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Guns and Fatal Police Shootings: Accuracy of Firearm Prevalence Proxies in a Panel Data Analysis

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Abstract

This paper explores the relationship between gun prevalence and fatal police shootings in the United States. Specifically, the study assesses the validity of using FSS (Firearm Suicides Per Total Suicides) as a proxy for gun prevalence. It examines whether FBI background check data can be a more reliable alternative. Through a panel data analysis, the study provides moderate evidence that FBI background check data performs better than FSS as a proxy for gun prevalence and that the relationship between firearm prevalence and fatal police shootings is positive. Additionally, the study finds that instrumenting FSS with background check data to account for measurement error in gun prevalence measures is ineffective. The research highlights the role of crime in fatal police shootings and calls for future studies to develop better proxies for gun prevalence.

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Introduction

The escalating frequency and severity of mass shootings in the United States have thrust the issue of firearm control legislation to the forefront of the American political discourse in recent years. While the United States has a higher rate of firearm deaths[1] and firearm deaths by police officers[2] than any other comparable developed nation, pundits and policymakers disagree on how to best resolve the issue. Furthermore, the United States exhibits the highest rate of firearm possession in the world; among other reasons, this is due to the deep historical roots of firearms in American society and an abundance of game-hunting land[3]. Ingrained in the culture so deeply, the prospect of restricting access to firearms seems to cause turmoil among many folks across the United States. Countless studies have tried to see if directing policy to restrict the possession of firearms will decrease violence or if it will do the opposite. With thousands of people dying yearly due to firearm-related injuries[4], and countless more being impacted by proximity, unbiased steps must be taken to arrive at the appropriate, optimal solution.

Simplistically, there are three ways in which one could argue in the deliberation over what to do with firearms. Presumably, one could say that there are not enough firearms so we should increase accessibility because doing so would improve outcomes. Those who believe that increasing the number of firearms in society may see them as a deterrence to crime and therefore police shootings. One could also argue the opposite- that we have too many firearms prevalent in society, and by reducing this prevalence, we will reach a more optimal outcome. The last stance rejects both previous arguments and suggests that any change in firearm prevalence would move the United States away from an already optimal level. Regardless of the outcome being

[1] Pallin et al. (2019)
[2] Zimring (2017)
[3] Kennett and Anderson (1975)
[4] Pew Research Center

examined and how optimality is defined, it seems evident that the third argument - that we have reached an optimal level of firearms in society - is incorrect. No notable literature posits that the status quo is at an optimum. Furthermore, the frequency of fatal police shootings has grown to an all time high in recent years (see figure 6 in appendix). Therefore, it is crucial to examine how a change in firearm prevalence would lead to improved outcomes. The question then becomes: do we need more or fewer firearms in society, and how should they be optimally allocated?

Of course, the problem can be simplified even further to suggest that it does not matter as long as the firearms are in the hands of law-abiding citizens; however, for policymaking, it is far more challenging to manipulate the level of firearms that are being obtained illegally than legally, and therefore it is legal firearm prevalence that is of interest in most studies in the field. Given some exogenous levels of illegally obtained firearms, a policy can manipulate the level of legally obtained firearms to reach an optimum. If legal firearm prevalence has a negative relationship with crime but a positive relationship with fatal police shootings, illegally obtained firearms that are not being impacted by legislation are likely decreasing risk for criminals to commit crime and therefore forcing police into more situations where they have to use their gun. However, if legal firearm prevalence has a positive relationship with crime, the impact of criminal firearms can be dismissed because legal and illegal guns impact outcomes in the same direction, meaning that the true impact isn't obscured.

This study examines the relationship between legal firearm prevalence and fatal police shootings in the United States. If a decrease in firearm prevalence is associated with a lower frequency of fatal police shootings, this is likely due to the violence that firearms invite by emboldening criminals or increasing the stakes in otherwise nonfatal confrontations. In this vein, police may also be more prone to using their firearm in potentially dangerous situations.

Inversely, firearms may deter crime by increasing the risk associated with committing a crime. This paper seeks to add to the literature by employing a panel-data analysis to determine the relationship between firearm prevalence and fatal police shootings. I will test the use of multiple proxy variables for firearm prevalence, as no national data on firearm sales and resales are collected. Correcting measurement errors is essential because firearm ownership estimates must be correct to accurately address the impact of firearm prevalence on society. To correct potential measurement errors, I will employ a multiple indicator method which utilizes variance that is common between firearm prevalence proxies to eliminate unwanted noise in each of the individual proxies.

Results of my analysis suggest that background checks act as a more viable proxy for firearm prevalence than firearm suicides per total suicides in panel data. In the fixed effect regression, firearm prevalence measured by background checks is statistically significant while firearm prevalence measured by firearm suicides per total suicides is not. The random effects model shows the same trend and reinforces the hypothesis that more firearms leads to more fatal police shootings. While neither the random effects model nor the fixed effects models reflect the actual relationship of gun prevalence on fatal police shootings, on account of potential measurement error and endogeneity, the truth lies somewhere between the two. This paper also calls for better measurement of firearm prevalence.

Literature Review

There has been abundant literature dedicated to examining the effect of firearm ownership in the United States on crimes and firearm-related death. In 1998, Lott and Mustard (1998) proposed the more firearms, less crime hypothesis[5]. The hypothesis states that when there is a higher chance of coming across an armed citizen, a criminal will be less likely to commit the intended crime. The presence of firearms also allows citizens to protect themselves, their families, and their property, reducing the risk of becoming a victim of crime. This view suggests that higher levels of firearm ownership may be dominated by a so-called "deterrence effect," by which the risk of being harmed as an offender increases, leading to a decrease in violent crime. The policy implications of a theory like this are relatively straightforward: legally limiting access to firearms could ultimately result in more violent crime, as such a policy would inherently reduce at a greater rate the number of firearms that are in the hands of law-abiding citizens compared to less risk averse criminals. Responding to the Lott and Mustard (1998) hypothesis, many studies have attempted to revisit the actual social cost of firearms to uncover whether more is better, as Lott and Mustard (1998) suggested. Many findings contradict the original Lott and Mustard (1998) hypothesis[6].

