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Guns and Homicides: A Multiscale Geographically Weighted Instrumental Variables Approach

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This article assesses the locally varying effects of gun ownership levels on total and gun homicide rates in the contiguous United States using cross-sectional county data for the period 2009–2015. Employing a multiscale geographically weighted instrumental variables regression that takes into account spatial nonstationarity in the processes and the endogenous nature of gun ownership levels, estimates show that gun ownership exerts spatially monotonically negative effects on total and gun homicide rates, indicating that there are no counties supporting the "more guns, more crime" *hypothesis for these two highly important crime categories. The number of counties in the contiguous United States where the* "more guns, less crime" *hypothesis is confirmed is limited to at least 1258 counties (44.8% of the sample) with the strongest total homicide-decreasing effects concentrated in southeastern Texas and the* deep south. *On the other hand, stricter state gun control laws exert spatially monotonically negative effects on gun homicide rates with the strongest effects concentrated in the southern tip of Texas extending toward the* deep south.

Introduction

A year after the Sandy Hook elementary school shooting, gun production in the United States reached 10.8 million per year with an average annual growth rate of about 8.3% in the 2000–2013 period (ATF 2015). The Congressional Research Service report shows that the estimated number of firearms available to civilians in the United States reached about 310 million in 2009 or about 1 gun per capita (Krouse 2012). This amounts to slightly less than double that of Yemen, the country with the second highest private gun ownership rate (Karp 2007). The latest figures from the Center for Disease Control show that firearm-related death rate in the United States is 11.6 per 100,000 population, the highest in the developed world and accounts for about 75% of all homicides and 51% of all suicides in 2016.

Social gun culture, high levels of gun ownership, and high firearm-related crime rates make America a unique geography to study the effects of gun ownership levels on crime and violence.

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The theoretically ambiguous sign of this effect and the statistical disunity makes the scholarly research even more complicated and challenging with dozens of articles piled up on both sides of the debate. Increasing gun levels could deter crime by arming potential victims and thereby increasing the prospective criminals' risk of facing an armed response and the expected costs of illegal activity (*the deterrent effect*) (Becker 1968; Ehrlich 1973). This argument is rooted in the concept known as the *instrumentality effect*. If an individual were committed in the act of killing someone, the individual would substitute the gun for another weapon. Hence, the weapon itself has no bearing on fatal outcomes and the intention plays a key role (Wolfgang 1958). Adopted by gun rights advocates, the *instrumentality effect* suggests that higher gun ownership, therefore, should not escalate violence and that restricting gun availability is futile (Gabor 2016).

On the other hand, higher gun availability in the overall population may increase the likelihood of gun theft and increase illegal firearm availability (Khalil 2017). This in turn may facilitate the commission of certain types of crimes, such as robbery (*the facilitating effect*). The sight of a gun during an altercation may also trigger violence (Kleck, Kovandzic, and Bellows 2016). Proponents further argue that the risk factors associated with the choice of weapon are greater for firearm due to its lethality. If the deterrent (facilitating) effect dominates the facilitating (deterrent) effect, then increasing gun levels would lead to a net decrease (increase) in crime rate.

The way gun control laws could affect crime rates is even more intricate. Gun control laws are designed to reduce crime and violence by blocking acquisition, possession, and use of guns in the high-risk population, such as drug addicts, convicted criminals, and severely mentally ill individuals, who are susceptible of committing criminal acts (Kleck, Kovandzic, and Bellows 2016). In the presence of high gun ownership in the overall population, whether stricter gun control laws would reduce crime and violence is a knotty problem.

While a jurisdiction may have perfect control over the possession and the use of guns in the high-risk population (say through the right-to-carry (RTC) repeal and background checks), it may not have perfect control over the acquisition of guns because of spillover effects and leakages (e.g., gun theft). These spillover effects make laws unable to perfectly distinguish high-risk groups from the rest of the population. Another spillover is that stricter gun control laws incentivize criminals to acquire guns from neighboring jurisdictions where laws are lenient. If laws are able to distinguish high-risk groups from the rest of the population, but only imperfectly, stricter gun control laws may disarm low-risk population, reduce their ability to defend themselves in a criminal event, and consequently increase crime rates (Kleck, Kovandzic, and Bellows 2016).

This article aims to bring the debate on gun policy to a whole new level by allowing the effects of gun ownership levels (and those of all other observable predictors) to vary over space and thereby relaxing the assumption of spatially monotonic effects of gun ownership level while at the same time controlling for its endogenous nature. For this purpose, this article invokes a multiscale geographically weighted instrumental variables regression (MGWIVR) approach. The method estimates a unique parameter of gun ownership and gun control laws for every county in the contiguous United States and exploits spatial heterogeneity in the processes. Allowing these effects to vary over space enables us to identify locations where gun levels and gun laws exert the strongest, the weakest, and possibly the most perverse effects on crime rates. Most importantly, our approach enables us to identify whether the locally varying effects of gun ownership levels and gun laws are monotonic across space. If the impact of gun levels on crime rates is spatially nonmonotonic and therefore nonstationary, then both the "more guns, less crime" and "more guns, more crime" hypotheses may hold and coexist at different locations. Identifying the local variation in the effects of gun levels is particularly useful to implement county-specific policy prescriptions to combat gun violence and crime.

The locally varying surface of estimates of the effects of gun ownership levels on total and gun homicide rates using the MGWIVR approach shows that gun ownership levels exert spatially monotonically negative effects on total and gun homicide rates, indicating that there are no counties in the United States supporting the "more guns, more crime" hypothesis for these two highly important crime categories. The number of counties in the contiguous United States where the "more guns, less crime" hypothesis is confirmed at conventional test levels is limited to at least 1258 counties with the strongest total homicide-decreasing effects concentrated in southeastern Texas and the deep south. For the rest of the contiguous United States, gun ownership levels have no (spatially varying) effects on total homicide rates. Consequently, the one-size gun policy does not fit all and local efforts may be more efficient and cost-effective in combating crime. Consistent with the local results, the global estimates that equivalently take the endogeneity of gun ownership levels into account but that fail to deal with spatial heterogeneity in the processes, also favor the "more guns, less crime" hypothesis. On the other hand, stricter state gun control laws exert spatially monotonically negative effects on gun homicide rates with the strongest effects concentrated in the southern tip of Texas extending toward the deep south.

Section "Prior literature" briefly assesses the prior literature on gun control; Section "Gun policy and causal reasoning" emphasizes the indispensableness of controlling for firearm availability along with gun control laws (and vice versa) using a simple signed directed acyclic graph (DAG) representation and examines its implications regarding the sign of the effect of gun ownership levels on crime rates; Section "A multiscale geographically weighted instrumental variables regression approach" introduces the empirical strategy where gun ownership level is treated as an endogenous covariate of interest and the effects of gun ownership levels and state gun control laws on homicide rates are allowed to vary over space; Section "Results" reports the comparative results of the global estimates and those of MGWIVR and Section "Concluding remarks" concludes.

Prior literature

The stalemate in the gun control debate, fueled by the seminal Lott and Mustard (1997) analysis, appears to be the product of ideological predisposition, statistical cherry picking, and technical issues surrounding the identifying assumptions of the effects of gun policy or gun ownership. Even the background of the researcher contributes to how the controversial issues of gun control have been analyzed, presented, and published. Gun control debate in the United States academic environment is largely driven by three groups. The first group of law and economics scholars typically focuses on aspects of gun control laws that are thought to be the most crucial in shaping gun policy. Studies on the impact of RTC laws on crime rates are divided between those that support the more guns, more crime hypothesis (Ludwig 1998; Ayres and Donohue 2003; Donohue 2004; Aneja, Donohue, and Zhang 2011), those that support the more guns, less crime hypothesis (Bronars and Lott 1998; Moody 2001; Plassmann and Whitley 2003; Barati 2016) and those that find either mixed (Olson and Maltz 2001) or no effect (Black and Nagin 1998; Helland and Tabarrok 2004). These studies appear in highly prestigious law and economics journals and law reviews with 30–60 pages devoted to convey the credibility of the analysis. While they are highly regarded for scientific rigor, less than a handful of studies control for gun ownership levels (Dezhbakhsh and Rubin 1998; Rubin and Dezhbakhsh 2003) and other substantive provisions of the law along with the main covariate of interest. A number of studies in this group focuses on other substantive areas such as the permit-to-purchase (PTP) and stand your ground (SYG) laws. While those that elucidate the impact of SYG laws suggest of a homicide-increasing effect (McClellan and Tekin 2016; Gius 2016), the impact of PTP laws on violence is either positive (Webster, Crifasi, and Vernick 2014), negative (Rudolph et al. 2015), or insignificant (Gius 2017).

