Trigger Warning: The Causal Impact of Gun Ownership on Suicide*

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Abstract

With a growing debate over tighter firearm regulations, we consider an important social consequence of increased firearm access: increased firearm suicides. Using data from the federal criminal background check system, we consider the impact of firearm ownership of firearm suicide rates. To deal with concerns of identification, we instrument for firearm background checks with state-year level Google search intensity for phrases that reflect fear of future gun shortages and learning about the constitutional rights of firearm owners. We find that an increase in firearm ownership has a sizable and statistically significant impact on firearm suicide rates. A 10% increase in firearm ownership increases firearm suicide rates by 3.1%, which is five times larger than OLS estimates. Furthermore, we find no effect of gun ownership on non-firearm suicide rates, suggesting our findings are not simply capturing a suicide method substitution effect. The results are consistent with a variety of validity and robustness tests. Our results make clear the link between firearm ownership and firearm suicide rates, both of which have increased dramatically over the last decade.

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1 Introduction

In 2014, 21,334 suicides were committed by the use of firearms in the U.S. This represents a 28% increase since the start of the century. Suicides by firearm were nearly twice as prevalent as homicides by firearm in 2014. The social cost of suicide is staggering. A public health crisis of this magnitude necessitates further consideration of policy options that can reduce the social and economic consequences of suicide.

To understand the determinants of this public health crisis, we focus on the impact of firearm availability on suicides. Figure 1 shows the evolution of firearm background checks in the US along with the evolution of suicides by firearm over the last 15 years. First, note the downward trend in firearm suicides in the early 2000s, broken by a sudden sharp increase in suicides. This sharp increase coincides with a spike in firearm background checks.[1]

There is a danger in drawing sweeping conclusions from simple charts like those in Figure 1. For example, a rise in depression could cause both an increase in firearm sales and an increase in suicides, generating a spurious correlation. Or perhaps, an increase in suicidal tendencies could lead to a similarly large increase in gun purchases. Alternatively, the evolution of these two phenomena may be completely unrelated and only chance led to similar patterns.

Nonetheless, the patterns are interesting and suggestive, and in the present work, we seek to disentangle and uncover any causal relationships if present. We proceed by looking for a plausible source of exogenous variation in gun sales and gun ownership unrelated to issues of mental health and suicide. Such a source of exogenous variation will allow us to make causal statements about the impact of increased firearm sales on suicides by firearm.

Our identification strategy uses concern regarding future access to guns as an instrument for present gun sales. That is, if agents are worried about access to guns in the future, they will intertemporally substitute future gun ownership with present gun ownership. To capture variation in this concern, we use Google Trends search intensity for key words related to the

[1]Kentucky is an outlier in the data as they started additional background checks of concealed carry permit holders in 2006, regardless of whether the permit holder was attempting to acquire a new gun at the time. As Figure 1 shows, excluding Kentucky has no effect on the overall national trend.

second amendment and gun bans. Agents searching for these terms are likely concerned about future access to gun ownership, but not directly contemplating suicide. In response to this concern about future access, agents are likely to buy guns today. With this intertemporal substitution in mind, we consider the impact of moving future gun sales into the present on the incidence of suicides by firearm.

Using state-level variation in background checks predicted by Google Trends search intensity for future gun restrictions, we estimate the impact of these plausibly exogenous additional guns on suicide rates. Our first stage results suggest that intertemporal gun ownership concerns do predict current gun sales well, and these estimates are robust to additional socio-economic controls.

The second stage results imply that additional guns do have a statistically significant and economically meaningful effect on suicide by firearm rates. Our estimates imply that a 10% increase in gun sales leads to a 3.1% increase in suicides by firearm. The identification strategy deployed here is important as estimates based on IV are more than 5 times larger than OLS models, underscoring the importance of selection bias in the present context.

To test the robustness of our results and methodological approach, we consider a variety of alternative specifications. Importantly, when we consider the impact of instrumented gun sales on non-firearm suicides, we find no statistically significant effect. This finding is key for two reasons. First, our approach would predict no effect on non-firearm suicides as the chain of events leads from additional access to guns to additional suicides by firearm because of greater access to firearms. The lack of a statistically significant finding thus supports the implied mechanism. Second, access to guns could simply substitute suicide by alternative means to suicide via firearm. This would imply an increase in suicides by firearm and an equal decrease in suicide by other means. Our lack of such a finding implies these additional suicides would likely not have occurred otherwise, representing a true social cost of additional firearms.

We test the validity of our instrument by replicating the exercise using Google Trends searches for the 27th Amendment as an instrument. Our concern is that, if our instruments really do measure a chain of events starting with fear of future gun shortages and ending with intertemporal

substitution of firearm purchases, then our results should not spuriously replicate with other Google Trends search phrases. To test our identification claim, we instrument for gun sales using searches for the 27th amendment, and find no statistically significant effects, providing additional support for our instruments.

The results presented here suggest that gun ownership has significant social costs in the form of additional suicides. The notable run-up in gun ownership over the last decade has coincided with a startling increase in suicides. Our findings suggest these processes are not unrelated, and that addition gun ownership leads to increased suicide by firearm rates.

The rest of the paper proceeds as follows. Section 2 discusses related literature, while Section 3 introduces the data. Section 4 lays out the empirical approach and results. Section 5 concludes.

2 Related Literature

Focus on the determinants of suicide has gained new-found interest from economists recently, as the increase in suicide rates has become more pronounced over the last decade. Case and Deaton (2015) find that changes in self-reported measures of well-being are poor predictors of changes in suicide rates. However, they do find that physical pain is a strong predictor of suicide in many contexts. Daly et al. (2011), however, find that happier areas tend to have higher suicide rates, while Daly et al. (2013) provides evidence that interpersonal income comparisons may influence suicide decisions. With increased focus on the importance of social factors, the economics literature is moving away from an purely individual framework of suicide determination.

Perhaps due to data limitations, or perhaps on account of the stigma surrounding topics like suicide and gun ownership, there have been limited attempts at establishing a causal impact of gun ownership on firearm suicide rates. The previous empirical literature on the topic, much of which comes from medical research, is based on exploring partial correlations. While there is an abundance of empirical papers on the topic of suicide, there are fewer economic theory papers on the topic. One possible explanation may be the inappropriateness of a rational framework for

understanding suicides. We proceed by summarizing the theory underpinning our economic understanding of suicide thus far, and then follow up with a synopsis of the econometric investigations into the relationship between gun ownership and suicide.

An economic theory of suicide was first proposed in Hamermesh and Soss (1974), who built a mathematical model to show that, given a cost of maintaining oneself through the aging process, there is a point where the (marginal) cost of living exceeds the (marginal) benefit of each year of life. The implication of this marginal analysis is that a rational agent would take the appropriate suicidal measures at the point where the marginal cost of living exceeds the marginal benefit.

An updated approach to this rational framework is presented in Marcotte (2003), who proposes an innovation that allows for suicide attempts to affect both the maintenance cost of living and the probability of reaching next year, conditional on making it to this year. This model is consistent with survey data that finds most suicide attempts are not fatal, and individuals who attempt suicide but survive have higher incomes than individuals who report feeling suicidal but do not actually attempt suicide.

Becker and Posner (2004) introduces greater uncertainty over the life cycle of an agent when considering rational utility-maximizes behavior for unhappy individuals. The framework provides valuable corrections and extensions to the Hamermesh and Soss (1974) optimizing approach, allowing for greater testable predictions of the rationality theory of suicide.

Fundamentally, the criticism of Becker and Posner (2004), Marcotte (2003), and Hamermesh and Soss (1974) are the same. These models approach an issue that is inappropriate for the rational choice framework. The "suicide contemplating agent" may lack the ability required to precisely and accurately calculate both the monetary and non-monetary gains from years of life to come, while simultaneously overestimating the costs of maintenance as a result of their current emotional state of being. There are many reasons to believe that an economic agent contemplating suicide has behavior that may not fit the *homo economicus* paradigm. For example, it is possible that mental illness is cognitively taxing in such a way that the agent's judgment is clouded, prohibiting that agent from forming a complete ordering of preferences regarding future states of the world.

Such agents would be unable to weigh the gains and losses appropriately.