According to Donohue et al. 2019, the "More firearms, Less Crime" hypothesis may have been a flawed study for several reasons[7], prompting the question if policymakers were right to consider a move toward the Right-to-Carry Laws (RTCs). Since the original Lott and Mustard (1998) hypothesis, most studies in support have used aggregate-level data and have yet to account for population or geographic differences. They also note that most studies have relied on

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[5] Lott and Mustard (1998)
[6] Donohue et. al (2019); Duggan (2001); Siegel Ross and King (2013)
[7] Donohue et. al (2019)

cross-sectional or time-series designs, which may need to be revised to account for potential confounding variables. Using panel data from the Uniform Crime Reports and the National Incident-Based Reporting System, as well as a state-level synthetic control analysis, Donohue et al. 2019 find that RTC laws are associated with an increase in overall violent crime rates and that the effects of RTC laws vary depending on the type of crime and the characteristics of the state. This finding, in contrast with the findings of Lott and Mustard (1998), points toward the potential for a "substitution effect," whereby the decrease in violent crime due to the deterrence effect is counteracted by an increase in violent crime due to the increased availability of firearms.

Speculatively, with more firearms in circulation, there is a higher likelihood that a confrontation that would otherwise be nonfatal has elevated stakes when firearms are involved. This may result in more firearm homicides in the data. However, the findings on the topic are mixed. Kovandzic, Schaffer, and G. Kleck. 2013 cite the deterrence effect to suggest increased firearm ownership decreases homicide rates. Yet other studies (Duggan 2001; Siegel Ross and King 2013) argue that increased firearm ownership rates lead to increases in homicide rates. Concerning other outcomes such as property crimes which include burglary, theft, larceny, or motor vehicle theft, among other crimes, the impact of firearm ownership is not clear. Some studies suggest that keeping a firearm at home will likely not provide a positive externality through burglary deterrence because the value a firearm may possess as loot offsets its deterrence effect, which could even engender a higher burglary rate altogether [8].

Moreover, other studies have suggested that firearm ownership may increase suicide rates or unintentional shootings[9]. Bangalore and Messerli (2013) conducted a study to evaluate the relationship between firearm ownership rates and firearm-related deaths in the United States. The

[8] Cook and Ludwig (2002)
[9] Ludwig and Cook (2000); Duggan (2002)

authors found that states with higher levels of firearm ownership had higher rates of firearm-related deaths[10]. They also found that the effects of firearm ownership on firearm-related deaths were more significant than those of other factors such as poverty, unemployment, and mental illness. This directly opposes the more firearms, less crime hypothesis that Lott and Mustard (1998) put forth.

Police Shootings as the Outcome of Interest

Turning to police shootings, fatalities at the hands of officers have been at the forefront of national attention recently, prompting a growing demand for police reform. Considering the high rate of police shootings in the United States, it is not surprising that firearms also play a central role in this political debate. For law enforcement officers in the United States, the increased likelihood of confrontation with an armed suspect may make them more rash in their decision-making, even if a suspect is not armed. This is consistent with the data, as police officers in the U.S. encounter a significantly greater likelihood of being killed by assault while on duty, with over 90% of these fatal attacks involving firearms[11].

The topic has garnered increasing interest recently with high-profile incidents such as the murders of George Floyd, Eric Garner, and Breonna Taylor, all amplifying the public's outcry for a systematic change. According to Kivisto et al. 2017, states in the U.S. with more stringent firearms laws have lower rates of fatal police shootings[12]. Additionally, numerous other studies have suggested that the relationship between firearms prevalence and fatal police shootings is positive[13]. These results, however, have been based upon cross-sectional analysis, which, despite

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[10] Bangalore and Messerli (2013)
[11] Zimring (2017)
[12] Kivisto, Ray and Phalen (2017)
[13] Hemingway et al. (2018); Nagin (2020); Shepphard et al. (2021)

a parsimonious set of control variables, still may suffer from an omitted variable problem. Specific state characteristics such as political affiliation, education, or demographic makeup could affect both firearm prevalence and frequency of fatal encounters with police. Moreover, cultural differences between states may drive the relationship between the level of firearm regulation and rates of police killings. Consequently, Rogna and Nguyen (2022) attempts to aid the shortcoming of previous research by employing a panel data analysis to determine more accurately causality from correlation. The study finds a negative relationship between higher firearm regulation and fatal police shootings, consistent with previous findings confirming the hypothesis that the use of fatal force by police officers is positively linked to the degree of danger which they encounter. However, the study also finds no significant effect of firearm prevalence on fatal police shootings. This puzzling finding could signify that it is ostensibly a state's laws and attitudes toward firearms that impact the level of police shootings, but the lowering of firearm prevalence itself isn't what is important. Specifically, it could be the case that states which regulate firearms more strictly also produce police departments that are less prone to firearm violence. Alternatively, the insignificant results could result from measurement error in the proxy used for firearm prevalence. The coefficients could either be biased to zero in the presence of a noisy indicator, or insignificant due to lack of temporal variance in a fixed effect model. *A priori*, it is hard to say what is driving the results. But, if legislation intended to restrict the availability of firearms significantly reduces fatal police shootings, it is reasonable to assume this reduction operates through the legislation's impact on firearm availability.

