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Reexamining the Evidence on Gun Ownership and Homicide Using Proxy Measures of Ownership

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Abstract

Limited by the lack of data on gun ownership in the United States, ecological research linking firearms ownership rates to homicide often relies on proxy measures of ownership. Although the variable of interest is the gun ownership rate, not the proxy, the existing research does not formally account for the fact that the proxy is an error-ridden measure of the ownership rate. In this paper, we reexamine the ecological association between state-level gun ownership rates and homicide explicitly accounting for the measurement error in the proxy measure of ownership. To do this, we apply the results in Chalak and Kim (2020) to provide informative bounds on the mean association between rates of homicide and firearms ownership. In this setting, the estimated lower bound on the magnitude of the association corresponds to the conventional linear regression model estimate whereas the upper bound depends on prior information about the measurement error process. Our preferred model yields an upper bound on the gun homicide elasticity that is nearly three times larger than the fixed effects regression estimates that do not account for measurement error. Moreover, we consider three point-identified models that rely on earlier validation studies and on instrumental variables respectively, and find that the gun homicide elasticity nearly equals this upper bound. Thus, our results suggest that the association between gun homicide and ownership rates is substantially larger than found in the earlier literature.

Keywords: *Firearms, homicide, measurement error, multiple equations, partial identification, sensitivity analysis, suicide.*

JEL codes: C30, I18, K42.

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1 Introduction

In the United States, nearly 40,000 people died from gun related injuries in 2017, with about 14,500 homicides and another 24,000 suicides. Despite these alarming numbers, there is a lack of data suitable to draw credible inferences on even the most basic questions about guns and violence (National Research Council (NRC), 2005). To overcome the lack of comprehensive data on gun ownership in the United States, ecological research linking firearms ownership rates to homicide (and other violent events) often relies on proxy measures of ownership (Azrael et al., 2004; NRC, 2005).¹ Duggan (2001), for example, proxies for the gun ownership rate using the subscription rate to *Guns and Ammo* magazine, and Cook and Ludwig (2006) use the fraction of suicides committed with a firearm (FSS).² Unlike the limited survey information on gun ownership, these proxies are measured each year at state and county geographic levels. Using state-level panel data on homicide rates from the FBI’s Uniform Crime Reports and linear fixed effects panel data models, Duggan (2001) estimates an elasticity of the homicide rate with respect to the proxy measure of 0.2 and Cook and Ludwig (2006) report an estimated elasticity of just over 0.4.

In this paper, we reexamine the ecological association between state-level gun ownership rates and homicide explicitly accounting for the measurement error in the proxy measure of gun ownership. Although the variable of interest is the gun ownership rate, not the proxy, the existing research does not formally account for the fact that the proxy is an error-ridden measure of the ownership rate.

Limited validation research assesses the correlation between different proxy measures and self-reported ownership measures from surveys where firearms ownership information is collected at higher geographic units and/or less frequent intervals. In particular, Azrael,

¹Surveys with information on ownership do not consistently cover the geographic areas of interest (e.g., states or counties). For example, the General Social Survey (GSS), which collects individual and household information on firearms ownership over time, is representative of the nine census regions and the nation as whole. Other surveys – the Behavioral Risk Factor Surveillance System (BRFSS) and the Harvard Injury Control Research Center Survey (HICRC) – collect information on gun ownership prevalence rates representative of individual states in certain years. The BRFSS included firearm ownership questions in the 1992-1995 surveys conducted in 21 states. The HICRC can be used to draw inferences on ownership by states in 1996 and 1999.

²Miller, Azrael, and Hemenway (2002), Siegel, Ross, and King (2013), and Cook and Ludwig (2019) also use the FSS to proxy for gun ownership rates. Other proxies used in this literature include the fraction of homicides committed with a gun and the average of the percentages of homicides and suicides involving guns, referred to as the “Cook Index” (Azrael et al., 2004).

Cook, and Miller (2004) and Cook and Ludwig (2006), find that the fraction of suicides committed with a firearm (FSS) has the highest correlation with observed ownership rates among all of the applied proxy measures. They find that the correlation coefficient between FSS and observed measures of gun ownership within a state ranges between 0.81 when using the BRFSS to 0.90 when using the HICRC (see footnote 1). Other proposed proxy measures had much lower correlation coefficient estimates (Azreal et al., 2004; Cook and Ludwig, 2006). For example, the correlation coefficient between the Cook Index proxy (see footnote 2) and observed measures of gun ownership within a state ranges between 0.52 and 0.88. Based on this evidence, researchers have concluded that FSS is a superior proxy for state level ecological analyses of gun ownership and homicide.³

While the FSS proxy may be highly correlated with gun ownership, it is still a noisy measure of the true ownership rate. As such, the estimated mean regression of homicide rates on firearms ownership rates is biased. We study the extent of this bias using a battery of set- and point-identified models.

Assuming a classical measurement error model, we apply the results in Chalak and Kim (2020) to provide informative bounds on the mean association between homicide and firearms ownership. Given uncertainty about the true data generating process linking the observed proxy to the unobserved gun ownership rate, we assess the sensitivity of inferences to the underlying assumptions on the measurement error. We also show how the related literature evaluating the association between suicide and firearms ownership can narrow the bounds. Further, we report estimates from three point identified models, one that assumes the correlation between the proxy and the ownership rate is known based on prior information and others that use instrumental variables. Finally, we use multiple proxies to derive less attenuated lower bound estimates.

Section 2 summarizes the data and replicates the basic results from Cook and Ludwig (2006). This analysis uses a basic linear panel data model to estimate the association between the state homicide rate and FSS. The parameter of interest, however, is the association

³Cerqueira et al. (2018) develop a more refined proxy which relies on the socioeconomic characteristics (e.g., gender) of the suicide victims. They find that this new proxy has a slightly higher correlation than FSS with the actual state level gun ownership data from BRFSS. Similarly, Schell et al. (2020) produce a model based proxy of state gun ownership using several sources of survey data including BRFSS and proxies including FSS. Cook (2020) finds the cross-section correlation between the Schell et al. (2020) proxy and FSS to be 0.91. Neither of these new proxy variables have been applied in this literature.

between homicide and gun ownership rates.