Additional economic approaches to suicide have focused on decision theory modeling, but with less demanding rationality requirements. Cutler et al. (2001) study the startling rise of youth suicides over the last half of the 20th century, and consider alternative theoretical frameworks to explain this rise, especially in light of the declining rates at the time for other age groups. Their preferred theoretical interpretation for the youth results focuses on a signaling theory of suicide attempts, since most youth suicide attempts fail, where a suicide attempt is interpret as a signal for help. They also find evidence that supports a contagion view of suicide, which relies on social pressures plus variability in emotions for youths.

Especially noteworthy for our purposes here, they consider an instrumental view of suicide where access to easier suicide might increase the rate of suicide. The data they considered does not allow for a clean test of this instrumental theory of suicide view, leaving an open question we consider more rigorously here.

Seiden (1977) found that many suicides appear to be the result of impulsive behavior, where individuals who take their own lives often do so when confronting a severe, but temporary crisis. Simon et al. (2002) found that, among people who made near-lethal suicide attempts, 24% took less than five minutes between the decision to kill themselves and the actual attempt, and 70% took less than one hour. Rich et al. (1986) found that at-risk teenagers are more likely to act impulsively in suicidal ideation, and are more likely to be affected by the means at hand.

The impulsive decision-making process of suicide is also addressed in the literature by studies of survivors. Chapdelaine et al. (1991) found that, in cases of men who survived a self-inflicted gunshot wound, subsequent suicide attempts were uncommon. Peterson et al. (1985) found that, of self-inflicted gunshot wounds which were considered fatal without emergency medical treatment, none of the 30 subjects studied had written a suicide note, and more than half reported having suicidal thoughts for less than 24 hours. Furthermore, within two years of follow-up, none had attempted suicide or died.

If suicide attempts are not strictly rational, then opportunity and method may have significant

effects on suicide rates. Using data from Canada, Chapdelaine et al. (1991) find that 92% of gun attempts resulted in death, compared to 78% by carbon monoxide or hanging, 67% by drowning, and 23% by drug overdose. Hemenway et al. (1995) found that 21% of gun owners store a gun both loaded and unlocked, and that in 14% of gun-owning homes with children, a gun is stored both loaded and unlocked. If guns are more plentiful and available, and if suicides are not a purely rational decision, then the increased availability of guns could lead to more suicide attempts and suicide deaths given the higher firearm success rate.

The substitutability of suicide method may be an important factor in understanding suicide patterns. Under the rational suicide framework, substituting one method for another would depend on the relative opportunity cost of each method. If access to a gun is made more difficult, the agent would move to the next best suicide method. Assuming the cost of the next best method was not significantly greater, the rational framework would predict only minor changes in suicide attempts as a result of slightly higher method costs. On the other hand, if suicide attempts are often impulsive, then easier access to firearms could lead to greater firearm suicides without a commensurate decline in non-firearm suicides.

Fischer et al. (1993) find that there is an imperfect substitutability among methods of suicide. They find that restricting access to a frequently used means of suicide such as firearms can reduce total completed suicides by altering the composition of suicides to less effective methods and because alternative methods are less socially acceptable, thus decreasing the probability of being used. Related research has found that factors other than intent matter with respect to the completion of a suicide attempt. Seiden and Spence (1984) analyze data from suicide patterns at the Golden Gate Bridge and the Oakland Bay Bridge, and find that availability, suggestion, and symbolic factors affect the choice of suicide method and location.

Correlational evidence of the relationship between gun ownership and suicide rates is much more prevalent in the literature from the medical profession than it is for economics. Using a matched pairs research design, Kellermann et al. (1992) find that keeping a firearm in the home was strongly associated with an increased risk for suicide, estimating an adjusted odds ratio of

4.8. A matched pairs identification strategy depends upon the belief that after matching on certain observable characteristics, all other differences are randomly distributed. In this study, authors matched on sex, race, age, and neighborhood. However, unobservable characteristics such as mental health could easily lead to households acquiring more guns and being more likely to commit suicide.

Furthermore, their investigation is conducted only in two counties, both selected for being large and being at opposite "extremes" of racial composition, which leaves concerns regarding external validity. Nonetheless, the approximation of a more credible research design to tease out the treatment effect in Kellermann et al. (1992) is an improvement over correlative studies that use regional or international cross-sectional variation, such as those in Kaplan and Geling (1998), Markush and Bartolucci (1984), or Molina and Duarte (2006).

In the empirical economics literature, the focus on guns and suicide was hampered since firearm suicide was actually used as a proxy for gun ownership (see Cook and Ludwig (2006)). The closest paper to our approach here is Lang (2013), who uses NICS background checks as a proxy for gun ownership and studies the correlation between this proxy and suicide rates using panel data. The attempt to deal with endogeneity in that paper focuses on youth suicides under the assumption that youth are not able to legally purchase guns. However, as discussed previously the challenges to identification are severe and sample disaggregation is unlikely to eliminate all identification concerns related to measurement error, simultaneity, and omitted variables. Our use of Google trends data to proxy for intertemporal gun ownership substitution provides a credible path to identification.

The relationship between gun ownership and crime has received more attention in the economics literature, with rigorous debate surrounding the hypothesis that guns increase/decrease crime. The evolution of this literature is summarized in Aneja et al. (2011). While the topic is different, many of the econometric challenges are the same. From this econometric discussion, we focus our attention on panel data that allows us to control for state and time trends as well as confounding covariates. However, whereas the "more guns, more/less crime" literature struggles to deal with

the endogeneity of crime and Right-to-Carry (RTC) laws, our identification strategy provides plausible exogenous variation in gun ownership through intertemporal substitution based on lack of future access to guns.

Our empirical strategy will rely on instruments that provide time varying measures of consumer interest in various topics. Choi and Varian (2012) gives a thorough overview of the search volume index. They show the utility of SVI as a time varying measure of consumer preferences and interests for predicting and forecasting economic statistics like motor vehicle sales, home sales, unemployment claims, and tourism. The field of finance has recently taken interest in the search volume index as an improvement for covariates formerly proxied on account of limited data availability. As an example, Da et al. (2011) use Google Trends data to measure investor attention. They find evidence that increases in the search volume index for stock tickers correlate highly with increases in stock prices and eventual reversals of the high prices. Vlastakis and Markellos (2012) use search volume for a stock ticker to proxy demand for information about the company for interested investors. Vitt (2017) uses Google Trends to instrument for e-commerce use intensity with search intensity for various keywords like "porn" and "cat videos", and we borrow aspects of this strategy to confirm robustness of the approach. Our intent is to use Google Trends data to measure demand for information that might lead an agent to intertemporally substitute a future gun purchase into a present gun purchase.

3 Data

The first step in our analysis is to define a measure of gun ownership. Given that there is no standardized database that directly tracks ownership of guns, we rely on the National Instant Criminal Background Check System (NICS), maintained by the Federal Bureau of Investigations since 1998. This system is used by firearms vendors to determine the worthiness of a prospective firearm buyer. Prior to completing the sale, a call to the FBI or other designated agency is conducted to ensure the customer is not prohibited from purchasing a firearm, and this is recorded

as a NICS check.[2] This metric does not represent the total gun ownership or the number of firearms in a given state, but it does proxy changes in the stock of gun owners as well as changes in the accumulation of firearms in the state, while also capturing changes in intent to own a firearm. Thus, we can think of the NICS metric as being a measure of gun ownership that is observed with some measurement error that will necessitate an instrumental variable approach to isolate the exogenous variation in gun ownership.

The validity of background checks as a proxy for gun ownership is discussed in Lang (2013). The literature has considered other proxies for firearm ownership at the national and census level using the General Social Survey, but this source is inappropriate for lower levels of observation such as the state in a given year. The CDC Behavioral Risk Factor Surveillance System also collects some data on firearm ownership, but not with sufficient granular information to be useful at the state-year level. Duggan (2001) uses subscriptions to *Guns & Ammo* as a proxy for firearm ownership, with limited success. Although there are concerns with non-compliance, private gun purchases, and transfers across state lines, Lang (2013) shows that NICS background checks are comparable to alternative measures of gun ownership at the national and census level, and thus likely to be useful proxies at lower levels of aggregation over time.[3]

Our data on suicides by firearm come from the Center for Disease Control's mortality records. In particular, we use the Public-Use files for Multiple-Cause-of-Death records. These records are drawn from all death certificates filed in the United States in a given year. Causes of death are classified according to the International Classification of Disease 10th edition (ICD-10) standards. We focus on Intentional Self Harm codes, which we further distinguish between intentional self-harm with a firearm and all other intentional self-harm deaths. CDC suicide data has been used in a variety of economic contexts recently, ranging from international trade (Pierce and Schott (2016)) to pain epidemics (Case and Deaton (2015)), among others.

