Gun-Use Regulation and Firearm Mortality: Evidence from Deer Hunting Season

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Abstract

This paper studies gun-use regulation and firearm mortality. I gather historical schedules of deer hunting season in North Carolina and Virginia and pair them with morgue records at county-day frequency. Hunters increase firearm use during deer season and sharply decrease use at season's end. The end of deer season in these states reduces firearm homicide fatalities by 45 percent of its daily average, 1 homicide every 3 years. Deer season homicides spill over onto women, who rarely hunt, but do not affect non-firearm fatalities. Public policies that encourage safer gun-use, even among lawful firearm owners, may decrease firearm injuries.

1 Introduction

Firearms are popular consumer goods in the U.S. with public health implications. One in three U.S. households owns a firearm, and U.S. consumers buy 25 million firearms each year (Parker et al. 2017, Moshary et al. forthcoming). Downstream, there were 48,000 fatal firearm injuries in 2022, and at least as many non-fatal injuries, making firearm injuries in the U.S. "a serious public health problem" (Centers for Disease Control 2023).

A number of public policy proposals seek to minimize firearm injuries by changing how consumers use their legally acquired firearms. These include regulations on the safe storage of firearms, on how and where firearms may be carried in public, and on which events allow an individual to discharge a firearm (Smart et

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al. 2023). There is some evidence that gun-use policy can affect firearm violence downstream. Yet, a lack of data and policy experimentation hampers research into gun-use laws and restricts information available to policymakers seeking to minimize the harms of firearm violence (Smart et al. 2021, Smart et al. 2023).

In this paper, I analyze how tightening local gun-use regulation affects local gun mortality in the short-term. I isolate exogenous changes in gun-use policy by studying the gun-hunting season for white-tailed deer ("deer season") in North Carolina and Virginia. The annual deer harvest is a major event in the U.S., leading millions of individuals to spend billions of dollars on hunting permits and equipment for the privilege of actively using their guns to harvest deer (U.S. Department of Interior et al. 2016). To meet state game management objectives, deer season schedules vary temporally, even over narrow geographies, which induces local variation in the timing of hunting-related gun use. Most hunters own guns beyond those used for hunting (Parker et al. 2017) and store these guns together (Crifasi et al. 2018), which leads deer season to increase access to both hunting-related long guns and handguns that are at higher risk of use in criminal behavior (e.g., Braga and Cook 2018). In line with these forces, the rate of firearm homicide fatalities is 30 percent higher during deer season than during the rest of the year.

The characteristics of deer season suggest that its timing creates quasi-random changes in local gun-use regulations. State game managers assign deer seasons to small clusters of counties months in advance of opening day. Their assignments balance the timing of the annual deer rut, the specific needs of local deer population control, and long-run norms around season dates. During the statutory deer season, both Google Search interest in deer hunting and the physical deer harvest rise smoothly through a preliminary hunting period, remain strong during the primary season, and then fall abruptly at the season's end. A wave of interest in ammunition follows deer season, suggesting that these regulations affect gun use on the ground. Outside of hunter behavior, deer season schedules do not systematically vary with interest in other forms of gun use, including self defense and shooting sports (Parker et al. 2017). Nor do they relate to interest in alcohol—a known risk factor for firearm injury—or incidents of non-firearm injuries (Pear et al. 2023). The clear assignment mechanism, plausible effects on hunter behavior, and separation from background risks all support a research design based on the timing of deer season.

To utilize the research design created by the local timing of deer season, I construct a dataset for North Carolina and Virginia with complete information on deer season status and firearm fatalities at county-day level. I gather data on hunting schedules for each county-year from historical maps provided by the game commissioners of North Carolina and Virginia. Since deer hunting regulations in these states have changed over time, and are not indexed online, constructing this dataset required correspondence and feedback from experts in each state. I gather case-level mortality data at county-day frequency from morgue records available through a restricted-access version of the National Violent Death Reporting System. Linking these datasets provides day-level fatality counts during 2,604 deer seasons across county-years. I analyze the effect of a county exiting deer season using a stacked event study framework.

My preferred specification reveals that the end of deer season in a county leads to a decrease in firearm homicide fatalities by 45 percent of its average daily fatality count. Aggregating across North Carolina and Virginia, this effect represents about 1 fatality every 3 years. This finding is robust to a variety of estimators and placebo checks, validating that my main results are not driven by modeling choices or statistical noise inherent to the analysis of mortality in high-resolution data. Deer season also drives the share of firearm homicides in which hunting is a proximate factor and the frequency of accidental firearm fatalities, which often relate to hunter behavior (Conlin et al. 2009). This pattern suggests a common underlying mechanism—laxer gun-use regulations during deer season—that increases hunting-specific firearm injuries and firearm homicides overall.

The start of the primary deer season has no on-impact effect on firearm fatalities. This result mirrors Niekamp (2019), who finds that the start of deer season has no effect on violent crime. Statutorily, deer season in North Carolina and Virginia ramp up through eight weeks of archery and shotgun/muzzleloader hunting prior to the primary general firearms season, each attracting more hunters than the last. Although Niekamp (2019) discusses pre-season trends, I characterize these dynamics by gathering additional data on the timing of the physical deer harvest and Google Search interest in deer hunting. Gun-use on the ground increases smoothly across the statutory start of deer season, explaining why the season's start does not sharply affect violent crime. Unlike Niekamp (2019), I examine the middle and end of deer season, finding that both gun-use and firearm homicide fatalities rise throughout deer season and fall sharply at the season's end. Virginia and North Carolina have similar deer season regulations, simplifying the interpretation of these dynamics, while Niekamp (2019) aggregates hunting seasons in 13 states with heterogeneous regulations.[1]

There is minimal connection between the decrease in firearm homicide fatalities at the end of deer season and the occurrence of other violent injuries. Repeating my primary analysis using all morgue records from non-firearm homicide fatalities, I find no change at the end of deer season. I also apply my research design to data on non-fatal criminal incidents from police reports. Non-fatal assaults—perpetrated with or without a firearm—appear to rise during deer season and fall afterwards. This pattern suggests that deer season does not lead individuals to substitute from one form of violence to another. If there are spillovers from the tightening of gun-use laws at the end of deer season, they serves to decrease assault injuries of all kinds.

Permissive gun-use regulations may impose negative impacts on non-users, even when gun-use is driven by legal activities. By analyzing the characteristics of firearm homicide fatalities, I show that the share of

[1]For instance, Niekamp (2019) applies the same model to both the one-week deer season in Wisconsin and the seven-week deer season in Virginia. Since hunting is differentially scarce in these states, their deer seasons may induce heterogeneity in hunter behavior.

incidents in which a female is injured falls by 6 percentage points, 25 percent, at the end of deer season. Since 90 percent of deer hunters are male (U.S. Department of Interior et al. 2016), this patterns suggests an externality from gun use that harms non-users.

The magnitude of the decrease in firearm homicide fatalities at the end of deer season is large. As a semi-elasticity, this effect is more than twice as large as the effect of other public policies on firearm homicide fatalities. For instance, states that adopt waiting periods between an individual's firearm purchase and their physical firearm acquisition decrease subsequent firearm homicides by 18 percent (Luca et al. 2017). The decrease in firearm homicide fatalities at the end of deer season is $45/18 - 1 = 150$ percent larger than the effect of waiting periods. This gap in effect size between policies designed to prevent firearm injuries and the ancillary effects of gun-use during deer season highlights the connection between firearm injury prevention policy and firearm usage "on the ground." The existence of a gap in effects sizes also suggests that current policy may not be optimal and that, through further policy experimentation, it may be possible to design more effective firearm policies. My results suggest that gun-use regulations may be a valuable direction for such experiments.

This paper contributes to a variety of literatures. Most narrowly, this paper extends a set of studies on game hunting and firearm injuries, including the independent but concurrent work of Niekamp (2019). Since findings from this literature pertain to my research design and interpretation of results, Section 3 provides a more thorough discussion.