After presenting the basic framework and assumptions used to draw inferences with a proxy variable in Section 3, Sections 4 and 5 present the results. We begin in Section 4 by focusing on the particular models and results that seem most credible. In this analysis, the estimated lower bound on the magnitude of the association corresponds to the conventional linear regression model estimates whereas the upper bound depends on prior information about the measurement error process. Our preferred model analyzes multiple outcomes jointly and assumes that the FSS proxy is at least as accurate as the validation literature reports; that is, a correlation coefficient between FSS and true ownership rates of at least 0.81. This yields an upper bound on the gun homicide elasticity that is nearly three times larger than fixed effects regression estimates that do not account for measurement error. Section 5 then illustrates the sensitivity of inferences to different assumptions about the data generating process. In particular, we first expand the partial identification analysis by tracing out the estimates across variation in the lower bound on the correlation coefficient between FSS and true ownership rates. Then, we consider three point identified models, one where the correlation coefficients is known (e.g., 0.81) and two that use lagged or other proxies as instrumental variables. We also combine multiple proxies with weaker assumptions to estimate a less attenuated lower bound on the association between the ownership and homicide rates. The resulting point and lower bound estimates sometimes nearly equal the upper bound estimates reported in Section 4, suggesting that the association between gun homicide and ownership rates is substantially larger than found in the earlier literature.

2 Data and Replication

Our analysis uses state-level data on annual homicide rates (per 100,000 residents) from 1980 to 2014 (25 years) across 50 states and the District of Columbia.⁴ We examine rates of gun and non-gun related homicide, and the gun suicide rate. The focus on annual crime rates within states has been common in the literature. See, for example, Duggan (2001), Cook

⁴Our basic regression model replicates Cook and Ludwig (2006; 2019), who examine state and county level homicide data from 1980 to 1999. Although Cook and Ludwig (2006) focus mostly on homicide rates in the 200 counties with the largest populations in 1990, Cook and Ludwig (2019) argue that the state level analysis in their 2006 paper has the benefit of reducing measurement error in suicide rates and FSS. We extend the panel to 2014, the latest year of the published data from the Uniform Crime Reports. The basic results are not sensitive to adding the extra years of data.

and Ludwig (2006), and Siegel, Ross and King (2013).

Table 1 displays the means and standard deviations for the variables used in this analysis. Data on the number of homicides, suicides, and the state population come from the Vital Statistics Program mortality files. Likewise, the proxy measure of the fraction of suicides committed with a firearm is calculated using data from the mortality file data. Following Cook and Ludwig (2006), we control for other crime rates and state level socioeconomic and demographic characteristics. Socioeconomic and demographic controls for the fraction of blacks, households headed by a female, urban residents, and residents living in the same house 5 years ago come from the decennial Census and/or the American Community Survey, and rates of robbery and burglary come from the FBI’s Uniform Crime Reports.⁵

Table 1: Means and Standard Deviations, Full Sample and by FSS Quartile

	Full Sample		Bottom FSS Quartile		Top FSS Quartile	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Homicide Rate	7.3	3.7	6.6	4.4	9.6	3.4
Gun Homicide Rate	4.8	2.7	4.2	3.2	6.6	2.6
Non-Gun Homicide Rate	2.5	1.2	2.4	1.6	3.0	1.0
FSS	53.9	12.8	36.6	6.6	70.0	4.9
Robbery rate	179.6	113.7	231.1	156.4	146.2	63.4
Burglary rate	960.7	421.9	767.4	372.2	1167.9	390.0
Share black	12.4	8.1	12.1	7.1	17.4	10.4
Share urban	77.9	12.7	87.9	6.7	65.6	11.6
Share female head	30.8	3.8	32.5	4.2	29.8	4.0
Share non-mover	55.5	6.2	60.0	4.6	53.4	5.3

Note: The sample means are weighted by the state population. Crime rates are per 100,000 people. FSS and the other shares are per 100 people. In the regression analysis, all the variables are logged.

Table 1 displays the overall mean and standard deviation for the full sample as well as for state-years where the FSS is in the bottom and top quartiles of the distribution. Overall, there is an unconditional association between the homicide rate and FSS. In particular, the homicide rate is 6.6 per 100,000 when the FSS is in the bottom quartile and 9.6 when in the top quartile. While similar patterns are seen for the gun homicide rate, the non-gun

⁵To control for reverse causation, Cook and Ludwig (2006) condition on the robbery and burglary rates, which they argue can motivate the acquisition of a firearm for self-defense. The idea is that these other crime rates control for the latent propensity for crime that induces gun ownership.

homicide rate hardly varies across the FSS quartiles.

Using these data, we replicate the basic results found in the literature by estimating a standard linear fixed effect model. In particular, we regress the homicide rate in state i and year t on the observed covariates listed in Table 1, as well as state and year fixed effects. As in Cook and Ludwig (2006), all of the variables are logged and FSS is lagged one year.⁶ The primary interest is in learning the elasticity of the mean homicide rate with respect to FSS.

Table 2: Estimates of the log(lagged FSS) Coefficient and Standard Errors Under Different Models and Weights

	Homicides	Gun homicides	Non-gun homicides
Fixed Effects			
Unweighted	0.249 (0.077)	0.359 (0.110)	0.102 (0.091)
Weighted	0.414 (0.183)	0.562 (0.237)	0.102 (0.144)
Limited Controls, Unweighted	0.275 (0.078)	0.411 (0.125)	0.092 (0.093)
Limited Controls, Weighted	0.444 (0.175)	0.597 (0.246)	0.111 (0.140)
No Fixed Effects			
Unweighted	0.487 (0.188)	0.835 (0.216)	0.097 (0.170)
Weighted	0.505 (0.134)	0.865 (0.140)	-0.017 (0.134)

Note: Standard errors appear in parentheses below the point estimates. Both the homicide rate and all of the covariates (including the lag of FSS) are log-transformed. The weighted regressions are weighted by the state population. The limited control models include the controls for race, robbery, burglary, and fixed effects for state and year. The models without fixed effects include the full set of control variables.

Table 2 displays the estimate and standard error⁷ for the FSS parameter under a variety

⁶The literature uses lagged FSS, as opposed to contemporaneous FSS, to avoid contamination. This is intended to alleviate the concern that the correlation between FSS and gun-homicide rates may reflect a change in gun ownership in response to a change in gun-crime.

We note that the basic regression results are not sensitive to whether we control for contemporaneous or lagged covariates. This holds for the regressions with and without fixed effects. For example, the baseline estimate for gun homicides is 0.835 without fixed effects and 0.359 with fixed effects (see Table 2). When lagging the covariates by one year, these estimates are 0.850 and 0.331, respectively.