In a striking contrast, the second group of public health researchers primarily focuses on the effects of gun ownership levels or gun control laws on homicide rates using state-level panel data. As a consequence of using longitudinal data, they suffer from the use of proxy measures for gun availability whose validity is unwarranted and possibly fail to establish a valid statistical association between gun levels and homicide rates with the exception of two studies that appear to use appropriate gun measures (Siegel, Ross, and King 2013; Monuteaux et al. 2015). This group of studies too appear in very prestigious public health journals where the number of article pages is smaller than the number of authors. The unfortunate custom of significantly limiting the word count in these journals to save space diminishes the credibility of these studies. Empirical findings in this group unambiguously favor the "more guns, more crime" hypothesis, yet there is not a single study that even acknowledges the endogenous relationship between gun ownership levels and homicide rates or the severe consequences of ignoring the problem.

The third group of criminologists is probably the one with the utmost scientific rigor to elucidate the effects of gun control laws or gun ownership levels on crime. Criminologists, as opposed to public health scholars, tend to favor county- or city-level cross-sectional data in order to (1) mitigate abrupt yearly fluctuations in homicide data through averaging; (2) minimize aggregation bias and explore intrastate variation in violence that would have been not possible in a state-level analysis; (3) find valid proxies for gun ownership levels that are otherwise invalid or absent in longitudinal contexts; and (4) find valid sources of exogenous variation to instrument gun ownership levels that are otherwise extremely difficult to find in a longitudinal setting.

It is not just a matter of chance that only the third group of criminologists and 3 studies out of some 40, use a valid proxy for gun ownership, accounts for its endogenous nature, and includes more than five significant control variables (Kleck and Patterson 1993; Kovandzic, Schaffer, and Kleck 2012, 2013). All prior studies on the effects of gun ownership levels on crime that used a valid proxy for gun levels and valid instruments for gun ownership point to *more guns, at least, do not mean more crime* while those that neglect both aspects tend to favor the "more guns, more crime" hypothesis (Kovandzic and Marvell 2003; Kovandzic, Marvell, and Vieraitis 2005; Kleck 2015; Kleck, Kovandzic, and Bellows 2016; Moore 2017).

The extant empirical literature on the effects of gun policy overlooks the fact that death, violence, and crime are not randomly distributed across space. Although a number of studies attempt to capture spillover effects of gun control laws (Bronars and Lott 1998; Moorhouse and Wanner 2006; Coates and Pearson-Merkowitzz 2017) and illegal firearms (Khalil 2017), they informally and rather arbitrarily do so without any reference to the structure of the spatial associations or the spatial autocorrelation in the data. Failure to control for spatial dependence results in unreliable statistical inference depending on the actual patterns of spatial dependence. While in the best-case scenario, the standard errors of the estimated effects are inconsistent (Elhorst 2010), in the worst case the parameter estimates are biased and inconsistent (LeSage and Pace 2009; Gibbons and Overman 2012).

Perhaps the most striking drawback is that all prior studies that used county-level data have succumbed to the fallacy of "one size fits all". Studies ultimately estimated a single number, an average effect, that is supposed to accurately represent the effect of gun laws or gun levels on crime everywhere. This is certainly not realistic. What is more grave is that the fixed sign, rather than the magnitude, of this effect is assumed to reign everywhere (i.e., spatially monotonic). The inferences drawn from this “one size fits all” view could endanger public policy if the effects of gun policy are unequal and ambivalent across space (i.e., spatially nonmonotonic).

Gun policy and causal reasoning

In order to understand why any analysis that aims to identify the effects of gun control laws (gun levels) on crime should control for or condition on gun levels (gun control laws), consider the simple DAG given in Fig. 1. Let **G**, **P**, and **C**, respectively, denote gun control law, gun level, and crime rate. The association between gun levels and gun laws arises from the political risks involved with introducing stricter gun control laws in areas of profound social gun culture and high gun ownership (Kleck, Kovandzic, and Bellows 2016). This situation is depicted in Fig. 1a by the directed edge **P⟶G**. Hence, gun ownership is a confounder in the relationship between gun control laws and crime rates (Kleck, Kovandzic, and Bellows 2016). If **P** is left unconditioned for, the association between **G** and **C** not only reflects the causal effect of **G** on **C** but also the causal effect of **P** on **C** and **P** on **G**.

A different argument claims that gun control laws not only restrict access to guns by high-risk subpopulations but also by the general population. Hence, gun control law is a confounder in the relationship between gun levels and crime rates (Kleck and Patterson 1993). This situation is depicted in Fig. 1b by the directed edge **G⟶P**. If **G** is left unconditioned for, the association between **P** and **C** not only reflects the causal effect of **P** on **C** but also the causal effect of **G** on **C** and **G** on **P**.

If stricter gun control laws are associated with lower gun ownership levels, that is the directed path **P⟶G** in Fig. 1a and the directed path **G⟶P** in Fig. 1b are of negative sign and that stricter gun control laws are designed to reduce crime rates, that is the directed path **G⟶C**

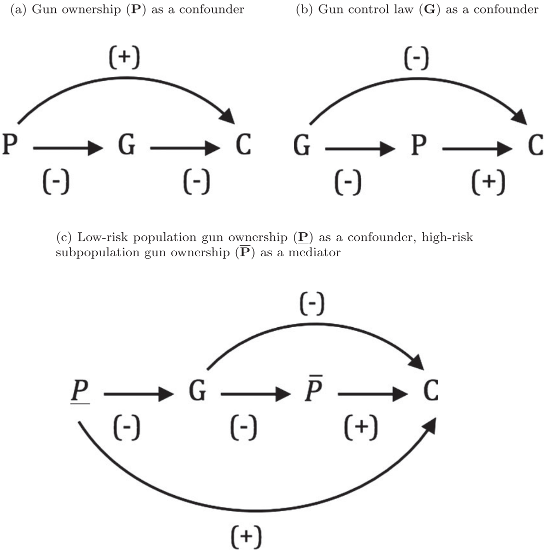


Figure 1. Conceptual framework—gun ownership, gun laws, and crime rates.

is of negative sign, the sign of the directed path $\mathbf{P} \longrightarrow \mathbf{C}$ must be the product of the signs of the edges that constitute that path (VanderWeele and Robins 2010). Hence, the sign of the directed path $\mathbf{P} \longrightarrow \mathbf{C}$ should be positive, suggesting that more guns lead to more crimes.

Even if gun control laws were assumed to restrict access to guns by high-risk groups only and not the overall population, the above inference would hold. The situation that reconciles both the confounding and the mediating effect of gun levels is depicted in Fig. 1c. We distinguish between high-risk ($\bar{\mathbf{P}}$) and low-risk ($\underline{\mathbf{P}}$) subpopulations. Accordingly, higher gun ownership in the low-risk population (legal firearm availability) is what makes it politically infeasible to enact stricter gun control laws, hence the sign of the directed edge $\underline{\mathbf{P}} \longrightarrow \mathbf{G}$ is negative. Gun control laws only restrict access to guns by high-risk subpopulation (illegal firearm availability) and thereby reduce crime rates; hence the sign of the directed edge $\mathbf{G} \longrightarrow \bar{\mathbf{P}}$ is negative. Higher gun ownership levels in the high-risk subpopulation are likely to increase crime rates; hence the sign of the directed edge $\bar{\mathbf{P}} \longrightarrow \mathbf{C}$ is positive. Since the sign of the directed edge $\mathbf{G} \longrightarrow \mathbf{C}$ is the product of the signs of the directed edges $\mathbf{G} \longrightarrow \bar{\mathbf{P}}$ and $\bar{\mathbf{P}} \longrightarrow \mathbf{C}$, the sign of the directed edge $\mathbf{G} \longrightarrow \mathbf{C}$ is negative. This would be true if stricter gun control laws reduce crime rates through, say, prohibiting unlicensed carrying of guns. This setting accounts for the possibility that gun ownership level in the low-risk population is a confounder while gun ownership level in the high-risk subpopulation is a mediator in the relationship between gun control laws and crime rates. This situation indicates that the sign of the directed edge $\underline{\mathbf{P}} \longrightarrow \mathbf{C}$ should be positive since the sign of this edge is the product of the signs of the edges that constitute that path (the directed edges $\underline{\mathbf{P}} \longrightarrow \mathbf{G} \longrightarrow \bar{\mathbf{P}} \longrightarrow \mathbf{C}$ and $\underline{\mathbf{P}} \longrightarrow \mathbf{G} \longrightarrow \mathbf{C}$ are both positive). Hence, more guns lead to more crime.

The statistical implication is that both variables should be controlled for in any analysis of gun policy on crime rates irrespective of the sign of the directed paths and that more guns lead to more crimes irrespective of whether the gun level is a confounder or a mediator in the relationship between gun control laws and crime rates. The "more guns, more crime" implicitly assumes that the facilitating effect must dominate the deterrent effect. For the deterrent effect to dominate the facilitating effect, that is, for the sign of $\underline{\mathbf{P}} \longrightarrow \mathbf{C}$ to be negative and hence "more guns mean less crime", one of the following three cases should prevail: (1) stricter gun control laws are plausible even with higher gun ownership levels in the low-risk population, that is, the sign of the directed edge $\underline{\mathbf{P}} \longrightarrow \mathbf{G}$ should be positive, or (2) stricter gun control laws should aggravate crime rates and gun ownership in the high-risk subpopulation should reduce crime rates, that is, the sign of the directed edge $\mathbf{G} \longrightarrow \mathbf{C}$ should be positive while the sign of the directed edge $\bar{\mathbf{P}} \longrightarrow \mathbf{C}$ should be negative, or (3) stricter gun control laws should increase gun ownership in the high-risk subpopulation which should in turn, reduce crime rates, that is the sign of the directed edge $\mathbf{G} \longrightarrow \bar{\mathbf{P}}$ should be positive while the sign of the directed edge $\bar{\mathbf{P}} \longrightarrow \mathbf{C}$ should be negative.