This paper also contributes to a large literature analyzing the impacts of specific firearm-related public policies on downstream public health and crime. These studies cover concealed carry laws (Lott and Mustard 1997, Donohue et al. 2019), waiting periods between gun purchase and pickup (Luca et al. 2017, Koenig and Schindler 2023), bans on the transaction of assault weapons (Koper and Roth 2001, 2002), restrictions on where firearm retailers may operate (Pear et al. 2023), laws requiring secure storage of firearms in homes with children (Schell et al. 2020), and gun buy-backs (Ferrazares et al. 2022). Smart et al. (2023) provides a systematic review of these policies. Unlike these studies, I analyze the effect of a gun-use regulation not designed to limit firearm injuries. By focussing beyond the set of policies that have been implemented in this past, this paper suggests that additional experimentation may yield even more effective tools for firearm injury prevention. This paper also utilizes a unique empirical approach, leveraging hundreds of policy changes and performing inference as the number of policy changes grows large.

Most broadly, my results speak to a long debate about the relationship between firearm ownership, public health, and crime (e.g., Krug 1968, Zimring 1968). Early work studied this relationship by analyzing changes in proxies for firearm ownership within a population over time (e.g., Duggan 2001, Cook and Ludwig 2006). While more recent papers directly analyze flows of firearm acquisitions (e.g., Lang 2013, 2016; Depetris-

Chauvin 2015; Levine and McKnight 2017; Studdert et al. 2020, 2022; Miller et al. 2022). Building on this work, a new literature on the economics of firearm injury prevention considers how market-based policies may affect firearm ownership and downstream public health outcomes (Cook 2018, McDougal et al. 2023, Moshary et al. forthcoming, Rosenberg 2024, Armona and Rosenberg 2024). I complement this literature by showing that, taking the stock and distribution of firearm ownership as given, well-designed regulations on how legal owners use their firearms may be able to limit firearm injuries in the U.S.

The remainder of the paper is organized as follows. Section 2 presents the data I use in analysis and Section 3 discusses deer season as a gun-use policy. Section 4 describes my research design and estimation strategy. Section 5 presents results, while Section 6 comments on magnitudes and policy implications.

2 Data

This section describes the sources of information used in this study.

2.1 Deer season schedules

I construct a dataset of deer hunting dates by encoding historical maps from the North Carolina Wildlife Resource Commission and the Virginia Department of Game and Inland Fisheries. Figure OA.1 provides an example of the raw data on deer hunting dates. I transform the raw data recorded in these maps, and clarified by a deer hunting expert from each state, using a script that assigns county-dates to deer season status.

Focusing on these states offers substantial data-analytic advantages. Both states were early adopters of the National Violent Death Reporting System, providing high-resolution data on firearm mortality. North Carolina and Virginia border one another, ensuring continuous economic, demographic, and ecological conditions within the study area. White-tailed deer are commonly hunted in both states, and are hunted at much higher rates than other animals, restricting attention to a single, salient hunting period (NC Wildlife 2017). In both states, game areas for white-tailed deer generally follow county borders, making it straightforward to match hunting dates with morgue records.

North Carolina and Virginia divide the broader deer season into three weapons periods: archery, shotgun/muzzleloader, and gun/general firearms. The hunting season dates are not uniform across counties or years in either state. North Carolina is divided into four contiguous regions that generally enter and exit seasons together. Virginia is divided into three.[2] Season dates are mostly set by weekdays within months.

[2] Virginia's deer project coordinator Matt Knox notes that some urban areas—especially around Washington D.C. and in independent cities—maintain deer hunting within city limits through much of the year, in order to control the local deer population. However, these extended seasons are highly regulated and draw few hunters.

Thus, hunting would begin on the Saturday prior to the fourth Monday in November, not on November 21. On average, each county's deer season consists of 43 days of archery starting in September, 17 days of muzzleloader, and 46 days of gun/general firearms, always progressing in that order. Some gun seasons cross into the January of the next calendar year. To maintain contiguity within a season, I treat these cross-year seasons as if they occur entirely during the year in which hunting began.

The final sample for analysis includes 226 counties observed over either 12 or 13 years, covering 2,604 county-level deer seasons and 101 state-season clusters. Figure 1 presents the spatial distribution of season clusters in 2015. Panel A of Table 1 summarizes the distribution of deer season status across counties.

2.2 Morgue records

To measure fatalities, I use administrative morgue records from the Centers for Disease Control ("CDC") between January 1, 2003 and December 31, 2015. These records are standardized across the U.S., completed by a medical professional after every known fatality, and collated by the CDC. Each record includes the date, location, and causes of death. Causes of death are recorded as ICD10 codes, twelve of which are principally gun-related. They reflect death by (i) assault, (ii) self-harm, (iii) accidental, and (iv) unknown means, each one crossed with indicators for death by (i) handguns, (ii) long guns, and (iii) unknown gun types.[3] A separate set of codes record firearm deaths from war, terrorism, and justifiable legal shootings, which I exclude from analysis.

I access the CDC morgue records through the National Violent Death Reporting System—Restricted Access Database (NVDRS-RAD), extracted on February 23, 2018.[4] This dataset provides records of violent deaths with county-day resolution for U.S. states that have opted into the reporting system (Centers for Disease Control 2023). The NVDRS began in 2003 with 7 states, and has since expanded to a broader section of the U.S. Virginia and North Carolina entered the system in 2003 and 2004, respectively. Each fatality is tagged with up to 11 ICD10 codes.

The NVDRS post-processes morgue records to ensure their reliability and to incorporate additional case-level characteristics of the incident. These include demographics of the deceased and suspected perpetrator, as well as additional context surrounding the incident. More detail is provided by the NVDRS web coding manual (Centers for Disease Control 2016).

I pair these records with a complete set of lower-resolution morgue records from the National Association for Public Health Statistics and Information Systems (NAPHSIS), collected by the National Center for Health Statistics (NCHS) and extracted on January 8, 2018. These records (Mortality - All County (micro

[3] Less than 1 percent of gun-related fatalities are attributed to unknown means. In contrast, 70 percent of gun-related fatalities are attributed to an unknown type of gun.

[4] For more information on the NVDRS-RAD, see Barber and Hemenway (2011) and Hemenway and Solnick (2015).

date)) are available as a restricted dataset with county-month resolution, reflecting all morgue records held by the CDC and tagged with up to 40 ICD10 codes.

I assign all fatalities in both sets of records to the county in which the fatality occurred.

These administrative data are distinct from records in the Uniform Crime Report (UCR), compiled by the FBI since 1930 (Regoeczi et al. 2014). The primary advantage of the NVDRS dataset is its day-level resolution, allowing for finer-grain analysis than the monthly data in the UCR or NAPHSIS datasets. In addition, NVDRS records any firearm fatality that appears in the U.S. healthcare system. This is in contrast to the UCR, which gathers data from a voluntary reporting system across police agencies, possibly introducing issues of non-random selective reporting of incidents to law enforcement or law-enforcement to the UCR. The improved resolution and recording in the NVDRS come at the cost of a decrease in coverage: being able to track firearm-related incidents over a shorter time horizon, across fewer localities, and without the inclusion of non-fatal incidents.

Panels B and C of Table 1 describes variation in fatality outcomes across the study region. Figure 2 shows how the occurrence of different types of fatalities evolves before, during, and after deer season.

2.3 Criminal incident reports

I complement the mortality data with information on criminal activity from the National Incident Based Reporting System (NIBRS) from Virginia between 2003 and 2015. NIBRS is a part of the Uniform Crime Reporting Program administered by the FBI that records detailed characteristics—including the calendar date, offenses, offenders, and victims—of criminal incidents. Criminal incidents are voluntarily reported to NIBRS by law enforcement agencies.

I restrict my NIBRS sample to neutralize potential biases from voluntary reporting. In particular, my analytic sample includes only law enforcement agencies operating in Virginia and making at least one NIBRS report each month between January 2003–December 2015. As no law enforcement agencies in North Carolina report to NIBRS during my sample period, its exclusion is unavoidable. The timing restriction neutralizes bias from changes in voluntary reporting within a law enforcement agency over time. As NIBRS covers all criminal incidents reported to police, there is little scope for this sample selection rule to remove law enforcement agencies with true zero criminal reports from the sample.