Lacking federal-level data on firearm ownership rates, all previous studies have resorted to using proxy measures to estimate firearm prevalence. The most commonly used proxy has been the fraction of firearm-related suicides over the total number of suicides (FSS). Other commonly used proxy variables to measure firearm ownership are background checks from the FBI, the fraction of firearm-related homicides over the total number of homicides, hunting license rates obtained from the National Fish and Wildlife Service, and subscriptions to Guns and Ammo magazine. While some studies have concluded that the FSS proxy most closely estimates actual firearm ownership rates, others raise concerns over its accuracy and call for the potential use of other measures in future studies[14].

All of the literature that has studied the impact of firearm prevalence on fatal police shootings with cross-sectional data has found a significant relationship between the two variables (Hemmingway et al. 2018; Nagin 2020; Shepphard, Zimmerman, Fridel 2021; Zimring 2017). Yet none of these studies have used panel data or diverted from using FSS to measure firearm prevalence. This analysis will add to the existing literature by exploring the use of background check data as a proxy for firearm prevalence in panel data. This is a direct extension to the results generated by Rogna and Nguyen (2022), the first panel-data analysis examining the impact of firearm prevalence on fatal police shootings.

Data and Methods

This analysis will attempt to build off previous research questions examining the causal mechanism underlying the relationship between firearm prevalence and fatal police shootings. Specifically, this paper will conduct a panel data analysis with each of the 50 United States as observations, with yearly aggregated data from January 1, 2012, to December 31, 2018. The data is therefore split into 50 categories across seven time periods. The primary focal point of this analysis will be to assess the differences between using FSS as a proxy for firearm ownership and using background checks (BRC) as a proxy for firearm ownership. According to Rogna and

[14] Kleck (2004); Wilbur and Kim (2022)

Nguyen (2022), the only other panel data analysis of this field of research up until this time, the level of firearm prevalence does not play a significant causal role in the number of fatal police shootings. Using BRC in place of FSS, this analysis will challenge the results mentioned above to determine whether the choice of the FSS proxy is driving these results. Other papers have used BRC data to capture the demand for firearms; however, it is essential to note that this data cannot be interpreted as a one-to-one correspondence with firearm purchases[15]. The metric reflects the total count of background checks sought rather than the number of background checks passed or the number of people who applied for background checks[16]. This is because one person could account for multiple FBI background checks in a single state/month. Additionally, the FBI data count checks that do not lead to firearm purchases alongside those that do lead to firearm purchases. Nonetheless, background checks are a strong indicator of overall firearm sales[17].

Mimicking the methodology of Rogna and Nguyen 2022 closely, this analysis will first report a baseline set of results using FSS to measure firearm prevalence. After the baseline results have been produced, background check data will be substituted into the model.

I collect data from six sources. The dependent variable, the number of fatal police shootings per million people (*pol_shoot*), is retrieved from the Fatal Encounters database. While the FBI maintains a database of police-related fatalities, it suffers from widespread underreporting, which makes it inadequate for econometric use[18]. Therefore, a privately operated database, namely Fatal Encounters, is preferred. Furthermore, to closely match preliminary results from Rogna and Nguyen (2022), it is advantageous to collect data from the same source when possible.

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[15] Briggs and Tabarrok (2014); Vitt et al. (2018); Pak, Tae-Young (2022)
[16] Firearms Checks (Nics)." FBI, 3 May 2016
[17] Kim and Wilbur (2022)
[18] Williams, Bowman and Jung (2019)

Control variables are obtained from various sources. From the United States Census Bureau, the poverty rate (*poverty*), per capita income in 2010 income-adjusted dollars (*pc_income*), Unemployment rate (*unemployment*), the percentage of adults with educational attainment lower than a high school diploma (*education*), the percentage of White people (*white*), and the percentage of young population, precisely 18-34 (*young*) are obtained. While previous research has also controlled for the percentage of people living in urban areas (*Urban*), this analysis treats it as time-invariant due to a lack of data. However, this will not be of concern in fixed effects models, where time-invariant state-specific effects are omitted. In the random effects model however, any effect that *Urban* may have will be a source of concern. All controls are included to avoid so-called "backdoor paths" with which the dependent and independent variables may be correlated.

Other controls are received from the FBI. The FBI's crime data explorer provides data on violent crime per million people (*crime)*. Violent crime is considered anything that falls in the category of rape, murder, aggravated assault, or robbery. This variable is included to control for ecological conditions that may play a role in determining the level of police involvement in a given state[19]. Also from the FBI, the number of background checks is retrieved from their National Instant Criminal Background Check System (NICS). Specifically, the sum of numbers from the "hand firearms", "long firearms", "other firearms", and "multiple" rows were taken. Missing or incorrect data for the other variables caused their values to be dropped. FSS proxy data is retrieved from the CDC's web based injury statistics query and reporting system (WISQARS).

With regards to legislative strength, in order to capture the general strength of firearm legislation of states, data is collected from the Giffords Law Center to Prevent firearm Violence.