⁷Standard errors are clustered at the state level, and are heteroskedasticity robust. Although this is the norm in the literature, there is some debate about whether to cluster the standard errors at the state level, especially in fixed effects models (see NRC (2005) and Abadie et al. (2017)). In this application, clustering

of different models that have been estimated in the literature, including unweighted and weighted regressions, regressions with and without fixed effects, and regressions with a more parsimonious set of covariates that only control for robbery, burglary and the fraction of the population who are black (see Seigal, Ross and King, 2013).⁸

Although the point estimates vary across the different models, the basic qualitative conclusions are robust. The FSS is positively associated with overall homicides and gun homicides. For non-gun homicides, the estimates are all statistically insignificant. The gun-homicide point estimates are notably larger in models without fixed effects. The estimates are not particularly sensitive to the set of covariates (see rows 3 and 4).

Finally, notice that the basic qualitative findings are not sensitive to weighting by the state population (also see Siegel et al., 2013). For example, in the fixed effect model, estimated elasticity with weights is 0.414 for homicide, 0.562 for gun homicide, and 0.102 for non-gun homicide, with this latter estimate being statistically insignificant at the 5% significance level. These findings are similar to those reported in Cook and Ludwig (2006). The unweighted estimates are somewhat smaller but have the same qualitative implications: the estimated elasticity is 0.249 for homicide and 0.359 for gun homicide. The estimate for non-gun homicide, at 0.102, is identical to the weighted estimated and statistically insignificant. In the models without fixed effects, the weighted and unweighted estimates are very similar.

In the next sections, we examine the implications of measurement error in the proxy variable, FSS, on inference. To do this, we evaluate regressions with all of the covariates listed in Table 1, and report results with and without fixed effects. Whether to weight by state population depends on the underlying assumptions about the data generating process (see Solon et al., 2015). In particular, weighting by the state population leads to an efficient estimator if there is homoskedasticity at the level of the individual, but not otherwise. Since this homoskedasticity assumption does not seem likely to hold in our application, we focus on the unweighted regressions with heteroskedasticity robust standard errors.⁹

leads to a modest increase in the estimated standard errors relative to those found without clustering, but does not in general impact conclusions about statistical significance.

⁸Estimates and standard errors for parameters associated with the covariates are available from the authors.

⁹Cook and Ludwig (2006) “prefer the weighted estimates because they provide a heteroskedasticity correction” and may therefore be more precise. As shown in Table 2, in our context, the weighted standard errors are not substantially smaller than the unweighted ones, and are in fact often larger.

3 Framework and Assumptions

While the results in Table 2 provide information on the association between homicide and FSS, our interest is in inferring the association between homicide rates and firearms ownership rates. To do this, one needs to formally account for the fact that FSS is a proxy measure for the unobserved ownership rate. To account for the measurement error created by the proxy variable of gun ownership, we adapt the framework developed in Chalak and Kim (2020).

Our interest is in identifying the association between gun ownership rates and gun and non-gun homicide rates. Let U_{it} be the log of the lagged unobserved gun ownership rate. The j^{th} outcome equation is given by

$$Y_{jit} = X'_{it}\beta_j + U_{it}\delta_j + \eta_{jit}, \tag{1}$$

where Y_{jit} for $j = 1, 2$ corresponds respectively to the log of the non-gun homicide and gun homicide rate per 100,000 people in state i and year t , X_{it} denotes observed controls (see Table 1), and η_{jit} is an unobserved disturbance. (β_j, δ_j) are unobserved coefficients, and our primary interest is in learning the δ_j coefficients. As in Cook and Ludwig (2006), all of the observed random variables are logged and the gun ownership rate is lagged one year.

As noted above, we consider models without fixed effects as well as with state and year fixed effects. In the latter case, we include year indicators in X_{it} and we remove the state fixed effects by applying a within transformation. In this case, we interpret the analysis relative to the within-transformed variables (see Chalak and Kim (2020, Online Appendix C) for further details).

3.1 Primary Assumptions

The problem is that while we observe the log of lagged FSS, W_{it} , the log of lagged gun ownership rate, U_{it} , is unobserved. To formalize the proxy variable problem, we decompose W_{it} into a “signal” component U_{it} and a “noise” or measurement error ε_{it} . After stacking the Y_{jit} outcomes in Y_{it} , we assume that the data is generated as follows.

Assumption A₁ *Data Generation:* For $t = 1, \dots, T$, let the latent variable U_{it} , measurement error ε_{it} , disturbance η_{it} , and covariates X_{it} be random variables with finite variances and

let the proxy W_{it} and outcomes Y_{it} be given by

$$Y_{it}' = X_{it}'\beta + U_{it}\delta + \eta_{it}' \quad (2)$$

$$W_{it} = U_{it} + \varepsilon_{it}. \quad (3)$$

The researcher observes realizations of $(X_{it}', W_{it}, Y_{it}')$, for $i = 1, \dots, n$ and $t = 1, \dots, T$, whereas realizations of $(U_{it}, \eta_{it}', \varepsilon_{it})$ are unobserved.

We maintain that n is large relative to T and treat any missing observations as missing at random. The equation for the proxy W_{it} involves logged variables, and therefore relates the percentage (as opposed to level) changes in the variables. Thus, in levels, we have that $FSS_{it} = b \times G_{it} \times error_{it}$ where G_{it} denotes the rate of gun ownership. After logging, our model absorbs the error and the slope coefficient b into $\varepsilon_{it} = \log(b \times error_{it})$. That is, under this specification, a percent increase in the rate of gun ownership is associated with an approximately one percent increase in FSS. This is less restrictive than the no proxy error model where $W_{it} = U_{it}$.¹⁰ In Section 5.3.3, we consider a model with two proxies for U_{it} , and we allow the coefficient on U_{it} in one of the proxy equations to be unrestricted. We find that we cannot reject the hypothesis in A_1 that this slope coefficient is equal to 1.

We let the latent firearm ownership prevalence U_{it} be freely correlated with X_{it} . Nevertheless, we maintain two standard assumptions about the other unobservables η_{it} and ε_{it} .

First, the disturbance η_{it} is uncorrelated with (U_{it}, X_{it}) .

Assumption A₂ *Uncorrelated Disturbance:* $Cov[\eta_{it}, (X_{it}', U_{it})'] = 0$ for $t = 1, \dots, T$.

A_2 refers to the classical linear regression assumptions, under which the regression disturbance is (by construction) uncorrelated with the right-hand side variables. Under this assumption, the parameter δ is the best linear least squares coefficient for predicting the crime rates based on gun ownership after controlling for X_{it} .¹¹

Second, the measurement error ε_{it} is classical.

¹⁰One might relax the assumption of a unit elasticity by introducing, for example, an unknown coefficient on U_{it} : $W_{it} = \gamma U_{it} + \varepsilon_{it}$. In levels, this corresponds to $FSS_{it} = b \times G_{it}^\gamma \times error_{it}$. In general, however, the scaling parameter γ is not separately identified from δ . This identification problem is not particular to the methods used in this paper but rather a generic feature in many latent variable models. In particular, this identification problem applies to the previous literature estimating the regressions under the no-error assumption $\varepsilon_{it} = 0$. Finally, in the case where we have two proxies for U_{it} , the parameter γ can be identified for one of the two proxies. We allow for this possibility in Section 5.3.3.