This simple representation, however, does not take into account the relative weights of the deterrent and the facilitating effects of gun levels on crime that move in opposite directions, nor does it take into account other statistical challenges such as the presence of other potentially confounding variables or methodological challenges such as the possibly endogenous or the nonmonotonic and spatially varying processes in $\mathbf{P}$. In the following section, we relax (1) the assumption of independence of errors, (2) the exogeneity of gun ownership levels, and (3) the spatial homogeneity in the processes that brings up the possibility that the spatially varying effects might be nonmonotonic across space.[1]

A MGWIVR approach

Data and sample

This study employs a cross-sectional design using county-level data, averaged over the 2009–2015 period. The sample period begins in 2009 because data on some of the covariates and on sources of exogenous variation are only available around this date. The sample ends in 2015 because it is the latest year for which data on crime rates and most of the control variables are available at the county level. The sample includes all counties in the contiguous United States, excluding the District of Columbia and Bedford City, Virginia due to missing observations. Furthermore, a number of counties in the United States, typically clustered along the border that separates the West from the Midwest and South divisions, have very small populations (<5,000 inhabitants). These small counties are likely to act as outliers and the percentage statistics will have huge uncertainties.[2] Therefore, a total of 293 counties in the contiguous United States that have <5,000 inhabitants are further removed from the sample. This results in 2,810 observations to be used in the analysis.

Total and gun homicides are retrieved from the Center for Disease Control-Wide-ranging Online Data for Epidemiologic Research (CDC–WONDER).[3] The overall strength of state gun control laws is obtained from Siegel et al. (2017). The index contains an uninterrupted cross-section time-series information on the overall strength of state gun control laws for all 50 states for the 1991–2016 period. Using Thomson Reuters Westlaw and the Everytown for Gun Safety databases, the authors developed a database that indicates the presence/absence of a total of 133 provisions, covering 14 aspects of state gun policies.[4]

One of the most important measures that should be controlled for in gun policy studies to isolate the effect of gun control law is gun ownership. However, uninterrupted time-series data are not available and the only available measures are based on General Social Survey conducted by the National Opinion Research Center in 2013 and in the 1973–2006 period, the survey conducted by the Behavioral Risk Factor Surveillance System for the 1992–1995 period, and the surveys conducted on behalf of the Harvard Injury Control Research Center in 1996 and 1999 (Azrael, Cook, and Miller 2004). These surveys are representative at the national or at the state level and they are likely to produce unreliable estimates due to small sample size, response bias, inadequate response rates, and sampling errors. However, ample evidence shows that the percentage of gun suicides is a valid proxy for gun ownership levels in cross-sectional studies (Kleck and Patterson 1993; Kleck 2004; Azrael, Cook, and Miller 2004; Kovandzic, Schaffer, and Kleck 2012, 2013; Moore 2017). Data on total suicides and suicides committed by a gun are retrieved from the CDC-WONDER to calculate gun suicide ratio.[5]

In addition to gun control laws and gun ownership levels, this study controls for 16 county-level variables that have been routinely used in the analysis of crime: unemployment rate (Kleck and Patterson 1993; Duggan 2001), population share of age groups (Duggan 2001; Kovandzic, Schaffer, and Kleck 2012; Kleck, Kovandzic, and Bellows 2016), population density, racial/ethnic composition (Cook and Ludwig 2006; Kleck, Kovandzic, and Bellows 2016; Kleck and Patterson 1993), educational attainment (Kleck and Patterson 1993; Kleck, Kovandzic, and Bellows 2016), income inequality (Kelly 2000; Fajnzylber, Lederman, and Loayza 2002; Stolzenberg, Eitle, and D'Alessio 2006; Choe 2008), and poverty rate (Kovandzic, Schaffer, and Kleck 2012; Siegel, Ross, and King 2013). Poverty rate, racial/ethnic composition, and the age interval–specific population measures are retrieved from the U.S. Census Bureau. Unemployment rates are retrieved from the Bureau of Labor Statistics (BLS). The Gini index of income inequality and the adult percentage with a bachelor degree are obtained from U.S. Census Bureau, the American Community Survey (ACS), two waves of 5-year estimates (2006–2010, 2011–2015) because the Census Bureau does not collect information for every year for all counties. Table 1 provides a description of each variable used along with their means and standard deviations.

Spatial heterogeneity

Global models assume spatial homogeneity in the processes and compute a single statistic that represents the average relationship between crime rate and its predictors (i.e., the relationship is stationary across space). The geographically weighted regression (GWR) of Brunsdon, Fotheringham, and Charlton (1996), Fotheringham, Charlton, and Brunsdon (1996), Brunsdon, Fotheringham, and Charlton (1998), and Fotheringham, Brunsdon, and Charlton (2003) relaxes the assumption of spatial homogeneity and allows the parameter estimates of a regression to vary locally to account for the case that counties may differ from each other not only in terms of crime rates but also in terms of its causes. Therefore, gun levels and gun laws may have different effects on different geographies. However, GWR constrains the local relationships within each model to vary at the same spatial scale.

An important extension of the GWR, called multiscale GWR or MGWR, allows each relationship in the model to vary at a unique spatial scale and therefore computes covariate-specific bandwidths as opposed to a single bandwidth (Fotheringham, Yang, and Kang 2017; Yu et al. 2019). This less restrictive extension minimizes overfitting, reduces bias in the parameter estimates, and mitigates concurvity (Oshan et al. 2019). The MGWR model takes the form:

$$\mathbf{y} = \eta_{bw}\left(u_i, v_i\right)\mathbf{p}_i + \sum_k \beta_{bwk}\left(u_i, v_i\right)\mathbf{x}_{k,i} + \boldsymbol{\varepsilon}_i \tag{1}$$

where $\mathbf{y}$ is the homicide rate per 100,000 population, $\mathbf{p}$ is gun ownership at location i, $\mathbf{x}_{k,i}$ is the kth covariate or included instrument at location i, $\eta_{bw}\left(u_i, v_i\right)$ and $\beta_{bwk}\left(u_i, v_i\right)$ are the locally varying coefficients where η_{bw} indicates the bandwidth used for the calibration of the conditional relationship between homicide rate and gun ownership, β_{bwk} indicates the bandwidth used for the calibration of the kth conditional relationship, $\left(u_i, v_i\right)$ is the x–y coordinate of the ith location, and $\boldsymbol{\varepsilon}_i$ is the Gaussian error at location i. $\mathbf{x}_i$s consist of control variables such as state gun control law, unemployment rate, Gini index of income inequality, poverty rate, percentage of population with a bachelor degree, percentage of African-American, Native-American, and Hispanic population, population density, urban dummies, and population share of age groups (0–19, 20–34, 35–44, 45–64 years of age).[6]

Our reasoning in Section "Gun policy and causal reasoning" suggests that any analysis that aims to identify the impact of gun control laws or gun ownership levels on crime rates should condition on both variables. If gun level (gun law) is left uncontrolled for, then the parameter estimate of gun control law (gun level) will be biased due to the omission of gun levels (gun control law) since it will be contained in the errors of the model. This situation leads to an endogeneity problem in which gun law (gun level) will be correlated with the unexplained determinants of crime rates (Kovandzic, Schaffer, and Kleck 2012). While this type of endogeneity bias can be circumvented by explicitly accounting for the gun level (gun law) variable, a second source of endogeneity bias arises due to reverse causation that runs from gun ownership levels to crime rates. That is, the fear of victimization induced by higher violence rates may drive up gun ownership levels (Kovandzic, Schaffer, and Kleck 2012, 2013; Kleck, Kovandzic, and Bellows 2016).

If gun levels are endogenous due to reverse causality, the estimate of the effect of gun ownership on crime rates will be biased and inconsistent. This problem persists in a MGWR framework. A way to get around this problem is to manually estimate a MGWIVR model in the same spirit as performing a global two-stage least squares (2SLS). In the first-stage, gun ownership is regressed on all the included and excluded instruments. The first-stage MGWIVR is:

$$\mathbf{p} = \sum_{q} \omega_{bwq} \left(u_i, v_i\right) \mathbf{w}_{q,i} + \sum_{k} \beta_{bwk} \left(u_i, v_i\right) \mathbf{x}_{k,i} + \boldsymbol{\epsilon}_i \tag{2}$$

where $\mathbf{w}_{q,i}$ is the qth excluded instrument at location i and $\omega_{bwq}\left(u_i, v_i\right)$ are the locally varying coefficients of the excluded instruments, indicating the bandwidth used for calibration of the qth conditional relationship. In the second-stage, crime rate is regressed on the predicted value of gun ownership ($\hat{\mathbf{p}}_i$) and all the included instruments from the first-stage.[7] The second-stage MGWIVR is:

$$\mathbf{y} = \eta_{bw} \left(u_i, v_i\right) \hat{\mathbf{p}}_i + \sum_{k} \beta_{bwk} \left(u_i, v_i\right) \mathbf{x}_{k,i} + \boldsymbol{\varepsilon}_i \tag{3}$$

where $\hat{\mathbf{p}}_i = \sum_q \hat{\omega}_{bwq}\left(u_i, v_i\right) \mathbf{w}_{q,i} + \sum_k \hat{\beta}_{bwk}\left(u_i, v_i\right) \mathbf{x}_{k,i}$.