[19] Zimring (2017)

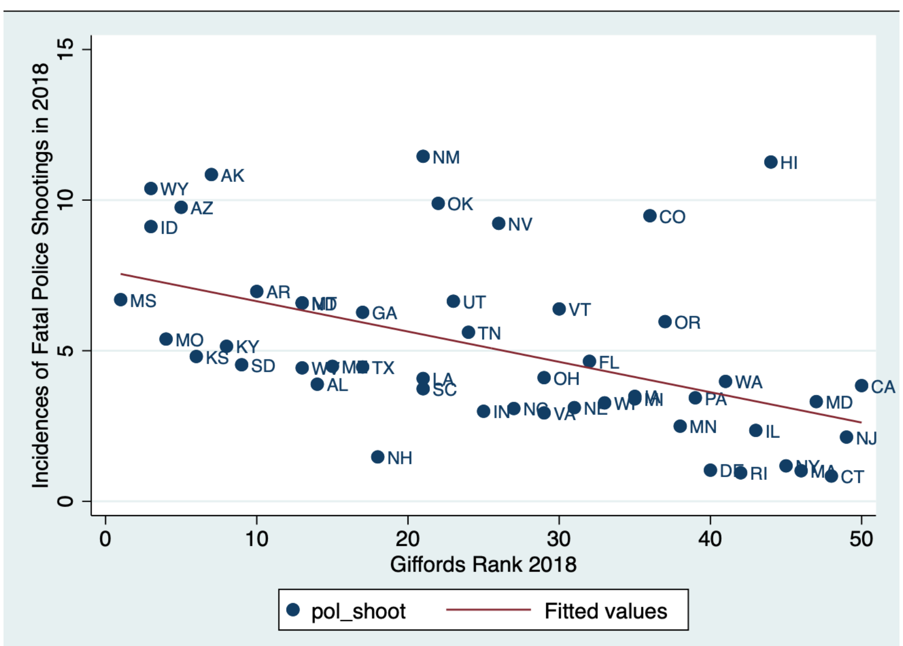
The Giffords organization publishes an annual composite scorecard which ranks all states based on the strength of their firearm legislation. The scorecard's final grade is the result of a composite score, made up from seven categories. These categories are: background checks and access to firearms (*BCAF*), other regulations of sales and transfers (*ORST*), classes of weapons and ammunitions/magazines (*CWAM*), consumers and child safety (*CCS*), firearm owner accountability (*GOA*), firearms in public places (*FPP*) and a class for other categories (*OTH*) which leaves room for other measures such as local legal schemes or microstamping technology requirements. There are other scorecard organizations which publish similar data, all highly correlated with each other. While Giffords spans the longest time period (2010-2023) which proves advantageous, the specific choice of scorecard should not impact the findings.

Importantly, disaggregated Giffords scorecard data is not accessible to the public unlike the other databases used thus far. However, data regarding where each state was ranked with respect to other states each year is available. Hence, this analysis will employ these rank scores (one to fifty) for each state for each year as a simplified version of the representational disaggregated composite Giffords score. In addition, because the effects of firearm legislative strength may take time to be observed, the scores will be introduced with one-year lags. While the effect of legislation may take longer to be observed, the limited number of years available to this study restricts the inclusion of longer time lags. This creates a limitation of this study, namely that both disaggregating the scorecards into each of their seven categories for further analysis and introducing lags for multiple years is not possible.

I will first employ a fixed effects model to examine the effect that firearm prevalence as measured by FSS has on fatal police shootings. The second portion of my analysis will hold fixed the model but substitute the background check proxy into the model in favor of FSS. Next, the same two sets of control variables will be used in a random effects model. Finally, I will endogenize the independent variable by performing a multiple indicator method with 2SLS, with FSS and BRC as the endogenous and instrumental variables respectively. Table 3 in the appendix reports the key statistics for variables under consideration.

The descriptive statistics match with expected values from prior research[20] that included the same set of variables, except for *pol_shoot* which differs by 0.3 standard deviations from previous literature and has a slightly higher mean value (+ 0.6). This should not affect the significance of analysis. Because Giffords rank scores are distributed from one to fifty on a yearly basis, the key descriptors are not useful to include, and are relatively straightforward. Figure 1 shows the relationship between Giffords rank score and police shooting for the year 2018. Figure 2 shows the relationship between BRC and fatal police shootings for the year 2018.

Figure 1 - Scatter plot of Giffords rank and fatal police shootings



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20 Rogna and Nguyen (2022)

Figure 1 shows that there is a negative correlation between Giffords ranks and incidences of fatal police shootings, which suggests that there is still an effect of the rank scores despite limitations to accessing true Giffords score.

Figure 2 - Scatter plot of Giffords rank and fatal police shootings

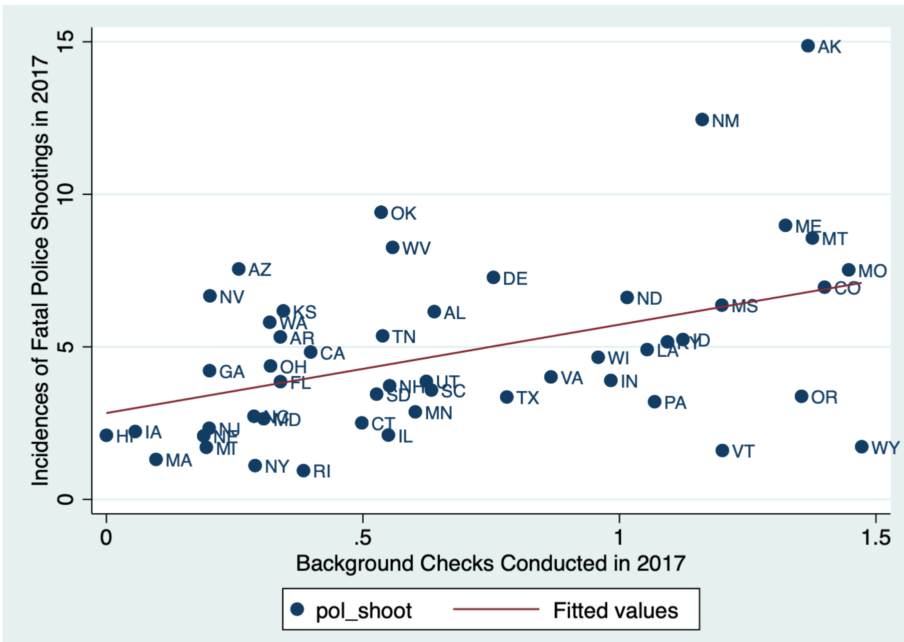


Figure 2 shows that there is a positive correlation between BRC in 2017 and police shootings in 2017. It indicates that more firearms demanded through background checks is correlated with higher levels of fatal police shooting, as predicted by previous findings. The fixed effect model is given by equation (1) where Y_{it} indicates the outcome of interest, *pol_shoot*, in entity *i* at time *t*. Theta and Phi represent the two way dummies to maintain the fixed effects.

