help determine if core inferences are robust to violations of parallel trends of a similar magnitude in the post-intervention period. It may be the case that a significant effect of the loophole rule change on the outcomes holds if the parallel trends assumption is exactly met but is not robust to slight violations of parallel trends. Rambachan and Roth’s (2023) approach, implemented with the honestdid program in Stata, allows us to assess whether our core inferences hold under increasingly larger violations of the parallel trends assumption.

Third, whereas our main analyses estimate the treatment effect on the treated (TOT) and exclude the four states receiving preliminary injunctive relief from the rule change from the analysis (i.e., Texas, Louisiana, Mississippi, and Utah), for robustness we will include the four states in an analysis of the intent to treat (ITT). Essentially, we include all 51 states and DC in the analysis, with the intervention being the treatment assigned rather than the treatment received. Such an analysis will give us some understanding of whether our inferences are robust to our decisions about how to handle these four states.

Fourth, thus far we have specified the treatment date to be the enactment of the rule change on May 20, 2024, with a design focused on examining twelve months of outcomes on either side of the treatment date. Nevertheless, recall that the initial action on the attempt to close the gun show loophole was the passage of the Bipartisan Safer Communities Act on June 25, 2022. In a final robustness test, when extend the time period under investigation to include twelve months prior to the passage of the Act (i.e., June 2021 to May 2025), thereby enabling us a broader view of any anticipatory changes occurring between when the Act was passed in 2022 and when the corresponding rule change took effect almost two years later.

**RESULTS**

We proceed first with the DiD results to show the average treatment effect on the treated states. We then proceed to the event study to assess the parallel trends assumption and to examine potential dynamic treatment effects such as an anticipatory effect or a delayed effect.

**FFLs**

Table 1 presents DiD results for the count of newly licensed FFLs. The coefficient on the treatment indicator in the model is 0.103 (SE = 0.059), which equates to an 11% increase in the number of new FFLs per month following the rule change .[[7]](#footnote-7) However, this finding is not statistically significant at a conventional p-value of 0.05. Therefore, we do not observe support for our first hypothesis, in which we specified an expected increase in new FFLs. The model yields a pseudo R2 of 0.359, indicating moderately strong fit.

**Table 1. DiD Estimates: New FFLs**

|  |  |  |
| --- | --- | --- |
|  | Coef. | Std. Err. |
| Treat | 0.103 | 0.059 |
| Constant | 1.750\*\*\* | 0.020 |
| Observations  Clusters | 1150  46 |  |

Notes: Poisson Pseudo-Maximum Likelihood model.

Standard errors clustered by state.

\* *p* < 0.05, \*\* *p* < 0.01, \*\*\* *p* < 0.001

To assess the validity of the parallel trends assumption underlying both the DiD and the event study design, we estimate our event study model and examine the coefficients for the pre-intervention periods. The coefficients for each month are interpreted as the average difference in the outcome between the control and treated states relative to the reference period. As is typical in event study designs, we use the month before treatment as the reference period, in this case April 2024, as it reflects circumstances just before the rule change went into effect (i.e., on May 20, 2024). This is denoted by the staggered vertical line at -1 in Figure 4.

Recall that the parallel trends assumption requires that, in the absence of treatment, treated and control units would have followed similar trajectories. As can be seen in Figure 4 (with coefficients displayed in Appendix Table A1), none of the pre-treatment coefficients are statistically significant at conventional levels, and their magnitudes fluctuate around zero. This finding suggests that the treated and control states exhibited similar trends in FFLs prior to the rule change.

**Figure 4. Event Study Estimates: New FFLs**

A graph showing the time period

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Now focusing on the post-intervention period, we observe a fluctuating pattern of coefficients, although none are statistically significant at p < 0.05. Just one coefficient, for time period 1, is marginally significant (p = 0.095). As with the DiD results, these findings suggest that the rule change did not prompt an increase in the issuance of new FFL licenses as we hypothesized.