The MGWR estimates a separate regression and uses a different weighting for each observation. Observations of closer locations have more influence on each other than observations that are spatially apart (Tobler 1970). The weight assigned to each observation is based on a distance decay function between the county centroids.[8]

The locally varying coefficient estimate of $\eta\left(u_i, v_i\right)$ yields an unbiased estimate of the effect of gun ownership level provided that a source of exogenous variation should be found such that it might plausibly be viewed as randomly moving. It should be strongly correlated with gun ownership (i.e., relevant), should exhibit an impact on crime rates through and only through gun levels, and should not be directly related to crime rates (excludable) or the errors of the model (i.e., clean). Following Kleck, Kovandzic, and Bellows (2016) and Kleck and Patterson (1993), the primal candidates that might satisfy such properties are a measure of political conservatism, captured by the county vote share for the Republican presidential candidate in the 2008 election obtained from *The Guardian*, and state hunting license rate obtained from the US Fish and Wild Service, Wildlife & Sport Fish Restoration Program.[9]

Estimating equations (2) and (3) manually is not ideal because the standard errors of the locally varying parameter estimates obtained from a manual second-stage MGWIVR are likely to be incorrect for the same reason that the standard errors of the estimates obtained from a global manual IV are incorrect. Currently, there is no known solution that simultaneously deals with endogeneity and incorrect standard errors of a manual IV in a MGWR framework. Therefore, the costs of ignoring endogeneity by estimating a conventional MGWR as in equation (1) (i.e., locally varying but biased and inconsistent parameter estimates if gun levels are truly endogenous) should be weighed against the costs of estimating a manual MGWIVR as in equations (2) and (3) (i.e., locally varying and unbiased but imprecise parameter estimates when endogeneity is accounted for) or against the costs of estimating a global IV (i.e., unbiased but erroneously fixed and monotonic estimates with correct standard errors). Our objective is to obtain a unique estimate of the effect of gun ownership levels on violence for each county and with a small bias

Table 1. Descriptive Statistics

Variable	Pop. weighted mean (SD)	Unweighted mean (SD)	Min	Max
Homicides				
Total homicide rate	5.18 (4.19)	4.14 (4.11)	0	61.16
Gun homicide rate	3.66 (3.58)	2.65 (3.14)	0	37.96
Gun ownership				
Gun suicide ratio, %	47.96 (15.05)	60.65 (18.59)	0	100
State Gun Control Law				
Strength index	38.13 (31.40)	21.42 (19.27)	4	100
Socioeconomic status				
Income inequality (Gini index)	0.45 (0.04)	0.44 (0.03)	0.32	0.60
Poverty rate, %	15.43 (5.35)	16.88 (6.33)	3.13	49.84
Unemployment rate, %	7.93 (2.02)	7.72 (2.58)	1.72	26.49
Bachelor degree or higher, %	18.07 (5.89)	12.93 (5.30)	2.72	42.19
Racial/ethnic composition				
African-American population, %	13.11 (13.04)	9.18 (14.61)	0	85.82
Hispanic population, %	16.79 (16.75)	8.58 (13.37)	0.09	95.84
Native-American population, %	1.19 (3.12)	1.92 (6.69)	0	95.28
Demographics				
Population share (0–19 yrs), %	26.34 (3.03)	25.60 (3.38)	9.78	43.49
Population share (20–34 yrs), %	20.61 (3.67)	18.00 (3.95)	9.07	47.13
Population share (35–44 yrs), %	12.98 (1.39)	11.83 (1.50)	5.92	19.15
Population share (45–64 yrs), %	26.27 (2.63)	27.76 (3.00)	8.38	42.02
Population share (65+ yrs), %	13.80 (3.61)	16.81 (4.22)	3.75	45.46
Urbanization				
Population density	2140 (6790.17)	264.1 (1772.1)	0.13	71073
Very small urban dummy [0 : 25K)	0.06 (0.23)	0.48 (0.50)	0	1
Small urban dummy [25K : 50K)	0.07 (0.26)	0.20 (0.40)	0	1
Medium urban dummy [50K : 75K)	0.05 (0.21)	0.08 (0.27)	0	1
Large urban dummy [75K : 100K)	0.04 (0.19)	0.05 (0.21)	0	1
Very large urban dummy [100K+]	0.78 (0.42)	0.19 (0.39)	0	1
Excluded IVs				
Republican vote share, 2008	45.83 (14.39)	56.89 (13.80)	8.58	92.64
State hunting license rate	0.12 (0.13)	0.20 (0.19)	0.02	1.02

Notes: N = 3,103. All statistics are based on the 2009–2015 averages except income inequality and the population share with a bachelor degree, averaged over the 2006–2015 period. Homicides are per 100,000 population. Standard deviations in parentheses.

than a precise one of a drastically wrong quantity (Rubin 2006). We, therefore, proceed with a manual MGWIVR despite its obvious drawback. The linearity of the MGWR on the other hand rules out the fallacy of estimating a *forbidden regression* (i.e., performing a manual IV on a nonlinear model by plugging the fitted values from the first stage).[10]

The selection of the bandwidth is the most important part of the MGWR analysis. The bandwidth determines the way each observation is weighted and the way these weights decline with distance. An adaptive bandwidth selects a different bandwidth for each location so that all regression points have the same number of nearest neighbors. The bandwidth is chosen by minimizing the small sample bias–corrected Akaike information criterion (AICc). Throughout all MGWIVR models, an adaptive bi-square kernel is chosen so that the kernel bandwidth increases (decreases) in areas where data points are sparse (plenty) (Fotheringham, Brunsdon, and Charlton 2003).[11] The adaptive bi-square kernel is $\mathbf{W}_{ij} = \begin{cases} \left[1-\left(\frac{d_{ij}}{G_i}\right)^2\right]^2 & \text{if } d_{ij} < G_i \\ 0 & \text{otherwise} \end{cases}$ where d_{ij} is the distance between location i and j, and G_i is the distance from point i to its Mth nearest neighbor where M is the optimal number of nearest neighbor (Fotheringham, Yang, and Kang 2017).[12]

Results

Global generalized method of moments estimates

We first perform a generalized method of moments (GMM) estimation that produces global, consistent and efficient parameter estimates in the presence of arbitrary heteroscedasticity. The results are reported in Table 2. In all specifications, gun ownership level is instrumented by the vote share for the Republican presidential candidate in the 2008 election and state hunting license rate. Column (1) of Table 2 reports the first-stage IV results from a regression of gun ownership levels on all included and excluded instruments, columns (2) and (3) show the parameter estimates of gun ownership on total and gun homicide rates, respectively[13]. Counties may share common unobservable characteristics, leading to intercounty error correlation and underestimated standard errors. This would lead to over-rejection of the null of no effects of gun levels. Therefore, standard errors are clustered at the state level.

In column (1) of Table 2, both excluded IVs exert statistically significant and positive effects on gun ownership levels. The first-stage F statistic reported at the bottom of column (1), as a suggested measure to assess the explanatory power of the excluded instruments, is well above 10, indicating that the instruments are not weak (Bound, Jaeger, and Baker 1995; Staiger and Stock 1997). At the bottom of the remaining columns of Table 2, we report an exhaustive set of diagnostics on instrument relevance, instrument redundancy, instrument validity, and endogeneity. In order to test for instrument relevance, we report the heteroscedasticity consistent version of the Anderson canonical correlation LM statistic (Kleibergen–Paap LM statistic). When the excluded instruments are only weakly correlated with the endogenous variable, the IV estimates will be biased in the same direction as the OLS and the significance tests will have an incorrect size and confidence interval. Therefore, we further report weak identification-robust inference test results (Moreira 2003).

Expectedly, the endogeneity test suggests that gun ownership levels cannot be considered exogenous at conventional test levels for total and gun homicide rates. Instrument validity, assessed by the Hansen J statistic, indicates that the instruments are uncorrelated with the unobservable factors for both types of homicides. The underidentification test results suggest that the null hypothesis that, the excluded instruments are irrelevant, can be rejected at conventional test levels. The redundancy test further suggests that the vote share for the Republican presidential candidate in the 2008 election as a proxy for political conservatism is not redundant. Overall, the GMM-IV diagnostics indicate that the excluded IVs are relevant, clean, and excludable. The weak identification-robust inference test results à la Moreira (2003) further show that the null hypothesis, that the coefficient on gun ownership is zero, can be rejected at the 5% level.

