The Short-Term and Localized Effect of Gun Shows:

Evidence from California and Texas

Mark Duggan

University of Maryland and NBER

Randi Hjalmarsson

Queen Mary, University of London

Brian A. Jacob

University of Michigan and NBER

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Abstract

We examine the effect of more than 3,400 gun shows using data from *Gun and Knife Show Calendar* and vital statistics data from California and Texas. Considering the one month following each show and a surrounding area ranging from 80 to 2,000 square miles, we find no evidence that gun shows increase either gun homicides or suicides. The similarity of our estimates for California and Texas suggest that the much tighter California gun show regulations do not substantially reduce the number of firearms-related deaths in that state. Using incident-level crime data for Houston, Texas, we also find no evidence of an effect on other crime categories.

JEL codes: H0, I1, K4

I. Introduction

Thousands of gun shows take place in the United States each year. Gun control advocates argue that the "gun show loophole" makes it easier for potential criminals to obtain a gun; the loophole basically allows unlicensed vendors at gun shows to sell firearms without conducting background checks on purchasers. In support of this claim, gun control advocates commonly cite selected extreme events, such as the April 20, 1999 Columbine High School shooting during which Eric Harris and Dylan Klebold shot 26 students, killing 13. Subsequent investigations by the Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF) revealed that some of the weapons used in the shooting were purchased for Harris and Klebold by a friend at a gun show (Brady Campaign, 2005). Though not an issue generally raised by gun control advocates, one might also be concerned that gun shows increase suicide rates by providing individuals considering suicide with a more lethal means of ending their lives.

On the other hand, proponents argue that gun shows are innocuous since potential criminals can acquire guns quite easily through other black market sales or theft. Gun lobbyists often cite a Bureau of Justice Statistics survey that finds that only 0.7% of state prison inmates who had ever owned a gun reported that they obtained it at a gun show (Harlow, 2001).

In response to the concerns about gun shows, 18 states have closed the "gun show loophole" by passing legislation that regulates the private transfer of firearms and 6 states have imposed additional regulations on gun shows.[1] Despite this legislative activity, there is little empirical evidence regarding the effect of gun shows and, to our knowledge, there are no studies of the effect of gun shows on gun suicides.[2] This stems in large part from the difficulty of obtaining detailed information on gun shows and outcomes such as crime or mortality. Moreover, because the timing and location of gun shows is clearly not random, it is difficult to infer the causal impact of gun shows by simply comparing geographic areas with frequent gun shows to those with fewer shows.[3]

This paper consists of two analyses, which study the impact of gun shows on both mortality and crime. Specifically, the first analysis examines the impact of gun shows on gun and non-gun suicides and homicides using a unique zip code by week level data set of all gun shows and deaths in Texas and California from 1994 to 2004. During this period, there were more than 2,200 gun shows in Texas and almost 1,200 gun shows in California.

Texas and California were chosen for this analysis for a number of reasons. As the nation's two most populous states, they comprise approximately 20% of the total U.S. population and accounted for 18% of total U.S. gun deaths in 2000 (Office of Statistics and Programming, CDC). In addition, these two states account for more than 13% of the 8.3 million background checks for firearms transfers conducted by the FBI and state agencies in 2005 (Bowling et. al, 2006). They also rank amongst the top five states in terms of the number of gun shows (U.S. Departments of Treasury and Justice, 1999). Finally, the states' gun show regulatory environments differ significantly: California is known for having the most aggressive gun show regulations while Texas has none.

To address the potentially endogenous timing and location of gun shows, we examine outcome trends *within* jurisdictions where gun shows occur, exploiting the high frequency variation in deaths that we observe in the vital statistics data. Our baseline empirical specification estimates the impact of a gun show on the number of deaths in a zip code in the week of a show and the three subsequent weeks relative to the four weeks preceding a show, controlling for zip code by year fixed effects as well as month fixed effects. Because zip codes are quite small and because the zip codes in which gun shows occur may be primarily commercial (e.g., a convention center) and attract many attendees from outside the immediate zip code, we also estimate specifications that utilize the number of gun shows that take place within various distances to the "home" zip code.

Overall, we find little evidence that gun shows have a significant effect on each of our four mortality measures: gun homicides, non-gun homicides, gun suicides, and non-gun suicides. This finding persists across a variety of specifications. First, we find the same thing when estimating our baseline specification separately for California and Texas zip codes, despite the differing regulatory environments in each state. Second, the results do not depend on the geographic area considered: 0, 5, 10, and 25 miles from the zip code in which the show occurs. Third, the findings are not sensitive to the estimation strategy used or alternative sets of fixed effects. Finally, we do not find evidence of heterogeneous effects over time or across zip codes with differing degrees of poverty, urbanicity, and gun ownership.

Using a similar empirical methodology, the second analysis considers the impact of gun shows on gun and non-gun violent crimes as well as property crimes using a census tract by week data set of all FBI part one crime incidents recorded by the Houston Police Department from 1994 to 2004. We identify the number of shows in each census tract and within various radii of each tract. As in our mortality analysis, we find no evidence of gun shows having a significant effect on crime.

There are two important limitations to our analysis. We only examine the geographic areas in and around where gun shows take place. To the extent that guns obtained at shows are transported elsewhere, we will not pick up these effects. In addition, our identification strategy relies on high frequency variation that, by definition, focuses on short-term effects. Specifically, most of our specifications look for spikes in various measures of mortality and crime in the four weeks immediately following a gun show. However, guns are durable and can be used many years in the future and thus our estimates will not capture these long-run effects. Despite these limitations, we believe that this analysis makes an important contribution to understanding the influence of gun shows, the regulation of which has arguably been the most active area of federal, state, and local firearms policy during the past decade.

The remainder of the paper proceeds as follows. Section II provides background information on gun ownership and the institutional and legal arrangements surrounding gun shows. Section III describes the data used in the analysis and Section IV outlines our empirical strategy. Section V presents our main results and Section VI concludes.

II. Background

Gun Ownership in the U.S.

Firearms manufacturing and ownership in the United States are substantial. Approximately five million new firearms were for sale in the U.S. in 2006, including net imports.[4] Using the 1994 National Survey of Private Ownership of Firearms (NSPOF), Cook and Ludwig (1996) estimate that approximately 192 million privately-owned firearms, including 65 million handguns, exist in the United States and that about 35% of households own a gun. In contrast, the Bureau of Alcohol, Tobacco and Firearms (U.S. Department of the Treasury, 2000) estimated that approximately 242 million firearms were available for sale or owned by civilians in the U.S. at the end of 1996, including roughly 72 million handguns, 76 million rifles, and 64 million shotguns.

Previous research generally suggests that there is a positive relationship between gun ownership rates and both gun suicide and homicide rates. That is, increases in gun ownership rates are associated with increases in gun homicide (Duggan, 2001; Cook and Ludwig, 2004) and gun suicide (Kellermann et al., 1992; Sloan et al., 1990; Azrael, Hemenway, and Miller, 2002).[5]

Institutional Background on Gun Shows

Thousands of gun shows are held in the U.S. each year.[6] Shows are generally open to the public, though attendees often pay a modest admission fee to attend. Most shows are held over the weekend and last for two days, drawing an average of 2,500 to 5,000 people per show. To rent a table from a promoter, vendors pay fees typically ranging from $5 to $50. The number of tables at gun shows ranges widely, from as few as 50 to as many as 2,000 (U.S. Departments of Justice and Treasury, 1999).[7]

The share of guns acquired by private citizens through gun shows appears relatively small compared to other channels. The NSPOF estimated that approximately 239,000 firearms per year were bought at U.S. gun shows and flea markets in 1993 and 1994. This represents just 4% of both long guns and handguns acquired by private individuals from all sources in those years (Cook & Ludwig, 1996). Similarly, a 1997 survey of 18,000 state prison inmates by the Bureau of Justice Statistics found that only 0.7% and 1.7% of inmates who had ever owned a gun said they had obtained it at a gun show or flea market, respectively (Harlow, 2001). Yet, an ATF study (2000) found that 14% of their criminal trafficking investigations between 1996 and 1998 involved guns purchased from gun shows; about 46% involved straw purchases (i.e. when an individual purchases a gun for someone else) and 20% involved unlicensed sellers.[8]

The Gun Show Loophole

Certain individuals – primarily felons and those convicted of domestic abuse – are prohibited from purchasing or possessing a firearm under federal law.[9] The "gun show loophole" refers to the fact that federal law requires "federal firearms licensees" (FFLs) (i.e., those licensed by the government to manufacture, import, or deal in firearms) to conduct background checks on non-licensed persons seeking to obtain firearms, but does not require such checks by those who transfer firearms and do not meet the statutory test of being “engaged in the business” to do so (Krouse, 2005). Therefore, while a gun dealer operating a gun shop is obliged to conduct background checks on potential buyers, private sellers at gun shows who “transfer” firearms do not have to do so. FFLs comprise 50% to 75% of the vendors at most gun shows, so some private venders could use this “loophole” to entice potential customers to their tables (U.S. Departments of Justice and Treasury, 1999).