**Background Checks**

Now turning to background checks in a DiD model in Table 2, the estimated coefficient on the treatment variable is 0.004 (SE = 0.020), indicating a negligible and statistically insignificant change in the expected count of background checks.[[8]](#footnote-8) Hence, we again fail to find support for our hypotheses. The model exhibits strong overall fit (pseudo R² = 0.985), illustrating that the inclusion of state and month-year fixed effects explains nearly all the variation in background checks.

**Table 2. DiD Estimates: Background Checks**

|  |  |  |
| --- | --- | --- |
|  | Coef. | Std. Err. |
| Treat | 0.004 | 0.020 |
| Constant | 10.352\*\*\* | 0.006 |
| Observations  Clusters | 1150  46 |  |

Notes: Poisson Pseudo-Maximum Likelihood model.

Standard errors clustered by state.

\* *p* < 0.05, \*\* *p* < 0.01, \*\*\* *p* < 0.001

Turning to our event study, results are shown visually in Figure 5, with the full model results shown in Appendix Table A2. Arguably the most visually stark finding is the increase in background checks in treated states relative to control states exactly 12 months apart, in months -6 and -5 and again in months 6 and 7. These months correspond to November and December 2023 (pre-treatment) and November and December 2024 (post-treatment). As noted earlier, the month-year fixed effects in the model account for seasonality common to both the treatment and control group. However, as visually suggested by the raw seasonal patterns displayed in Figure 3, it may be the case that there are differential seasonality patterns not picked up by the model, such as a huge spike in firearm sales in treated states during the holidays without an equivalent spike in control states. After further discussing findings related to the pre- and post-intervention periods, we will return to this point.

Focusing on the pre-intervention leads, we see in all pre-treatment months positive point estimates, and several of the pre-treatment coefficients are statistically significant, notably months -6 and -5. This suggests, in likely violation of the parallel trends assumption, that prior to the loophole rule change, background checks in the treated states were already increasing relative to control states. In the absence of parallel trends before the rule change went into effect, we should be cautious in our interpretations of the effect of the rule change on background checks. It could be the case that increases in the post-treatment period reflect a continuation of the increases occurring pre-treatment rather than a causal effect of the rule change.

As for the post rule change lags, results reveal several statistically significant associations (i.e., lags 6 to 9), all indicating an increase in background checks after the rule change. For instance, the coefficient of the effect of lag 6, 0.215, means that in the 6th month after the rule change went into effect, background checks were 24% greater in treated states compared to what would have been expected based on the control group trend . The pattern observed in the post-treatment period in Figure 5 may be interpreted in a couple different ways. It may be consistent with a delayed increase in background checks, with the effect lasting for a few months before returning to baseline. Or it could simply reflect differential seasonality in firearm purchasing and background checks between treated and control states rather than a causal effect of the loophole rule change.

**Figure 5. Event Study Estimates: Background Checks**

A graph showing the time period

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If one were to exclude focus on the months of the holiday buying season and more narrowly focus on the ten month period in Figure 5 between time periods -4 and 5 (corresponding to January to October 2024, four months pre-intervention and five months post), there is still suggestive evidence of an increase in background checks. Pre-intervention, there is a shrinking gap in background checks between treated and control states between months -4 and -1 that appears to reverse once the loophole rule change came into effect (i.e., coefficients around 0.1 that decline to zero in the pre-intervention period). Figure 5 visually reveals increasing point estimates in the immediate post-intervention period, with effects marginally significant at p < 0.10 for time periods 3 to 5.

Whether focusing on the full post-intervention period or just the first few months following the rule change, results thus far provide suggestive evidence that the loophole rule change spurred an increase in background checks. Before drawing firm conclusions about this finding, we use Rambachan and Roth’s (2023) sensitivity test to assess the extent to which the observed increases in background checks are robust to violations of the parallel trends assumptions.

Rambachan and Roth’s method requires a linear specification. Accordingly, to assess the sensitivity of our inferences, we use an inverse hyperbolic sine transformation of the count of background checks in a linear model. Adapting equation 1 to a linear model, we estimate the following:

(3)

Figure 6 reveals estimates from this equation. Comparing figures 5 and 6, the general pattern of pre-treatment and post-treatment differences remains consistent. The pre-treatment period mostly has positive coefficient estimates, with several statistically significant, thereby suggesting that our treatment and control groups do not follow parallel paths. Post-treatment, months 6 and 7 (November and December 2024) have substantial differences between treated and control states.