¹¹The literature often interprets this as a structural or causal assumption where δ is interpreted to be

Assumption A₃ *Uncorrelated measurement error:* $Cov[\varepsilon_{it}, (X'_{it}, U_{it}, \eta'_{it})'] = 0$ for $t = 1, \dots, T$.

Thus, the classical model assumes the measurement error, ε_{it} , is white noise, uncorrelated with the log of the lagged ownership rate, U_{it} , and the observed covariates, X_{it} . This provides a parsimonious and reasonable weakening of the no measurement error assumption applied in this literature. The classical error model is implicit in the validation studies examining the correlation between different proxy measures and self-reported ownership measures (e.g., Azrael, Cook, and Miller, 2004). In addition, although the classical measurement error model does not restrict FSS to lie between 0 and 1, this is not a chief concern for this application where the minimum and maximum values of FSS are 0.143 and 0.882.

For $T = 1$, A₁-A₃ are the classical error-in-variables assumptions, where it is well known that β and δ are not point identified. When analyzing the Y_j equation separately, Klepper and Leamer (1984), Bollinger (2003), and Chalak and Kim (2021) characterize the sharp identification bounds for β_j and δ_j . In particular, δ_j is bounded under A₁-A₃ using the forward and reverse regressions (see e.g. Klepper and Leamer, 1984; Bollinger, 2003). Chalak and Kim (2020, Corollary 3.3) show that jointly analyzing the J equations weakly narrows the sharp identification regions. The basic intuition is that there is one mismeasured variable and the noise-to-signal ratio in equation (3) applies to all J equations. This common measurement error process imposes implicit cross-equation restrictions that weakly narrow the identification regions. When estimating the association between firearms ownership and non-gun or gun homicide ($J = 2$), we report the joint equation bounds in Chalak and Kim (2020).

Given the identification gain from analyzing multiple equations, we also estimate models with a third equation ($J = 3$) where the outcome is the gun suicide rate across states. While our focus is on the homicide rate, there is a related literature which evaluates the

the average treatment effect. In this setting, papers from the literature take several steps to render A₂ plausible (see Section 2). First, the specification in A₁ conditions on covariates to control for demographic and socioeconomic characteristics as well as the burglary and robbery crime rates. Second, the proxy is lagged by one or more periods. Finally, the literature uses a type of placebo test by evaluating non-gun homicides. See, for example, the discussion in Rosenbaum (2020, section 5.2.4) on the use of “unaffected outcomes” when “we think a treatment will affect one outcome and not affect another, and we wish to exploit the anticipated absence of effect to provide information about unmeasured biases.” The finding that gun ownership rates are associated with gun-homicides but not with non-gun homicides, makes it less likely that the specification failed to account for unobserved confounders that jointly affect homicide outcomes and gun ownership. While these steps address several concerns regarding the validity assumption A₂ in a structural model, our analysis interprets the estimates of δ as an association rather than an average treatment effect.

association between suicide rates and gun ownership rates using proxy measures of ownership (see Duggan, 2003 and NRC, 2005). The Chalak and Kim (2020) bounds imply that bringing together these two related literatures may serve to reduce the uncertainty about the extent of the measurement error in the proxy.

Last, as discussed in Rosenbaum (2020, p. 138), considering multiple outcomes can also bolster the plausibility of estimates if, e.g., one finds that “where an effect is plausible, treatment and outcome are associated; where an effect is not particularly plausible, treatment and outcome are not associated.” This makes it less likely that the model failed to account for key unobserved confounders.

3.2 Auxiliary Assumptions

To tighten the identification regions obtained under A_1 - A_3 , we consider the auxiliary assumptions A_4 - A_5 . Chalak and Kim (2020) characterize the identification region under any configuration of these auxiliary assumptions.

First, we bound the reliability ratio of the observed proxy, W_{it} , for the unobserved ownership rate, U_{it} . For a given t , let $R_{W_t}^2$ denote the “reliability ratio,” that is the population coefficient of determination (R-squared) from a linear regression of W_{it} on U_{it} . Specifically, if there is no measurement error then W_{it} is a perfect proxy for U_{it} and $R_{W_t}^2 = 1$. More generally, with classical measurement error, we have that $R_{W_t}^2 \leq 1$. The existing literature has implicitly assumed the no measurement error assumption even though the correlation between FSS and firearms ownership is less than one. As noted above, Azrael, Cook, and Miller (2004) use auxiliary data on ownership to estimate a correlation between W_{it} and U_{it} of at least 0.81. This implies a reliability ratio of $R_{W_t}^2$ of at least 0.6561 (i.e. 0.81^2).

Our first auxiliary assumption weakens the “no measurement error” assumption $R_{W_t}^2 = 1$ by imposing a lower bound κ on the reliability ratio.¹²

Assumption A_4 *Bounded Reliability Ratio: $\kappa \leq R_{W_t}^2$ for $t = 1, \dots, T$.*

The no measurement error assumption sets the lower bound $\kappa = 1$. To weaken this assumption, we conduct a sensitivity analysis that varies κ away from 1. Based on the validation literature discussed above, our preferred value is $\kappa = 0.6561$ and we report estimates

¹²More precisely, we impose the weaker assumption $\kappa \sum_{t=1}^T \text{Var}(W_{it}) \leq \sum_{t=1}^T \text{Var}(U_{it})$ (or its equivalent after projecting W_{it} and U_{it} on the covariates).

using this value in Section 4. In Section 5, we trace out the estimated bounds under different values of the reliability ratio, κ , and also report estimates under the assumption that the ratio is known rather than bounded. Chalak and Kim (2020, equation 5) show that if the reliability ratio is known then δ is point identified.

Second, we allow for cross-equation information. For a given t , let $r_{j,h}^t$ denote the correlation among the cross-equation disturbances η_{jit} and η_{hit} . Our second auxiliary assumption restricts the correlation between the shocks in the non-gun homicide, gun homicide, and (when included) gun suicide equations to be non-negative.¹³

Assumption A₅ *Positive Disturbance Correlation Restriction:* $0 \leq r_{j,h}^t$ for $j, h = 1, \dots, J$, $j < h$, and $t = 1, \dots, T$.