A number of states, however, have passed legislation regulating at least some of these sales. For example, California, Rhode Island, and the District of Columbia require background checks on all gun purchases, including those at gun shows. Colorado, Connecticut, Illinois, New York, and Oregon have less comprehensive regulations but also require background checks for firearms purchased at gun shows (LCAV, 2008). The vast majority of states, however, do not require a background check for transactions occurring at gun shows.

One recent study (Wintemute, 2007) compared gun shows in California, which is considered to have “a uniquely restrictive regulatory environment for gun shows”, with shows in Arizona, Nevada, Texas, and Florida. These four states do not regulate any private party firearms sales, including those at gun shows. In addition to having background checks, California requires that any individual who purchases a gun, whether at a gun show or anywhere else, wait ten days before receiving the gun.[10] The results from this study suggest that there were fewer illegal “straw purchases” and undocumented gun sales at California’s shows. Thus, one might expect to find a very different effect of gun shows in an aggressively regulated state (California) than in one with no regulations (Texas). We investigate this issue in the sections that follow.

III. Data Description

We create two data sets to investigate whether the number of deaths and crimes change in the weeks leading up to or following a gun show. The first data set is a week by zip code panel of gun show and mortality information for Texas and California for all weeks from 1994 to 2004. The second data set, spanning the same years, is a week by census tract panel of gun shows and seven categories of Part I crime incidents for Houston, Texas. Both data sets are aggregated to the week level (rather than the date) to (i) increase statistical precision and (ii) account for the fact that gun shows typically occur on weekends, when mortality and crime rates are likely to differ from other days for reasons unrelated to gun shows. Using a symmetric time period as our unit of observation also reduces the possibility that pre-post comparisons will be driven by factors other than the existence of a gun show. Because approximately 99 percent of Texas and California gun shows begin on either Friday or Saturday, we begin each week on a Friday and end it on a Thursday. This results in 573 weeks of data (January 7, 1994 to December 30, 2004) for 1,861 zip codes in Texas, 1,664 zip codes in California, and 446 Houston census tracts.[11]

Information on gun shows was obtained from *Gun and Knife Show Calendar*, a national magazine that lists the dates and locations of gun shows throughout the country.[12] For each Texas and California gun show from 1994 to 2004, we noted the zip code and date(s) of the show. For those in Harris County, Texas (which contains Houston), we also noted the 1990 census tract.[13] For each zip code in California and Texas as well as each Houston census tract, we then determined the number of gun shows in each week of our sample. Finally, since gun show attendees may not live in the show zip code or tract, we calculated the number of gun shows each week within various distances of each zip code or tract using the latitude and longitude of the centroid of each location.[14]

There were 2,187 gun shows in Texas and 1,179 in California from 1994 to 2004. Figure 1 presents the combined annual number of shows in Texas and California. There is a decrease from a high of 394 shows in 1995 to a low of 232 shows in 2001. This decline is seen in both California and Texas. It is also important to note that gun shows are not geographically evenly distributed. Only 120 Texas zip codes and 98 California zip codes have at least one gun show over the sample period. Likewise, the 338 gun shows in Harris County occur in just eight census tracts, five of which are in Houston.

To examine the impact of gun shows on mortality, we utilize individual-level vital statistics data for the deaths of all residents of Texas and California.[15] For each death, we identify the date and zip code of residence.[16] Consistent with previous research, we use the International Classification of Disease cause-of-death codes to focus on the number of gun and non-gun homicides and suicides per week in each zip code.[17] Figure 1 also plots the combined number of annual gun homicides and suicides in Texas and California. The number of gun homicides and suicides declines from 8,034 in 1994 to 4,845 in 2000, and then starts to slowly trend up. This pattern is not being driven by any one state or category of death. In a typical year, approximately 62% and 64% of Texas suicides and homicides, respectively, are committed with a gun compared to 48% and 72% in California.

The second data set is created by merging the Harris County gun show data with data on the number of Part I crime incidents in each Houston census tract in each week from 1994 to 2004.[18] We group the seven Part I crime categories (homicide, rape, robbery, assault, burglary, motor vehicle theft, and other thefts) into property crimes (burglary, motor vehicle theft, and other theft) and violent crimes (homicide, rape, assault, and robbery) committed with and without a gun.[19] Violent crimes committed with a gun decreased by almost 28 percent from 1994 to 1998; by 2002, however, they had increased back to the 1994 level. In contrast, a general upwards trend in both violent non-gun crimes and property crimes was seen over the same period.

Table 1 provides summary statistics for the Texas and California mortality data and demographic data from the 2000 Census. In on-line appendix Table A1, we present comparable statistics for Texas and California separately. We present the average weekly number of gun-related deaths per 100,000 residents and also list the corresponding averages for gun homicides and suicides for the 3,525 zip codes in our analysis sample. As the first column of the table shows, the average weekly number of gun-related deaths in a zip code is 0.309; almost two-thirds is accounted for by gun suicides and most of the rest accounted for by gun homicides.[20]

In the next column, we present analogous information for the 218 zip codes that have one or more gun shows during our sample period. Interestingly, the average weekly number of gun-related deaths per 100,000 residents of 0.217 is almost 30 percent lower than the corresponding average for all zip codes; this is primarily driven by a lower number of gun suicides per capita. Column (3) provides similar information for the 1,596 zip codes with one or more shows within 10 miles during the eleven-year period; though gun deaths per capita are also much lower in this set of zip codes, gun homicides per capita are almost 15% higher.

Table 1 also indicates that zip codes with at least one show within 10 miles during our sample period are significantly different than other zip codes in terms of demographic characteristics. These zip codes have an average population of 25,730, which is 74 percent greater than the corresponding average of 14,751 for all other zip codes. Additionally, the average fraction of the population that is in a rural area is substantially lower in zip codes close to a gun show than in zip codes with no gun shows (14.5 versus 48.5 percent). The fraction of the population that is poor is similar between the two groups of zip codes, while the fraction that is black or of Hispanic origin is substantially greater among those zip codes that are close to a gun show (37.0 versus 30.5 percent). These differences and the others summarized in the table indicate that gun shows tend to occur in more urban areas with lower rates of gun ownership and with higher population densities.

The second panel of Table 1 provides analogous information for the 446 census tracts in the city of Houston, Texas during the 1994 to 2004 period. We see that the 221 census tracts within 5 miles[21] of one or more shows had higher crime rates (non-gun violent, gun violent, and property) than the rest of the city. This contrasts with the previous comparison in the first panel of Table 1, though it is important to note that there are only 8 tracts in all of Harris County (of which 5 are in Houston) with one or more shows during the period of interest.

IV. Empirical Strategy

We are interested in examining the impact of gun shows on mortality and crime. The primary challenge stems from the fact that gun shows may occur in places, or at times, that have more deaths (or greater crime) for other, unobserved reasons. For example, as we saw in Table 1, there is some evidence that gun shows occur in places where relatively fewer people own guns. Failing to account for this could lead to spurious estimates of the impact of gun shows on mortality. Similarly, the number of gun shows occurring in Texas and California during weeks in the second quarter of the year is significantly less than the number of gun shows in other quarters; once again, this could yield spurious results given the seasonal nature of homicide and suicide. To address this potential endogeneity, we examine outcome trends *within* jurisdictions where gun shows occur, exploiting the high frequency variation in deaths that we observe in the vital statistics data. The key identifying assumption of this model and subsequent ones is that the *timing* of gun shows is not correlated with other factors that might directly influence the homicide or suicide rate. The discussion below focuses on our analysis of mortality in Texas and California, but we follow an identical approach when examining crime in Houston.

We begin by estimating models that take the following form:

$$(1) \qquad y_{zt} = \sum_{k=-K}^{K} \beta_k nshows_{z,t+k} + \lambda_t + \gamma_{zt} + \varepsilon_{zt}$$

where y_{zt} is the number of deaths in zip code z in week t, and the *nshows* variables indicate the number of shows that occurred in zip code z in the week $t + k$. That is, the *nshows* variables are leading/lagging indicators. Specifically, the coefficient β_k measures the impact on mortality in the given week of having a gun show k weeks ago. Since 99% of gun shows take place over the weekend and weeks are defined to run from Friday to Thursday, the coefficient β_0 captures the effect of a gun show on gun deaths *during* the show and in the four or five days immediately following the show. Analogously, β_{-1} measures the effect of a gun show that took place one week ago while β_1 measures the "effect" of a gun show that will take place one week in the future.