These records track all deaths and report not only the cause of death, but demographic variables

[2]Prohibitions include people convicted of a crime punishable by imprisonment for at least one year, people who have been documented as addicted to controlled substances, and people who have been adjudicated as mentally defective, among others.

[3]For a recent survey of gun ownership acquisition without a background check, see Miller et al. (2017).

of interest such as race, sex, and age. We use this mortality data to construct the total number of suicide deaths in a state over time, as well as to partition suicide deaths in firearm suicides and non-firearm suicides. Doing so allows us to investigate whether an increase in gun ownership has a substitution effect on the method of suicide, or if gun ownership increases suicide rates at the margin by enabling those already considering it to more easily make a rash decision and commit suicide. We present the average firearm suicide rate and average firearm background check rate nationally over time in Figure 1.

Search intensity data is available on the Google Trends page.[4] Search intensity can be refined by geographic unit (countries, states, MSAs, etc.) and over time from 2004 until present. The search volume index is reported on a monthly basis as the total monthly query volume for the particular keywords or phrases as a fraction of the total number of search queries in the geographic area that month. Google then normalizes the maximum query share for the time period of interest to 100. We aggregate these monthly values to an annual search volume index by averaging over the months in the year.

We believe that spikes in gun purchases today are affected by an aversion to expected future firearm shortages. It should be the case that news about future gun restrictions prompts risk averse consumers to substitute away from future personal security purchases in favor of purchasing that equipment today. This line of thought drives our decision to include several keywords from Google Trends as instruments in order to measure consumer concerns about future firearm shortages. We start with state level search intensity for the phrase "second amendment", which captures concern about constitutional rights associated with firearm ownership. To further supplement intertemporal demand for firearm ownership, we include a second instrument based on state level search intensity for "gun bans". Concern about future gun bans would be a significant motivator to substitute future gun ownership for present gun ownership. To get a sense of the variation in our instruments over time, we present the state level variation in the search intensity for these phrases in Figure 2 for selected states. In this graph, we see that there is significant variation in

[4] `www.google.com/trends` or `trends.google.com`

the search volume index for these phrases within states, as well as across-state differences in the search intensity relative to the peak search intensity for the state.

Additional state and year controls were collected to control for additional social and economic factors that may be correlated with our instruments. If these factors are also correlated with suicide rates, failure to include these would bias our instrumented estimates. As will be shown below, inclusion of additional controls does not statistically affect our IV estimates, but we present results with and without additional controls for completeness. Demographic data such as population estimates, median income, the percentage of the population between 18-24, the percentage of the population that is African-American, and the veteran population are sourced from the U.S. Census Bureau. Data on the state unemployment rate is sourced from the U.S. Bureau of Labor Statistics. Crime data is taken from the FBI's Uniform Crime Reporting (UCR) program, while prison population statistics come from the Bureau of Justice Statistics.[5] In a series of robustness checks, we use proxies for mental health from the CDC's annual Behavioral Risk Factor Surveillance Survey (BRFSS). Our complete sample covers all 50 states from 2004 until 2013.[6] Summary statistics are reported in Table 1.

4 Empirical Analysis

First, to get a sense of the relationship between firearm suicide rates and guns, we consider an empirical strategy without instruments. Panel data estimates are presented in Table 2. The first observation is that a naive approach that did not fully exploit panel data information would conclude that gun background checks are positively associated with firearm suicide rates, although the effect is small. Column (1) uses fixed effects and a linear time trend, while columns (2) and (3) include additional covariates, and column (4) weights the observations by state population size.

[5]We include these on account of how they seem to be plausible sources for differences in relative well-being, which is in turn an important determinant of suicide rates as explored in Daly et al. (2013).

[6]Our primary sample has 500 observations, but we lose some observations due to missing Google Trends data and missing socio-economic controls. For the Google Trends data, four states (Delaware, Rhode Island, Vermont, and Wyoming) are missing data for "gun ban" searches because the intensity was not above the threshold Google Trends sets for reporting. 8 other states are missing a single observation, mostly from 2004 or 2005.

The estimated effect is small: for a 10% increase in gun checks, firearm suicide rates increase by 0.6%.

This initial exercise provides some evidence consistent with the trends presented in Figure 1. Serious concerns about selection bias, measurement error, and simultaneity, however, suggest extreme caution in interpreting the results in Table 2 as causal.

For a causal estimation and interpretation, we proceed following our instrumental variable estimation strategy. Our preferred specification estimates the elasticity of firearm suicide rates with respect to gun background checks per capita, while controlling for additional factors that may influence firearm suicide rates and be correlated with our instrument. Our preferred specification is:

$$\begin{aligned} \ln(\text{Firearm Suicide Rate}_{st}) = \theta_0 &+ \alpha \ln(\text{Gun Background } \hat{\text{C}}\text{hecks Per Capita}_{st}) \\ &+ \mathbf{Controls_{st}}\boldsymbol{\beta_z} \\ &+ \text{State}_s + g\ t + \epsilon_{st} \end{aligned} \tag{1}$$

where the dependent variable is the log number of firearm suicides per 100,000 population in state s for year t. Our variable of interest, ln(Gun Background $\hat{\text{C}}$hecks Per Capita$_{st}$), is the log predicted number of NICS background checks per capita in the state for the given year from the first stage results. Included in $\mathbf{Controls_{st}}$ are time-varying state characteristics that could be correlated with our instrument and also correlated with firearm suicides. In Eq. (1), $State_s$ represents a state fixed effect that accounts for any time-invariant determinants of suicide, such as differences in cultures towards suicide across geographic borders as explored in Neumayer (2003). Time trends are included to account for unobserved national forces driving suicide rates. To address the concerns regarding undersized standard errors when shocks may be correlated at a geographic level, as in Bertrand et al. (2004), we cluster observations at the state level as we view this as the most appropriate level for likely correlations in shocks to suicide, although results are robust to alternative assumptions about error term correlations.

Given concerns regarding our proxy for gun ownership via background checks, a classic measurement error problem, and to address (omitted) time varying confounding factors, we adopt an instrumental variables strategy. We instrument for gun background checks using Google search intensity for "second amendment", and in robustness checks, "gun bans". To isolate exogenous variation in gun ownership, our first stage specification is as follows:

$$\begin{aligned}\ln(\text{Gun Background Checks Per Capita}_{st}) = \theta_1 &+ \pi_1 \ln(\text{second amendment search intensity}_{st}) \\ &+ \pi_2 \ln(\text{gun ban search intensity}_{st}) \\ &+ \mathbf{Controls}_{st}\boldsymbol{\delta} \\ &+ \text{State}_s + \pi_3 t + r_{st}\end{aligned} \tag{2}$$

Our identification strategy relies on the idea that, through intertemporal substitution, consumers respond to a fear of future firearm shortages (perhaps due to anticipation of regulatory changes) by substituting away from future firearm purchases in favor of firearm purchases in the current period.

Furthermore, we argue that these search phrases, which measure the expectation of future difficulty to attain firearms and learning about constitutional rights respectively, influence suicide only through increasing the stock of guns today. An increase in the stock of guns today increases the ease of making an impulsive decision to kill oneself with a gun. It seems unlikely that searches for these keywords would be direct determinants of suicide themselves. This exclusion restriction is akin to saying that the fear of future gun shortages in and of itself is not the reason people are committing suicide. To account for possible correlations between search intensity and other factor that may influence suicide rates, we include additional economic and demographic controls in our IV estimation.