For example, when $J = 2$ and after accounting for the gun ownership rate and the covariates, A₅ encodes that the shocks to the gun and non-gun homicide rates are (weakly) positively correlated, reflecting an overall change in violent outcomes. We emphasize that our framework allows imposing A₄ and/or A₅ but does not require them.

4 Empirical Results

In addition to reporting the plug-in estimates, we apply the estimation procedure in Chalak and Kim (2020) to report 50% (this conveys information similar to median unbiased estimates) and 95% confidence intervals (thereafter CI) for the parameters of interest.¹⁴ These intervals account for state level clustering and arbitrary heteroskedasticity.

Table 3 displays the estimated bounds for δ_j in the basic specification with the full set of logged covariates (see Table 1).¹⁵ In addition to imposing assumptions A₁-A₃, we also restrict the reliability ratio to be greater than 0.6561 (A₄). We present estimates with and without fixed effects, and with and without the restriction that the disturbances are positively correlated (A₅). Finally, in columns A, we display results using two outcome equations –

¹³Here too, it suffices to impose the weaker restriction $0 \leq \sum_{t=1}^T Cov(\eta_{jit}, \eta_{hit})$ for $j, h = 1, \dots, J$, $j < h$.

¹⁴Chalak and Kim (2020) show how A₁-A₅ lead to intersection bounds on the reliability ratio. As discussed in Manski and Pepper (2000), plug-in estimators of intersection bounds will be biased inward (also see Kreider et al, (2012)). To address this bias, Chalak and Kim (2020) apply the results in Chernozhukov et al. (2013, theorem 4 and example 1).

¹⁵Estimated bounds for the covariates coefficients are available from the authors.

Table 3: Bounds on the Association between Ownership and Violent Deaths Under the Assumption that the Reliability Ratio is at least 0.6561

No Fixed Effects				
	A. Without Gun Suicide Equation		B. With Gun Suicide Equation	
	Cross Equation Correlation		Cross Equation Correlation	
	No Assumption	Non-negative	No Assumption	Non-negative
Non-gun Homicide Rate				
Plug-in Estimate	[0.097, 0.651]	[0.097, 0.651]	[0.097, 0.171]	[0.097, 0.115]
50% CR	[-0.179, 1.816]	[-0.225, 1.862]	[-0.044, 0.450]	[-0.059, 0.487]
95% CR	(-2.247, 3.884)	(-2.820, 4.457)	(-0.556, 0.961)	(-0.737, 1.165)
Gun Homicide Rate				
Plug-in Estimate	[0.835, 5.573]	[0.835, 5.573]	[0.835, 1.467]	[0.835, 0.988]
50% CR	[0.684, 8.274]	[0.677, 8.332]	[0.684, 2.048]	[0.677, 2.178]
95% CR	(0.372, 10.898)	(0.285, 11.625)	(0.372, 2.698)	(0.285, 3.039)
Gun Suicide Rate				
Plug-in Estimate			[1.332, 2.341]	[1.332, 1.576]
50% CR			[1.271, 2.895]	[1.269, 3.063]
95% CR			(1.145, 3.157)	(1.110, 3.411)
State and Year Fixed Effects				
	Cross Equation Correlation		Cross Equation Correlation	
	No Assumption	Non-negative	No Assumption	Non-negative
Non-gun Homicide Rate				
Plug-in Estimate	[0.102, 0.287]	[0.102, 0.287]	[0.102, 0.287]	[0.102, 0.287]
50% CR	[0.040, 0.463]	[0.037, 0.471]	[0.040, 0.463]	[0.037, 0.471]
95% CR	(-0.252, 0.826)	(-0.352, 0.927)	(-0.252, 0.826)	(-0.352, 0.927)
Gun Homicide Rate				
Plug-in Estimate	[0.359, 1.011]	[0.359, 1.011]	[0.359, 1.011]	[0.359, 1.011]
50% CR	[0.283, 1.223]	[0.280, 1.232]	[0.283, 1.223]	[0.280, 1.232]
95% CR	(0.127, 1.662)	(0.084, 1.784)	(0.127, 1.662)	(0.084, 1.784)
Gun Suicide Rate				
Plug-in Estimate			[0.168, 0.474]	[0.168, 0.474]
50% CR			[0.122, 0.604]	[0.120, 0.610]
95% CR			(0.027, 0.872)	(0.001, 0.946)

Note: The 50% and 95% confidence regions (CR) are computed using the intersection bounds procedure in Chalak and Kim (2020). We set $\kappa = 0.6561$ in A_4 and report the results with and without A_5 .

one for non-gun homicides and one for gun homicides – and in columns B, we add the gun suicide equation.

As in the linear models evaluated in Section 2, the estimated plug-in bounds on non-gun homicide are close to zero on one side of the bound, and the 95% confidence intervals contain zero. Consider, for example, the models without fixed effects under A_1 - A_4 . The elasticity of non-gun homicide rates with respect to gun ownership rates is estimated to be as low as 0.097 and as high as 0.651, with a 95% CI ranging from -2.247 to 3.884. Adding in the suicide equation tightens the upper bound to 0.171 and the 95% CI to (-0.556, 0.961). Thus, the sign of this association for non-gun homicides is indeterminate.

For gun homicide rates, the estimated association is strictly positive, substantial, and statistically significant. Without fixed effects, the plug-in estimates in the basic model imply an association of at least 0.835 and at most 5.573. Adding in the gun suicide equation reduces the upper bound to 1.467.¹⁶ Thus, under this model, a one percent increase in the gun ownership rate is associated with a roughly 0.835 to 1.467 percent increase in the gun homicide rate. Finally, with the assumption that the disturbances are positively correlated (A_5), the upper bound falls further to 0.988. Thus, in this model, the estimated elasticity lies within the narrow bound of 0.835 to 0.988, but there is substantial sampling uncertainty reflected in the 95% CI of (0.285, 3.039).

In the fixed effect model, the association between the gun homicide and ownership rates is estimated to lie in [0.359, 1.011]. This estimated bound is not sensitive to adding in the suicide equation or imposing the restriction that the disturbances are positively correlated (A_5).¹⁷ Thus, these estimated bounds imply that the elasticity of gun homicide with respect to ownership may be nearly three times larger than when estimated using models that do not account for proxy errors.

Overall, the results for gun homicide rates imply a substantial positive association. With

¹⁶The estimates for gun suicide are similar to those reported by Duggan (2003).