This event history approach allows one to trace out the mortality in the weeks leading up to and following a gun show. The leading indicators serve two purposes. First, they serve as a test for the presence of unobserved factors that occurred close to the time of a gun show and that may give rise to spurious correlations between gun shows and mortality. Second, they allow us to explore temporal substitution in the number of deaths that might be related to the presence of a show. For example, if potential criminals "wait" to commit their crimes until a gun show provides the chance to purchase a firearm, then one might see a decline in deaths leading up to a gun show followed by a spike in deaths immediately after. In practice, the inclusion of leading indicators does not change our results and does not yield any indication of temporal substitution.[22]

To account for unobservable location and period specific factors that might be correlated with the occurrence of gun shows as well as the number of gun-related deaths, we include a set of location*time period fixed effects, denoted above by γ_{zt}. In our baseline model, γ_{zt} represents zip code*year fixed effects and captures location-specific factors that are either time-invariant or change slowly over time (e.g., demographic shifts, changes in police practice, etc.). Our baseline model also includes month fixed effects (i.e., separate indicators for January, February, etc.) to capture common seasonality-related trends across zip codes; this is denoted above by λ_t. We later show that our results are robust to a variety of alternative controls for unobserved location and/or time effects.

In Figures 2 and 3, we present event history figures that graph the coefficients on 10 leading and 10 lagging gun show variables. These provide a full and transparent picture of the potential impacts. For the sake of parsimony, we then estimate models that focus exclusively on mortality in the 4 weeks following a gun show. We compare the post-month mortality rate to the mortality rate in the 4 weeks leading up to a gun show in order to "difference out" any unobserved zip code*time specific factors that might be correlated with gun show timing and mortality. Specifically, we estimate the following models:

(2) $$y_{zt} = \beta_1 nsh8wk_{zt} + \beta_2 nsh4wk_{zt} + \lambda_t + \gamma_{zt} + \varepsilon_{zt}$$

where the first term measures the number of gun shows in zip code z in the 8-week window around week t (i.e., $nsh8wk_{zt} = \sum_{k=-3}^{4} nshows_{z,t+k}$) and the second term measures the number of gun shows in the four weeks prior to week t, (i.e., $nsh4wk_{zt} = \sum_{k=-3}^{0} nshows_{z,t+k}$). We present the estimate of β_2 , which reflects the difference in mortality in the four weeks following a gun show relative to the 4 weeks preceding a gun show. As in equation (1), λ_t and γ_{zt} represent month and zip code*year fixed effects, respectively.

In the discussion above, we have focused on the relationship between gun shows and gun-related deaths in a particular zip code. However, zip codes are quite small. The median zip code in California (Texas) is only 17 (52) square miles, and the urban zip codes in which many gun shows occur are considerably smaller in terms of land area. Indeed, in some cases, the zip code in which a gun show occurs is primarily a commercial area with a negligible residential population. While there is no data on the residential location of gun show patrons, it seems likely that gun shows attract many people outside the immediate zip code.[23] Thus, one might expect the presence of a gun show in a particular zip code to influence the number of gun-related deaths in neighboring zip codes.

If one had a strong reason to believe, ex ante, that gun shows attracted patrons within a certain geographic area, then one would want to use this information in determining the proper specification. In the absence of any compelling evidence on this matter, we experiment with specifications that allow gun shows to influence mortality in zip codes located within various distances of the show itself. In our baseline specification, we allow gun shows to influence mortality within a 10-mile radius of the zip code in which the show took place, which includes more than 300 square miles. In these specifications, the *nshows* variables reflects the number of gun shows that took place in zip codes located within 10-miles of the zip code whose mortality we measure. Note that the unit of observation for these regressions is still the zip code*week, and the outcome still measures the number of deaths in zip code *z* in week *t*.

We then present results for 5-mile and 25-mile radii (which includes approximately 80 and 2,000 square miles, respectively) along with results limiting the impact of gun shows to the zip code of the show itself. It is worth noting that, unlike the other sensitivity analyses we present, the results from these alternative specifications of distance to show will not provide a "falsification test" for our baseline model. While we have some intuition that a gun show in one zip code will likely influence mortality in neighboring zip codes, we have no reason to believe that effects we find within, say, a 25-mile radius are "better" than the effects within a 10-mile radius. Instead, one should view this exercise as identifying where potential effects may exist.

<u>Estimation</u>

Throughout the analysis, our outcome will be some measure of the number of gun-related deaths in a particular location at a particular time. However, the choice of the correct specification depends in large part on the way in which one believes that gun shows influence gun-related deaths. If one believes that a gun show will reduce the "shadow price" of purchasing a gun by the same amount for all individuals in each location, then (all else equal) one might expect the impact of the gun show to be proportional to the population in the relevant jurisdiction. For example, a gun show that takes place in a town of 10,000 people might allow the one person who is contemplating suicide sufficiently easy access to a firearm to induce her to kill herself, resulting in one additional gun death. In an otherwise comparable town of 100,000 people, one would expect there to be 10 such individuals who might be induced to commit suicide by the "gun show-induced" availability of a firearm. This type of proportional effect suggests a specification in which the outcome variable is measured per capita (e.g, deaths per 100,000 residents), or is measured relative to the average number of deaths in the location.[24]

On the other hand, if one believes that gun shows will have a similar impact on the number of deaths across locations regardless of population, one could estimate an OLS model using the number of gun deaths in a particular location*week as the outcome. This specification would be reasonable if one believed that gun shows induce a smaller change in gun availability (i.e., a smaller price reduction) for the average person in larger geographic areas because transportation costs limit the access to gun shows in large areas, or perhaps because there are already many alternative ways to obtain a firearm without going through standard background checks in larger areas. If the supply of guns available at gun shows is limited relative to the demand, this might also be a reason that the effect is not proportional to the population.

Because we believe that the effects of gun shows are likely to be only partially proportional to the population size, we estimate several different specifications. To begin, we estimate an OLS model in which the outcome is the number of deaths.[25] In addition, we estimate negative binomial and Poisson regression models. In these models, the mean number of deaths (μ) is modeled as an exponential function of the predictors (i.e., $\mu = e^{x\beta}$), so that the resulting estimates reflect the proportional effect of gun shows. Specifically, the exponentiated coefficients from these models can be interpreted as incidence rate ratios, which reflect the percent effect of gun shows on the number of deaths in a zip code.[26] Both the negative binomial and Poisson regressions are consistent under our identifying assumptions. The negative binomial is a generalization of the Poisson regression model that allows for the variance of the outcome measure to differ from the mean. This technique is ideal for dealing with count data with over-dispersion since it provides more efficient estimates than Poisson regression. In order to accommodate the zip code*year fixed effects in our model, we use the fixed effects negative binomial model developed by Hausman, Hall, and Griliches (1984). However, Allison and Waterman (2002) have shown that this model is not a true fixed effects estimator in the sense that it does not necessarily control for all stable unit-specific covariates as does the standard linear fixed effects model. Thus, we present estimates from a fixed effects Poisson regression, which does provide consistent estimates in the presence of time-invariant unit-specific confounding factors.

In all models, we will account for possible serial correlation within jurisdictions and other forms of heteroskedasticity. In the OLS models, we estimated Eiker-White standard errors that are clustered by zip code. In the negative binomial and Poisson models, we use a block bootstrap where the blocking variable is the zip code.

V. Results

In this section, we present our findings. The first subsection describes our main results on how gun shows influence mortality rates in California and Texas. The next subsection presents results that speak to the sensitivity and heterogeneity of these estimates. The final subsection explores the effect of gun shows on gun and non-gun violent crime and property crime using data from Houston.

The Effect of Gun Shows on Mortality in California and Texas

Following equation (1), Figures II (a) through (d) present coefficients reflecting the effect of gun shows in the 10 weeks prior to and 10 weeks following the occurrence of the show. These coefficients come from our baseline model that combines California and Texas, and focuses on shows that occur within a 10-mile radius of the zip code. The dependent variable is the number of gun homicides, non-gun homicides, gun suicides, and non-gun suicides, respectively, in the zip code*week. All models are estimated via OLS.

The results shown in Figure II suggest that gun shows do not have a significant effect on any of our four mortality measures. Moreover, the confidence intervals indicate relatively precise estimates. For example, the upper bound of the confidence interval for gun homicides rarely goes above .002 deaths. Given a mean of roughly .022, this suggests we can rule out effects larger than a 9 percent increase.

Interestingly, for some outcomes like gun homicides and non-gun suicides, it appears that mortality rates in a zip code during the 20-week window surrounding a show are somewhat lower than in other weeks during the year in the same zip code. While this may be a sampling variability, it is possible that this pattern reflects the presence of some unobserved factor that is correlated both with the occurrence of gun shows and a reduction in mortality. For this reason, our more parsimonious specifications measure the effect of a gun show as the difference between mortality rates in the 4-week period following a gun show relative to the 4-week period preceding a gun show (i.e., the specification detailed in equation (2)).