Before turning to the first stage results, we consider some graphical anecdotal evidence of the relationship between the instruments and our proxy of gun ownership. Figure 3 shows state-level partial correlations between Google search intensity for "Second Amendment" and NICS background checks, after controlling for state population and linear time trends. The figure shows

significant heterogeneity across states, which is important for the implementation of the IV strategy. Although the evidence is anecdotal, it provides initial empirical support for our approach, which we formally and systematically confirm by estimating Eq. (2).

From Table 3 we note that our hypothesis regarding the relationship between expectations of future gun shortages, learning about gun owner's rights, and gun background checks is supported by the significance of "gun ban" and "second amendment" search intensity. For our primary instrument, the second amendment search intensity is highly significant (0.1% in columns (1)-(4)), and the excluded instrument F is above 10, suggesting the approach does not suffer from a weak instruments problem. Column (5) considers search intensity for gun ban, with similar individual statistical significance and passes the weak instrument test. Column (6) includes both search intensities, with second amendment search intensity significant at 1% and gun ban search intensity significant at 10%. The specification also passes the weak instrument test. Our first stage results strongly suggest that each of our instruments generates significant variation in the number of gun background checks within the state over time. Given the statistical value of the first stage, we now move to a full estimation of the impact of gun background checks on firearm suicide rates.

Armed with an empirical strategy suited to address measurement error, endogeneity, and omitted variable bias problems, we are able to paint a clearer picture of the relationship between gun ownership and suicide. First, consider Table 4, which presents IV estimation. Column (1) includes state fixed effects and linear time trends, and estimates an elasticity of 0.38. The effect is highly significant, and implies that a 10% increase in gun ownership results in a 3.8% increase in firearm suicides. This estimate is notable because it is nearly six times larger than the equivalent non-instrumented estimate in Table 2, underscoring the importance of a credible identification strategy and confirming concerns that measurement error, endogeneity, and omitted variables are biasing the estimates in Table 2.

When additional time varying controls are added in columns (2) and (3) to address independence concerns regarding the instrument, the point estimate is slightly smaller, although not

statistically distinguishable, and continues to be highly significant. The estimates are again 5-6 times larger than the equivalent estimates in Table 2. Column (4) weights the regression by the population of the state, resulting in an estimate that is about 50% smaller. Unweighted regressions treat all suicide rates equally so that changes in the suicide rate in California are given equal weight as changes in suicide rates in Delaware. However, while the estimated effect is smaller, the same 50% reduction is present in Table 2, so that the estimated effect is still roughly 5 times larger when our instrument is utilized. The lower weighted regression estimate suggests that smaller states (in terms of population) have higher elasticities on average, although this might be partially driven by the fact that smaller population states have noisier suicide rates since an additional suicide has a larger impact on the suicide rate than in larger population states. The weighted regression results imply that a 10% increase in background gun checks results in a 2.3% increase in firearm suicides.

To consider the robustness of our underlying identification, we next consider the search intensity for gun bans, an alternative approach to capturing a desire for intertemporal gun ownership substitution. The estimates presented in Column (5) are very similar to Column (3), with an implied elasticity of 0.26. In column 6, we include both proxies for intertemporal gun ownership substitution, and find that for a 10% increase in gun background checks, firearm suicide rates increase by 2.9%, which is 5 times larger than the OLS estimate.

The results from Table 4 strongly support the view that gun ownership, as proxied by gun background checks, results in higher firearm suicide rates. Furthermore, an identification strategy that accounts for endogeneity, measurement error, and omitted variables is important. Failure to account for selection bias would result in estimates that understate the true causal impact by a factor of five. Social costs of gun ownership in terms of the impact of suicide rates are thus significantly understated without a clear identification strategy.

To further consider the validity of our approach, and our hypothesized mechanism, we first consider the effect of guns sales on non-firearm suicide rates. The mechanism we have in mind is that an increase in the abundance of guns translates directly into more opportunities for someone

to make a rash decision and kill themselves with one. If our experimental approach is valid, and more gun sales lead to more firearm suicides, then we would not expect to find an effect on non-firearm suicides.

Any effect, positive or negative, could cast doubt on our approach. If we find a positive effect on non-firearm suicides, this would imply that whatever effect we are picking up, it is a more general affect related to suicides, and therefore our hypothesized causal mechanism of more guns leading to more gun suicides would be questionable. Alternatively, if we find a negative effect on non-firearm suicides, this would imply that more guns may shift the composition of suicide method without impacting the fundamental forces driving suicide attempts.[7]

To test this placebo hypothesis of no relationship between gun ownership and non-firearm suicide rates, we run a regression similar to Eq. (1) with non-firearm suicide rates as the dependent variable. Fixed effects instrumental variable results from this placebo specification appear in Table 5, providing strong evidence that our instrument is not capturing some suicide specific force unrelated to firearms. We find no evidence that gun background checks impacts non-firearm suicides. In our preferred specification (column (3)), the estimated elasticity is essentially zero. Perhaps not unsurprisingly given our identification strategy, the OLS (not reported) and IV estimates are nearly identical, -0.024 compared to -0.016. We thus rule out either story related to general suicide trends or suicide method substitution effects, while providing additional support for our hypothesized mechanism.

To test the validity of our instruments, we show that our selection of instruments is meaningful in the sense that if we used the search intensity for another constitutional amendment as an instrument, our results should disappear. If we use a different constitutional amendment search intensity, and find an effect on suicide rates, this would imply that our primary results are likely spurious and should be discounted. For this purpose, we choose to collect state-year level search intensity for the phrase "27th amendment". The 27th amendment, our most recent, prevents

[7]It is, however, possible that by substituting a firearm suicide attempt for a non-firearm suicide attempt may increase successful suicides if firearm suicides attempts are on average more likely to result in successful suicides, as in Chapdelaine et al. (1991).

congress from passing any law that would increase or decrease the salary of congress members until the beginning of the next term. While we see no reason for searches for such an amendment to be correlated with gun sales, it may be correlated with general unhappiness with social and political institutions. If this general unhappiness is symptomatic of some deeper unhappiness, it may be related to suicide rates.

For this validity test, we run a first stage similar to Eq.(2), using only 27th amendment search intensity as an instrument. Results from this first stage appear in column (1) of Table 6. The first stage results show no apparent relationship between 27th amendment search intensity and gun background checks. The estimated coefficient on search intensity of the 27th amendment is essentially zero and highly insignificant, while the excluded instrument F statistics is less than 2, consistent with a weak instrument. Unlike second amendment and gun ban search intensity, we find no evidence that 27th amendment search intensity is a good predictor of gun background checks. Nonetheless, we use this first stage result to regress suicide rates on predicted gun background checks, and present the results in column (2). Gun background checks, when instrumented with search intensity for the 27th amendment, are statistically unrelated to firearm suicide rates. The fact that not just any Google search phrase gives significant results lends credibility to the instruments selected and our hypothesized mechanism.

To further demonstrate the validity of our instruments, we present Limited Information Maximum Likelihood (LIML) estimates for a validity check similar to the Cruz and Moreira (2005) check of Angrist and Krueger (1991). In column (3) of Table (6) we use LIML to estimate the effect of interest and compare it to the OLS estimate in Table 2 and the IV estimate in Table 4. In light of Stock et al. (2002), since the F statistic of the excluded instrument test is sufficiently high, we should not suffer from a weak instruments problem. If our instruments were weak and we were committing a Type I error regarding the existence of weak instruments, then the LIML estimates will be close to the OLS estimates. Since the LIML estimates in column (3) of Table 6 are close to our preferred IV specification replicated in column (4) of Table 6, we find additional support for the validity and strength of our search phrase intensity instruments.

For a final series of robustness checks, we try to eliminate alternative stories and mechanisms driving the results. First, we consider whether Google search intensity may just be capturing general Internet usage or even specific Internet usage that may be correlated with anti-social behaviors associated with suicide risk. For example, perhaps people who are suicidal feel isolated from their local communities and retreat to virtual communities. These people may spend a lot of time at home on the Internet, and while on the Internet, they may search for a variety of phrases, including constitutional amendments. This would result in a biased overestimation of the impact of gun ownership on suicides, since our instrument would be correlated with people who were predisposed to committee suicide. Alternatively, robust online communities may reduce the sense of isolation, resulting in happier individuals who are less likely to commit suicide. This would result in a biased underestimation of the impact of gun ownership on suicide, since our instrument would be correlated with people were less likely to commit suicide as a result of their online participation. From our identification perspective, our concern is that our Google trends search intensity is correlated with general online usage intensity, which could be correlated with suicide risk. To separate our proxy for intertemporal gun ownership substitution from general Internet usage, we include a covariate of Internet use intensity.