I match law enforcement agencies reporting to NIBRS with counties using the 2012 Law Enforcement Agency Identifiers Crosswalk. The matched data covers 202 law enforcement agencies reporting from 124 counties.

2.4 County population

I compute county population in 2015 using the 2016-vintage of data from the Surveillance, Epidemiology, and End Results (SEER) Program, produced by the National Cancer Institute.

2.5 North Carolina season harvest

I gather deer harvest counts in North Carolina for the 2021 season. The North Carolina Wildlife Resources Commission publishes the cumulative North Carolina deer harvest on their Live Deer, Turkey & Bear Reported Totals online tool.[5] Each day of the 2021 deer season, I visited this page and recorded the cumulative 2021 North Carolina deer harvest and the cumulative deer harvest within each county. Per the North Carolina Wildlife Resources Commission, "end-of-season harvest totals are subject to change until harvest records have been finalized by the NCWRC," making these daily numbers provisional.

These data are restricted to North Carolina during the 2021 deer season. The publicly available tool from the North Carolina Wildlife Resources Commission is updated in real time and does not provide historical records. Virginia does not provide a comparable tracker of the within-season harvest.

2.6 Google Trends queries

I measure public interest in various firearm-related topics by querying Google Trends (e.g., Levine and McKinght 2017, Koenig and Schindler 2023). Each query is associated with a single term, a state—either North Carolina or Virginia—and the fiduciary year spanning the deer season.[6] I make queries for the six final hunting seasons in my primary dataset: fiduciary years 2010–2015. Given these query inputs, Google Trends returns a weekly series of search interest scaled from 0—for the week with the lowest search interest in the term within a state-year—to 1 in the week with the highest search interest. Table OA.1 presents the exact terms used in queries. In analysis, I compute average search interest within a week by averaging that week's search interest scores across state-years. In some cases, I further aggregate the averages across years and/or search terms.

3 Hunting schedules as gun-use regulation

In this section, I discuss the use of deer hunting schedules in North Carolina and Virginia as a source of variation in gun-use regulation.

[5] Downloaded from https://www.ncalvin.org/harvestreports/.

[6] For instance, the deer season beginning in 2019 and ending in 2020 would be associated with the fiduciary year from June 2019 through June 2020.

The firearm injury prevention literature shows that hunting is an important driver of firearm ownership. In an early study of gun control Krug (1968) directly proxies firearm ownership by the number of hunting licenses issued per capita, and this method of measurement has been advanced by subsequent studies (e.g., Kleck and Patterson 1993; Kovandzic et al. 2013; Siegel et al. 2013, Schell et al. 2020). Nationally representative surveys of firearm owners find that more than 30 percent of owners hunt, and 40 percent of owners cite hunting as a primary reason for gun ownership, second only to personal protection (Parker et al. 2017, Azrael et al. 2017). The typical deer hunter is more likely to be a white, non-hispanic male than the average firearm owner and the overall U.S. population (U.S. Department of the Interior et al. 2016), and these demographic differences suggest that the hunting-motivated firearm owners may represent a distinct consumer- and epidemiological-segment of the population.

Deer season is a major driver of gun use across the U.S. Each year, it leads millions of individuals to spend billions of dollars for the ability to actively use guns to harvest deer (U.S. Department of Interior et al. 2016). Relative to all hunting, deer account for 70 percent of total hunters and 60 percent of total hunting days (U.S. Department of Interior et al. 2016). Conlin et al. (2009) show that Pennsylvanian counties with stricter regulation of deer hunting have fewer hunting-related accidental gun deaths. Niekamp (2019) demonstrates that the onset of deer hunting season meaningfully increases rates of on-person firearm carry and decreases short-run labor supply among likely hunters.

Deer season is an important driver of gun use in North Carolina and Virginia. In North Carolina in 2016, around 250 thousand hunters—40 percent of all licensees—spent 3.7 million days to harvest 220 thousand deer.[7] The second most popular animal to hunt was turkey, drawing one-third as many hunters. Virginian hunters in 2017 harvested 190 thousand deer.[8] The single-season, statewide deer hunting license fees for adult residents of North Carolina and Virginia in 2017 were $39 and $46, respectively (N.C. Wildlife Resources Commission 2019, Virginia DWR 2023).). Each license grants a hunter the right to harvest up to three bucks and six deer total during the hunting season. For these harvest privileges, deer hunters across these two states pay approximately $20 million in licensing fees each year. The demographics of hunters in North Carolina and Virginia are similar to the national average for hunters (U.S. Department of Interior et al. 2011a,b).

The statutory deer hunting season is long in North Carolina and Virginia. Most counties in most years begin archery season in September and end the gun season in mid- or late-December. These two seasons last more than a month, on average, and they bookend a two-week muzzleloader season in late October. Season

[7] Estimates of deer hunter engagement are from the North Carolina Wildlife Commission https://www.ncwildlife.org/Portals/0/Hunting/Documents/2016-17-NC-Hunter-Harvest-Survey-Estimates-Summary.pdf.

[8] Deer kill estimates are from the Virginia Department of Game and Inland Fisheries https://www.dgif.virginia.gov/wildlife/deer/harvestsummary/.

dates are set according to game management objectives and the timing of the annual deer rut. Among these three seasons, the gun season is most popular. In Virginia in 2017, approximately 60 percent of the deer harvest occurred during gun season, with 15 and 25 percent of the harvest in the archery and muzzleloader seasons, respectively.[9] Guns were slightly more used in North Carolina in 2017: approximately 80 percent of the deer harvest occurred during gun season, with bow and muzzleloader each contributing approximately 10 percent.[10]

Figure 3 presents the daily relationship between the start and end of the primary gun deer season and the cumulative deer harvest in North Carolina. Panel A shows that most of the deer harvest is accomplished during the gun season but that the rate of harvest begins to increase quickly during the two-week muzzleloader season prior to the gun season's start date. Panel B shows that the average county's deer harvest rises smoothly towards the end of the gun season and then stops abruptly, consistent with deer season regulations.

Figure 4 uses Google Trends to measure the dynamics of hunter behavior throughout deer season. Panel A shows that interest in deer hunting matches the pattern of the deer season harvest: it rises smoothly as gun season approaches and falls quickly at the season's end, closely tracking the staggered pattern with which counties exit deer season. Panel B shows that interest in deer season precedes an interest in ammunition, consistent with an increase in gun use during deer season. Panel C shows that search interest in the other two most commonly stated reasons for gun ownership—personal protection and shooting sports—are constant throughout the year.[11] Panel D shows that search interest in alcoholic beverages—a known contributor to firearm mortality (Pear et al. 2023)—is roughly constant across the year, with a small increase around New Year's Eve.

When hunters increase access to hunting firearms for deer season, they tend to increase access to firearms of every kind. Representative surveys (e.g., Parker et al. 2017, Azrael et al. 2017) find that U.S. gun owners are approximately evenly distributed among owning a single gun, two-to-four guns, and five or more guns. Moreover, owners of multiple guns tend towards variety, with more than half of gun owners possessing a handgun (70 percent), rifle (60 percent), or shotgun (54 percent). In a survey of 1,440 respondents representative of adult firearm owners in the U.S., Crifasi et al. (2018) finds that most respondents store their weapons in multi-gun appliances, such as gun safes, cabinets, or racks. Most of these firearms are stored assembled, unloaded, and in the same physical location as ammunition. Few respondents in either survey report permanently keeping their weapon on their person or outside their home.

[9] Harvest estimates are from the Virginia Department of Game and Inland Fisheries https://www.dgif.virginia.gov/wildlife/deer/harvestsummary/.

[10] Harvest estimates are from the North Carolina Wildlife Resources Commission https://www.ncwildlife.org/Portals/0/Hunting/Documents/Deer/Annual%20Reported%20Deer%20Harvest%20Summaries/Harv_ByWeaponType17.pdf.

[11] The spike in search interest for self defense reflects the Sandy Hook massacre (e.g., Wallace 2015).