¹⁷Notice that the estimated 50% and 95% confidence interval with the positive sign restriction are wider than without the restriction. In the population, the joint bounds with the sign restrictions are at least as tight as the bounds without the sign restrictions. This is reflected in the plug-in estimates. The bounds with the sign restriction depend on the sign of certain nuisance parameters which are known in the population. In the sample, they are estimated. The inference procedure in Chalak and Kim (2020) proceeds by (a) estimating a confidence interval for the nuisance parameters, (b) taking the union of the (intersection bounds) confidence regions over all admissible nuisance parameters values, and (c) splitting the significance level across these steps appropriately. Because of this conservative procedure, the confidence regions may be wider than the ones without imposing sign restrictions (the statistical uncertainty can offset the identification gain).

the fixed effect model, a one percent increase in ownership rates is associated with a 0.359 to 1.011 percent increase in gun homicides, whereas in the models without fixed effects the estimated bounds imply an elasticity of slightly less than 1.¹⁸

Notice that the estimated lower bounds in Table 3 are equal to the regression estimates in Table 2. This is the well-known attenuation bias in the classical measurement error model. This paper’s framework allows us to obtain an informative upper bound, thereby clarifying the otherwise unknown extent of the attenuation bias. In most of the models, the estimated upper bounds are substantially larger, suggesting that the association between gun-homicide and ownership rates may be much larger than previously reported in models that do not account for proxy errors. Moreover, the point estimates from the identified models considered in Section 5 are (nearly) equal to the estimated upper bounds displayed in Table 3.

5 Sensitivity Analysis

In this section, we examine the sensitivity of the estimates to alternative assumptions on the measurement error. We first study the sensitivity of the identification regions to variation in κ , the lower bound on the reliability ratio. Then, we evaluate the point estimates from one model where the reliability is known (Section 5.2) and two others model where a lagged or different proxy variable serves as an instrumental variable (Section 5.3). Finally, we combine multiple proxies to estimate a less attenuated lower bound (Section 5.3).

5.1 Variation in the Reliability Ratio Lower Bound, κ

Under A₁-A₄, we can study the sensitivity of the identification regions for δ_j , $j = 1, 2, 3$, to variation from κ . Figure 1 plots the plug-in estimates, the 50%, and 95% confidence regions (thereafter CR) for the partially identified δ_j as κ deviates from 1 for the model without fixed effects. Figure 2 plots the estimates and confidence intervals for the models with fixed effects. The graphs set the lower bound on the reliability ratio on the x axis starting at

¹⁸When using the noisier “Cook Index” as a proxy (the estimated reliability ratio from the validation studies is $0.52^2 = 0.270$ versus 0.651 for FSS) the estimated bounds are qualitatively similar but much wider. For example, in the fixed effects model, a one percent increase in the ownership rate is associated with a 0.783 to 9.696 percent increase in gun homicides. The results for non-gun homicide are all statistically insignificant at the five percent level.

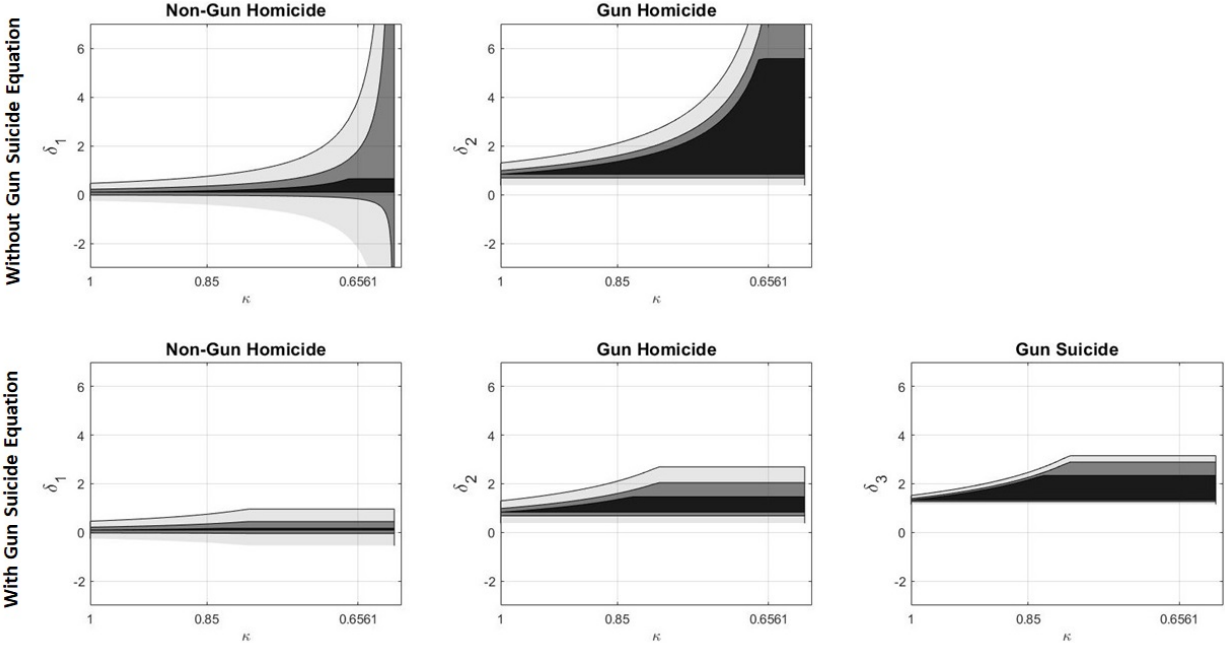


Figure 1: Sensitivity analysis for the bounds without fixed effects as κ varies. The darkest color corresponds to the plug-in bounds, the lighter color to the 50% CR, and the lightest color to the 95% CR.

1 (no measurement error) and decreasing away from 1. The columns correspond to the coefficient on non-gun homicide, gun homicide, and gun suicide. The rows correspond to the joint equation bounds without suicide, and the joint bounds with suicide. The darkest color corresponds to the plug-in bounds, the lighter color to the 50% CR, and the lightest color to the 95% CR. The darker regions are nested within the lighter regions.

Focusing first on the two-equation models, the basic conclusions for non-gun homicide rates are robust to variation in κ . In particular, the width of the δ_1 bounds is less sensitive to κ (relative to δ_2) and the associations are statistically insignificant in all of the estimated models. For gun homicide rates, however, the results are more nuanced. In all models, the estimates are positive and statistically significant. Yet, the upper bounds are highly sensitive to κ , implying substantial uncertainty about the true association when using proxy measures of ownership. For example, when $\kappa = 0.85$ the plug-in bounds on the gun homicide elasticity are $[0.835, 1.356]$ without fixed effects and $[0.359, 0.499]$ with fixed effects whereas setting $\kappa = 0.6561$ yields the bounds $[0.835, 5.573]$ and $[0.359, 1.011]$ respectively.