**Figure 6. Event Study Estimates: Background Checks (linear model)**

A graph showing the time period

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Figure 7 shows sensitivity analyses for the relatively sizable treatment effects in the sixth and seventh time periods post-treatment, under varying scenarios of, which is a parameter depicting the magnitude of possible deviations from parallel trends. When = 0, we are assuming exact parallel trends in the post-treatment period. In contrast, at = 1, we assume some level of deviation from the parallel trends assumption. In this case, the post-treatment violation of parallel trends is assumed to be no larger than the worst pre-treatment violation of parallel trends (Rambachan and Roth 2023).

Under the assumption of exact parallel trends at = 0, the 95% confidence interval (CI) of the treatment effect in post-treatment periods 6 and 7 is 0.121 to 0.332. In other words, as depicted in Figure 6, if we assume we have met the parallel trends assumption, we find a positive effect of the rule change on backgrounds checks in periods 6 and 7, with a CI of the estimate from 0.121 to 0.332. But of course, the pre-treatment trends revealed in Figure 6 call into question whether we have exact parallel trends.

**Figure 7. Sensitivity to Parallel Trends Violations, HonestDiD Test**

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Qualitatively, in Figure 7 = 0.5 can be roughly interpreted as a small violation of parallel trends, = 1 is a moderate violation, and = 2 is relatively large. We see in the figure that the CI of the estimate widens substantially, containing zero, once we no longer assume exact parallel trends. This is the case even at small hypothetical violations of parallel trends ( = 0.5; CI -0.622, 1.176). In short, even for the strongest effects we observe (i.e., the 6th and 7th post-treatment months), we find that our inferences are sensitive to even small violations of the parallel trends assumption. We therefore conclude that while evidence is suggestive of an increase in background checks following the loophole rule change, we cannot conclusively determine if it was because of the rule change versus confounding factors.

**Further Robustness Analyses**

Analyses thus far reveal a non-significant relationship between the rule change and FFLs and a positive effect of the rule change on background checks in some months, although we again caution whether to interpret the latter effect as causal given the lack of parallel trends in the pre-intervention period, which may be due to differential seasonality. Both analyses may be interpreted as the treatment effect on the treated (TOT) states. As stated, these analyses exclude the four states that received preliminary injunctive relief by a federal court from having to implement the rule change.

As a test of robustness, we re-estimate our event study models in an intention-to-treat (ITT) analysis that includes the four previously excluded states in the treatment group. None of these states — Texas, Louisiana, Mississippi, and Utah — have state background check laws. While they did not technically receive the intervention, they were assigned the intervention. It may be the case that some would-be sellers, because injunctive relief was temporary, decided to proceed with obtaining an FFL or directed sales through an FFL.

Results are presented in Table 3.[[9]](#footnote-9) Highly similar to the DiD analyses presented in Tables 1 and 2, the intention-to-treat analyses reveal no effect of the loophole rule change on the number of new FFLs or background checks for firearm purchases.

**Table 3. DiD Robustness Analyses: Intention-to-Treat**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | FFLs | |  | Background Checks | |
|  | Coef. | Std. Err. |  | Coef. | Std. Err. |
| Treat | 0.099 | 0.057 |  | 0.012 | 0.019 |
| Constant | 1.860\*\*\* | 0.021 |  | 10.422\*\*\* | 0.006 |
| Observations  Clusters | 1250  50 |  |  | 1250  50 |  |

Notes: Poisson Pseudo-Maximum Likelihood model.

Standard errors clustered by state.