$$Y_{it} = \sum_{j=1}^{k} \beta_j X_{itj} + \Theta_i + \Phi_t + \varepsilon_{it} \tag{1}$$

The random effect model takes the same form as specified by equation (1), where all of the variables are the same as in the fixed effect model. Importantly, the random effects model assumes that the observed predictors of fatal police shootings are not correlated with unobserved time invariant state specific characteristics that may be included in the error term, whereas the fixed effect model loosens this assumption.

Adjusting for Measurement Error Bias

As mentioned before, a potential limitation of previous research has been its reliance on FSS as a proxy measure for firearm prevalence. While the measure may indicate to some degree the availability of firearms in a given state, prior literature does not provide a theoretical framework for how or why legal firearm prevalence and suicide data are connected. Furthermore, it has been suggested that FBI background checks may offer a better measure for legal firearm prevalence than suicide-based proxies[21]. However, none of the available proxies are without weaknesses, and will all provide a somewhat imprecise measure of firearm prevalence. In order to adjust for the noise in each of them and obtain a more pure measure of firearm prevalence, I apply a multiple indicator method by utilizing both FSS and FBI background checks.

To illustrate the measurement error more formally, consider the true relationship that cannot be observed:

$$C = \beta_0 + \beta_1 X + \varepsilon \tag{2}$$

where C denotes the number of police shootings and X is the unobservable true level of firearm prevalence, which is mapped to C through β_1. However, instead of X, it is a noisy indicator variable, namely FSS, that is used to estimate C. Assuming a linear relationship, the indicator variable, *x*, can be mapped to the true X as follows:

$$X = b_0 + b_1 x + \epsilon \tag{3}$$

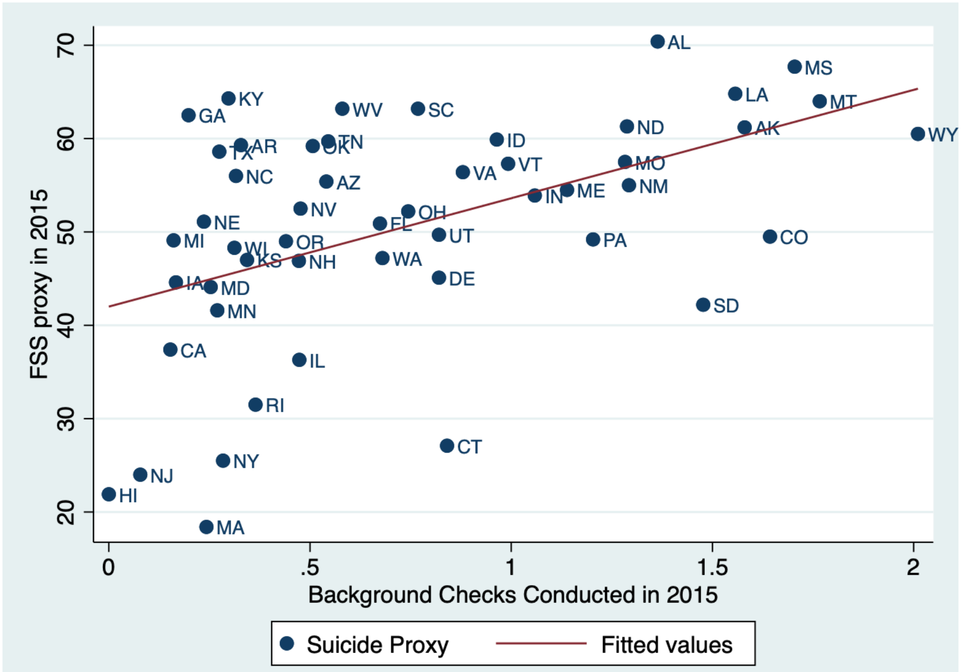
[21] Kim and Wilbur (2022)

Necessarily, b_1 will be equivalent to $\beta_1[1-\sigma_v/(\sigma_X+\sigma_v)]$, where σ_v and σ_X denote the variances of *x* and X, respectively. The consequence of this is that the addition of measurement error from *x* will bias the coefficient to zero. To mitigate the bias that is introduced through measurement error, extensive literature has formulated relevant methodology[22]. It is difficult to say whether or not potential measurement error is enough to bias the results significantly. Yet the contradicting results which previous literature has produced suggests that the measurement error problem could be a useful place to start.

In my analysis I will take advantage of the methods used in previous studies to adjust for measurement error. Specifically, in addition to *x*, another proxy *y* is included in the model. This IV framework is only used to adjust for empirical errors in the relationship between firearm prevalence and fatal police shootings, as opposed to causal effects which are often examined by IV methods. Specifically, the IV framework will reveal more accurately the degree to which firearm prevalence is correlated with fatal encounters. However, in order for the multiple indicator method to work in this case, it must be that there is correlation between FSS and background checks that emerges only from their shared dependence on the true firearm prevalence. Figure 3 shows the first-stage relationship between these two variables. Not surprisingly, FSS is positively correlated with background checks. I argue that this correlation is indeed only due to shared dependence on firearm prevalence.