Ideally, we would pick a search phrase that would capture as wide a cross-section of the state's Internet users as possible. A 2012 study found that 30% of all Internet traffic is pornography related, with the dominant firm PornHub reporting the most popular keyword search across states being "lesbian". For this reason, we include Google Trends data on the state level search intensity for "lesbian porn" as an additional control for Internet access and intensity. Results from adding this Internet use intensity variable as a control are found in column 1 of Table 7. Our point estimate on instrumented gun background checks is essentially unchanged, while we do find a weakly significant negative effect on our Internet usage proxy. These findings together imply that our instrument was not biased correlated with general Internet usage, and that Internet access does seem to lower suicide rates, consistent with the view that online communities provide valuable social connections.

Next, we consider alternative measures of mental health. In our primary specifications, we include Google trends searches for psychiatry. Here we consider an alternative measure based on the CDCs annual Behavioral Risk Factor Surveillance System (BRFSS). We calculate state specific measures of self-reported mental health. Based on survey response data, we calculate the percent of survey respondents in a state in a given year reporting having had at least twenty not good days of mental health in the last month, proxying for extreme unhappiness. In Column (2), we find no statistically significant difference in the estimated effect of gun background checks on firearm suicide rates. When we include both this alternative measure of mental health and our measure of general Internet usage intensity, the results are similar unchanged. These results should not be surprising since it is unclear why mental health would be correlated with searching for a specific constitutional amendment related to gun rights or related to concerns over future gun shortages, which would violate the independence claim of our identification strategy.

For a further check of model robustness, we consider an alternative time trend assumption with a random trend model, where we estimate a trend coefficient specific to each state. The random trend model is included to address the concerns in Aneja et al. (2011), namely we may not want to impose that the trend in suicide rates from a vacation state like Florida is the same as the trend in suicide rates for more isolated states like Wyoming or Montana. In column (4) of Table 7, we include state specific linear trends with no noticeable effect on our point estimate. There is disagreement about the appropriateness of state specific time trends within the guns and crime literature, but this appears not to matter in our context.[8]

Finally, data from Kentucky may be biasing our findings. As discussed in Lang (2013), in 2006, Kentucky began performing background checks on concealed carry permit holders, who were typically exempt from NICS background checks when purchasing firearms. The NICS measures from Kentucky thus may not be an equivalent proxy for gun ownership as in other states, although this would not impact the identification unless Kentucky's background check of concealed carry permit holders was correlated with our instrument. To confirm that our results are not being

[8]See Aneja et al. (2011)for a thorough discussion of the literature and the disagreement over this assumption.

driven by this outlying behavior, we drop Kentucky from the sample, and estimate the model in column (5). The results are unchanged.

Lastly, we consider alternative error term assumptions to test the validity of our inference. In Table 8, we estimate our preferred second stage specification, given in Eq. (1), with a sequence of different methods for estimating the standard errors of our point estimates. This sequences begins with conventional standard errors, then robust standard errors a la White (1980), regional clusters, jackknife, and bootstrap standard errors. In each case, statistical significance varies between 5% and 0.1%, confirming the inferential resilience of the model.

5 Conclusion

Understanding the relationship between firearm ownership and suicides by firearm is an important social policy question given the increasingly large social cost of suicide in the U.S. A lack of rigorous empirical evidence previously hindered painting a clear picture of the causal relationship between the two. In this paper, we attempt to make this causal impact clear: an increase in the number of firearms available today means increased opportunities for costly impulsive decisions like suicide by firearm.

We utilize an instrumental variables approach in order to address the measurement error in current measures of gun ownership, and omitted variable problems inherent in models of suicide determination. With this strategy, we find that an increase in the number of gun background checks within a state indeed causes a significant and sizable increase in the rate of firearm suicides within that state. To show that these suicides deaths are truly suicides that would have not otherwise occurred, we also show that increased firearm access does not induce a substitution across the various methods of committing suicide.

Our estimates yield a natural way to estimate the economic impact of firearm regulation policies. Consider a modest policy that would reduce handgun ownership by 20%. This would be met with a reduction in the firearm suicide rate of approximately 6%. Given that there were

21,000 firearm suicides in 2014, this reduction in ownership would result in nearly 1,300 fewer firearm suicides each year.

The social costs of gun ownership have mostly focused on the impact on crime, and in particular violent crime. While the relationship between gun ownership and firearm suicides has been observed in the data, confidence in the underlying relationship was limited by the correlative studies used to estimate the relationship. Our IV estimates are nearly five times larger than the OLS estimates, suggesting that the true costs of firearm ownership have been significantly understated. Our results here suggest a meaningful causal relationship between firearm ownership and firearm suicide. Preventable suicides impose enormous costs on society, and the expanding prevalence of gun ownership has contributed notably to these increased costs. Future public policy should carefully weigh the benefits and costs of gun ownership, and the impact on suicide should be included in that cost-benefit analysis.

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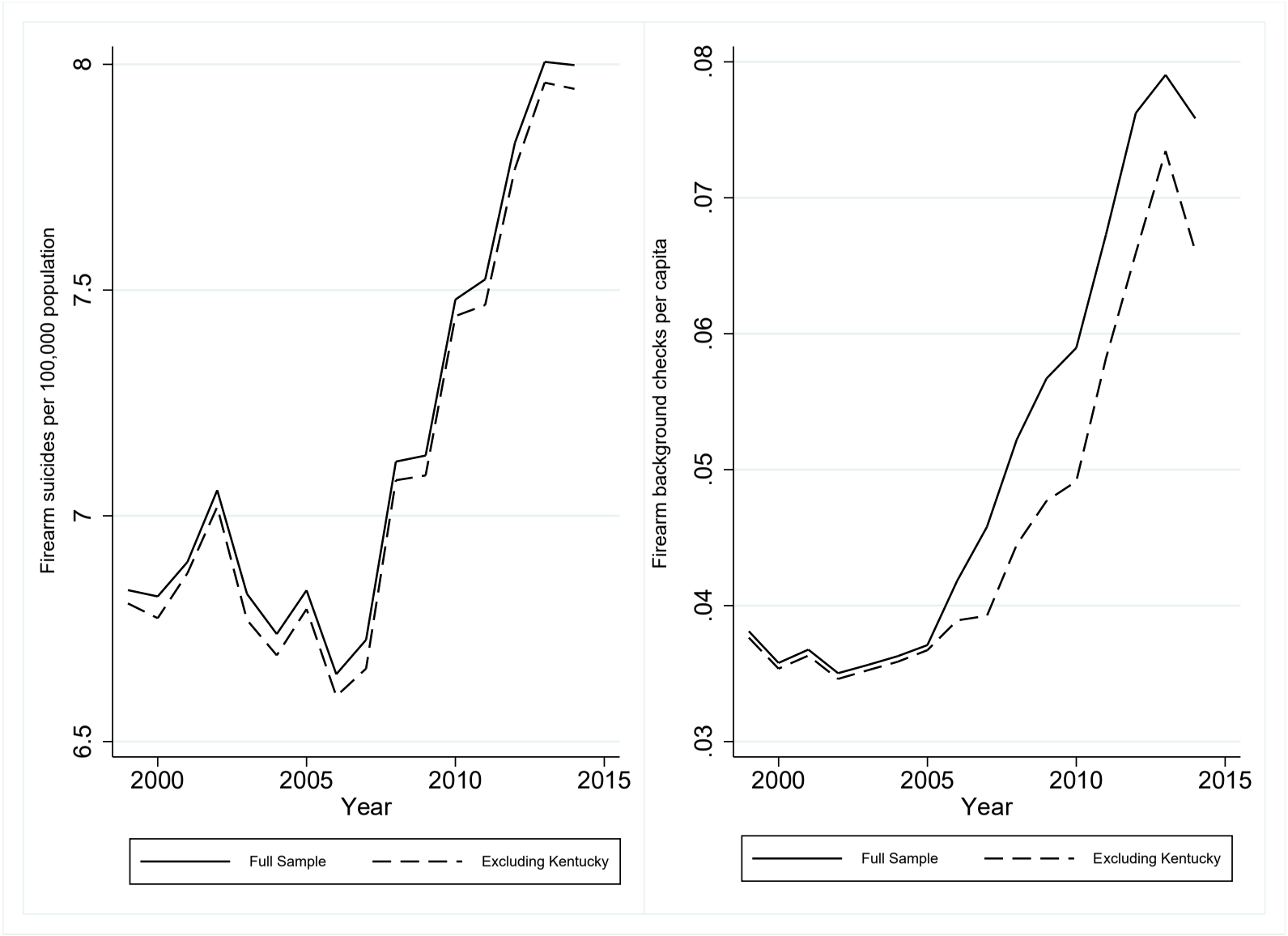

Figure 1: Variation in the average firearm suicide rate across states in every year is presented in the left panel, and variation in the average number of firearm background checks across states in every year is presented in the right panel.