\* *p* < 0.05, \*\* *p* < 0.01, \*\*\* *p* < 0.001

To conclude the analysis, we consider the longer timeframe of the Biden Administration’s policy and regulatory efforts to close the gun show loophole. Given that action to close the loophole was initiated with the passage of the Bipartisan Safer Communities Act on June 25, 2022, in figures 8 and 9, we extend the time period of our analyses to include twelve months prior to the passage of the Act (i.e., June 2021 to May 2025, 35 months prior to rule change and 12 months after). Results of an extended FFL analysis presented in Figure 8 are consistent with our previous FFL results presented in Figure 4.

As for background checks (Figure 9), extending the timeframe of the event study back to June 2021 reveals a bifurcated picture. In the first half of the observation period, through time period -12 (June 2023), coefficient estimates of the difference between treated and control states are typically negative and non-significant. Then the pattern reverses, to mostly positive coefficients. However, a clear explanation for the shift is elusive. The Bipartisan Safer Communities Act passed in June 2022 and the proposed rule change was announced and opened for public comment in late August 2023. It remains unclear why there might have been an upward shift in background checks in the treated states (relative to control) starting in June 2023. Given that the apparent increase in background checks does not coincide with the major actions associated with the closing of the gun show loophole, it may be premature to conclude that the rule change led to an increase in background checks. Figure 9 also reveals that the cyclical increase in background checks during the December holiday buying season that we observed in figures 3 and 5 holds when we extend the lens of observation back to 2021. This suggests that the post rule change increases in background checks that we observed are, to some extent, due to pre-existing cyclical patterns.

**Figure 8. Event Study Estimates: New FFLs, June 2021 – May 2025**

A graph showing the time period

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**Figure 9. Event Study Estimates: Background Checks, June 2021 – May 2025**

A graph showing the time period

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**Discussion**

Results presented in this study fail to reveal evidence of an increase in new FFLs following the loophole rule change. For background checks, we find suggestive evidence of a delayed and short-term increase following the rule change. However, we caution against interpreting this finding as causal, as the analysis reveals violations of the parallel trends assumption—particularly due to recurring seasonal spikes in November and December that are of differing magnitudes between treated and control states. These differential seasonal patterns suggest that the observed effects may reflect heterogeneity in holiday-related firearm purchasing rather than a true impact of the loophole rule change.

In this section, we consider why the rule change appears to have had negligible effects on FFLs and background checks. We consider three plausible and overlapping reasons for the muted effect of the rule change: (1) the nature of the rule change was ambiguous, leading some people to believe that nothing had really changed; (2) the rule change had little deterrent effect because it was minimally enforced, perceived to be minimally enforced, or because sellers were unaware of the change; or (3) not that many sellers were affected by the rule change (i.e., few private sellers were actually redefined as being “engaged in the business” of dealing firearms to “predominantly earn a profit”).

With respect to ambiguity with the rule change, at the outset of the article we noted that the distinction between the old rule and new rule was the addition of language on the intent of sellers to “predominantly earn a profit.” While ATF (2024) did publish a guidance to help firearm sellers navigate the implications of the rule change, ambiguity over the phrases “engaged in the business” and “predominantly earn a profit” likely led at least some would-be firearm sellers to conclude that either the rule change was not applicable to them or that the rule change really did not change anything. This view is validated by sentiment across several online gun forums, including the following post on Reddit’s r/law forum (sh1tpost1nsh1t 2024):

“Nothing much has really changed. Private sales are still allowed. I can still post up at a gun show or post a classified to sell off parts of my collection to fund new purchases. The rule doesn't close a loophole, it just creates (or really mostly restates) the existing rules about straw purchases and operating without a license. The rule was and remains that private sales don't require a background check. And the people regularly buying and selling guns for a profit, or otherwise acting as straw purchasers, were already breaking the law and continue to be breaking the law, and this rule doesn't seem to be creating new mechanisms for identifying and stopping those people. So really this seems like a preservation of the status quo…At best I think a bunch of government lawyers got paid to waste a bunch of ink on a rule that will serve as a symbolic but ultimately meaningless gesture to people who want to see politicians do something about gun laws, but don’t fundamentally understand gun laws.”