Deer season may increase gun use through both physical and psychological mechanisms. Physical mechanisms may reflect proximity to guns, changes in gun storage practices, or new-gun purchases. Psychological mechanisms (Witt et al. 2020) may reflect the salience of firearm availability or the framing of gun use.

Thus, there is a clear mechanism for the end of deer season to impact firearm mortality by changing gun-use regulations in North Carolina and Virginia. Nationwide, hunting is a driver of firearm ownership, and deer hunting is a driver of gun use. In North Carolina and Virginia, deer hunting is an important institution, and the design of deer season schedules leads the harvest to phase-in smoothly over time but conclude sharply at the season's end. Deer season in these states is a regulation that leads hunters to increase gun use throughout the full deer season, puts hunters in a position to access other ready-to-use firearms when they access those used for hunting, and sharply removes the statutes that encourage gun use at season's end. In line with these forces, Figure 2 shows that firearm homicide fatalities are 30 percent higher during deer season than in the periods before, and 50 percent higher than in the post-season period.

4 Research design and estimation

To estimate the effect of the end of deer season on firearm fatalities, I estimate a linear stacked event study model of the following form:

$$Y_{itd} = \alpha + \beta Days_{itd} + \tau Post_{itd} + \gamma Days_{itd} \times Post_{itd} + \varepsilon_{itd}, \tag{1}$$

where Y_{itd} is a mortality outcome for county i on day d of year t. I measure event time as days from the end of deer season $Days$, with $Post$ a binary indicator equal to one for observations after the end of deer season. As notational convention, I specify observations occurring before the end of deer season as $Days < 0$, the last day of deer season with $Days = -1$, and observations after deer season as $Days \geq 0$. The intercept α governs the level of firearm mortality prior to the end of deer season, while β governs its event-time trend. Parameters τ and γ govern the level and trend of mortality after the end of deer season. The coefficient of interest is τ, which captures the average change in mortality the day after deer season ends at $Days = 0$.

Equation (1) has an intuitive connection to the workhorse framework for regression discontinuity designs (Imbens and Lemieux 2008). In the model, event-time $Days$ plays the role of the running variable, with treatment indicated by $Post$. The coefficient of interest τ represents the effect of a sharp change in treatment across a smoother change in the running variable. Unlike the typical regression discontinuity setting, I observe each county-year unit at all values of the running variable $Days$ and on either side of the threshold $Post$. Thus, I do not estimate Equation (1) by comparing a cross-section of units just below the treatment threshold to another set just above. Rather, I compare days before the end of deer season to days after the season's end within a county-year, and estimate τ by averaging this difference across many observations in a panel of county-years. In this sense, the estimator and identifying variation for Equation (1) are similar to an event study design, with the parameter of interest τ representing the on-impact effect of the event.

I estimate the model via ordinary least squares and cluster standard errors by the interaction of state, year, and deer season end-date. This clustering scheme matches the state game-management process that assigns county-years to hunting seasons, reflecting the level of treatment assignment in the research design (Abadie et al. 2023). Inference is based on the 101 "policy changes" from deer season conclusions within the data. Although the model is fit to county-day outcomes, this high-frequency data does not contribute to variance estimates.

To construct an estimation sample, I specify a bandwidth b and restrict estimation to observations no more than b days from the end of deer season. My default bandwidth is the median season length reported in Table 1 of $b = 50$. As a robustness check, I re-estimate my model under alternative bandwidths from $b = 1$ to $b = 100$ and provide the mean square error optimal bandwidth from Cattaneo et al. (2020) as a benchmark.

I interpret the regression in Equation (1) under bandwidth b as a linearly-smoothed analogue of the difference in means estimator for matched-pair experiments. At the minimal bandwidth of $b = 1$, the regression collapses to a two-parameter model, with α the average mortality on the last day of deer season, and τ the average change on the day after. Under this interpretation, each county-year is a strata, and the two days within the bandwidth per strata are a matched pair. Although valid under quasi-random assignment of deer season schedules, the noise inherent to mortality data likely makes this matched-pair estimator imprecise. For greater precision—and in analogy to the regression discontinuity literature (Hausman and Rapson 2018)—I increase the bandwidth to $b = 50$, utilizing additional information up to 7 weeks from the end of deer season. This additional information is smoothed linearly by the slope parameters β and γ. Geometrically, my estimate of the effect of the end of deer season is the difference between linear imputations of mortality on the last day of deer season and the day after deer season. In practice, narrower bandwidths lead to point estimates that are larger in magnitude and less precise, suggesting that smoothing decreases estimator variance at the cost of some attenuation bias.

To further reduce estimation noise, I standardize mortality outcomes Y_{itd} within each county. Let Y_{itd}^l denote the level of a mortality outcome, μ_i^l denote the outcome's full-sample mean within county i, and σ_i^l denote its within-county daily standard deviation. Then the outcome measure I use in estimation is the

z-score

$$Y_{itd} = \frac{Y_{itd}^l - \hat{\mu}_i^l}{\hat{\sigma}_i^l}.$$

When a county has complete zeros for a mortality outcome, this ratio is undefined, and I omit the county from the analytic sample for that outcome. This transformation serves to reduce noise by measuring level changes in mortality at the end of deer season against a county-specific benchmark for variability in mortality levels σ_i^l. In practice, this transformation down-weights changes in mortality for counties with high day-to-day variability and up-weights changes in less variable counties.

In implementing the linear model, I conduct a formal test of the null hypothesis that there is no systematic variation in mortality outcomes Y_{itd} over time within the estimation bandwidth b. In particular, I use a cluster-robust Wald test of the hypothesis that $\beta = \tau = \gamma = 0$. The stronger the evidence against this null, the more variation is available in the data to estimate Equation (1). If there is only weak evidence against this null, then there is minimal systematic variation in the mortality outcome Y_{itd} across event-time, and the causal effect estimator $\hat{\tau}$ is statistically unlikely to reflect the true causal effect of deer season's end τ. This hypothesis test is a formal, low-dimensional accompaniment to visually inspecting patterns in raw mortality data around the end of deer season.

4.1 Assumptions

There are three key assumptions on Equation (1) that allow τ to be interpreted as the causal effect of the end of deer season. In this section, I outline these assumptions, provide evidence in their support, and discuss how potential violations would likely affect my results.

First, I make a Stable Unit Treatment Value Assumption ("SUTVA") that separates mortality outcomes in each county from the deer season status of all other counties (Rubin 1980). As the treatment status of days within each county-year is predetermined to meet state game management objectives, SUTVA is a mild assumption in this context. To violate this assumption, hunters would need to travel from one county to another in a different deer season cluster *because* of both county's hunting seasons in a way that affected at least one county's potential mortality outcomes. This type of violation creates a spillover that constitutes a form of measurement error. The Online Appendix describes this assumption in greater detail and provides evidence in its support.

Second, I assume that changes in hunting season status other than the end of the deer hunting season do not affect morality outcomes Y_{itd} (i.e., there is no intercept representing the start of deer season). This modeling decision is based on parsimony, reducing the number of parameters that need to be given a causal interpretation. This assumption is further supported by my results, where I estimate that the start of deer season has no effect on mortality, suggesting that these other changes in hunting season can be left unmodeled. Moreover, Figure 3 shows that, on average, the deer harvest progresses smoothly as the end of the season approaches, suggesting that there are not large changes in hunter behavior before the end of deer season. To ensure that this assumption does not confound my causal effect estimates, I fit some models under an estimation bandwidth b narrow enough so that no unmodeled changes in deer season status occur within the estimation sample.