In Table 2, gun ownership levels, proxied by gun suicide ratio, exert statistically significant and negative effects across both specifications when gun levels are treated endogenously, indicating that higher gun ownership levels lead to lower homicide rates. This initial evidence that favors the "more guns, less crime" hypothesis is in contradiction with the implications of the conceptual framework of Section "Gun policy and causal reasoning". However, neither the conceptual framework, nor the global models of Table 2 are able to assess whether the relationship between gun levels and homicide rates is nonstationary and possibly nonmonotonic across space. It might be the case that increases in gun levels may exhibit an impact of varying degrees and of alternating sign. This situation is of particular importance with respect to gun control debate because if the impact of gun levels on homicide rates is nonstationary and nonmonotonic across space, then both the "more guns, less crime" and "more guns, more crime" hypotheses may hold and coexist at different locations. A similar argument may be postulated with respect to possibly spatially heterogenous effects of state gun control laws. In Table 2, stricter state gun control laws reduce total and gun homicide rates. However, it is unrealistic to assume that gun control laws in any state would have the same effect on every sub-state region although the law exercises the same "degree" of strictness over its jurisdictional domain. Allowing the effects of state gun control laws to vary over space enables us to assess whether certain counties of a state are more responsive to stricter laws than others.

MGWIVR estimates

The assumption of spatial homogeneity is relaxed by estimating a MGWIVR that computes a unique parameter estimate for every county and for every explanatory variable. In order to control for the endogenous nature of gun ownership levels, we first estimate a first-stage MGWIVR by regressing gun ownership levels on all included and excluded instruments and obtain the fitted values. In the second stage, total and gun homicide rates are regressed on the fitted gun ownership values from the first stage and all the included instruments. In all summary statistics for the locally varying coefficients reported in Panel A of Tables 3–5, the bandwidths are covariate specific and the significance of the parameter estimates have been adjusted using the procedure proposed by da Silva and Fotheringham (2016).

Local effects of political conservatism on gun ownership

Panel A of Table 3 reports the summary statistics for the locally varying coefficients along with the geographic distribution of the first-stage MGWIVR results, mapped in Fig. 2a. The local estimates of the effects of political conservatism are captured by the 2008 vote share of the Republican presidential candidate. The strongest effects of increasing political conservatism are observed in the counties of Nevada, Utah, Colorado, Northern Pacific, upper Mountain states, Midwest, and Maine. The weakest effects on the other hand are observed in the East North Central Divisions. Expectedly, all statistically significant local parameter estimates are negative, indicating that political conservatism exerts spatially monotonic effects and that the rising republican presidential vote share is associated with increased gun ownership.

Fig. 2b maps the surface of estimates for the local effects of state gun control laws on gun ownership. Expectedly, stricter gun control laws are associated with spatially monotonically decreasing gun ownership throughout the United States, with stronger effects concentrated in Texas, Oklahoma, Kansas and Nebraska, Indiana, and Kentucky. All these states are known to have lenient gun control laws, relative to the rest of the United States. On the other hand, the weakest effects are concentrated in the Pacific division and the bordering states, and in counties of the states along the Middle Atlantic shore, extending to North Carolina. Albeit exceptions do exist, such as the upper Mountain states, most of them are characterized by very strict gun control laws. This suggests that gun control laws are most effective in reducing gun ownership in counties of the states where the laws are already lenient and are still effective but to a smaller extent in counties of the states where the laws are already strict.

Local effects of gun ownership on total homicide rates

Fig. 3a maps the geographic distribution of the local relationship between gun ownership levels and total homicide rates along with a summary in Panel A of Table 5. Again, all statistically significant parameter estimates of gun ownership on total homicide rates are of the same sign, indicating spatial monotonicity. However, the negative sign suggests that higher gun ownership is associated with lower total homicide rates in counties where this effect is statistically significantly different from zero at conventional test levels; hence “more guns, less crime”.

The spatial distribution of the locally varying effects of gun ownership on total homicide rates, shown in Fig. 3a, is concentrated in the East South Central and South Atlantic divisions with strongest effects in southeastern Texas and the deep south, comprising of Louisiana and Mississippi. Accordingly, more guns lead to lower total homicide rates in 1258 counties (about 45% of the sample). In the remaining 1552 counties or 55% of the contiguous United States, gun ownership does not affect total homicide rates.

Local effects of gun ownership on gun homicide rates

Fig. 3b maps the geographic distribution of the local relationship between gun ownership levels and gun homicides along with the model summary in Table 5. Again, all statistically significant parameter estimates of gun ownership levels on gun homicide rates are negative, indicating spatial monotonicity and confirm the “more guns, less crime” hypothesis.

The surface of parameter estimates for the gun homicide model are different than that of the total homicide model even though both models are identically specified except for the outcome variable. First, the number of counties with a statistically significant local estimate is much higher (2,307 counties or 82% of the sample) compared to that of the total homicide model and the surface of these estimates extend toward the entire Western United States. Second, the range of the absolute magnitude of these estimates is slightly smaller, relative to those reported in Section “Local effects of gun ownership on total homicide rates”.

Finally, Fig. 3c maps the surface of estimates for the local effects of state gun control laws on gun homicide rates. Again, these effects are spatially monotonically negative and are concentrated in the deep south and Southern Texas and comprises of 449 counties (about 16% of the sample) in the region. The overall snapshot in Fig. 3c is also consistent with the global GMM-IV models of Table 2 that suggest that stricter gun control laws are associated with reduced gun homicide rates.

Model diagnostics

Panel B of Tables 3–5 assesses the model performance of the MGWIVR over the GMM-IV by a comparison of the AICc; the adjusted $\mathbf{R}^2$ and the residual Moran’s I statistic (Moran 1950; Anselin 1995; Anselin et al. 1996; Anselin 1998). For both the first and the second stages, the AICc of the MGWIVR is lower than that of the GMM-IV regression (about 0.56% lower for the

Table 2. Global GMM-IV Estimates of the Effect of Gun Ownership on Homicide Rates

	Gun ownership	Total homicide	Gun homicide
Outcome variable	(1)	(2)	(3)
Constant	12.750 (14.060)	−8.488** (4.114)	−8.152** (3.360)
Gun ownership			
Gun suicide ratio, %	–	−0.065** (0.031)	−0.059** (0.024)
State Gun control law			
Strength index	−0.176*** (0.027)	−0.016** (0.008)	−0.016** (0.006)
Socioeconomic status			
Income inequality	−6.041 (15.50)	10.349*** (2.903)	8.081*** (2.311)
Unemployment rate, %	0.782*** (0.149)	−0.022 (0.037)	0.019 (0.028)
Poverty rate, %	0.241** (0.112)	0.163*** (0.039)	0.110*** (0.027)
Bachelor or + degree , %	−0.083 (0.071)	−0.101*** (0.022)	−0.067*** (0.016)
Racial/ethnic composition			
African-American population, %	0.107*** (0.038)	0.128*** (0.013)	0.106*** (0.011)
Hispanic pop., %	−0.108*** (0.025)	−0.024* (0.012)	−0.024*** (0.009)
Native-American population, %	−0.258*** (0.065)	0.072*** (0.020)	−0.010 (0.019)
Demographics			
Population share (0–19 yrs), %	0.041 (0.147)	0.119*** (0.036)	0.124*** (0.028)
Population share (20–34 yrs), %	0.392*** (0.116)	0.002 (0.034)	0.009 (0.028)
Population share (35–44 yrs), %	−0.307 (0.231)	0.028 (0.053)	−0.020 (0.044)
Population share (45–64 yrs), %	1.098*** (0.240)	0.231*** (0.049)	0.213*** (0.048)
Urbanization			
Population density	−0.0005*** (0.0001)	0.000 (0.000)	0.000 (0.000)
Small urban dummy [25K : 50K)	−1.222* (0.737)	0.384** (0.178)	0.261* (0.141)
Medium urban dummy [50K : 75K)	−2.305*** (0.770)	0.529*** (0.182)	0.383*** (0.143)
Large urban dummy [75K : 100K)	−4.251*** (1.020)	0.532*** (0.202)	0.308* (0.159)
Very large urban dummy [100K+]	−6.541*** (1.124)	1.057*** (0.256)	0.790*** (0.203)
Excluded IVs			
Republican vote share, 2008	0.280*** (0.038)	–	–
State hunting license rate	6.192** (2.527)	–	–
Spatial features			
Longitude	−0.120*** (0.042)	−0.049*** (0.010)	−0.040*** (0.008)
Latitude	−0.384*** (0.140)	−0.119*** (0.026)	−0.094*** (0.022)

Table 2. (Continued)

Outcome variable	Gun ownership (1)	Total homicide (2)	Gun homicide (3)
F-test of excluded instruments	35.213 [0.0000]	–	–
Underidentification test	–	21.977 [0.0000]	21.977 [0.0000]
Redundancy test	–	20.046 [0.0000]	20.046 [0.0000]
Hansen J statistic	–	0.1308 [0.7176]	0.0333 [0.8551]
Conditional LR	–	4.39 [0.0409]	6.04 [0.0166]
Endogeneity test	–	4.3113 [0.0379]	6.5617 [0.0104]
Weak identification test	–	35.213	35.213