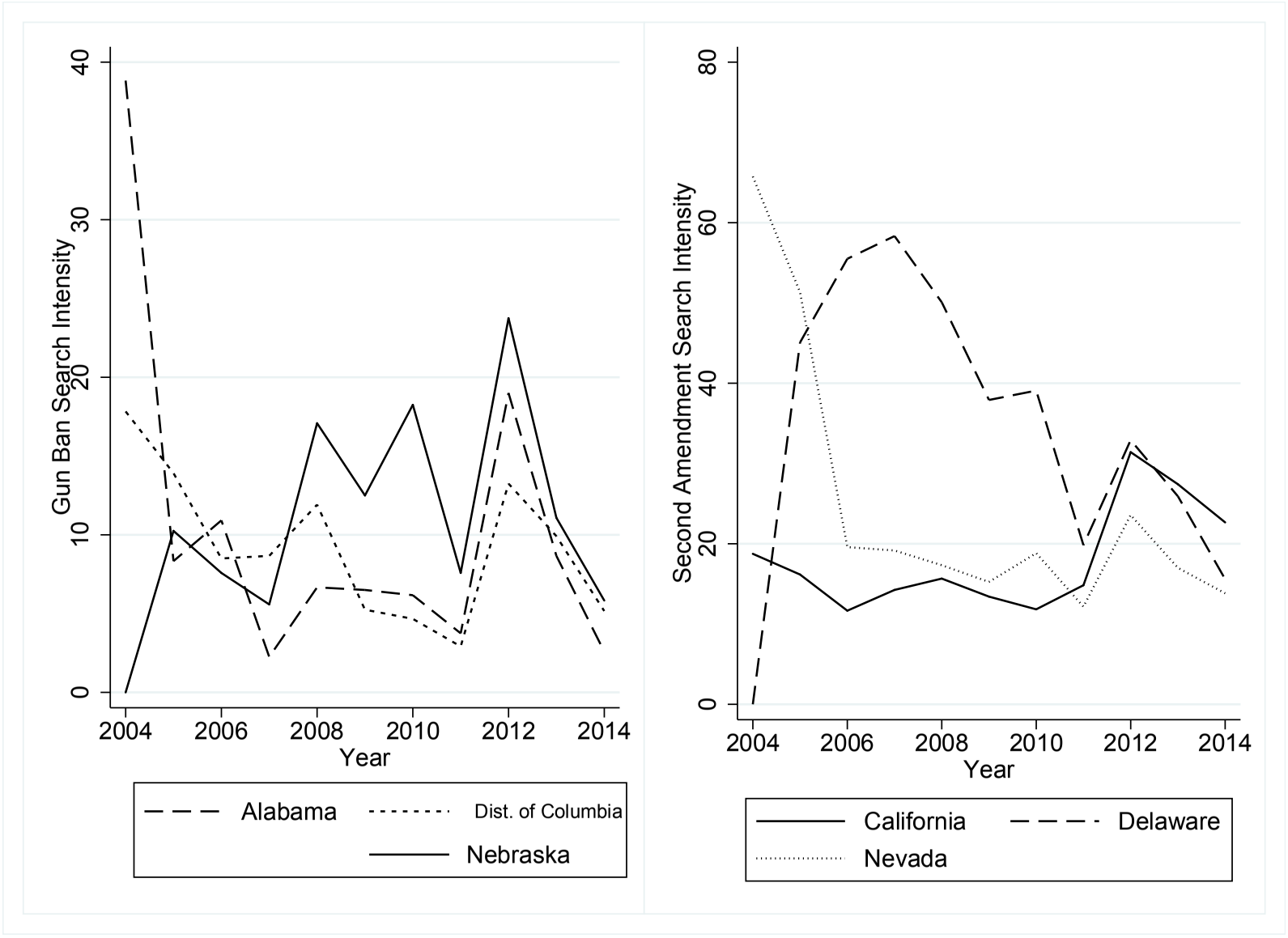

Figure 2: Variation in Google search intensity for phrases "gun ban" and "second amendment" across and within select states over time

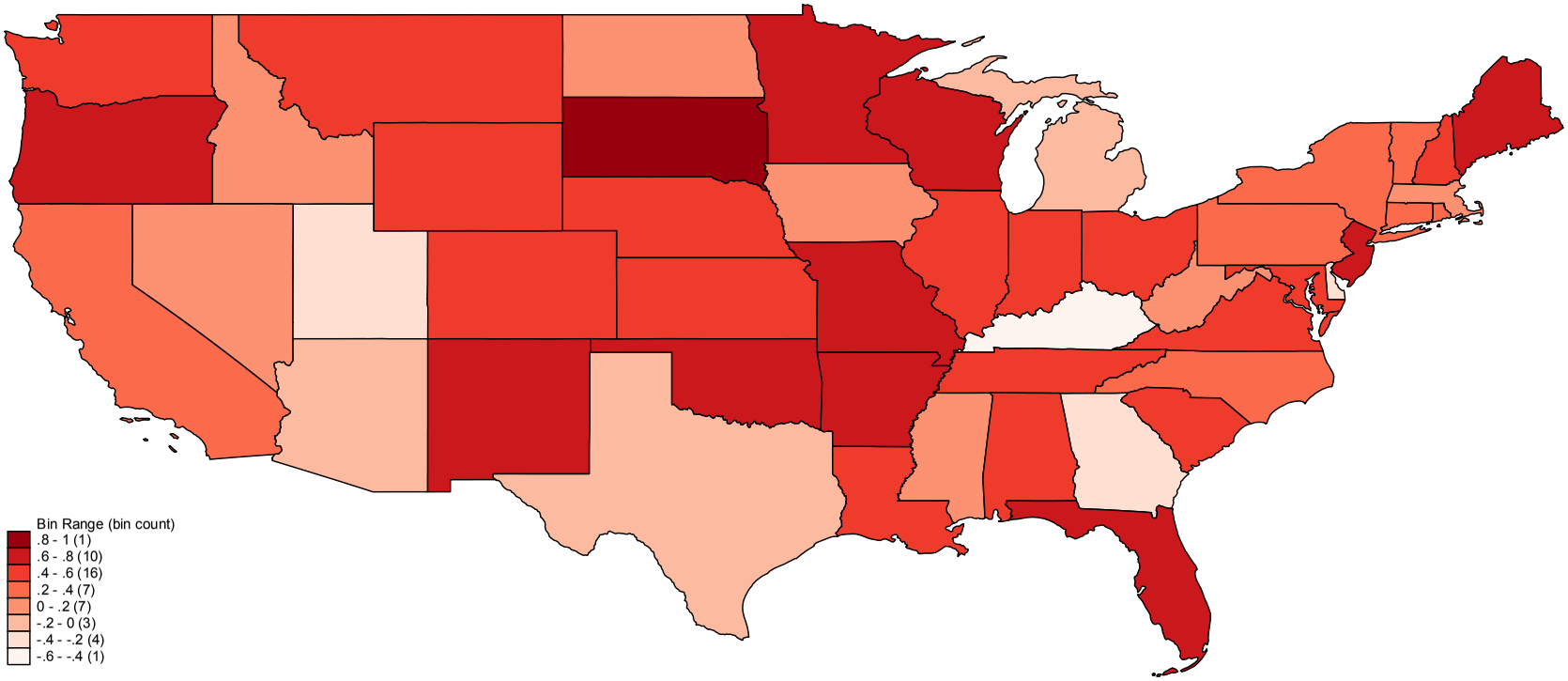

Figure 3: State-level Partial Correlations between Google Search Intensity for 2nd Amendment and NICS Background Checks