With respect to the second possibility, we acknowledge that the loophole rule change took effect during a divisive presidential election and the early months of its implementation involved a change in presidential administrations to one that has redefined the role of the Department of Justice and the rule of law in the United States. While easing gun regulations has not been among the highest priorities during the first year of the current presidential administration, the Attorney General has nevertheless signaled that the gun control pursuits of the Biden era are not shared by the current administration and are being reviewed with an eye towards rescinding them (ATF 2025). Hence, the rule change may have been a reasonably sound idea, but its implementation was dead on arrival given the political context of the United States. If that is the case, there should be evidence of the failure of ATF to effectively enforce a regulation that was technically active for the entire 12-month post-treatment observation period examined in this study.

Accordingly, we submitted a Freedom of Information request to ATF directly asking for information about enforcement, specifically investigations and prosecutions over the last several years against any individuals for willfully engaging in the business of dealing in firearms without the required license (i.e., an alleged violation of 18 U.S.C. §922(a)(1)(A)). ATF provided a partial response thus far, about enforcement activity through the end of the Biden Administration. There appears to have been little change in enforcement. In the year prior to the loophole rule change (i.e., May 2023 to April 2024), ATF closed an average of 25 investigative cases per month related to violations of dealing firearms without a license. In the first seven months after the rule change went into effect, ATF averaged 26 case closures per month. The stability in cases could reflect ATF investigate capacity (e.g., enforcement activity remained steady even if violations of the law increased). Alternatively, it could reflect the fact that the rule change did not actually affect the potential ATF caseload, which is a point we consider shortly.

As noted, we examined several online discussion forums for gun owners and enthusiasts, to understand the reaction to the rule change as well as its efficacy. A common view among posters is that the rule change is unconstitutional, in violation of the Second Amendment, and therefore unenforceable. For instance, one respondent on Reddit’s r/progun forum commented: “The new rule (all the rules in fact, along with the ATFs very existence) are a violation of Bruen, not to mention the 2nd Amendment itself. Bottom line, it’s unenforceable” (JohnnyGalt129 2024).

Yet assertions about unconstitutionality were not limited to Second Amendment arguments. A poster, on Shotgun World’s online forum, questioned ATF’s legal authority to enforce firearm regulations in light of the U.S. Supreme Court’s 2024 overruling of the *Chevron* doctrine (i.e., *Loper Bright Enterprises v. Raimondo*, 603 U.S. \_\_\_ (2024)) (kknurick 2024). Under *Chevron*, established in *Chevron U.S.A., Inc. v. Natural Resources Defense Council, Inc.*, 468 U.S. 837 (1984), courts deferred to reasonable agency interpretations of ambiguous statutes. With Chevron overruled, some individuals may question ATF’s authority to define and interpret what it means to be “engaged in the business” of selling firearms. If that authority is in doubt, then ATF’s threat of punishment may have little deterrent effect.

Shifting from a focus on constitutionality to views on enforcement, we did observe online posts describing ways to circumvent the rule change. For instance, another post on Shotgun World’s forum encouraged individuals to become members of the Gun Owners of America (GOA), given that the GOA was granted injunctive relief from the rule change in the lawsuit that included relief for Texas, Louisiana, Mississippi, and Utah (top\_prop 2024). Whether injunctive relief actually extends beyond the GOA to the individual members of the GOA is a legal question that is unclear as of this writing. Nevertheless, the post highlights the rule change’s potentially limited deterrent effect. A long line of criminological research suggests that the certainty of punishment presents a stronger deterrent effect than severity (Chalfin and McCrary 2017; Nagin 2013). Based on a multitude of factors, including the granting of injunctive relief to four states from having to implement the rule change and the perceived and perhaps real toothlessness of ATF to enforce regulations, the likelihood of punishment for violating the loophole rule change is far from certain.