Third, I assume that the mean of the error term ε_{itd} does not change as the event-time variable $Days$ crosses the hunting season end date. This is the sharp RD assumption, vital to all estimators that approximate changes in treatment assignment at a point (Imbens and Lemieux 2008). I bolster this assumption in several ways. My estimates are based on hundreds of changes in county-level deer seasons across dozens of state-season clusters, so that any truly idiosyncratic noise in the error term ε_{itd} is likely to average out to zero. Moreover, Figure 4 shows that Google search interest in alcohol and non-hunting firearm activities is constant around the end of deer season, suggesting that other changes in gun use do not coincide with the end of deer season. Similarly, I examine placebo outcomes based on non-gun mortality, that would likely capture signal from other changes in mortality-related behavior embodied in the error term. I also estimate some regressions with rich covariates that improve model fit and better account for the distribution of the error ε_{itd} (Hausman and Rapson 2018). As an additional model specification check, I increase the estimation bandwidth b, reducing the leverage of errors ε_{itd} that may be unrelated to hunting but co-occur with the end of deer season. To explicitly remove confounders that may occur *exactly* at the end of deer season, I estimate "doughnut hole" models, in which the estimation bandwidth b excludes the days closest to the end of hunting season (Cattaneo et al. 2020). As violent deaths are, fortunately, rare events at the county-day level, serial correlation in the error ε_{itd} is theoretically unlikely (Hausman and Rapson 2018).

4.2 Permutation tests

I use two permutation tests to validate that my estimate of the effect of the end of deer season τ from Equation (1) is not driven by statistical noise in the data. Both tests compute two-sided p-values for a test statistic defined as the share of simulated effects that are at least as large in magnitude as the observed effect. The observed effect is included in the calculation of the share.

The first test considers the possibility of estimating a spuriously large change in mortality from this research design. The null hypothesis is that, for each county-year, there is no day within my default estimation sample on which there is a discrete change in average mortality. Under this sharp null, the distribution of my causal effect estimator $\hat{\tau}$ is unchanged if any day is (fallaciously) treated as the end of deer season. I simulate data under this null by treating each of the 100 days within my default estimation bandwidth as the end of deer season, forming a new 100-day estimation sample for each county-year around the simulated season end, and simulating event-time as the number of days from the simulated end date. I then estimate $\hat{\tau}$ on each of these 100 simulated datasets, computing the exact distribution of $\hat{\tau}$ under the null and, therefore, exact two-sided p-values. This permutation test addresses the following question: Is there a particular change in mortality at the end of deer season, or could this estimate be found by applying the same method to any calendar day?

The second test considers the possibility that the estimation strategy drives a spuriously large causal effect estimate. The null hypothesis is that there is no effect of the end of deer season on mortality. This hypothesis is different from the one above because it concerns the effect conditional on a specific date—the end of deer season—rather than a variety of alternate dates. Under this sharp null, an observation's outcome is uncorrelated with its event-time from the end of deer season. I simulate data under this null by randomly and without replacement permuting observed outcomes Y_{itd} across the days used in estimation within each county-year, while holding event-time fixed at its observed values. This procedure removes all correlation between simulated outcomes and observed event time. I then estimate $\hat{\tau}$ on the simulated data. Unlike the first test, the number of possible simulated datasets experiences exponential growth in the bandwidth b. I therefore approximate the exact two-sided p-value based on 10,000 simulations. This permutation test addresses the following question: Is there a particular change in mortality at the end of deer season, or could this estimate be found even if there was no effect whatsoever?

5 Results

This section implements the model and tests described in Section 4 to estimate how changes in deer season status affect mortality in North Carolina and Virginia. It further considers spillover to other criminal incidents and the heterogeneity in the characteristics of firearm homicides around the change in deer season.

5.1 Causal effect estimates

Panel A of Figure 5 visualizes the main result. Incidents of firearm homicide rise during deer season, drop sharply at the season's end, and remain constant thereafter. The vertical red line visualizes my estimator for the average effect of the end of deer season $\hat{\tau} = -0.02$. This estimate implies that homicide fatalities in the average county-year fall by 0.02 standard deviations the day after hunting season ends, relative to the day before.

The estimated effect of the end of deer season is meaningful, relative to the summary statistics in Table

1. The average county's standard deviation of daily firearm homicides is 0.07. Thus, the end of deer season, on average, corresponds to a decrease in firearm homicides of $0.02/0.07 \approx 30$ percent of the standard day-to-day variation in firearm homicide fatalities. If the decrease in fatalities at the end of deer season could be scaled-up to a full year, it would lead to $0.02 \times 0.07 \times 365 \approx 0.5$ fewer firearm homicides in the average county-year.

The patterns in Panel A of Figure 5 are consistent with the underlying mechanics of deer season in North Carolina and Virginia. Panel B of Figure 3 shows that the deer harvest grows smoothly until the season's end, at which point it stops abruptly. Since the median deer season is 50 days in length, the increasing pattern of firearm homicide fatalities on the left-hand side of the figure may reflect an increase in the share of observations that are within their statutory deer season. My estimate $\hat{\tau}$ reflects the increase in firearm access during deer season, followed by a decrease at the end of deer season, when gun-use restrictions become more stringent. In line with these visual patterns and the underlying mechanics of deer season, my estimates reject that expected firearm homicide fatalities are constant around the end of deer season ($p \approx 0.004$). Instead, firearm homicide fatalities are higher during deer season and lower afterwards.

Panel B of Figure 5 shows that there is no sharp change in firearm homicide mortality at the statutory start of deer season. Visually, patterns of firearm homicide fatalities over the 50 days prior to the primary deer season are similar to those 50 days after its start. Reflecting this visual pattern, there is minimal evidence that firearm homicide fatalities are non-constant over this period ($p \approx 0.3$). Thus, my estimate of the effect of the start of deer season is 70 percent smaller in magnitude than my estimate of the season's end. Importantly, the patterns of firearm homicide fatalities in the 10 days prior to the start of deer season and the 40 days after its end are consistent with the mechanics described in Section 3. The deer harvest accelerates in the week prior to deer season, remains brisk in the following weeks, and then slows after several weeks. This is the exact pattern observed in firearm homicides around the start of deer season. The null effect of the start of deer season on firearm homicide fatalities matches estimates of the start of deer season on violent crime reported by Niekamp (2019).

Table 2 summarizes regression estimates of the effect of the end of deer season on different sources of firearm fatalities. Accidental firearm fatalities fall by 0.04 standard deviations after the end of deer season, a decrease twice as large as the fall in firearm homicides, suggesting the importance of gun-use regulations in controlling accidental firearm injuries (e.g., Conlin et al. 2009). Unlike with firearm homicides and accidents, there is minimal evidence of a difference in firearm suicide fatalities before and after the end of deer season ($p \approx 0.15$). This result mirrors the patterns in Figure 2 and suggests a potentially limited role of gun-use laws in controlling self-inflicted firearm injuries.

The Online Appendix visualizes the data underlying the regression estimates in Table 2. It also shows that estimates are quantitatively similar when conditioning the regression in Equation (1) on fixed effects for each day of the week, holidays, and county-year fixed effects. This final set of fixed effects allows a separate baseline level of fatalities for each deer season in each county, so that all identifying variation comes through the passage of event time within a county-year. The Online Appendix also reports a complementary set of results for the start of deer season, finding that the pattern for firearm homicides extends to other sources of firearm fatalities.

5.2 Validation Tests

I consider a number of exercises to validate my estimates of the causal effect of deer season on firearm fatalities.

Panel A of Figure 6, visualizes how my estimate of the causal effect of the end of deer season on firearm homicide fatalities varies with the bandwidth used when estimating the regression in Equation (1). I find estimates with similar magnitudes and precision for bandwidths from 30–100 days around the end of deer season. For even narrower bandwidths—including the minimal bandwidth comparing the last day of deer season to the day after—estimates are larger in magnitude and less precise than under my preferred bandwidth. This pattern supports my interpretation of the linear model in Equation (1), as a device to increase the precision of my causal effect estimate through the use of additional data away from the end of deer season. Further supporting this bias-variance interpretation, when explicitly optimizing a mean-squared error criterion, the tools from Cattaneo et al. (2020) suggest an intermediate bandwidth of around 23 days. The "doughnut hole" bandwidth, excluding the last day of and the day after deer season, produces a causal effect estimate similar to my preferred bandwidth. This result suggests that my estimates are not driven by an unobservable shock that correlates with firearm mortality and co-occurs with the end of deer season. Instead, patterns in firearm homicides appears to be driven by changes in gun use around deer season, as discussed in Section 3.