[22] Bjerk (2006)

Figure 3 - First stage relationship between firearm prevalence proxies



By instrumenting FSS on background checks, we can obtain a more accurate measure for β_1 in equation 2 by mitigating the noise that each one of the indicators experiences on their own. However, the use of BRC as an instrument for FSS is not intended to correct for endogeneity, but rather measurement error. It is common practice to make use of an instrumental variable as a sort of natural experiment to mitigate endogeneity. If an instrument is able to manipulate the independent variable orthogonally to the error term, then the method acts like a randomized control trial which can eliminate confounders. BRC as an instrument for FSS is not one of these cases. BRC may be related to unobservable state specific characteristics, for example it is likely that the culture of a state influences the demand for firearms. The IV framework here does not account for this endogeneity.

Results

The preliminary regression shows the baseline results for the panel data analysis similar to that performed in previous studies[23], using the FSS proxy as a measure for firearm prevalence. Both a two way fixed effects model and a random effects model are used. Because different states may have individual characteristics such as cultural attitude toward firearms, the fixed effects model is used to eliminate these potential confounders. Time fixed effects are also added to control for potential trends across all groups over time.

In models one and two of table 1, firearm prevalence is proxied by the suicide proxy (*suicideproxy*). In models three and four, this proxy is substituted for the background check proxy (*pc_brc*).

[23] Rogna and Nguyen (2022)

Table 1 - Regression results for Fixed Effects and Random Effects models

VARIABLES	(4) FE FSS	(5) RE FSS	(8) FE BRC	(9) RE BRC
suicideproxy	-0.00975	0.0206		
	(0.0251)	(0.0174)		
pc_brc			0.425*	0.603***
			(0.246)	(0.232)
young	-0.277	-0.0414	-0.283	-0.0317
	(0.192)	(0.125)	(0.191)	(0.122)
white	-0.00616	0.0107	0.0131	0.00661
	(0.153)	(0.0175)	(0.153)	(0.0170)
dummy2017	-0.502*	-0.484	-0.514*	-0.506*
	(0.298)	(0.297)	(0.296)	(0.296)
dummy2016	-0.947***	-0.962***	-1.026***	-1.044***
	(0.327)	(0.320)	(0.327)	(0.320)
dummy2015	-1.639***	-1.671***	-1.678***	-1.753***
	(0.374)	(0.347)	(0.372)	(0.345)
dummy2014	-1.755***	-1.858***	-1.790***	-1.921***
	(0.460)	(0.409)	(0.458)	(0.407)
dummy2013	-2.076***	-2.217***	-2.175***	-2.304***
	(0.566)	(0.499)	(0.565)	(0.497)
dummy2012	-2.501***	-2.616***	-2.590***	-2.692***
	(0.657)	(0.579)	(0.656)	(0.576)
Unemployment	0.293	0.302	0.299	0.294
	(0.210)	(0.187)	(0.209)	(0.185)
Poverty	-0.204*	-0.192*	-0.190*	-0.170*
	(0.108)	(0.0986)	(0.107)	(0.0973)
Pc_income	-8.42e-05**	-9.81e-05**	-8.27e-05*	-0.000101***
	(4.23e-05)	(3.96e-05)	(4.21e-05)	(3.91e-05)
LowEduc	0.159	0.0778	0.145	0.0702
	(0.102)	(0.0848)	(0.101)	(0.0836)
Crime	0.000968***	0.000771***	0.000975***	0.000756***
	(0.000293)	(0.000142)	(0.000290)	(0.000138)
Rank	0.0155	-0.0333**	0.0158	-0.0366***
	(0.0237)	(0.0154)	(0.0236)	(0.0135)
stateID				
Constant	10.55	5.435	8.361	6.316*
	(12.18)	(3.968)	(12.15)	(3.778)
Observations	350	350	350	350
R-squared	0.247		0.254	
Number of stateID	50	50	50	50

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Column 1 and 3 of table 1 shows the results of the fixed effects model. Column 2 and 4 show the random effects model. FSS is insignificant in each of the models, while the direction of the coefficient also switches from negative to positive between the two. BRC (*pc_brc*) is significant at the one percent and ten percent levels across the two models, and the direction of the coefficients are both as expected. Importantly, there seems to be a significant increase in the number of police shootings over the years, as the omitted year is 2018 and all of the year dummies have negative coefficients. Additionally, the rate of violent crime for a given state is statistically significant at the one percent level across all specifications, and positively correlated with fatal police shootings. This reinforces the hypothesis that police killings are largely driven by the level of violence in which officers work. Lack of significance associated with the FSS coefficient is consistent with the findings of Rogna and Nguyen (2022), however when swapping FSS for background check data, *ceteris paribus* firearm prevalence is statistically significant. This provides modest evidence that BRC is a superior measure of firearm prevalence to FSS.

The fixed effect and random effect models maintain important differences which bias these results. Assuming fixed effects, the model removes all of the time invariant group specific characteristics that may influence the number of fatal police shootings. This could be culture, location, borders with other states, or local police department attitudes. On one hand, this is very good at addressing endogeneity concerns, as the only thing that could endogenize independent variables would be a time variant state specific factor. However, as addressed earlier, perhaps the true Gifford score does not capture variability and therefore the true effect of legislative attitude is captured in the error term.

Importantly, if there is not enough variation in an independent covariate, its effects will be largely omitted. This is of specific concern for the FSS and background check proxies, as they are the variables of interest. Their effects could be dampened by the fixed effect model. If they are time invariant, they are ostensibly treated as state specific time invariant effects (see appendix for trends in average BRC and FSS across time). For this reason, I turned to the random effect model to obtain more information on what is potentially happening.