While the lack of new FFLs or a consistent increase in background checks may have been due to ineffective deterrence, a third explanation for our findings is that few private sellers were actually subject to the redefinition that is the foundation of the rule change. In other words, it could be that the vast majority of private sellers are not doing so to “predominately earn a profit” through repetitive purchase and resale of firearms. Rather, most may be doing occasional transactions out of their private collection in which profit motive is not the predominant goal. Assessing this possibility is tricky given the severely limited data available to researchers and even the government on firearm transactions. Perhaps one way to assess changes in the nature of private firearm selling is to examine trends in the types of sellers engaging in firearm sales online. Future research should examine whether patterns of firearms for sale in the online marketplace by types of sellers (i.e., private vs. licensed) shifted following the rule change. For instance, were firearms for sale on online sites less likely to be available from private sellers than prior to the rule change, particularly in treated states?

To conclude, the loophole rule change originated in a 2022 bipartisan act of Congress that was spurred by a Covid-era surge in violence. Yet its impact appears negligible. Whether this failure stemmed from the deeply polarized politics of gun regulation, the toothlessness of ATF, or from targeting to narrow a group of sellers remains unclear. Consequently it seems unlikely that the rule change has had any meaningful impact on broader outcomes like firearm violence.

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**Appendices**

**Appendix Table A1. Event Study Estimates: New FFLs**

|  |  |  |
| --- | --- | --- |
|  | Coef. | Std. Err. |
| Pre-treatment (t = -12) | -0.128 | 0.225 |
| Pre-treatment (t = -11) | 0.066 | 0.227 |
| Pre-treatment (t = -10) | -0.185 | 0.228 |
| Pre-treatment (t = -9) | -0.055 | 0.254 |
| Pre-treatment (t = -8) | -0.119 | 0.243 |
| Pre-treatment (t = -7) | -0.332 | 0.257 |
| Pre-treatment (t = -6) | -0.378 | 0.249 |
| Pre-treatment (t = -5) | 0.137 | 0.281 |
| Pre-treatment (t = -4) | -0.417 | 0.267 |
| Pre-treatment (t = -3) | -0.166 | 0.200 |
| Pre-treatment (t = -2) | -0.161 | 0.277 |
| Pre-treatment (t = -1) | 0.000 | . |
| Treatment (t = 0) | -0.147 | 0.183 |
| Post-treatment (t = 1) | -0.326 | 0.196 |
| Post-treatment (t = 2) | 0.321 | 0.249 |
| Post-treatment (t = 3) | 0.074 | 0.251 |
| Post-treatment (t = 4) | -0.348 | 0.280 |
| Post-treatment (t = 5) | 0.366 | 0.228 |
| Post-treatment (t = 6) | 0.012 | 0.238 |
| Post-treatment (t = 7) | 0.221 | 0.274 |
| Post-treatment (t = 8) | -0.076 | 0.341 |
| Post-treatment (t = 9) | -0.203 | 0.216 |
| Post-treatment (t = 10) | 0.099 | 0.254 |
| Post-treatment (t = 11) | -0.127 | 0.223 |
| Post-treatment (t = 12) | 0.009 | 0.224 |
|  |  |  |
| Constant | 1.829\*\*\* | 0.107 |
| Observations  Clusters | 1150  46 |  |

Notes: Poisson Pseudo-Maximum Likelihood model.

Standard errors clustered by state. Estimates are

displayed visually in Figure 4.