Table 2 presents the results from this specification. In addition to showing results for our baseline 10-mile radius, we report results from specifications in which we allow gun shows to influence mortality in locations at various distances from the show. In panel A, for example, we restrict gun shows to influence mortality only within the actual zip code in which the show occurred. In panels B through D, gun shows are allowed to influence mortality in zip codes within respectively a 5-mile, 10-mile and 25-mile radius of the zip code in which the show occurred. Regardless of the geographic proximity to the show (0, 5, 10 and 25 miles) or the outcome (gun homicide, non-gun homicide, gun suicide or non-gun suicide), it appears that gun shows have no substantively important nor statistically significant impacts on mortality.

Table 2 reports the average effect of gun shows in California and Texas. However, as noted earlier, California has much stricter regulations on gun shows than Texas, so one might expect the effect of shows to be larger in Texas. To explore this potential heterogeneity, Table 3 reports results separately by state for our baseline specification that measures the influence of a show within a 10-mile radius of where it occurred. We find no significant effects of gun shows on any of the four outcomes in either state. In an on-line appendix (Table A2), we present state-specific estimates for our alternative geographic catchment areas (0-miles, 5-miles and 25-miles). These results tell a similar story as described above.

Sensitivity of Mortality Results

Table 4 explores whether the results above are sensitive to the model specification. Column 1 reproduces the baseline results for gun homicides (i.e., Table 2, panel C, column 1). Column 2 shows estimates that are weighted by the total population in the zip code (from the 2000 census). The results are extremely similar to the unweighted estimates. Columns 3 and 4 show the exponentiated coefficients (and p-values in parentheses) from negative binomial and Poisson regressions where the outcome is the number of deaths. The sample sizes for these regressions are smaller than the baseline because these models are estimated off of the set of observations for which there is at least one gun death in a given zip code*year. Column 5 shows the OLS estimates for this same sample to allow one to distinguish between differences due to sample size and those due to model specification. Columns 6 through 10 present parallel specifications for gun suicides. The basic pattern of results – both magnitude and significance –

is comparable across specifications for homicides and suicides. (Table A3 in the on-line appendix shows these results separately by state).[27]

In our baseline specification, we include zip code*year and month fixed effects. In the on-line appendix (Table A4), we present results from three alternative sets of fixed effects: separate main effects for month, year and zip code; separate main effects for week defined over the entire sample period (i.e., 1-573) and zip code; and week-of-year (i.e., 1-52) and zip code*year. The results do not change appreciably across any of these alternative specifications.

Heterogeneous Effects

Table 5 reports estimates of our baseline specification by zip code characteristics. To explore whether the effect of gun shows differs by poverty and urbanicity, we present estimates for three mutually exclusive zip code categories: (1) zips in which at least 30 percent of the population is living in a rural area (as defined by the census); (2) zips in which less than 30 percent are living in a rural area and fewer than 10 percent are below the poverty line; and (3) zips where fewer than 30 percent are living in a rural area but at least 10 percent are in poverty. These groups are meant to roughly capture rural, urban non-poor and urban poor areas. We do not see any noticeable patterns across groups (and none of the individual estimates are significantly different than zero). These basic results are not sensitive to moderate changes in the definition of these groups and are similar if one looks within Texas and California separately (Table A5 in the on-line appendix).

To the extent that gun shows influence homicide and suicide rates by increasing the availability of guns, one might think that the effects would be smaller in locations where guns are more readily available. While we do not have any direct measures of gun ownership, following Azrael, Cook, and Miller (2004), we use the fraction of suicides committed with a gun as a proxy for gun ownership. Specifically, Table 6 shows the results of splitting the sample into three groups: low (bottom 25% of zip codes), moderate (middle 50% of zip codes), and high (top 25% of zip codes) gun ownership. Once again, none of the individual estimates are significantly different than zero. (On-line appendix Table A6 shows comparable results separately by state.)

Our sample spans 11 years, during which there were substantial economic and political changes along with corresponding changes in national crime statistics. For example, from 1994 to 1999, the number of homicides in the U.S. decreased by more than 33%; from 1999 to 2004, the annual number of homicides began to slowly increase. As mentioned previously and shown in Figure I, similar patterns are seen in the annual number of gun deaths in Texas and California. For this reason, we estimate our models separately for 1994-1999 and 2000-2004. The results, shown in on-line appendix Table A7, indicate no notable difference across time periods in either state (all effects are statistically indistinguishable from zero, and relatively small in magnitude).

<u>The Effect of Gun Shows on Violent and Property Crime in Houston</u>

The results presented above suggest that gun shows in California and Texas do not have a significant short-run effect on local homicide or suicide rates. However, it is possible that the occurrence of a gun show might influence the prevalence of other types of crime such as assaults or robberies. To examine this issue, we estimate models similar to those discussed above on tract-level crime data for Houston from 1994 to 2004. In our preferred specification discussed below, we focus on effects within a 5-mile radius of the gun show, and discuss results from OLS regressions where the outcome is the simple number of crimes in a tract-week.

Figure III presents coefficients reflecting the effect of gun shows in the 10 weeks prior to and 10 weeks following the occurrence of the show, which are based on estimates from equation (1). The solid line reflects the point estimates and the dashed line reflects the corresponding

95% confidence band. Looking across the figures for gun violent crimes (panel a), non-gun violent crimes (panel b), and property crimes (panel c), we see no evidence that gun shows influence the prevalence of any type of crime. With regard to gun violent crimes, the point estimates bounce around zero, never exceeding 0.02, and the 95% confidence interval for most weeks is between -0.02 and 0.02. Given that the number of gun violent crimes in the average tract-week observation is 0.386, this suggests we can rule out effects larger than 6 percent. For non-gun violent and property crimes, our estimates rule out effects larger than 2 and 1 percent, respectively.

To more precisely and parsimoniously summarize these results, Table 7 reports estimates of crime effects from equation (2), which describes the impact of a gun show on crime in the 4-week period following the show relative to the 4-week period preceding the show. None of the estimates are statistically different than zero, and all are relatively precise. For example, given that the average number of gun violent crimes within a tract-week observation in Houston during this period is 0.386, our estimates imply that we can rule out effects larger than 1.5 percent of the mean. The estimates for non-gun violent and property crimes are even smaller in relative magnitude and equally insignificant.

In results available in the on-line appendix, we show that the general conclusion of no effect is robust to negative binomial, Poisson and population-weighted OLS estimates (Table A9), to specifications that include alternative controls for time and location (Table A10), and to alternate choices of geographic proximity to a gun show (Table A11). Finally, in results not reported but available upon request, we demonstrate that gun shows have no effect on more fine-grained crime categories including robberies, burglaries, motor vehicle thefts, assaults, rapes, etc.

In summary, it appears that the occurrence of a gun show does not significantly affect the short-run crime rates in localized areas.

VI. Conclusion

Thousands of gun shows take place in the U.S. every year. Gun control advocates argue that the "gun show loophole" that exists in many states makes it easier for potential criminals to obtain a gun. Gun shows may also affect suicide rates by increasing the ease with which individuals who are contemplating suicide can obtain a more lethal device. On the other hand, opponents of gun show regulations argue that gun shows are innocuous because potential criminals and other individuals can acquire guns easily through other channels.

In this paper, we have investigated the effect of gun shows using eleven years of data on the date and location of every gun show in the states of California and Texas, the nation's two most populous states. To study the effect on mortality (homicide and suicide, in particular), we have combined this with information on the date, location, and cause of every death occurring in these same two states during our eleven-year study period. To study the effect of gun shows on violent and property crime, we combine the gun show data with crime data provided by the Houston Police Department from 1994 to 2004.

Using both event study techniques and specifications that estimate the difference in mortality in the four weeks following a gun show relative to the four weeks preceding a show, we find no evidence that gun shows have an effect on any of our outcome measures: gun and non-gun homicides, gun and non-gun suicides, gun and non-gun violent crimes, and property crime. In addition, the mortality results are the same in both Texas and California, despite the fact that California arguably has the strictest gun show regulations while Texas' regulations are amongst the least stringent. Thus, our results suggest that gun shows do not increase the number of homicides or suicides and that the absence of gun show regulations does not increase the number of gun-related deaths as proponents of these regulations suggest.

There are, however, two important caveats to our analyses. First, we are considering only the effect in the geographic area immediately surrounding gun shows. To the extent that firearms purchased at gun shows are transported more than 25 miles away from the show, our identification strategy will not capture this effect. Additionally, we consider the effect only in the four weeks immediately following a gun show. However, guns are durable, and thus to the extent that effects occur much later, our analysis will not capture this.

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[1] States became particularly attentive to the gun show loophole after the 1999 Columbine incident and again after the 2007 Virginia Tech massacre. Even though the weapons used in the Virginia Tech shooting were purchased at federally licensed stores and not gun shows, the Virginia Tech review panel put together a report that recommended requiring background checks for firearms sales at gun shows. Governor Kaine made it a priority to enact such a law in 2008, but it was defeated in the Virginia Senate.