Panel B of Figure 6 conducts the first of the two permutation tests described in Section 4.2. This test presents strong evidence ($p = 0.03$) against the hypothesis that my preferred estimate of the end of deer season $\hat{\tau} = -0.02$ could be produced from data in which deer season status did not actually change. Treating each of the 100 days in my preferred estimation bandwidth as a placebo end of deer season yields only two placebo estimates as large in magnitude as the estimator applied to real data. This result shows that the end of deer season has a uniquely large effect on firearm homicide fatalities in event time. It also demonstrates that my preferred estimate is unlikely to be an artifact from the estimation procedure, as applying the same procedure to data in which no effect is present yields few estimates of similar magnitude.

Panel C of Figure 6 conducts the second permutation test. This test provides strong evidence ($p \approx 0.01$) that random variation in mortality data could produce an estimate of the effect of the end of deer season as large in magnitude as my preferred estimate of $\hat{\tau} = -0.02$. When randomly permuting fatality outcomes used in estimation across days within a county-year—constructing placebo data with no correlation between deer season and fatalities—fewer than 2 percent of estimates are as large in magnitude as my preferred estimate.

These validation tests demonstrate that the change in firearm homicide fatalities at the end of deer season, as reflected by my estimator, likely captures a real change in the epidemiology of firearm violence. Appealing to the discussion of deer season in Section 3, I attribute this decrease in firearm homicide fatalities to the stricter regulations on gun use that prevail after deer season concludes.

The Online Appendix presents complementary robustness tests for fatalities from firearm-related accidents and suicides. The tests for accidental fatalities reflect a similar pattern of results, while results for self-harm fatalities are less robust. The lack of robustness follows from the poor performance of the regression model in Equation (1) when applied to self-harm fatalities.

5.3 Minimal spillover to other criminal incidents

This section considers whether the decrease in firearm homicide fatalities at the end of deer season is offset by a contemporaneous increase in other forms of injurious incidents. The magnitude of such spillovers in response to firearm regulation is a source of disagreement among experts in firearm policy (Smart et al. 2021). My setting, in which there are many changes in gun-use regulations available to study, offers a unique opportunity to analyze the force of these spillover effects.

Figure 7 shows that the end of deer season has no effect whatsoever on incidents of non-gun homicide and suicide fatalities. This result suggests that the decrease in firearm homicides at the end of deer season is a one-for-one decrease in homicides overall. These data suggest minimal spillover from gun to non-gun homicide in response to a tightening of gun use regulations.

Figure OA.6 considers spillovers between firearm mortality and other forms of criminal activity around the end of deer season. I find that non-fatal assaults—committed either with or without a firearm—rise during deer season and fall afterwards. This pattern suggests a *positive* spillover from firearm homicides to non-fatal assaults. A positive spillover would suggest that a homicide-reducing gun use regulation would also reduce non-fatal assaults.

5.4 Heterogeneity in homicide characteristics

This section uses the high resolution of the morgue records in the NVDRS to document patterns in the characteristics of firearm homicides around the end of deer season.

In analysis, I estimate a regression model analogous to Equation (1) with two changes in implementation. I condition this analysis on the set of firearm homicides occurring in the data. Thus, the number of observation is the 2,176 firearm homicides within the estimation bandwidth, rather than the total number of county-days. I also specify the dependent variable as a binary indicator equal to one if a firearm homicide is coded as having a specific characteristic, and zero otherwise. This analysis is thus a linear probability model, with the parameter τ reflecting the expected percentage-point change in the probability that a realized firearm homicide has a certain characteristic.

Table 3 documents several important dimensions of heterogeneity in the characteristics of firearm homicides at the end of deer season. Column 2 shows that the share of incidents that involve hunting fall sharply—almost to zero— at the end of deer season. This pattern further supports the causal interpretation of my estimates as reflecting changes in gun use at the end of deer season.

Column 3 shows that the share of firearm homicides with a female victim falls at the end of deer season. Notably, nearly 90 percent of hunters identify as male (U.S. Department of Interior et al. 2016) This pattern shows that non-hunters bear a burden from the increase in gun use created by laxer regulation during the deer hunting season.

Despite these two important patterns of heterogeneity, Table 3 documents minimal evidence of heterogeneity in the other characteristics of firearm homicides I investigate. This lack of evidence could reflect that the increase in firearm homicide at the end of deer season represents an increase in firearm homicides across the board. However, it could also reflect that the available data is not adequately powered to estimate heterogenous effects.

6 Magnitudes and policy implications

This section transforms my estimate of the effect of the end of deer season on fatalities $\hat{\tau}$ into economically meaningful magnitudes for interpretation. I focus this discussion around point estimates for firearm homicides, while Table OA.4 presents a full suite of estimates, standard errors, and additional details used in computation.

Table 4 considers the causal effect of the end of deer season on the level of firearm homicides in the average year in North Carolina and Virginia. I compute this quantity for a single county i as the product of my estimated causal effect with the county-specific daily standard deviation in firearm homicides $\hat{\tau} \times \sigma_i^l$. Summing this product across counties estimates the level decrease in firearm homicides at the end of deer season for the entire study area.

In the average year, the tightening of gun-use laws at the end of deer season saves 0.33 lives across North Carolina and Virginia, or 1 life every 3 years. This effect is comparable to the number of firearm homicides on the average day in Washington D.C. Assuming the entire causal effect operates through the population of deer hunters in the study area, the drop in homicide rates at the end of deer season is approximately equal to the difference in homicide rates between Washington D.C. and Baltimore MD.

Table 5 compares the decrease in firearm homicides at the end of deer season to the effects of other sources of firearm fatalities in the literature. To compare across studies, I transform the estimated effect of the end of deer season into a semi-elasticity by computing the level decrease in homicides within a county, scaling this decrease by the county's average daily homicide rate, and averaging this ratio across all counties in the study area. In the average county in North Carolina and Virginia, the end of deer season decreases firearm homicides by 45 percent of the daily average.

The estimated 45 percent decrease in firearm homicides at the end of deer season is large relative to the effects of existing firearm policies studied in the literature. For instance, Luca et al. (2017) find that, on average, a state's firearm homicide rate falls by 18 percent after it adopts a law mandating a waiting period between the purchase and physical acquisition of a firearm. Following a longstanding debate in the literature (e.g., Lott and Mustard 1997, National Research Council 2005), a recent study finds that concealed carry laws increase the firearm homicide rate by 15 percent (Donohue et al. 2019). The findings by Luca et al. (2017) and Donohue et al. (2019) speak directly to the impacts of firearm policies that have been implemented in the past, and are relevant for regulators who seek to design effective firearm policy. That these existing policies have a smaller effect on firearm homicides than the change in gun-use restrictions at the end of deer season suggests that regulators may be able to further decrease firearm fatalities by experimenting with the design and implementation of alternative firearm policies.

The estimated effect of the end of deer season on firearm homicide mortality also speaks to a debate about the effect of gun ownership on crime. Past studies have tended to find that when a proxy for gun ownership within an area increases, so too does the area's rate of violent crime (RAND 2018), with an elasticity for firearm homicides of around 0.3. My results imply that the laxer gun use regulations during deer season increase firearm homicides by 45 percent, with no change in gun ownership. To generate a comparable increase in firearm homicides, the proxy-ownership literature suggests that gun ownership would need to increase by .45/.3=150 percent. This highlights an important distinction between the consequences of firearm ownership and firearm use. It may be possible for regulators to decrease firearm violence by changing gun-use laws, without restricting firearm ownership at the individual level.

Evidence on the epidemiology of firearm violence underscores the power of gun-use regulations as a policy tool. Using household-level administrative data, Studdert et al. (2022) find that, among individuals who do not own a handgun, the probability of death by firearm homicide is 180 percent higher among individuals who reside with a handgun owner, relative to those that do not. This estimate suggests an important direct effect of firearm ownership on firearm homicides. The laxer gun-use restrictions during deer season increase the risk of firearm homicide by $.45/1.8 = 25$ percent of the effect of handgun exposure within a household. That the effect of deer season laws is so strong, relative to the myriad epidemiological pathways connecting firearm ownership to firearm homicide, further emphasizes the potential impact of well-specified gun-use regulation as a policy tool for decreasing firearm violence.