\* *p* < 0.05, \*\* *p* < 0.01, \*\*\* *p* < 0.001

**Appendix Table A2. Event Study Estimates: Background Checks**

|  |  |  |
| --- | --- | --- |
|  | Coef. | Std. Err. |
| Pre-treatment (t = -12) | 0.016 | 0.042 |
| Pre-treatment (t = -11) | 0.053 | 0.043 |
| Pre-treatment (t = -10) | 0.091\* | 0.045 |
| Pre-treatment (t = -9) | 0.060 | 0.041 |
| Pre-treatment (t = -8) | 0.040 | 0.037 |
| Pre-treatment (t = -7) | 0.087 | 0.052 |
| Pre-treatment (t = -6) | 0.153\*\* | 0.057 |
| Pre-treatment (t = -5) | 0.180\*\* | 0.061 |
| Pre-treatment (t = -4) | 0.094\* | 0.042 |
| Pre-treatment (t = -3) | 0.109\* | 0.045 |
| Pre-treatment (t = -2) | 0.058 | 0.030 |
| Pre-treatment (t = -1) | 0.000 | . |
| Treatment (t = 0) | 0.018 | 0.034 |
| Post-treatment (t = 1) | 0.006 | 0.075 |
| Post-treatment (t = 2) | 0.055 | 0.063 |
| Post-treatment (t = 3) | 0.068 | 0.040 |
| Post-treatment (t = 4) | 0.054 | 0.032 |
| Post-treatment (t = 5) | 0.079 | 0.041 |
| Post-treatment (t = 6) | 0.215\*\*\* | 0.056 |
| Post-treatment (t = 7) | 0.288\*\*\* | 0.055 |
| Post-treatment (t = 8) | 0.109\* | 0.043 |
| Post-treatment (t = 9) | 0.121\*\* | 0.040 |
| Post-treatment (t = 10) | -0.002 | 0.042 |
| Post-treatment (t = 11) | -0.024 | 0.026 |
| Post-treatment (t = 12) | 0.049 | 0.036 |
|  |  |  |
| Constant | 10.303\*\*\* | 0.018 |
| Observations  Clusters | 1150  46 |  |

Notes: Poisson Pseudo-Maximum Likelihood model.

Standard errors clustered by state. Estimates are

displayed visually in Figure 5.

\* *p* < 0.05, \*\* *p* < 0.01, \*\*\* *p* < 0.001

1. Maine passed a universal background check law on April 26, 2024; however, it did not take effect until after the loophole rule change took effect on May 20, 2024. [↑](#footnote-ref-1)
2. Whereas the final rule was signed by the Attorney General on April 10, 2024, and effective May 20, 2024, the redefinition of the key criteria, “engaged in the business” of dealing firearms, was initially addressed in the Bipartisan Safer Communities Act, passed by the U.S. Congress and signed by President Biden on June 25, 2022. To implement the Act, the Department of Justice (DOJ) then initiated a process to amend the associated regulation. DOJ proceeded by first announcing the proposed regulatory change on August 31, 2023, followed by an open public comment period until December 31, 2023. [↑](#footnote-ref-2)
3. Beyond the four states, plaintiffs in the suit also receiving injunctive relief include Jeffrey W. Tormey, Gun Owners of America, Inc., Gun Owners Foundation, Tennessee Firearms Association, and Virginia Defense League. [↑](#footnote-ref-3)
4. FFLs are required to conduct background checks prior to transferring a firearm, unless the purchaser presents to the FFL in certain states a permit to possess a firearm (i.e., a permit is used in lieu of the background check). See the following for a list of state permits that qualify as exceptions to a background check: https://www.atf.gov/rules-and-regulations/laws-alcohol-tobacco-firearms-and-explosives/gun-control-act/brady-law/brady-permit-chart [↑](#footnote-ref-4)
5. See: https://www.atf.gov/firearms/apply-license [↑](#footnote-ref-5)
6. Upon publication, data, computer code, and detailed metadata will be made freely and publicly available on Harvard Dataverse. The study has been pre-registered on Open Science Framework. [↑](#footnote-ref-6)
7. In this model, the observations for the District of Columbia are omitted because the number of new FFLs remained zero for every month of the observation period. Accordingly, the model perfectly predicts the number of new FFLs using the fixed effects. The results displayed in Table 1 are based on the 46 states where the loophole rule change was in effect. [↑](#footnote-ref-7)
8. In this model, the observations for Hawaii are omitted because the number of background checks was zero for every month of the observation period. Accordingly, the model perfectly predicts the number of background checks using the fixed effects. The results displayed in Table 2 are based on the other 46 states and DC where the loophole rule change was in effect. [↑](#footnote-ref-8)
9. As in the DiD analyses in Tables 1 and 2, in the models in Table 3 the observations for the District of Columbia are omitted in the estimates of FFLs because the number of new FFLs was zero for every month of the observation period and the observations for Hawaii were omitted from the model of background checks because Hawaii had zero background checks each month. [↑](#footnote-ref-9)