[2] Lott (2003) examines violent crime rates before and after the introduction of state laws to require background checks for private transfers of handguns. Comparing nine states that closed this loophole by 1994 to 33 states that never implemented such laws, he finds no evidence that these gun show laws reduced violent crime and, in fact, he finds that such laws are positively associated with murder, robbery, and auto theft.

[3] Lott (2003) finds that laws requiring background checks at gun shows as well as laws banning assault weapons and/or imposing waiting periods are negatively associated with the prevalence of gun shows in a state. He also finds that western and rural states tend to have the greatest number of gun shows per capita and that states with higher gun ownership rates have significantly more shows.

[4] Specifically, in 2006, approximately 1.4 million handguns and 2.2 million shotguns and rifles were manufactured in the U.S., with just 0.3 million of these exported (U.S. Department of the Treasury, 2008). According to Census Bureau statistics published by *Shooting Industry* magazine an additional 1.1 million handguns and 0.7 million rifles and shotguns were imported

into the U.S. (See http://www.shootingindustry.com/Pages/SpecRep6.html#importhand accessed on August 29, 2008).

[5] In contrast, Lott and Mustard (1997) find that crime declined in those states that passed concealed weapons laws, suggesting that gun ownership reduces crime through deterrence. Though Moody (2001) supports these results, Ayres and Donohue (1999) and Black and Nagin (1998) found that the results were not robust to a variety of assumptions and modeling choices.

[6] Lott (2003) states that roughly 1,900 gun shows were held in the U.S. in 1991 and that this number increased to a high of 2,907 in 1996, but then declined to roughly 2,400 in 2001. Using the same data source (a periodical titled *Gun Show Calendar*), the U.S. Departments of Justice and Treasury (1999) came up with a much higher figure for the overall number of gun shows in 1998 – 4,442 shows compared with the 2,600 reported by Lott (2003).

[7] Various types of firearms are sold at gun shows. These include new and used handguns, shotguns, rifles, semi-automatic assault weapons, and curio or relic firearms (e.g., firearms of historical interest) (U.S. Department of Justice, 2007). In addition, gun show vendors also usually sell ammunition, gun literature, and gun accessories. Gun shows often include knife vendors and sellers of air guns. For the most part, gun shows offer firearms for both those seeking to purchase handguns, as well as the sportsman and hunter. (U.S. Departments of Justice and Treasury, 1999).

[8] See http://www.atf.treas.gov/pub/fire-explo_pub/pdf/followingthegun_internet.pdf.

[9] There are eight categories under the Brady Act that render individuals ineligible to purchase or possess firearms. These include: felony convictions, misdemeanor convictions, fugitive status, an adjudication of mental illness, issuance of a restraining order against the individual, people convicted of drug-related offenses, underage status, or alien status. Many state laws contain

these same prohibitions. In addition, some state laws also prohibit people convicted of alcohol offenses and juvenile offenses from buying or possessing firearms (U.S. Department of Justice, Bureau of Justice Statistics, 2003).

[10]Additional gun show specific regulations that exist in California are described in California Penal Code 12071.4, which is also known as the Gun Show Enforcement and Security Act of 2000. For instance, this includes the requirement that each vendor at a gun show submits his personal information (name, birth date, driver's license number) as well as that of his employees to the producer of the show. It is also important to note that regulations that apply to the purchase of a firearm at a location other than a gun show generally apply to gun show purchases as well.

[11] Note that zip codes not listed in the 2000 Census were dropped from the analysis since distances to nearby gun shows cannot be calculated. We also omitted those zip codes with either zero population or zero land area according to the 2000 Census. These zip codes are omitted (i) to allow us to consider the number of deaths on a per capita basis and (ii) because the mortality data is based on zip code of residence rather than zip code of death. Similarly, one Houston census tract was dropped.

[12] Communications with Garen Wintemute raised the issue that not all gun shows are reported in *Gun and Knife Show Calendar*. Wintemute and his colleagues found that 298 gun shows were held in California and Texas in 2007 according to both *Gun and Knife Show Calendar* and the *Big Show Journal*. But, only 79% of these shows were listed in *Gun and Knife Show Calendar*. The failure to identify such shows could potentially bias our estimates towards zero. While this is a valid concern, we do not believe this omission will substantially bias our estimates. The shows that are not contained in our data are relatively small shows, which might be expected to have a smaller impact on gun-related deaths. However, even if these shows are on average no smaller, a

simple back-of-the-envelope calculation reveals that the bias introduced by these omissions is likely to be small. Suppose for simplicity that the true number of homicides in the week after a gun show is X + Y and in any other week is X. Thus, in this simplified example, the true "effect" of the gun show in this first week is Y. Assume that the shows we miss occur only in weeks with no gun show (a conservative assumption in that it will maximize the estimated bias). Given that our analysis suggests that 4 percent of zip*week observations have one or more gun shows within ten miles, the figures of Wintemute and his colleagues suggest that an additional 1 percent of zip*week observations might have them. Thus, our weighted average for the "off" weeks (1 out of 96 of which would actually be "on") would be approximately X + (1/96)Y, which would introduce a bias of approximately 1 percent (leading us to estimate .9896Y instead of Y) in our pre-post analysis. And to the extent that these shows are smaller and/or also occur in "on" weeks, the actual bias is likely to be even lower.

[13] The 1990 census tract is used since that is the unit of identification used internally by the Houston Police Department.

[14] For the mortality analysis, zip code centroids were obtained from the 2000 census. For the crime analysis, tract centroids were obtained from the 1990 census.

[15] The California data was obtained from the Office of Health Information and Research in the California Center for Health Statistics (CAHS). The Texas data was obtained from the Center for Health Statistics in the Texas Department of State Health Services. We focus on deaths of 'state' residents to be consistent across states. For instance, while the California data set also includes deaths of non-Californians occurring in the state of California, the Texas data set does not.

[16] Deaths with incomplete zip code information were dropped from the analysis (0.9% of deaths in Texas and none in California).

[17] Deaths due to the accidental discharge of a firearm and those that are firearm related, but for which the cause is undetermined (i.e. accidental or committed with intent) can also be identified. We focus on just homicides and suicides given that gun homicides and suicides comprise 96% of gun deaths over our sample period.

[18] This data was obtained through an Open Records Request to the Houston Police Department.

[19] Whether a firearm was used can be determined for each of the four violent crimes.

[20] Four percent of gun-related deaths in our sample are gun accidents or gun deaths with an undetermined cause.

[21] For our Houston census tract analyses, we use five miles rather than ten miles as our baseline distance given that the city of Houston is much smaller than the entire state of Texas with just 600 square miles. For example, a ten-mile radius for a show in the middle of the city would include more than half of the city's geographic area.

[22] Other studies of the determinants of suicide have used similar specifications. For instance, Bollen and Phillips (1982) studies the effects of publicized suicides (i.e. an imitation effect) using daily data and 10 leads and lags for news coverage.

[23] Wintemute (2007) provides some evidence that this is the case. In his study, he recorded vehicle licensure at two gun shows in Reno, Nevada and found that 31% and 32% of vehicles bore California license plates at both of these shows.

[24] To the extent that the total population is highly correlated with the number of gun-related deaths in a jurisdiction, models that estimate proportional effects relative to a population base will be quite similar to those that use the number of deaths as the base.

[25] This model will suffer from extreme heteroskedasticity given the variation in zip code size. Because we present cluster-robust standard errors (clustering by zip code), our standard errors will be consistent, but not efficient.

[26] One might also estimate models with a binary outcome indicating whether the location experienced at least one gun-related death in a given week. This approach may attenuate any effects of gun shows, however. The reason for this is that large jurisdictions almost always experience at least one death and, conversely, small jurisdictions almost never experience a death. This will tend to bias the coefficients on our gun show indicators toward zero. To see this, consider a large jurisdiction such as Los Angeles that has at least one gun death every week. Here the coefficient on our gun show measures will be zero by construction. The same will be true for jurisdictions where no gun-deaths occur.

[27] On-line appendix Table A8 shows results separately by zip code population. Consistent with the similarity of weighted and unweighted results in Table 4, we find no significant patterns by zip code population size.

Figure I. Gun Shows and Gun Homicides and Suicides in California and Texas from 1994 to 2004.