Tables

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Firearm Suicide Rate	7.32	3.01	1.37	17.70	500
Non-Firearm Suicide Rate	6.31	1.56	3.43	12.50	500
Gun Ban Search Intensity	8.87	6.76	0	38.83	500
Second Amendment Search Intensity	18.47	9.31	0	65.75	500
27th Amendment Search Intensity	16.64	13.48	0	90.17	410
Psychiatrist Search Intensity	46.13	13.79	8.83	83.17	500
20 or More Days "Not Good Mental Health" Share	0.076	0.015	0.041	0.133	499
Population	6,086,456	6,707,250	509,106	38,332,521	500
Unemployment Rate	6.3	2.2	2.5	13.8	500
Median Income ($)	54,230	8,350	37,173	78,632	500
Violent Crime Rate (per 100,000 residents)	384.7	158.9	87.7	789.9	500
% Veteran Population	8.3	1.4	4.5	11.1	500
% Young	9.4	0.9	7.2	13.5	500
% Black	10.2	9.2	0.3	36.4	500
% Prison	0.004	0.002	0.002	0.009	500

Table 2: OLS and FE Regression of Firearm Suicide Rates on Background Checks and Controls

	(1)	(2)	(3)	(4)
	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate
Log Gun Background Checks Per Capita	0.0612***	0.0630***	0.0597***	0.0405**
	(0.0200)	(0.0215)	(0.0215)	(0.0170)
Log Population		-0.351	-0.362	-0.176
		(0.251)	(0.307)	(0.223)
Unemployment Rate			-0.0000367	0.000134
			(0.00251)	(0.00211)
Median Income			-0.00000318	-0.00000300
			(0.00000279)	(0.00000194)
Violent Crime Rate			-0.000101	-0.000100
			(0.000135)	(0.0000995)
% Veteran Population			3.513*	3.415**
			(1.969)	(1.492)
% Young			0.00664	-0.000238
			(0.00537)	(0.00506)
% Black			-0.00141	-0.00609
			(0.0139)	(0.00898)
% Prison			12.33	28.86**
			(20.40)	(13.31)
Psychiatrist Search Intensity			0.000652	0.000369
			(0.000468)	(0.000274)
Instruments	None	None	None	None
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster
Weight	Unweighted	Unweighted	Unweighted	Weighted by Population
Observations	451	451	451	451

Standard errors in parentheses. All models presented in this table include both state fixed effects and a linear time trend.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 3: First Stage Regression of Background Checks on Instruments and Controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Gun Background Checks Per Capita	Log Gun Background Checks Per Capita	Log Gun Background Checks Per Capita	Log Gun Background Checks Per Capita	Log Gun Background Checks Per Capita	Log Gun Background Checks Per Capita
Log Second Amendment Search Intensity	0.111***	0.113***	0.118***	0.142***		0.0831**
	(0.0288)	(0.0263)	(0.0245)	(0.0220)		(0.0298)
Log Gun Ban Search Intensity					0.0572***	0.0321*
					(0.0143)	(0.0171)
Log Population		0.677	0.784	0.207	0.774	0.850
		(1.185)	(1.022)	(0.881)	(1.000)	(0.998)
Unemployment Rate			-0.0136	-0.0214***	-0.0113	-0.0117
			(0.00895)	(0.00615)	(0.00848)	(0.00842)
Median Income			-0.00000875**	-0.00000700*	-0.00000847**	-0.00000853**
			(0.00000390)	(0.00000403)	(0.00000379)	(0.00000386)
Violent Crime Rate			0.000426	0.0000232	0.000268	0.000416
			(0.000281)	(0.000363)	(0.000278)	(0.000279)
% Veteran Population			-0.0687	-0.275	-1.107	-0.920
			(2.799)	(4.268)	(3.006)	(2.822)
% Young			-0.00986	-0.0112	-0.00802	-0.00908
			(0.0175)	(0.0132)	(0.0176)	(0.0176)
% Black			-0.00221	-0.00738	0.00377	-0.00335
			(0.0231)	(0.0233)	(0.0214)	(0.0218)
% Prison			-39.62	37.37	-35.96	-29.53
			(51.06)	(43.27)	(47.70)	(49.38)
Psychiatrist Search Intensity			0.0000476	-0.000483	0.000660	0.000202
			(0.00102)	(0.000795)	(0.000994)	(0.00104)
Excluded Instruments F	14.41	17.85	22.26	39.61	15.30	12.89
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster
Weight	Unweighted	Unweighted	Unweighted	Weighted by Population	Unweighted	Unweighted
Observations	451	451	451	451	451	451

Standard errors in parentheses. All models presented in this table include state fixed effects and a linear time trend.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 4: FEIV Regression of Firearm Suicide Rates on Background Checks and Controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate
Log Gun Background Checks Per Capita	0.382***	0.367***	0.314**	0.226***	0.257**	0.291**
	(0.140)	(0.131)	(0.136)	(0.0831)	(0.125)	(0.118)
Log Population		-0.514	-0.491	-0.162	-0.462*	-0.479*
		(0.353)	(0.305)	(0.292)	(0.272)	(0.289)
Unemployment Rate			0.00409	0.00459	0.00317	0.00372
			(0.00372)	(0.00355)	(0.00318)	(0.00336)
Median Income			-0.000000900	-0.00000146	-0.00000141	-0.00000111
			(0.00000312)	(0.00000220)	(0.00000311)	(0.00000308)
Violent Crime Rate			-0.000133	-0.0000429	-0.000126	-0.000130
			(0.000132)	(0.000110)	(0.000129)	(0.000131)
% Veteran Population			3.271	3.060**	3.325	3.293
			(2.267)	(1.543)	(2.185)	(2.232)
% Young			0.00891	0.00124	0.00840	0.00870
			(0.00643)	(0.00556)	(0.00594)	(0.00620)
% Black			-0.00518	-0.00911	-0.00434	-0.00484
			(0.0162)	(0.00909)	(0.0157)	(0.0160)
% Prison			30.15	32.43**	26.16	28.54
			(26.36)	(13.86)	(24.97)	(25.45)
Psychiatrist Search Intensity			0.000458	0.000457	0.000501	0.000475
			(0.000559)	(0.000369)	(0.000527)	(0.000545)
Instruments	2nd Amendment	2nd Amendment	2nd Amendment	2nd Amendment	Gun Ban	2nd Amendment, Gun Ban
Excluded Instruments F	14.41	17.85	22.26	39.61	15.30	12.89
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster
Weight	Unweighted	Unweighted	Unweighted	Weighted by Population	Unweighted	Unweighted
Observations	451	451	451	451	451	451

Standard errors in parentheses. All models presented in this table include state fixed effects and linear time trends.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 5: Second Stage FEIV Regression of Non-Firearm Suicide Rates on Per Capita Gun Background Checks Search Intensity ent Instruments and Controls