The random effects model does not punish lack of variation in the proxy variables. Instead, the random effect model assumes there is no correlation between time invariant state specific effects and firearm prevalence. This assumption drives an increase in significance when using the background check proxy, but even when using FSS, the random effects model does not yield significance. The random effects model has its own drawbacks, namely that it opened the door to endogeneity of the independent variable, and it is likely that state specific effects do in fact influence police shootings. Both models are therefore not perfect. However, it seems as though the background check proxy yields better results across both models.

The table below indicates results for the 2SLS fixed effect regression (Model 1), the 2SLS random effect regression (Model 2), and the corresponding first-stage regression (Model 3). The first stage relationship between background checks and FSS is not statistically significant for the fixed effects nor the random effects model. This suggests that BRC is not a strong instrument for FSS. The coefficient on the FSS proxy becomes statistically significant at the one percent level and its direction is as expected only random effects are assumed. An increase in FSS of one percentage point is associated with an increase of 0.23 fatal police shootings per million people. Interpretation of FSS is not clear, as it does not measure firearms directly. Regardless, these results suggest little advantage in use of the multiple indicator method to account for measurement error in FSS. This could be because there is little variation that is common between FSS and BRC across time, considering there is little variation across time just for FSS. The results from the 2SLS model suggest that instrumenting FSS with background check data is not an avenue to consider in future inquiries which require a measure for firearm prevalence.

Table 2 - Regression results for IV Fixed Effects and Random Effects models

VARIABLES	(1) IV RE	(2) IV FE	(3) First Stage FE	(4) First Stage RE
suicideproxy	0.233***	-1.652		
	(0.0808)	(3.857)		
pc_brc			-0.257	0.371
			(0.585)	(0.621)
young	0.108	0.370	0.395	0.404
	(0.111)	(1.704)	(0.453)	(0.423)
white	-0.0167	0.371	0.217	0.158*
	(0.0142)	(1.078)	(0.363)	(0.0841)
dummy2017	-0.246	-0.927	-0.250	-0.508
	(0.498)	(1.555)	(0.704)	(0.759)
dummy2016	-0.815	0.243	0.768	0.475
	(0.517)	(3.086)	(0.776)	(0.827)
dummy2015	-1.077*	-3.686	-1.215	-1.335
	(0.580)	(5.032)	(0.882)	(0.900)
dummy2014	-1.265**	-3.610	-1.102	-0.906
	(0.641)	(4.728)	(1.086)	(1.080)
dummy2013	-1.856***	-1.239	0.566	0.831
	(0.714)	(3.000)	(1.342)	(1.334)
dummy2012	-2.057**	-2.063	0.319	0.642
	(0.813)	(2.827)	(1.557)	(1.554)
Unemployment	0.332	-0.104	-0.244	-0.655
	(0.230)	(1.255)	(0.495)	(0.518)
Poverty	-0.279**	-0.0730	0.0709	0.382
	(0.139)	(0.529)	(0.255)	(0.267)
Pc_income	6.42e-06	-7.68e-05	3.59e-06	-8.95e-05
	(6.50e-05)	(0.000170)	(9.99e-05)	(0.000106)
LowEduc	0.0579	0.522	0.229	0.0471
	(0.0963)	(0.945)	(0.240)	(0.247)
Crime	0.000454***	-0.00122	-0.00133*	0.000352
	(0.000138)	(0.00527)	(0.000688)	(0.000538)
Rank	0.0573	0.0183	0.00153	-0.236***
	(0.0419)	(0.0954)	(0.0561)	(0.0486)
Constant	-10.54	55.29	28.40	35.56***
	(6.707)	(115.8)	(28.85)	(13.68)
Observations	350	350	350	350
Number of stateID	50	50	50	50
R-squared			0.074	

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Limitations

My analysis of firearm prevalence's role in fatal police shootings is constrained by various limitations. Principally, the access restriction on disaggregated Giffords scores has not only limited the extent to which the categories can be separated and examined individually, but also obscured the true effect that firearm legislation carries. By reducing the scores to a set rank from one to fifty, information is lost about the magnitude of relative legislative strengths that states have to one another and themselves across years. California, for example, ranks as number one in the nation for every year in the sample period. However, this does not mean that the score which California was given in every year was the same. In fact, all it means is that California received the highest score across all of the years used. Furthermore, regarding fatal encounters with police officers, there is a limited number of years recorded by the private dataset used. Were the FBI dataset to be more reliable, this would open the study to a broader time period.

Importantly, my analysis examines the impact of firearm prevalence at the state level. More disaggregated data would open the door to more region specific results. This is an important next step in future research which should aim to isolate the effects across localities. Because some states contain diverse environments and populations, it is not necessarily appropriate to assume that states exhibit homogeneous effects with respect to firearm prevalence.

Across the field of study, it is a characteristic of using proxy data for firearm ownership that interpreting the meaning of the coefficient is obscured. While results do not provide a direct relationship to the number of firearms, ostensibly this is not what is important. The coefficient provides insight into the direction and responsiveness of the relationship between firearm prevalence and police shootings which can inform policy. It is through policy that changes can be made, rather than directly manipulating the level of firearms in society.

The panel data analysis presents an important tradeoff with regards to variation in the independent variables. While the use of the random effects model makes strong assumptions about the endogeneity of the independent variables, the fixed effect model is not without weakness because of the reasons discussed above. The assumptions made by the random effects model may not be accurate, but they are included to contrast the fixed effects model, to highlight potential shortcomings of the fixed effects model. In this way, FE and RE are somewhat extremes, where the truth may lie in between. This highlights a shortcoming of this paper, as both the FE and RE models give partial insight into a true relationship.

Conclusion

The present paper finds that more background checks leads to more fatal police shootings. My analysis also shows that choice of firearm prevalence measure in a panel data analysis can impact results greatly. FSS is not equipped well to deal with panel data, and should be substituted for BRC when possible.