Notes: The unit of observation is the county. $N = 2{,}810$. Total and gun homicides are per 100,000 population. Gun ownership is proxied by gun suicide ratio and is instrumented by the vote share for the Republican presidential candidate in elections of 2008 and hunting license rate. Underidentification test reports the Kleibergen–Paap rk LM statistic and the *P*-value for the null hypothesis that the equation is underidentified (i.e., the excluded instruments are irrelevant). Weak identification test reports the Kleibergen–Paap rk Wald F statistic and the *P*-value for the null hypothesis that the equation is weakly identified. Stock and Yogo (2005) weak identification test critical values for 10 and 15% maximal IV size are 19.93 and 11.59, respectively. Moreira (2003)'s conditional likelihood ratio reports the weak identification-robust inference likelihood ratio and the *P*-value for the null hypothesis that the coefficient of gun ownership is zero. Hansen J statistic reports the chi-square and the *P*-value for the joint null hypothesis that the instruments are uncorrelated with the error term and that the excluded instruments are correctly excluded from the estimated equation (i.e., the instruments are valid). The redundancy test reports the chi-square and the *P*-value for the null hypothesis that the instruments are redundant. The endogeneity test reports the chi-square and the *P*-value for the null hypothesis that gun ownership is exogenous. All variables are averaged over the 2009–2015 period except income inequality and the population share with a bachelor degree which were averaged over the 2006–2015 period. Standard errors in parentheses are clustered at the state level and are robust to arbitrary heteroskedasticity.
*Statistical significance at the 10%; **Statistical significance at the 5%; ***Statistical significance at the 1% level respectively.

first stage and 6.04% and 8.2% lower for the second stage of the total homicide and gun homicide models respectively), suggesting that the global regression is inadequate. On the other hand, the adjusted $\mathbf{R}^2$ for the first and second stage of MGWIVR models are conspicuously higher than their respective GMM-IV counterparts. Specifically, the adjusted $\mathbf{R}^2$ for the second stage of total and gun homicide rates under the MGWIVR models are 29% and 42% higher than those of the GMM-IV (Panel B of Tables 4 and 5).

Finally, the residual Moran's I is displayed in Panel B of Tables 3–5 to compare the extent of spatial autocorrelation in the residuals of the GMM-IV against the MGWIVR. Due to the exclusion of counties with <5,000 inhabitants in the contiguous United States, it proved not possible to compute a residual Moran's I statistic using contiguity-based measures. This problem was overcome by specifying a distance-based spatial weight matrix. Therefore, the residual Moran's I statistic is computed using a *spectral inverse distance* weighting where each element of the weight matrix contains the inverse of the distance between the centroid of counties *i* and *j*, divided by its largest characteristic root. Notice that the residual spatial autocorrelations are positive but extremely

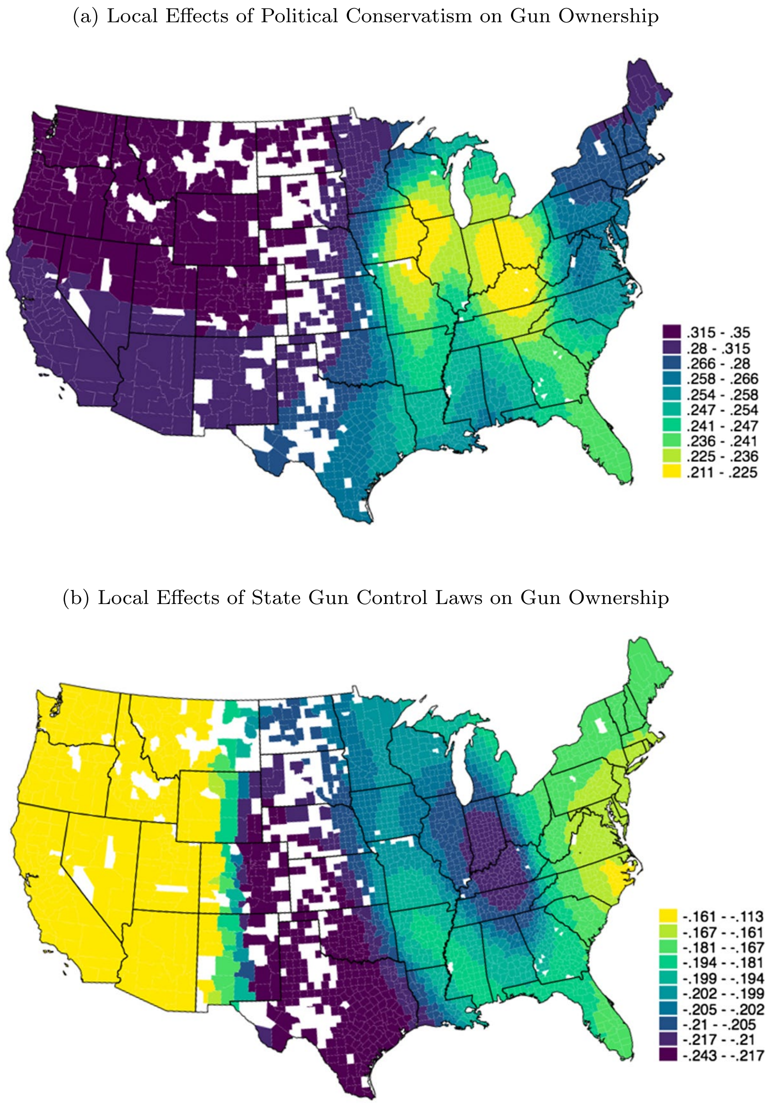


Figure 2. Spatial distribution of MGWIVR parameter estimates. Notes: The effective number of observations is 2,810. Counties with fewer than 5,000 inhabitants, shown by the blank polygons, are removed from the analysis (293 counties). All locally varying estimates are statistically significant at 5% level.

weak for both the first- and the second-stage GMM-IV residuals to begin with. Although the residual spatial autocorrelations of the first- and second-stage MGWIVR models turn out to be negative, they are even weaker (about −0.003) than their GMM-IV counterparts, suggesting that the residuals of the MGWIVR models exhibit virtually no global spatial autocorrelation.

Comparative results

The comparative summary results of the MGWIVR, the GMM-IV and the DAG of Section "Gun policy and causal reasoning" are given in Table 6. While the GMM-IV models for the total and gun homicide rates in columns (2) and (3) of Table 2 indicate negative and statistically

Table 3. Geographic Variability of Local Coefficient Estimates, MGWIVR (First Stage)

Panel A: Summary for local coefficients

	Outcome: Gun ownership level						
	Bandwidth	Adj. α (95%)	Mean	SD	Min	Median	Max
Intercept	87	0.001	21.536	14.83	−10.14	20.724	50.675
State Gun control law							
Strength index	1217	0.015	−0.19	0.028	−0.243	−0.199	−0.113
Socioeconomic status							
Income inequality	271	0.008	2.108	30.537	−76.041	15.7	31.832
Unemployment rate, %	657	0.016	0.212	0.21	−0.079	0.211	0.549
Poverty rate, %	1190	0.019	0.57	0.054	0.448	0.569	0.719
Bachelor or + degree , %	898	0.011	0.011	0.076	−0.17	0.012	0.141
Racial/ethnic composition							
African-American population, %	1522	0.028	0.086	0.05	−0.074	0.108	0.136
Hispanic pop., %	2394	0.027	−0.106	0.045	−0.196	−0.091	−0.053
Native-n population, %	2409	0.027	−0.21	0.05	−0.27	−0.222	−0.099
Demographics							
Population share (0–19 yrs), %	514	0.011	−0.051	0.137	−0.235	−0.098	0.26
Population share (20–34 yrs), %	538	0.010	0.148	0.138	−0.102	0.153	0.422
Population share (35–44 yrs), %	323	0.010	−0.106	0.438	−1.062	−0.111	0.787
Population share (45–64 yrs), %	223	0.005	0.757	0.261	0.176	0.84	1.151
Urbanization							
Population density	2333	0.020	−0.001	0.001	−0.001	−0.001	0
Small urban dummy [25K : 50K)	2809	0.037	−1.399	0.072	−1.476	−1.428	−1.227
Medium urban dummy [50K : 75K)	2809	0.033	−2.69	0.311	−3.552	−2.57	−2.3
Large urban dummy [75K : 100K)	2809	0.032	−5.141	0.1	−5.335	−5.16	−4.869
Very large urban dummy [100K+]	1798	0.014	−6.223	1.148	−8.588	−6.411	−3.788

(Continues)

Table 3. (Continued)

Panel A: Summary for local coefficients							
	Outcome: Gun ownership level						
	Bandwidth	Adj. $\boldsymbol{\alpha}$ (95%)	Mean	SD	Min	Median	Max
Excluded IVs							
Rep. pres. candidate vote share, 2008	485	0.010	0.26	0.031	0.211	0.254	0.35
State hunting license rate	1176	0.014	3.674	5.127	−4.761	3.418	15.543
Panel B: Model diagnostics							
	AICc	Adj. $\mathbf{R}^2$	Residual Moran's I (distance based)				
GMM-IV (first-stage)	21891.46	0.4041	0.009*** [13.057]				
MGWIVR (first-stage)	21769.25	0.4570	−0.003*** [−3.379]				

Note: * and *** denote statistical significance at 1% level. Z-scores in brackets.