An important caveat to the policy implications above is that deer season is a once-per-year event. It is not possible to end deer season every day. Moreover, the results do not suggest that abolishing deer hunting would decrease firearm homicides by 45 percent, as this requires extrapolation away from the observed changes in hunting season status. Instead, the results suggest that changes in gun-use regulations—and their effects on firearm usage among gun owners—may drive changes in firearm homicides. This is a portable lesson that regulators may be able to use in designing firearm policy, even when the policy is unrelated to deer hunting.

7 Conclusion

This paper studies the impact of gun-use regulations on firearm fatalities. My approach exploits changes in local gun-use regulation around the end of deer season. Hunting season schedules are set to meet state game management objectives months in advance of the season's start, leaving the exact timing of deer season as a strong, exogenous shock to gun use, both in statute and in behavior on-the-ground. Averaging across 2,604 conclusions of county-year deer seasons in North Carolina and Virginia, I find that the change in gun-use regulations at the end of deer season reduces a county's firearm homicide fatalities by 45 percent of its daily average.

The tightening of gun-use regulation at the end of deer season produces a substantial decrease in firearm homicides, relative to the effects of overall firearm ownership. This pattern highlights a distinction between individual firearm ownership and firearm use. By directly modifying patterns of usage, deer season acts directly on the downstream behaviors related to firearm violence, without changing individual firearm ownership upstream. Policymakers may be able to modify gun ownership, for example through gun buybacks (Ramchand and Saunders 2021), but the results in this paper suggest that it may be possible to decrease firearm injuries by changing gun use. Other policies within this class include safe-storage laws and extreme risk protection orders (Smart et al. 2023)

The connection between gun-use regulations and firearm homicides documented in this paper may support the design of more efficient firearm policy. The end of deer season has a larger effect on firearm homicides than other commonly debated policies, including waiting periods and concealed carry laws. This suggests that the current slate of firearm policies up for public debate may not represent the best effects that a well-designed policy can achieve. Research and policy experimentation may yield new, more efficient firearm policies. The results in this paper suggest that gun-use regulations may be a promising direction for this additional work.

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9 Tables

Table 1: Summary of Study Area

Statistic	#Counties	Mean	SD	P0	P25	P50	P75	P100
Panel A: Hunting season days per year								
None	234	256.02	10.15	235.77	244.00	257.92	267.75	291.08
Bow	234	42.87	10.86	24.50	43.08	43.08	43.08	60.08
Muzzleloader	234	17.15	10.16	0.00	10.50	14.08	14.08	36.23
Gun	234	45.99	20.83	17.00	20.42	50.15	50.17	78.75
Panel B: Fatalities per year								
Gun homicide	234	2.84	6.20	0.00	0.38	0.84	2.50	54.17
Gun suicide	234	5.12	5.83	0.17	1.62	3.44	6.38	40.50
Gun accident	234	0.17	0.21	0.00	0.00	0.08	0.25	1.17
Non-gun homicide	234	0.98	1.63	0.00	0.15	0.38	1.16	12.08
Non-gun suicide	234	3.65	5.85	0.00	0.77	1.76	3.86	48.00
Panel C: Daily fatality standard deviations								
Gun homicide	234	0.07	0.07	0.00	0.03	0.05	0.09	0.42
Gun suicide	234	0.11	0.05	0.02	0.07	0.10	0.13	0.34
Gun accident	234	0.02	0.01	0.00	0.00	0.02	0.03	0.06
Non-gun homicide	234	0.04	0.03	0.00	0.02	0.04	0.06	0.19
Non-gun suicide	234	0.08	0.06	0.00	0.05	0.07	0.10	0.37

Table presents summary statistics across counties. Panel A summarizes the number of days each county spends in different hunting seasons in the average year. Panel B summarizes fatalities in each county during the average year. Panel C summarizes the daily standard deviation in fatality counts within each county.

Table 2: Effect of Deer Season Ending on Firearm Fatalities

	Firearm Fatalitiy			
	Homicide	Accident	Suicide	All
	(1)	(2)	(3)	(4)
Intercept α	0.018	0.035	-0.014	0.004
	(0.006)	(0.009)	(0.005)	(0.005)
Days β	0.001	0.001	-0.0002	0.0002
	(0.0002)	(0.0003)	(0.0001)	(0.0001)
Post τ	-0.019	-0.037	0.013	-0.006
	(0.008)	(0.012)	(0.006)	(0.007)
Days$\times$Post γ	-0.001	-0.001	0.00005	-0.0004
	(0.0002)	(0.0004)	(0.0002)	(0.0002)
Constant-only (p)	0.00375	4.73e-05	0.154	0.0307
Clusters	101	101	101	101
Observations	279,392	198,717	289,502	289,502

Table presents estimates of Equation (1), summarizing the change in standardized firearm fatalities at the end of deer season under the default estimation bandwidth of $b = 50$ days around the season's end. Observations are the number of county-days used in estimation. Standard errors in parentheses are adjusted for clustering by state and date of season change. Constant-only presents the p-value from a cluster-robust Wald test of the joint hypothesis that all coefficients in a column, other than the intercept, are equal to zero. Columns ordered according to the value of the Contant-only statistic.

Table 3: Heterogeneity in Firearm Homicides at the End of Deer Season

	Firearm Fatalitiy Characteristic										
	White suspect	Hunting	Female injured	Alcohol	White injured	Intimate partner	At home	Male suspect	Handgun	Injured in home county	Family member
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Intercept α	0.205	0.017	0.225	0.251	0.249	0.177	0.328	0.752	0.118	0.869	0.016
	(0.023)	(0.006)	(0.021)	(0.038)	(0.025)	(0.020)	(0.026)	(0.031)	(0.033)	(0.022)	(0.009)
Days β	0.001	0.0003	0.002	0.001	−0.001	0.001	0.0003	0.001	−0.0001	0.001	0.0002
	(0.001)	(0.0002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0003)
Post τ	0.012	−0.014	−0.058	−0.050	0.058	−0.021	0.023	−0.045	0.019	−0.015	−0.007
	(0.043)	(0.007)	(0.032)	(0.037)	(0.043)	(0.032)	(0.037)	(0.039)	(0.038)	(0.028)	(0.009)
Days×Post γ	−0.001	−0.0005	−0.001	0.001	0.001	−0.0004	0.0003	0.0004	0.001	−0.001	−0.0002
	(0.001)	(0.0002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.0003)
Constant-only (p)	0.00879	0.0115	0.0268	0.0404	0.0517	0.0569	0.128	0.491	0.501	0.625	0.757
Clusters	94	94	94	94	94	94	94	94	94	94	94
Observations	2,176	2,176	2,176	2,176	2,176	2,176	2,176	2,176	2,176	2,171	2,176

Table presents estimates of Equation (1) as a linear probability model, summarizing the percentage point change in the share of firearm homicides with a characteristic at the end of deer season under the default estimation bandwidth of $b = 50$ days around the season's end. Observations are the number of firearm homicides used in estimation. Standard errors in parentheses are adjusted for clustering by state and date of season change. Constant-only presents the p-value from a cluster-robust Wald test of the joint hypothesis that all coefficients in a column, other than the intercept, are equal to zero.

Table 4: Level of Firearm Homicides at the End of Deer Season and in Surrounding Cities

Geography	Daily gun homicides	Population (1k)	Gun homicides per 100k per year
End of deer season	**-0.33**	**410**	**-29.4**
Washington D.C.	0.29	670	15.97
Baltimore MD	0.84	621	49.64
Charlotte NC	0.17	1033	6.01

Table shows daily firearm homicide fatalities at the end of deer season in North Carolina and Virginia and in three related geographies. The first row presents an estimate of the number of lives saved each year on the last day of deer season and the number of deer hunters, described in Sections 6 and 3, respectively. Remaining rows present the daily average number of gun-related assault fatalities in each city and the city's population. Both are computed for 2015. Fatalities are computed using morgue records from NAPHSIS. Population is reported from SEER. Estimates for Charlotte N.C. are those for Mecklenberg county.