I also reinforce the claim that the level of crime is indeed an important factor in fatal police shootings, which is consistent with previous literature. Policy implications are clear: while restrictive firearm legislation may contribute to fewer incidences of fatal police shootings [24], it is also important to tackle the issue from other angles like decreasing crime. Future analysis should examine the disaggregated impacts of crime on police shootings.

While this paper provides modest evidence that BRC is a more viable proxy for firearm prevalence than FSS in panel data, future researchers should also focus their efforts on refining

[24] Rogna and Nguyen (2022)

firearm prevalence proxies. Endogeneity concerns that arise in my analysis should be further examined by exploring sub-state level data and county police department characteristics.

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Appendix

Figure 4 - National mean Fraction of Suicides committed with Firearm

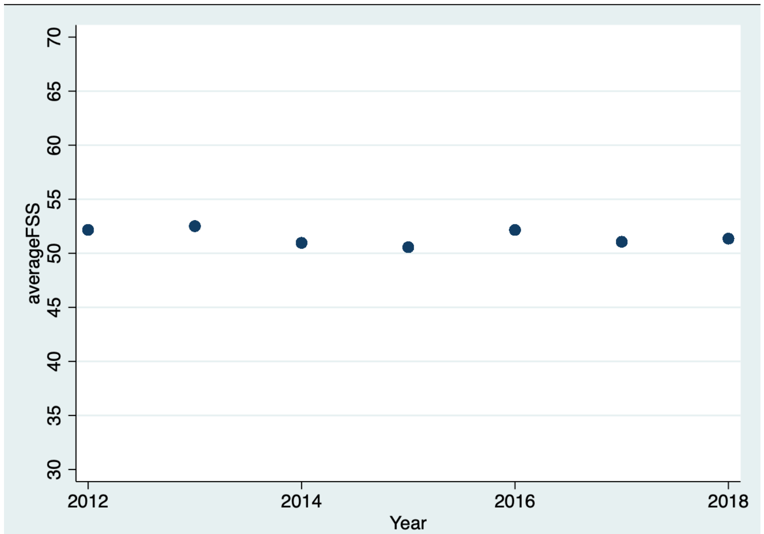

Figure 5 - National mean per capita background check applications

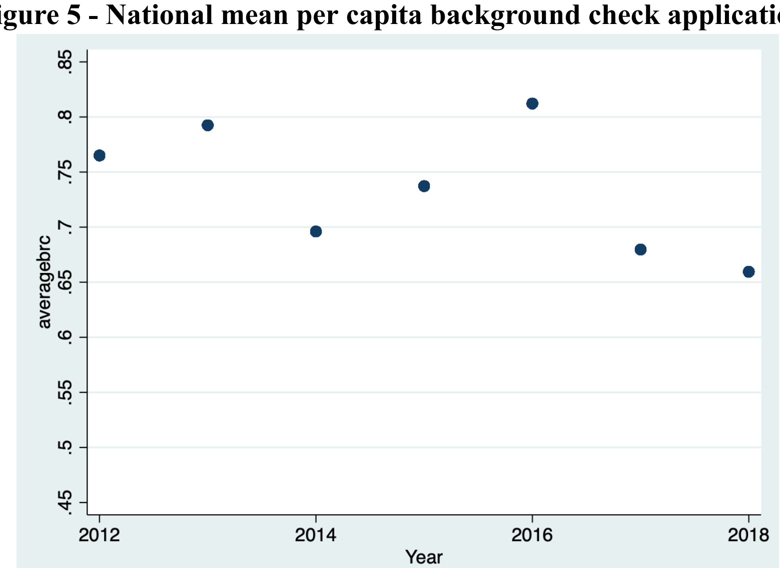

Figure 6 - Average National Fatal Police Shootings over time

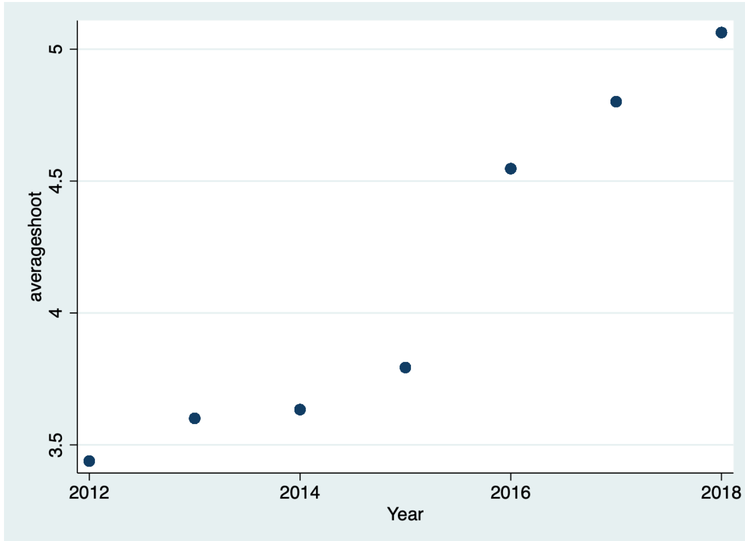

Table 3 - descriptive statistics for for all variables

Variable	Obs	Mean	Std. Dev.	Min	Max
Poverty	350	9.988	2.803	4	19.2
Pc income	350	29712.583	4828.934	20119	52500
Unemployment	350	3.963	1.177	1.8	7.9
LowEduc	350	11.187	2.968	6.1	18.6
Crime	350	3628.012	1388.99	1025.9	8849.558
white	350	76.954	12.67	24.3	95.1
young	350	23.155	1.309	19.8	27.5
suicideproxy	350	51.54	12.354	13.2	74.3
pol shoot	350	4.125	2.431	0	14.869
pc brc	350	.735	.487	0	2.233