Table 4. Geographic Variability of Local Coefficient Estimates, MGWIVR (Second Stage)

Panel A: Summary for local coefficients

	Outcome: Total homicide rate						
	Bandwidth	Adj. $\boldsymbol{\alpha}$ (95%)	Mean	SD	Min	Median	Max
Intercept	543	0.018	−0.11	0.756	−1.598	−0.005	1.21
Gun ownership							
Gun suicide ratio, % (fitted values)	255	0.004	−0.055	0.031	−0.117	−0.051	−0.004
State Gun control law							
Strength index	1290	0.018	−0.001	0.008	−0.024	−0.002	0.012
Socioeconomic status							
Income inequality	690	0.022	1.388	0.762	0.444	1.158	3.204
Unemployment rate, %	578	0.014	0.191	0.029	0.085	0.202	0.211
Poverty rate, %	566	0.012	−0.049	0.075	−0.232	−0.031	0.049
Bachelor or + degree , %	798	0.013	−0.134	0.039	−0.209	−0.136	−0.063
Racial/ethnic composition							
African-American population, %	46	0.000	0.193	0.303	−1.031	0.155	3.952
Hispanic pop., %	2732	0.042	−0.036	0.002	−0.038	−0.037	−0.031
Native-American population, %	153	0.002	0.02	0.703	−2.2	0.112	1.542
Demographics							
Population share (0–19 yrs), %	521	0.014	0.151	0.036	0.084	0.147	0.211
Population share (20–34 yrs), %	746	0.021	−0.107	0.028	−0.149	−0.108	−0.065
Population share (35–44 yrs), %	591	0.019	−0.14	0.053	−0.242	−0.137	−0.063
Population share (45–64 yrs), %	514	0.015	0.147	0.027	0.087	0.149	0.183
Urbanization							
Population density	182	0.002	0.002	0.002	−0.002	0.001	0.011
Small urban dummy [25K : 50K)	1217	0.010	0.345	0.321	−0.085	0.199	1.077
Medium urban dummy [50K : 75K)	2588	0.026	0.497	0.21	0.258	0.413	1.064

(Continues)

Table 4. (Continued)

Panel A: Summary for local coefficients							
	Outcome: Total homicide rate						
	Bandwidth	Adj. $\boldsymbol{\alpha}$ (95%)	Mean	SD	Min	Median	Max
Large urban dummy [75K : 100K)	2809	0.033	0.717	0.067	0.654	0.689	0.953
Very large urban dummy [100K+]	1934	0.017	0.881	0.187	0.513	0.865	1.325
Panel B: Model diagnostics							
	AICc	Adj. $\mathbf{R}^2$	Residual Moran's I (distance based)				
GMM-IV (second stage)	13502.87	0.5223	0.012*** [16.769]				
MGWIVR (second stage)	12686.88	0.6740	−0.003*** [−3.033]				

Notes: ***Statistical significance at 1% level. Z-scores in brackets.

Table 5. Geographic Variability of Local Coefficient Estimates, MGWIVR (Second Stage)

Panel A: Summary for local coefficients

	Outcome: Gun homicide rate						
	Bandwidth	Adj. α (95%)	Mean	SD	Min	Median	Max
Intercept	541	0.014	−2.718	0.71	−4.235	−2.639	−1.556
Gun ownership							
Gun suicide ratio, % (fitted values)	605	0.015	−0.056	0.016	−0.082	−0.057	−0.034
State Gun control law							
Strength index	1194	0.016	−0.01	0.007	−0.033	−0.008	−0.001
Socioeconomic status							
Income inequality	591	0.016	3.034	1.307	0.445	3.149	5.651
Unemployment rate, %	1300	0.032	0.116	0.008	0.101	0.117	0.136
Poverty rate, %	954	0.022	0.061	0.045	0.009	0.051	0.19
Bachelor or + degree , %	1005	0.016	−0.035	0.046	−0.091	-0.043	0.047
Racial/ethnic composition							
African-American population, %	46	0.000	0.234	0.263	−0.286	0.161	2.531
Hispanic pop., %	2649	0.040	−0.023	0.002	−0.027	−0.024	-0.016
Native-American population, %	173	0.002	−0.144	0.409	−1.727	−0.033	0.673
Demographics							
Population share (0–19 yrs), %	682	0.016	0.158	0.026	0.115	0.16	0.204
Population share (20–34 yrs), %	616	0.014	−0.069	0.027	−0.106	−0.077	−0.016
Population share (35–44 yrs), %	687	0.018	−0.151	0.034	−0.203	−0.158	−0.083
Population share (45–64 yrs), %	360	0.007	0.135	0.037	0.075	0.129	0.205
Urbanization							
Population density	182	0.002	0.001	0.002	−0.009	0	0.011
Small urban dummy [25K : 50K)	2429	0.025	0.003	0.064	−0.149	−0.005	0.169
Medium urban dummy [50K : 75K)	2809	0.034	−0.018	0.03	−0.073	−0.025	0.05

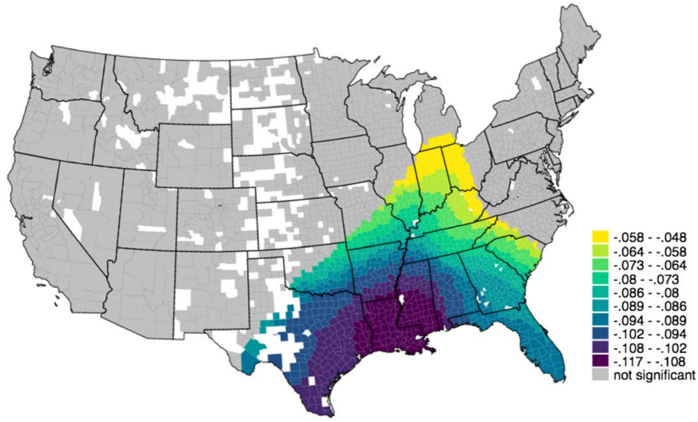
(Continues)

Table 5. (Continued)

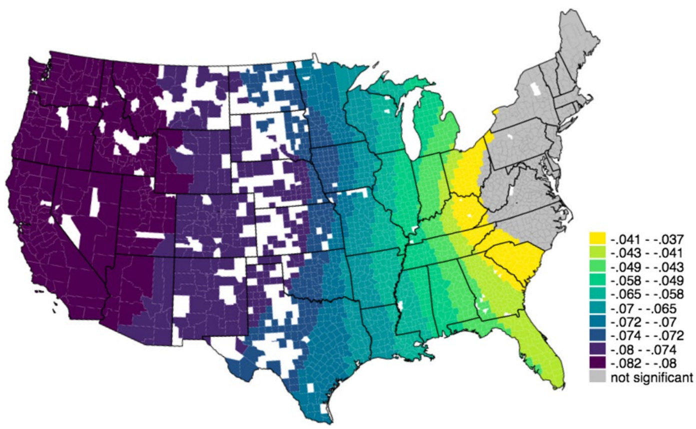
Panel A: Summary for local coefficients							
	Outcome: Gun homicide rate						
	Bandwidth	Adj. $\boldsymbol{\alpha}$ (95%)	Mean	SD	Min	Median	Max
Large urban dummy [75K : 100K)	2809	0.032	−0.032	0.05	−0.212	−0.01	0
Very large urban dummy [100K+]	2077	0.020	−0.133	0.24	−0.704	−0.089	0.167
Panel B: Model diagnostics							
	AICc	Adj. $\mathbf{R}^2$	Residual Moran's I (distance based)				
GMM-IV (second stage)	12617.66	0.4634	0.012*** [16.203]				
MGWIVR (second stage)	11583.63	0.6590	-0.003*** [3.797]				

Notes: ***Statistical significance at 1% level. Z-scores in brackets.

(a) Local Effects of Gun Ownership on Total Homicide Rates



(b) Local Effects of Gun Ownership on Gun Homicide Rates



(c) Local Effects of State Gun Control Laws on Gun Homicide Rates

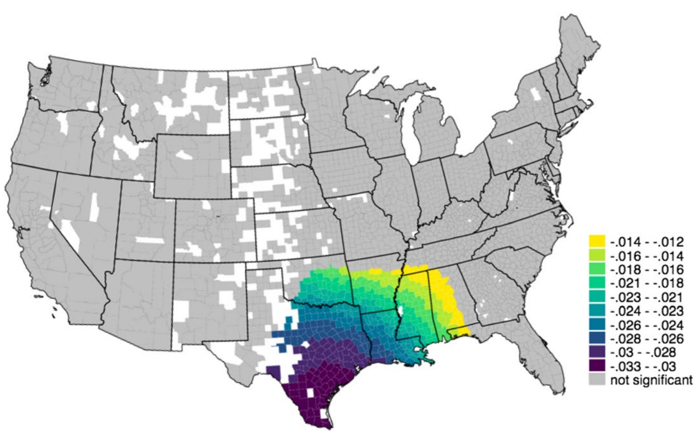


Figure 3. Spatial distribution of MGWIVR parameter estimates (continued). Notes: The effective number of observations is 2,810. Counties with fewer than 5,000 inhabitants, shown by the blank polygons, are removed from the analysis (293 counties). All locally varying estimates are statistically significant at 5% level.