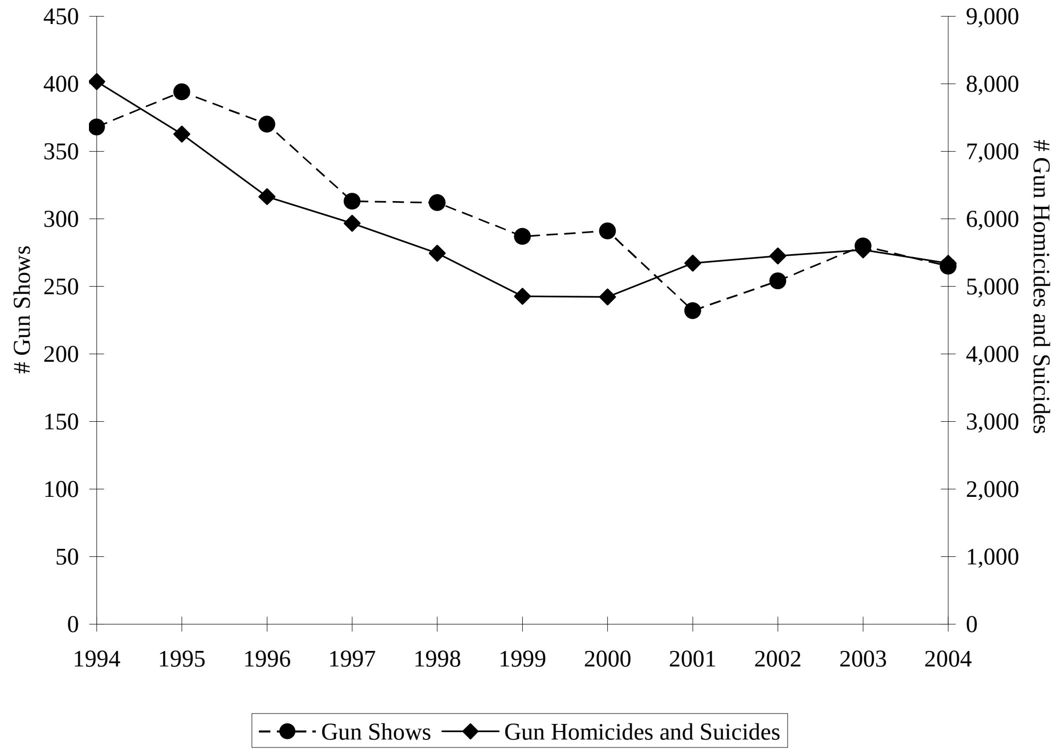
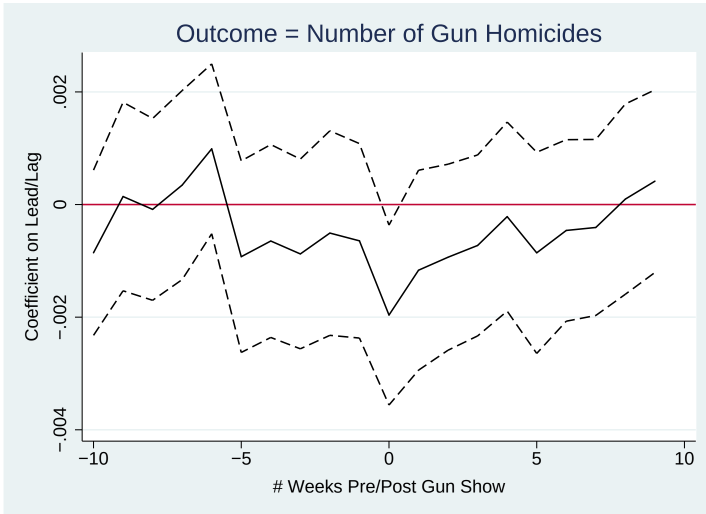
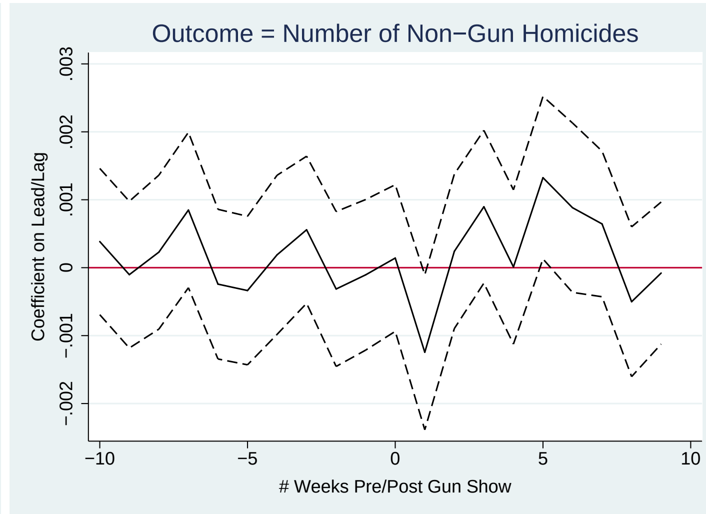


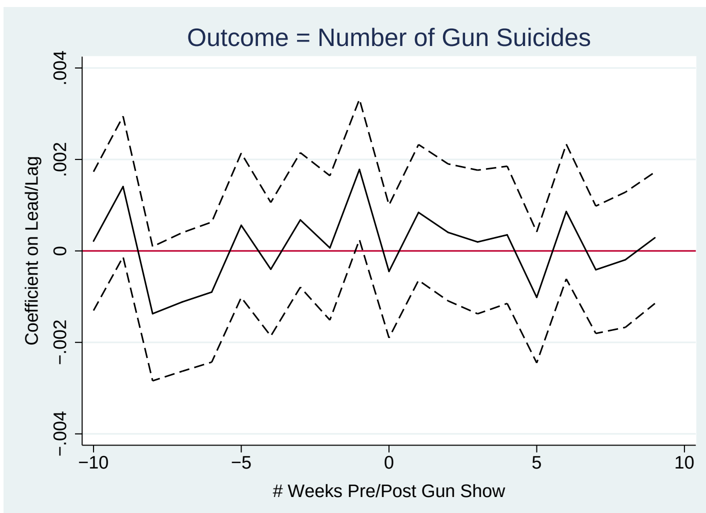
Figure II: Effect of Gun Shows on Homicides and Suicides in CA and TX



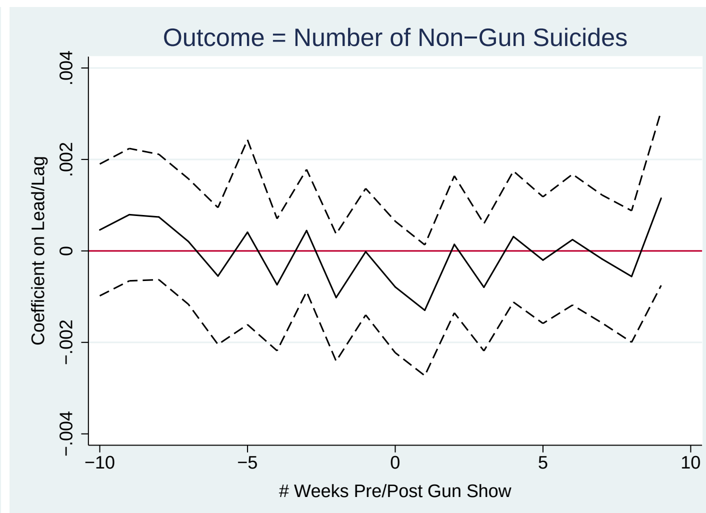
(a) Gun Homicides



(b) Non-Gun Homicides



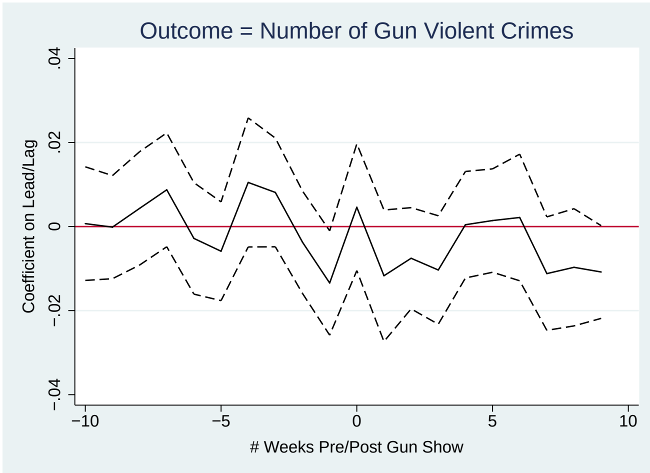
(c) Gun Suicides



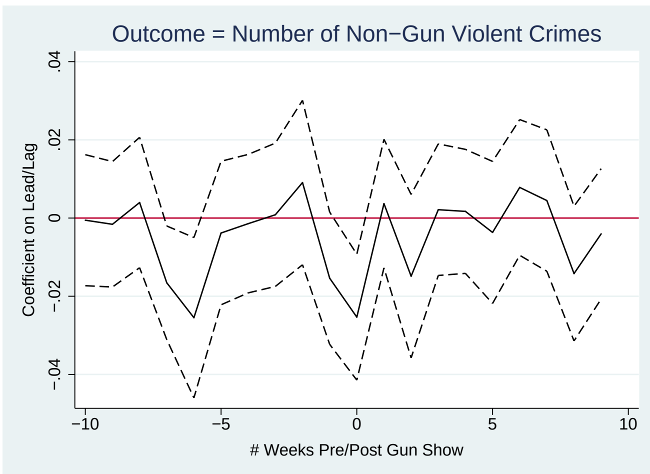
(d) Non-Gun Suicides

Notes: Estimates come from the specification shown in equation (1) in the text. Rather than a one month lag variable, however, there are 10 lag and lead variables that indicate the number of deaths in the zip code during that week. Week 0 indicates the week of the gun show. The sample is all zip codes in CA and TX that have at least one gun show within a 10 mile radius during the sample period.