	(1)	(2)	(3)	(4)	(5)
	Log Non-Firearm Suicide Rate	Log Non-Firearm Suicide Rate	Log Non-Firearm Suicide Rate	Log Non-Firearm Suicide Rate	Log Non-Firearm Suicide Rate
Log Gun Background Checks Per Capita	-0.0917	-0.104	-0.0155	0.0250	-0.123
	(0.139)	(0.134)	(0.140)	(0.0830)	(0.152)
Log Population		-0.404	-0.395	-0.464	-0.340
		(0.379)	(0.346)	(0.294)	(0.409)
Unemployment Rate			0.00543	0.00545*	0.00368
			(0.00453)	(0.00287)	(0.00454)
Median Income			-0.00000289	-0.00000287	-0.00000386
			(0.00000339)	(0.00000180)	(0.00000363)
Violent Crime Rate			0.000476****	0.000422****	0.000489****
			(0.000132)	(0.000123)	(0.000143)
% Veteran Population			0.957	1.150	1.059
			(2.490)	(1.705)	(2.462)
% Young			0.00463	0.00989**	0.00367
			(0.00703)	(0.00478)	(0.00722)
% Black			-0.00656	-0.0227**	-0.00496
			(0.0149)	(0.0108)	(0.0154)
% Prison			-26.19	-0.668	-33.73
			(23.38)	(22.66)	(26.28)
Psychiatrist Search Intensity			0.00000112	-0.000210	0.0000834
			(0.000395)	(0.000345)	(0.000413)
Instruments	2nd Amendment	2nd Amendment	2nd Amendment	2nd Amendment	2nd Amendment, Gun Ban
Excluded Instruments F	14.41	17.85	22.26	39.61	12.89
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster
Weight	Unweighted	Unweighted	Unweighted	Weighted by Population	Unweighted
Observations	451	451	451	451	451

Standard errors in parentheses. All models presented in this table include state fixed effects and a linear time trend.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 6: Instrument Validity Tests

	(1)	(2)	(3)	(4)
	Log Gun Background Checks Per Capita	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate
Log 27th Amendment Search Intensity	0.0222			
	(0.0205)			
Log Gun Background Checks Per Capita		0.355	0.294**	0.291**
		(0.498)	(0.120)	(0.118)
Log Population	0.428	-0.490	-0.481*	-0.479*
	(0.957)	(0.337)	(0.291)	(0.289)
Unemployment Rate	-0.0166	0.00393	0.00376	0.00372
	(0.0101)	(0.0104)	(0.00338)	(0.00336)
Median Income	-0.0000115**	0.000000612	-0.00000108	-0.00000111
	(0.00000502)	(0.00000665)	(0.00000309)	(0.00000308)
Violent Crime Rate	0.000273	-0.000113	-0.000131	-0.000130
	(0.000378)	(0.000219)	(0.000131)	(0.000131)
% Veteran Population	3.932	1.943	3.291	3.293
	(5.148)	(2.524)	(2.237)	(2.232)
% Young	-0.00236	-0.000805	0.00872	0.00870
	(0.0191)	(0.00796)	(0.00623)	(0.00620)
% Black	0.00681	-0.00476	-0.00488	-0.00484
	(0.0263)	(0.0143)	(0.0160)	(0.0160)
% Prison	-49.80	37.14	28.72	28.54
	(63.75)	(41.15)	(25.55)	(25.45)
Psychiatrist Search Intensity	-0.000135	0.000715	0.000473	0.000475
	(0.00149)	(0.000605)	(0.000546)	(0.000545)
Estimation Method	OLS	FEIV	LIML	FEIV
Instruments	First Stage	27th Amendment	Gun Ban, 2nd Amendment	Gun Ban, 2nd Amendment
Excluded Instruments F		1.175	12.89	12.89
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster
Weight	Unweighted	Unweighted	Unweighted	Unweighted
Observations	389	389	451	451

Standard errors in parentheses. Column (4) is a duplicate of our preferred specification in Table (4). All models presented in this table include state fixed effects and linear time trends.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 7: Model Robustness Tests

	(1) Log Firearm Suicide Rate	(2) Log Firearm Suicide Rate	(3) Log Firearm Suicide Rate	(4) Log Firearm Suicide Rate	(5) Log Firearm Suicide Rate
Log Gun Background Checks Per Capita	0.278**	0.273**	0.259**	0.253***	0.275**
	(0.115)	(0.127)	(0.124)	(0.0968)	(0.114)
Log Population	-0.448	-0.475*	-0.447	-0.0380	-0.461
	(0.281)	(0.288)	(0.279)	(0.686)	(0.284)
Unemployment Rate	-0.000299	0.00354	-0.000261	0.00561	0.00132
	(0.00376)	(0.00334)	(0.00397)	(0.00388)	(0.00343)
Median Income	-0.00000123	-0.00000143	-0.00000157	-0.00000346	-0.000000659
	(0.00000300)	(0.00000338)	(0.00000330)	(0.00000276)	(0.00000303)
Violent Crime Rate	-0.000149	-0.000136	-0.000157	-0.000209	-0.000131
	(0.000131)	(0.000127)	(0.000127)	(0.000198)	(0.000132)
% Veteran Population	3.122	3.861	3.697	2.883	3.135
	(2.186)	(2.434)	(2.395)	(2.503)	(2.206)
% Young	0.00862	0.00811	0.00797	0.0142**	0.0100
	(0.00611)	(0.00606)	(0.00594)	(0.00642)	(0.00636)
% Black	-0.00577	-0.00443	-0.00524	0.0183	-0.00520
	(0.0159)	(0.0152)	(0.0152)	(0.0228)	(0.0159)
% Prison	26.21	23.55	21.04	14.64	31.39
	(24.92)	(27.18)	(26.52)	(36.18)	(26.93)
Psychiatrist Search Intensity	0.000531			0.000470	0.000410
	(0.000531)			(0.000565)	(0.000533)
Lesbian Porn Search Intensity	-0.000616*		-0.000591	-0.000114	-0.000621*
	(0.000350)		(0.000367)	(0.000379)	(0.000354)
20 or More Days "Not Very Good" Share		1.617	1.645		
		(1.333)	(1.305)		
Instruments	2nd Amendment, Gun Ban	2nd Amendment, Gun Ban	2nd Amendment, Gun Ban	2nd Amendment, Gun Ban	2nd Amendment, Gun Ban
Excluded Instruments F	12.92	11.33	11.30	10.82	12.67
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster
Weight	Unweighted	Unweighted	Unweighted	Unweighted	Unweighted (Dropped Kentucky)
Observations	451	450	450	451	442

Standard errors in parentheses. All models presented in this table include state fixed effects and linear time trends. Column 4 includes a state specific linear time trend.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 8: IV and FEIV Regressions of Log Firearm Suicide Rates on Log Gun Background Checks w/ Differing Standard Error Assumptions

	(1)	(2)	(3)	(4)	(5)
	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate	Log Firearm Suicide Rate
Log Gun Background Checks Per Capita	0.291***	0.291**	0.291****	0.291**	0.291**
	(0.111)	(0.113)	(0.0448)	(0.129)	(0.136)
Log Population	-0.479*	-0.479*	-0.479	-0.479	-0.479
	(0.272)	(0.282)	(0.375)	(0.322)	(0.375)
Unemployment Rate	0.00372	0.00372	0.00372	0.00372	0.00372
	(0.00383)	(0.00341)	(0.00410)	(0.00341)	(0.00378)
Median Income	-0.00000111	-0.00000111	-0.00000111	-0.00000111	-0.00000111
	(0.00000226)	(0.00000255)	(0.00000111)	(0.00000345)	(0.00000328)
Violent Crime Rate	-0.000130	-0.000130	-0.000130	-0.000130	-0.000130
	(0.000135)	(0.000133)	(0.0000918)	(0.000149)	(0.000150)
% Veteran Population	3.293**	3.293	3.293	3.293	3.293
	(1.654)	(2.044)	(2.832)	(2.678)	(2.388)
% Young	0.00870	0.00870	0.00870*	0.00870	0.00870
	(0.00607)	(0.00654)	(0.00522)	(0.00621)	(0.00728)
% Black	-0.00484	-0.00484	-0.00484	-0.00484	-0.00484
	(0.0139)	(0.0136)	(0.0156)	(0.0178)	(0.0187)
% Prison	28.54	28.54	28.54*	28.54	28.54
	(22.00)	(22.50)	(14.58)	(28.25)	(27.87)
Psychiatrist Search Intensity	0.000475	0.000475	0.000475	0.000475	0.000475
	(0.000541)	(0.000545)	(0.000407)	(0.000559)	(0.000589)
Std. Error	Conventional	Huber-White	Region Cluster	Jackknife	Bootstrap
Instruments	2nd Amendment, Gun Ban	2nd Amendment, Gun Ban	2nd Amendment, Gun Ban	2nd Amendment, Gun Ban	2nd Amendment, Gun Ban
Observations	451	451	451	451	451

Standard errors in parentheses. All models presented in this table include state fixed effects and linear time trends.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$