Perhaps the most striking finding is that adding the gun suicide equation has substantial identifying power in models without fixed effects. With $\kappa = 0.6561$, for example, the plug-in

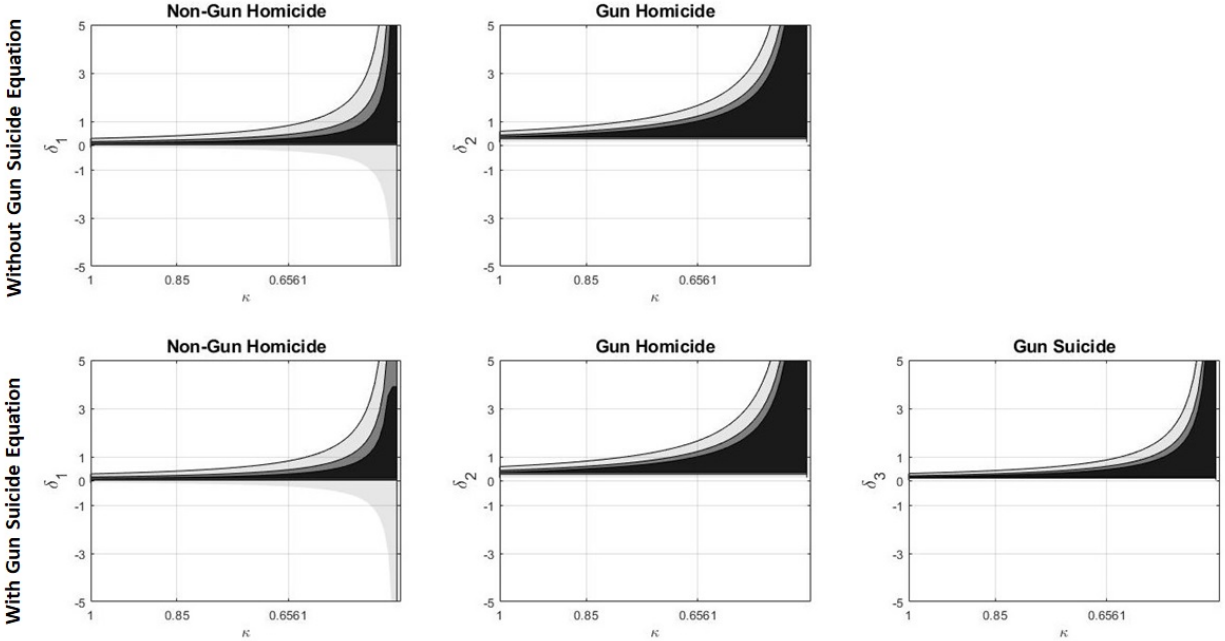


Figure 2: Sensitivity analysis for the bounds with fixed effects as κ varies. The darkest color corresponds to the plug-in bounds, the lighter color to the 50% CR, and the lightest color to the 95% CR.

bounds on the gun homicide elasticity is $[0.835, 1.467]$. Even when allowing for large degrees of measurement error, the estimated bounds are relatively narrow. In contrast, without the suicide equation, the analogous bounds are $[0.835, 5.573]$. Thus, in the model without fixed effects, there is substantial value added by bringing together these two related strands of the literature to address the measurement error associated with the proxy variable.

5.2 Known Reliability Ratio Model

In this section, we consider drawing inferences on the elasticity, δ , if the reliability ratio is known rather than bounded. In particular, we assume that the reliability ratio equals the smallest feasible value that is consistent with Assumptions A_1 - A_4 . The models estimated under A_1 - A_4 in Section 4 apply two restrictions on the reliability ratio, one directly from A_4 and another indirectly implied by A_1 - A_3 . Under A_4 , the reliability ratio is restricted to be no less than 0.6561, the lowest ratio estimated in earlier validation studies (Azrael et al., 2004). In the fixed effects models, A_1 - A_3 implicitly restrict the reliability ratio to exceed 0.48, a nonbinding restriction given A_4 . However, in the three equation model without fixed effects, A_1 - A_3 restrict the reliability ratio to be at least 0.8317. Thus, we examine the results

under the assumption that the reliability ratio equals 0.6561 for the fixed effect model and 0.8317 for the three equation model without fixed effects.

Under this assumption, Chalak and Kim (2020) show that the elasticity, δ , is point identified to equal the upper bound of the identification region. The estimated upper bounds are reported in Table 3. Thus, in the fixed effects models, where A_4 is binding, the reliability ratio is assumed to equal 0.6561 and the point estimate on the association between ownership rates and the gun homicide rate equals 1.011. This point estimate is nearly three times larger than the estimate of 0.359 derived under the assumption of no proxy errors (see Table 2). For the model without fixed effects, the estimated association between ownership and gun homicides equals 1.467. Further, adding the sign restriction A_5 and setting the reliability ratio to the smallest value consistent with A_1 - A_5 implies the point estimate 0.988, nearly 20 percent larger than the OLS estimator of 0.835.

These results suggest that the true association between ownership and firearms homicide rates is substantially larger than previously estimated. The estimates suggest a near unit elasticity; a one percent increase in the gun ownership rate is associated with a one percent increase in the gun homicide rate. In contrast, the elasticity estimates when assuming no proxy errors are 0.359 in the model with fixed effects and 0.835 in the model without fixed effect.

5.3 Instrumental Variable Models

Alternative restrictions on the measurement error process and proxy variables can be used to narrow the bounds on the association between the log homicide rate and the log ownership rate. In this section, we estimate two point identified models that rely on instrumental variable assumptions: one with lagged values of the FSS proxy and another that uses both the FSS and the fraction of homicides committed with a firearm (FHH) proxies. Finally, under weaker assumptions, we use both the FSS and FHH proxies to estimate a less attenuated lower bound than derived from the FSS proxy alone.

5.3.1 Point Estimated Using Lagged Value of the FSS Proxy

Without fixed effects, combining the j^{th} component of Equation (2) with Equation (3) gives

$$Y_{jit} = X'_{it}\beta_j + W_{it}\delta_j - \varepsilon_{it}\delta_j + \eta_{jit}.$$

Then, the lagged value of the proxy, W_{it-1} , can serve as a proper instrument if U_{it} is serially correlated (so that $Cov[(X'_{it}, W_{it-1})', (X'_{it}, W_{it})']$ is non-singular) and ε_{it} is not serially correlated (so that $Cov[(X'_{it}, W_{it-1})', (\varepsilon_{it}, \eta_{jit})'] = 0$). A similar argument applies to the model with fixed effects after first differencing (see e.g. Griliches and Hausman (1986)).