Table 6. Comparative Summary Results

	More guns lead to…			Stricter laws lead to…		
	MGWIVR	GMM-IV	DAG	MGWIVR	GMM-IV	DAG
Total homicide	*less crime*	*less crime*	*more crime*	*no effect*	*less crime*	*less crime*
Gun homicide	*less crime*	*less crime*	*more crime*	*less crime*	*less crime*	*less crime*

significant average effects of gun ownership levels (−0.065 and −0.059) and hence "more guns, less crime," the MGWIVR models that are built upon locality and that allow one to assess spatial heterogeneity in the processes, also confirm the "more guns, less crime" hypothesis. The investigation of locality through the MGWIVR method further suggests that the magnitude of these effects differ across geography. However, these consistent empirical results are not in line with those of the conceptual framework developed in Section "Gun policy and causal reasoning" that suggests that more guns should lead to higher homicide rates. With respect to the impact of state gun control laws, both the GMM-IV and the MGWIVR results are in line with the implications of the DAG representation, suggesting that stricter gun control laws reduce gun homicide rates.

Concluding remarks

This study employed a MGWIVR approach to identify the locally varying and spatially monotonic effects of gun ownership levels on total and gun homicide rates using county-level cross-sectional data for the period 2009–2015. The endogeneity of gun ownership levels caused by reverse causation is explicitly accounted for by applying a 2SLS reasoning under the MGWR framework. The significant local variation in the effects of gun levels suggests that their effects on crime rates should not be reduced to the mere use of global regression models that provide a single estimate that represents the average effect of these relationships.

Gun control is a multifaceted, contentious, and politically charged issue. The MGWIVR framework has the strong potential to address a multitude of methodological challenges and knotty inferential questions of controversial aspects of gun policy. By allowing the effects of gun ownership level to vary over space, our results suggest that it affects homicide rates to varying degrees. The central takeaway from this study is that the deterrent effect of gun ownership dominates the facilitating effect in every county where this effect is deemed statistically distinguishable from zero at conventional test levels; hence "more guns, less crime". This is particularly observed in the counties of the South; a region known for its "culture of honor". Therefore, region-tailored policies should be implemented to combat homicides and should be reinforced by the passage of stricter but *risk-selective* gun control laws that are able to distinguish high-risk population from the rest in the acquisition, possession, and use of guns.

This study has a number of limitations. First, the application of manual 2SLS in a MGWR framework yields unbiased and consistent estimates but possibly incorrect standard errors. While this is the only and currently known strategy to deal with endogeneity in a MGWR framework, future statistical work shall be extended to models where some of the covariates are endogenous so as to make manual 2SLS reasoning obsolete.

Second, the lack of longitudinal sources of exogenous variation to instrument gun ownership level and the lack of valid longitudinal proxies prevented us from considering a panel framework that enables us to explore the intertemporal variation in homicide rates. While a county-level panel analysis would suffer from the inability to find valid excluded time-varying instruments for gun ownership, a state-level panel analysis would suffer from serious aggregation bias. A state-level analysis is likely to suffer from the consequences of the first problem simply because the existing state-level time-varying instruments for gun ownership have not been tested for their validity and relevance.

Third, our assessment of the locally varying effects of the law only accounted for the interstate variation in gun control laws. A complete assessment should account for the effect of local ordinances related to gun control to capture intercounty variation. For example, the strength index of gun control laws in the state of Maryland is about two to three times higher than the average strength index level for the entire United States. However, at the local level, Baltimore City, for example, enforces even stricter gun control laws than those of the state level. This study does not capture this type of local variation due to difficulties and impracticalities to collect local level, time-varying information on the strength of gun control laws for every county in the contiguous United States. Even local data had been gathered, additional identification problems and data unavailability would have likely posed threats to valid causal inference. Strikingly, the average gun homicide rate in Baltimore City is about 9.4 times higher than the average gun homicide rate in the state of Maryland in the 2009–2015 period. The implication of this striking local versus state-level gap in the strictness of gun control laws and gun violence is that local gun control laws may be driven by violence rates and are likely to be endogenous even though state gun control laws are unlikely to be affected by a state-wide increase in violence. This anecdote can hardly be considered a solitary case in the United States, which brings additional identification problems into any analysis that aims to evaluate the effects of local gun control laws on violence in general and homicides in particular. Hence, an exogenous source of variation should be found to isolate the causal effects of the law at the local level. Although a measure has been proposed, no such source of variation has been validated so far in the literature (Luca, Malhotra, and Poliquin 2016).

Fourth, the MGWIVR approach is computationally intensive but the benefits of identifying the locally varying effects far outweigh the costs in areas of public policy as controversial and as delicate as gun control. Our analysis focused on the effects of the strength index of gun control laws based on the fact that the intertemporal variation is extremely low. However, the advantages of the MGWIVR should be explored in other substantive areas of the law such as the RTC, PTP, or SYG where the covariate of interest is binary. The caveat is that a cross-sectional MGWIVR is unable to explore the variation in the timing of such laws and therefore a panel MGWIVR approach should be followed.

Notes

[1]All spatially nonmonotonic effects are locally varying by definition but not all locally varying effects are necessarily spatially nonmonotonic.

[2]I would like to thank an anonymous referee for pointing this out.

[3]An earlier version of this article considered violent (rape, murder, robbery, aggravated assault) and property crimes (burglary, larceny, motor vehicle theft, arson), obtained from the National Archive of Criminal Justice Data (NACJD) as additional crime variables. However, they have been dropped due to serious undercount problems, inconsistencies across the NACJD and the CDC data, and other measurement errors.

[4]For more information on the measurement of this index, visit http://www.statefirearmlaws.org and http://everytownresearch.org/gunlawnavigator.

[5]Of the 2810, there were 2 counties where no suicide had taken place in the 2009–2015 period. This leads to an undefined gun suicide ratio due to a division by zero. We retained these observations in the sample and simply coded the gun suicide ratio as zero.

[6]The population share above the age of 65 and the urban dummy variable for counties with an average population of <25,000 are left out to avoid singularity.

[7]All first stage–included instrumental variables (IVs) should appear among the second-stage explanatory variables (and vice versa) in a manual IV in order to avoid covariate ambivalence. If a subset of the second stage–included IVs is absent in the first stage, it is likely to be correlated with the first-stage residuals and this correlation spills over to all coefficients in the second stage.

[8]See page 56 of Fotheringham, Brunsdon, and Charlton (2003) for the choice of the spatial weighting function in a GWR.

[9]https://github.com/tonmcg/County_Level_Election_Results_12-16/find/master.

[10]A number of studies advocate the use of count data and models such as the Poisson or the negative binomial in the analysis of crime (Osgood 2000; Plassmann and Tideman 2001). This principle also applies under spatial nonstationarity through the implementation of geographically weighted Poisson regression (GWPR) (Nakaya et al. 2005). Using the GWPR in the current context is inappropriate because if gun ownership level is truly endogenous, a direct application of 2SLS reasoning to a nonlinear model leads to an inconsistent estimator.

[11]In contrast, a fixed bandwidth implies a greater likelihood that some local calibrations will be based on only a few data points. As a result, the distribution of local estimates will exhibit greater variation, hence larger standard errors (Fotheringham, Brunsdon, and Charlton 2003). In the extreme scenario, a singularity problem might even render it impossible to obtain some of the parameter estimates upon the use of a fixed kernel.

[12]This study uses MGWR 2.0 software developed by Z. Li, T. Oshan, S. Fotheringham, W. Kang, L. Wolf, H. Yu, and M. Sachdeva, available at Arizona State University, School of Geographical Sciences & Urban Planning, Spatial Analysis Research Center (SPARC): https://sgsup.asu.edu/sparc/multiscale-gwr. The open-source python package is available at https://github.com/pysal/mgwr and may be used in conjunction with FastGWR of Li et al. (2019) for speed and memory considerations, available at: https://github.com/Ziqi-Li/FastGWR. MGWR is a computationally intensive procedure. Depending on the model, a single model estimation takes about 1–6 h to complete on an Intel i7-4790S 3.20 Ghz 4-core CPU and 8 GB of RAM.

[13]We use `xtivreg2` (Schaffer 2010) and `weakiv` (Finlay, Magnusson, and Schaffer 2013) commands in Stata, available at: http://ideas.repec.org/c/boc/bocode/s456501.html (xtivreg2), http://ideas.repec.org/c/boc/bocode/s457684.html (weakiv).

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