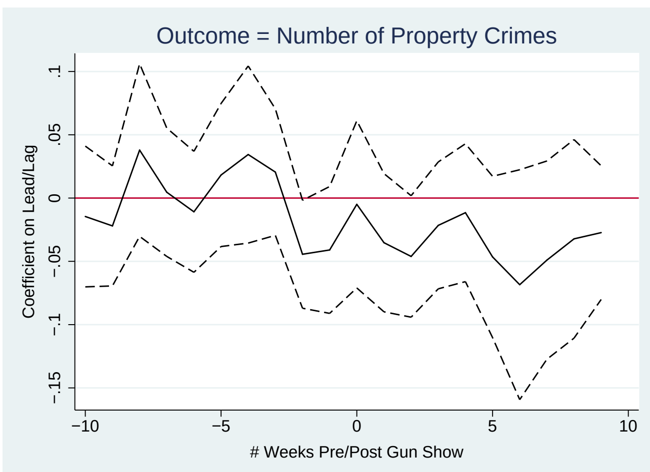
Figure III: Effect of Gun Shows on Crime in Houston



(a) Gun Violent Crimes



(b) Non-Gun Violent Crimes



(c) Property Crimes

Notes: Estimates come from the specification shown in equation (1) in the text. Rather than a one month lag variable, however, there are 10 lag and lead variables that indicate the number of crimes in the census tract during that week. Week 0 indicates the week of the gun show. The sample is all census tracts in Houston that have at least one gun show within a 5 mile radius during the sample period.

Table1. Descriptive Statistics

Panel A - CA and TX Zip Codes	All Zip Codes	1+ Gun Shows	Zip Codes with: 1+ Gun Shows within a 10 Mile Radius	No Gun Shows
	(1)	(2)	(3)	(4)
Number of gun-related deaths in zip code-week				
All gun deaths	0.309	0.217	0.249	0.315
Gun suicides	0.203	0.126	0.147	0.208
Gun homicides	0.083	0.081	0.095	0.083
Demographic characteristics of zip codes				
Total population	15,521	27,200	25,730	14,751
Population density (population/sq. mile)	1,966	1,916	3,944	1,970
Land area (sq. miles)	94.6	154.6	42.4	90.7
Fraction rural	0.466	0.176	0.145	0.485
Fraction Hispanic	0.245	0.298	0.281	0.241
Fraction Black	0.065	0.086	0.089	0.064
Fraction below poverty line	0.151	0.181	0.144	0.149
Zip is in an MSA	0.675	0.716	0.912	0.672
Fraction of suicides by gun	0.593	0.562	0.531	0.595
Number of zip codes	3,525	218	1,596	3,307
Number of zip*weeks	1,952,850	120,772	884,184	1,832,078

Panel B - Houston Census Tracts	All Census Tracts	1+ Gun Shows	Census Tracts with: 1+ Gun Shows within a 5 Mile Radius	No Gun Shows
	(1)	(2)	(3)	(4)
Number of crimes in tract-week				
Non-gun violent crimes	19.33	24.86	27.63	19.27
Gun violent crimes	14.10	12.86	20.83	14.12
Property crimes	221.37	386.62	327.97	219.50
Demographic characteristics of census tracts				
Total population	4,858	5,105	3,896	4,855
Population density (population/sq. mile)	4,008	4,425	4,826	4,003
Land area (sq. mile)	2.305	1.125	1.270	2.318
Fraction rural	0.028	0.000	0.011	0.028
Fraction Hispanic	0.249	0.210	0.296	0.249
Fraction Black	0.272	0.386	0.295	0.271
Fraction below poverty line	0.200	0.249	0.246	0.200
Number of census tracts	446	5	221	441
Number of tract*weeks	247,084	2,770	122,434	244,314

Notes: Each cell contains the mean of the row variable for the sample indicated by the column header. The unit of observation in Panel A (Panel B) is zip code*week (census tract*week). The number of gun related deaths per zip code*week in Panel A and crimes per census tract*week in Panel B are per 100,000 residents. Population numbers in Panel A (Panel B) are based on the 2000 (1990) census.The zip codes and census tracts in column (2) are a subset of those in column (3).

Table 2. Effect of Gun Shows on Mortality, By Geographic Distance

		Panel A: Within Zip Code				Panel B: Within 5 Mile Radius			
		Homicides		Suicides		Homicides		Suicides	
		Gun	Non-Gun	Gun	Non-Gun	Gun	Non-Gun	Gun	Non-Gun
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Both States	1st Month Post-Show	0.0014	0.0013	0.0018	-0.0019	-0.0001	0.0011*	0.0001	-0.0014
		(0.0027)	(0.0015)	(0.0024)	(0.0025)	(0.0011)	(0.0007)	(0.0009)	(0.0009)
	Observations (zip*weeks)	120,772	120,772	120,772	120,772	463,144	463,144	463,144	463,144
	Number of zip codes	218	218	218	218	836	836	836	836
	R-squared	0.056	0.027	0.032	0.033	0.101	0.032	0.032	0.034
	Mean (dependent var)	0.022	0.013	0.031	0.025	0.032	0.014	0.027	0.026
	Std. Dev. (dependent var)	0.159	0.119	0.178	0.159	0.194	0.122	0.167	0.162
		Panel C: Within 10 Mile Radius				Panel D: Within 25 Mile Radius			
		Homicides		Suicides		Homicides		Suicides	
		Gun	Non-Gun	Gun	Non-Gun	Gun	Non-Gun	Gun	Non-Gun
		(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	1st Month Post-Show	-0.0005	-0.0002	-0.0004	-0.0004	-0.0001	0.0001	-0.0003	-0.0003
		(0.0006)	(0.0004)	(0.0006)	(0.0005)	(0.0003)	(0.0002)	(0.0003)	(0.0003)
	Observations (zip*weeks)	884,184	884,184	884,184	884,184	1,550,646	1,550,646	1,550,646	1,550,646
	Number of zip codes	1,596	1,596	1,596	1,596	2,799	2,799	2,799	2,799
	R-squared	0.105	0.033	0.033	0.035	0.104	0.034	0.036	0.038
	Mean (dependent var)	0.029	0.012	0.025	0.023	0.019	0.008	0.019	0.016
	Std. Dev. (dependent var)	0.183	0.113	0.158	0.157	0.148	0.093	0.137	0.132

Notes: The sample for Panel A is zip codes that have at least one gun show during the sample period. For Panel B, C and D, it is zip codes with at least one gun show within a 5, 10 and 25 mile radius, respectively, during the sample period. The 1st Month Post-Show lag indicates the number of gun shows during the past month within zip code, and within a 5, 10 or 25 mile radius for the respective samples. The coefficient gives the effect of gun shows on deaths during the month following the show, as compared to deaths during the month prior to the show. The unit of observation is zip code*week. Standard errors (in parenthesis) are clustered by zip code. Uses month and zip code*year fixed effects. * significant at 10%; **significant at 5%; *** significant at 1%.

Table 3. Effect of Gun Shows on Mortality, By State

		Within 10 Mile Radius			
		Homicides		Suicides	
		Gun	Non-Gun	Gun	Non-Gun
		(1)	(2)	(3)	(4)
California	1st Month Post-Show	0.0002	0.0001	0.0005	-0.0001
		(0.0009)	(0.0006)	(0.0008)	(0.0009)
	Observations (zip*weeks)	499,154	499,154	499,154	499,154
	Number of zip codes	901	901	901	901
	R-squared	0.120	0.034	0.033	0.034
	Mean (dependent var)	0.035	0.013	0.025	0.028
	Std. Dev. (dependent var)	0.203	0.117	0.159	0.176

		Within 10 Mile Radius			
		Homicides		Suicides	
		Gun	Non-Gun	Gun	Non-Gun
Texas	1st Month Post-Show	-0.0010	-0.0004	-0.0011	-0.0006
		(0.0009)	(0.0006)	(0.0008)	(0.0006)
	Observations (zip*weeks)	385,030	385,030	385,030	385,030
	Number of zip codes	695	695	695	695
	R-squared	0.065	0.032	0.033	0.030
	Mean (dependent var)	0.021	0.011	0.024	0.016
	Std. Dev. (dependent var)	0.153	0.108	0.157	0.127

Notes: The sample for columns (1)-(4) is zip codes that have at least one gun show within a 10 mile radius during the sample period. The 1st Month Post-Show lag indicates the number of gun shows during the past month within a 10 mile radius. The coefficient gives the effect of gun shows on deaths during the month following the show, as compared to deaths during the month prior to the show. The unit of observation is zip code*week. Standard errors (in parenthesis) are clustered by zip code. Uses month and zip code*year fixed effects. * significant at 10%; **significant at 5%; *** significant at 1%.