To be clear, this model point identifies δ_j but the assumption that ε_{it} is not serially correlated may be too strong. Still, this assumption is weaker than assuming zero measurement error, and it is useful to examine the results from this point identified model that accounts for proxy errors.

Using a two-stage least squares estimator, the results without fixed effects corroborate our preferred partial identification model estimates. In particular, the point estimate for the gun homicide rate equation without fixed effects is 0.986, with a 95% CI of (0.539, 1.432). Thus, the point estimate found when accounting for measurement error is almost 20% larger than the estimate of 0.835 found using the OLS estimator. This is similar to the upper bound estimates of 0.988 when incorporating the suicide equation and the non-negative cross-equation correlation assumption (see Table 3). In the model with fixed effects, after first differencing, the two-stage least squares estimates are imprecise with wide confidence regions.

5.3.2 Point Estimates Using Two Proxies

Another model that point identifies the gun ownership coefficients uses both the FSS and FHH proxies. Let A_1 - A_3 hold for both proxies W_{1it} (log lagged FSS) and W_{2it} (log lagged FHH) where $W_{1it} = U_{it} + \varepsilon_{1it}$ and $W_{2it} = U_{it} + \varepsilon_{2it}$, and assume that $Cov(\varepsilon_{1it}, \varepsilon_{2it}) = 0$. Substituting W_{1it} for U_{it} , it follows that

$$Y_{it} = X'_{it}\beta + U_{it}\delta + \eta_{it} = X'_{it}\beta + W_{1it}\delta - \varepsilon_{1it}\delta + \eta_{it}$$

and the second proxy, W_{2it} , can serve as an instrument to point identify the equation coefficients. Symmetrically, one can substitute W_{2it} for U_{it} and use W_{1it} as an instrument. A similar argument applies to the within transformed variables in the fixed effect model.

We estimate this model using two stage least squares, with and without fixed effects. Similar to all the other models, the estimate of δ for the non-gun homicide coefficient is statistically insignificant at the five percent level. For the gun-homicide coefficient, estimates

without fixed effects are positive, significant, and substantially larger than the OLS estimate (0.835) which ignores the measurement error: 2.81 with 95% CI (1.86, 3.76) when FHH acts as an instrument and 2.46 with 95% CI (1.38, 3.54) when FSS acts as an instrument. The estimates with fixed effects are positive and large, but imprecise and statistically insignificant at the five percent level.

5.3.3 Lower Bound Estimates Using Two Proxies

While $Cov(\varepsilon_{1it}, \varepsilon_{2it}) = 0$ may not be a very credible assumption, it is weaker than the no measurement error assumption that $\varepsilon_{1it} = 0$ or $\varepsilon_{2it} = 0$. Although the coefficients are no longer point identified without this zero covariance assumption, δ can be bounded using the two proxy variables. In particular, Lubotsky and Wittenberg (2006) develop a method to use multiple proxies (e.g., FSS and FHH) to derive a less attenuated lower bound than found using a single proxy (e.g., FSS). The estimated lower bound using this approach is 1.24 with 95% CI (1.09, 1.40) in the model without fixed effects and 0.61 with 95% CI (0.38, 0.84) in the model with fixed effects.¹⁹ Thus, in the model without fixed effects, this lower bound estimate is similar to the upper bound estimates found in Section 4 when incorporating the suicide equation (see Table 3). In the model with fixed effects, this lower bound estimate is nearly 70% larger than the analogous estimate of 0.359 found when using the FSS proxy alone.

Finally, with multiple proxies, the procedure in Lubotsky and Wittenberg (2006) allows one of the slope coefficients in the proxy equations to be different from 1. Suppose $W_{1it} = \gamma_1 U_{it} + \varepsilon_{1it}$, so that a percent increase in the rate of gun ownership is associated with an approximately γ_1 percentage increase in FSS. In this case, the point estimate for γ_1 is 0.51 with standard error 0.29, and we cannot reject the hypothesis that $\gamma_1 = 1$ at the 5% level. Allowing for an unknown coefficient γ_1 on U_{it} in the FHH equation also leads to larger estimated lower bounds than when using the single FSS proxy. In the model without fixed effects, the resulting lower bound estimate is 0.94 with 95% CI (0.76, 1.11) and for the fixed effect model, the lower bound estimate is 0.46 with 95% CI (0.29, 0.63). We obtain even larger estimates of the lower bound if, instead, the slope coefficient in the FHH equation is unrestricted $W_{2it} = \gamma_2 U_{it} + \varepsilon_{2it}$. The estimate for γ_2 is 1.95 with standard error 1.11 and

¹⁹Following Lubotsky and Wittenberg (2006), we bootstrap the standard errors of the lower bounds in Section 5.3.3.

we cannot reject the hypothesis that $\gamma_2 = 1$ at the 5% level. The resulting lower bound estimates are 1.82 with 95% CI (1.44, 2.20) in the model without fixed effects and 0.89 with 95% CI (0.54, 1.25) in the model with fixed effects.

6 Conclusion

Given the high rates of gun violence and death in the United States, there has been keen interest in understanding the association between gun ownership rates and homicide. Are more guns associated with more violent crimes? Yet, there is a lack of basic data on gun ownership rates across states over time. Faced with this paucity of information, researchers have turned to using the fraction of suicides committed with a firearm (FSS) as a credible and reliable proxy measure of gun ownership that is consistently observed in each state and year. However, the existing research does not account for the fact that the proxy is an error-ridden measure of the ownership rate.

In this paper, we consider set- and point-identified models that formally account for the measurement error in the proxy for gun ownership. In particular, we apply Chalak and Kim (2020) to provide informative bounds on the mean association between homicide and firearms ownership. Assuming a classical measurement error model, the estimated lower bound on the magnitude of the association corresponds to the conventional linear regression model estimate which implies a notable positive association between gun ownership rates and gun homicide. The upper bound depends on prior information about the measurement error process. Based on the validation literature for gun ownership proxies, our preferred model restricts the FSS reliability ratio to be at least 0.6561. For most of the models we consider, this yields an upper bound on the gun homicide elasticity that is substantially larger than the regression estimates that do not account for measurement error. For example, in the models with state and year fixed effects, we find that the elasticity of homicide rates with respect to gun ownership rates is at least 0.359, the regression estimate, but may be as large as 1.011. Moreover, in Section 5, we consider three point-identified models that rely on earlier validation studies and instrumental variables respectively. We also consider less attenuated lower bound estimates using multiple proxies. We find point estimates and lower bound estimates that are substantially larger than the OLS estimates and, in some cases,

(nearly) equal the upper bound estimates reported in Section 4. Together, the result from these various models suggest that the association between gun homicide and ownership rates is substantially larger than found in the earlier literature.

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