Table 4. Effect of Gun Shows on Mortality, By Model Specification

		Gun Homicides					Gun Suicides				
		OLS, # of deaths (Baseline)	OLS, # of deaths (Wtd. by Pop.)	NB, # of deaths	Poisson, # of deaths	OLS, # of deaths (limited sample)	OLS, # of deaths (Baseline)	OLS, # of deaths (Wtd. by Pop.)	NB, # of deaths	Poisson, # of deaths	OLS, # of deaths (limited sample)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Both States	1st Month Post-Show	-0.0005	-0.0009	0.9836	0.9823	-0.0010	-0.0004	-0.0004	0.9834	0.9834	-0.0007
		(0.0006)	(0.0011)	(0.0214)	(0.0188)	(0.0012)	(0.0006)	(0.0008)	(0.0220)	(0.0190)	(0.0009)
	Observations (zip*weeks)	884,184	884,184	403,375	403,375	403,375	884,184	884,184	529,422	529,422	529,422
	Number of zip codes	1,596	1,596	1,379	1,379	1,379	1,596	1,596	1,491	1,491	1,491
	R-squared	0.105	0.115			0.078	0.033	0.029			0.017
	Mean (dependent var)	0.029	0.029	0.063	0.063	0.063	0.025	0.025	0.041	0.041	0.041
	Std. Dev. (dependent var)	0.183	0.183	0.267	0.267	0.267	0.158	0.158	0.203	0.203	0.203

Notes: Standard errors (clustered by zip code) are in parenthesis for the OLS regressions. For NB and Poisson regressions, exponentiated coefficients are reported with p-values in parenthesis. The sample for columns (1), (2), (6) and (7) is zip codes with at least one gun show within a 10 mile radius during the sample period. The sample for columns (3)-(5) and (8)-(10) is a subset of those zip codes - specifically, the subset of zip*weeks for which there is at least one non-zero gun death outcome variable during a given year. The 1st Month Post-Show lag indicates the number of gun shows within a 10 mile radius during the past month. The coefficient gives the effect of gun shows on deaths during the month following the show, as compared to deaths during the month prior to the show. The unit of observation is zip code*week. All regressions use month and zip code*year fixed effects. * significant at 10%; **significant at 5%; *** significant at 1%.

Table 5. Effect of Gun Shows on Mortality, By Poverty and Urbanicity

		Gun Homicides			Gun Suicides		
		Rural	Urban Non-Poor	Urban Poor	Rural	Urban Non-Poor	Urban Poor
		(1)	(2)	(3)	(4)	(5)	(6)
	1st Month Post-Show	0.0002	-0.0002	-0.0008	-0.0019	0.0009	-0.0011
		(0.0008)	(0.0006)	(0.0010)	(0.0014)	(0.0009)	(0.0008)
	Observations (zip*weeks)	142,378	298,606	443,200	142,378	298,606	443,200
	Number of zip codes	257	539	800	257	539	800
	R-squared	0.026	0.033	0.103	0.035	0.030	0.032
	Mean (dependent var)	0.003	0.011	0.049	0.011	0.026	0.028
Both States	Std. Dev. (dependent var)	0.061	0.108	0.238	0.105	0.161	0.170
		Non-Gun Homicides			Non-Gun Suicides		
		Rural	Urban Non-Poor	Urban Poor	Rural	Urban Non-Poor	Urban Poor
	1st Month Post-Show	0.0001	0.0001	-0.0004	-0.0008	0.0003	-0.0007
		(0.0006)	(0.0005)	(0.0006)	(0.0011)	(0.0009)	(0.0007)
	Observations (zip*weeks)	142,378	298,606	443,200	142,378	298,606	443,200
	Number of zip codes	257	539	800	257	539	800
	R-squared	0.024	0.023	0.031	0.029	0.029	0.035
	Mean (dependent var)	0.002	0.006	0.019	0.006	0.025	0.027
	Std. Dev. (dependent var)	0.048	0.084	0.141	0.076	0.170	0.166

Notes: The sample is zip codes that have at least one gun show within a 10 mile radius during the sample period. Rural is defined as at least 30 percent rural as defined by the census. Urban Non-Poor is defined as less than 30 percent rural and less than 10 of percent of population below the poverty line. Urban Poor is defined as less than 30 percent rural and at least 10 percent of population above the povery line. The 1st Month Post-Show lag indicates the number of gun shows within a 10 mile radius during the past month. The coefficient gives the effect of gun shows on deaths during the month following the show, as compared to deaths during the month prior to the show. Standard errors (in parenthesis) are clustered by zip code. The unit of observation is zip code*week. Uses month and zip code*year fixed effects. * significant at 10%; **significant at 5%; *** significant at 1%.

Table 6. Effect of Gun Shows on Mortality, By Gun Ownership

		Gun Homicides			Gun Suicides		
		Low Gun Ownership	Moderate Gun Ownership	High Gun Ownership	Low Gun Ownership	Moderate Gun Ownership	High Gun Ownership
		(1)	(2)	(3)	(4)	(5)	(6)
	1st Month Post-Show	0.0007	-0.0003	-0.0022	-0.0005	0.0002	-0.0018
		(0.0012)	(0.0008)	(0.0018)	(0.0009)	(0.0008)	(0.0015)
	Observations (zip*weeks)	216,060	434,336	204,426	216,060	434,336	204,426
	Number of zip codes	390	784	369	390	784	369
	R-squared	0.099	0.108	0.089	0.031	0.029	0.035
	Mean (dependent var)	0.025	0.036	0.020	0.015	0.031	0.025
Both States	Std. Dev. (dependent var)	0.170	0.206	0.153	0.125	0.177	0.160
		Non-Gun Homicides			Non-Gun Suicides		
		Low Gun Ownership	Moderate Gun Ownership	High Gun Ownership	Low Gun Ownership	Moderate Gun Ownership	High Gun Ownership
	1st Month Post-Show	0.0000	-0.0004	0.0000	0.0010	-0.0012	-0.0001
		(0.0008)	(0.0005)	(0.0009)	(0.0011)	(0.0008)	(0.0008)
	Observations (zip*weeks)	216,060	434,336	204,426	216,060	434,336	204,426
	Number of zip codes	390	784	369	390	784	369
	R-squared	0.034	0.032	0.032	0.037	0.029	0.028
	Mean (dependent var)	0.012	0.014	0.009	0.030	0.027	0.010
	Std. Dev. (dependent var)	0.113	0.124	0.097	0.191	0.165	0.099

Notes: The sample is zip codes that have at least one gun show within a 10 mile radius and one suicide during the sample period. The 1st Month Post-Show lag indicates the number of gun shows within a 10 mile radius during the past month. The coefficient gives the effect of gun shows on deaths during the month following the show, as compared to deaths during the month prior to the show. Fraction of suicides commited with a gun is used to proxy gun ownership. Low gun ownership is defined as the bottom 25% of CA and TX zip codes; moderate is the middle 50%; and high is the top 25%. Standard errors (in parenthesis) are clustered by zip code. The unit of observation is zip code*week. Uses month and zip code*year fixed effects. * significant at 10%; **significant at 5%; *** significant at 1%.

Table 7. Effect of Gun Shows on Crime in Houston, TX

	Non-Gun Violent Crimes	Gun Violent Crimes	Property Crimes
1st Month Post-Show	-0.0089	-0.0040	-0.0121
	(0.0080)	(0.0052)	(0.0199)
Observations (tract*weeks)	122,434	122,434	122,434
Number of census tracts	221	221	221
R-squared	0.311	0.193	0.735
Mean (dependent var)	1.063	0.386	5.085
Std. Dev. (dependent var)	1.416	0.768	5.090

Notes: The sample is census tracts that have at least one gun show within a 5 mile radius during the sample period. The 1st Month Post-Show lag indicates the number of gun shows within a 5 mile radius during the past month. The coefficient gives the effect of gun shows on crimes during the month following the show, as compared to crimes during the month prior to the show. The unit of observation is census tract*week. Standard errors (in parenthesis) are clustered by census tract. Uses month and census tract*year fixed effects. * significant at 10%; **significant at 5%; *** significant at 1%.