Table 5: Elasticity of Firearm Homicides to the End of Deer Season and in Comparable Literature

Paper	Variation	Percent change
Studdert et al. (2022)	Handgun ownership by cohabitant of non-owner	+183
This paper	**End of deer season**	**-45**
Luca et al. (2017)	Waiting periods for gun pickup	-18
Donohue et al. (2019)	Concealed carry law	+15
Depetris-Chauvin (2015)	Low gun demand after Obama Election	-8
Duggan (2001)	10% decrease Gun and Ammo Circulation	-3
Koenig and Schindler (2023)	Waiting periods after Sandy Hook	-2
Cook and Ludwig (2006)	10% decrease FS/S	-2
Duggan et al. (2011)	Gun show within 10 miles last month	+1

Table shows elasticity of firearm homicide fatalities to the end of deer season and in comparable literature. The table is not a comprehensive review of the literature, and further estimates may be found in Smart et al. (2023).

10 Figures

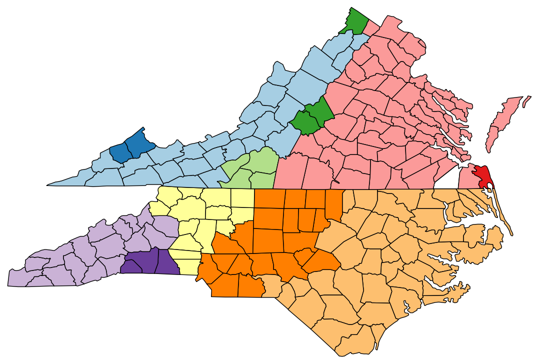

Figure 1: Spatial Distribution of Deer Season Clusters

Figure shows spatial distribution of deer season schedules in 2015. Counties are colored based on the start and end dates of deer season.

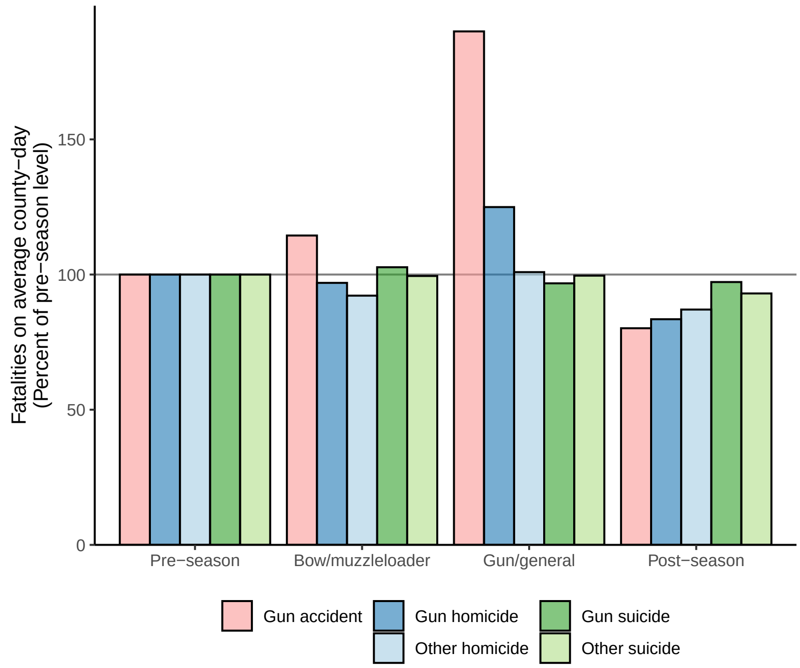

Figure 2: Percent change in Fatalities around Deer Season

Figure presents percent changes in fatalities across county days by hunting season status and cause of death. Horizontal axis is hunting season type. Pre-season includes the 90 days prior to the start of the bow hunting season. Post-season includes the 90 days following the end of the gun hunting season. Vertical axis is fatalities on the average county day, relative to the pre-season level.

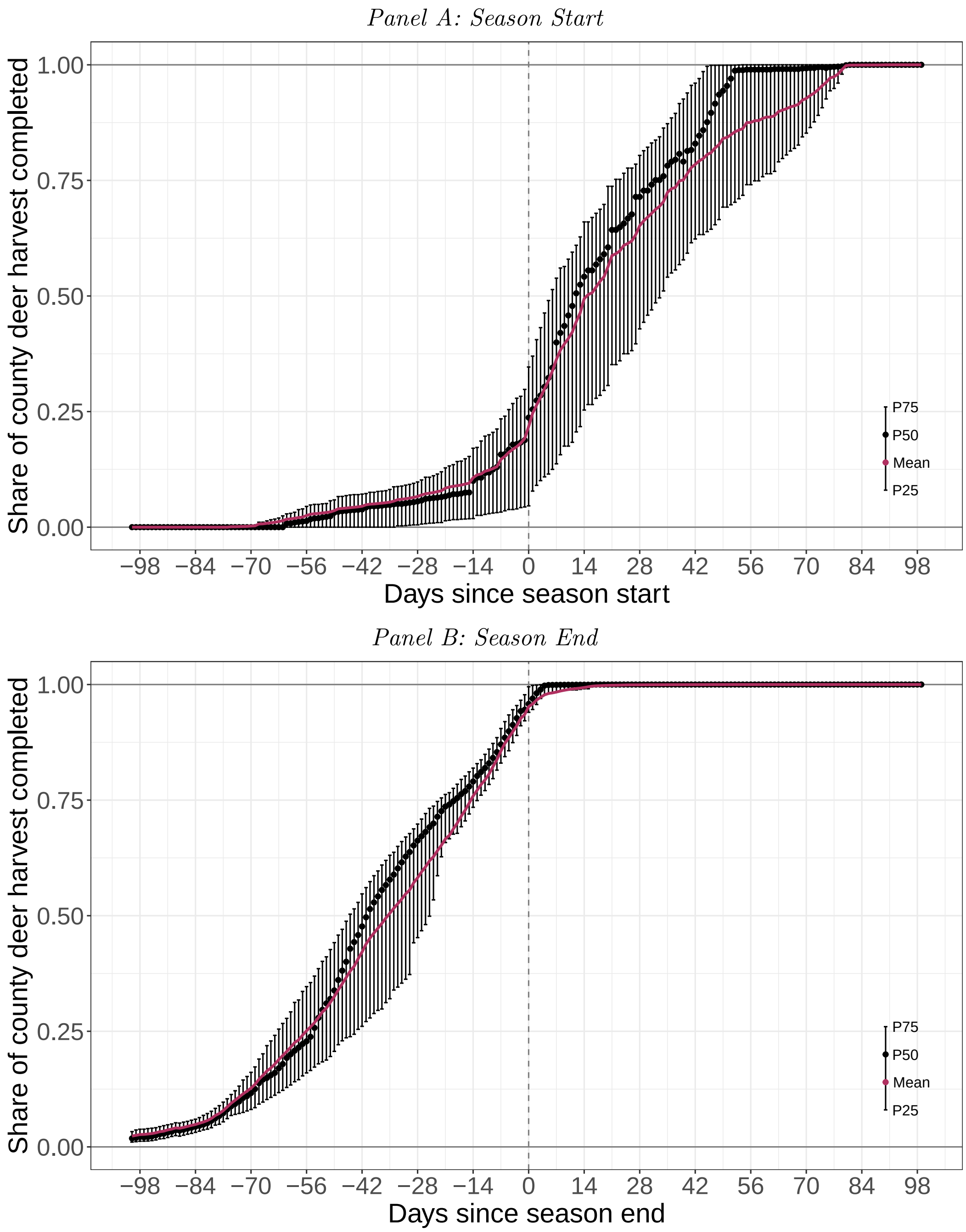

Figure 3: Timing of Harvest within Deer Season

Figure shows daily relationship between deer season and deer harvest in North Carolina. Panel A presents relationship centered around the start of the gun deer hunting season. Panel B presents relationship centered around season end. Statistics—mean, median, and inter-quartile range—computed across county-years for each day of event time.

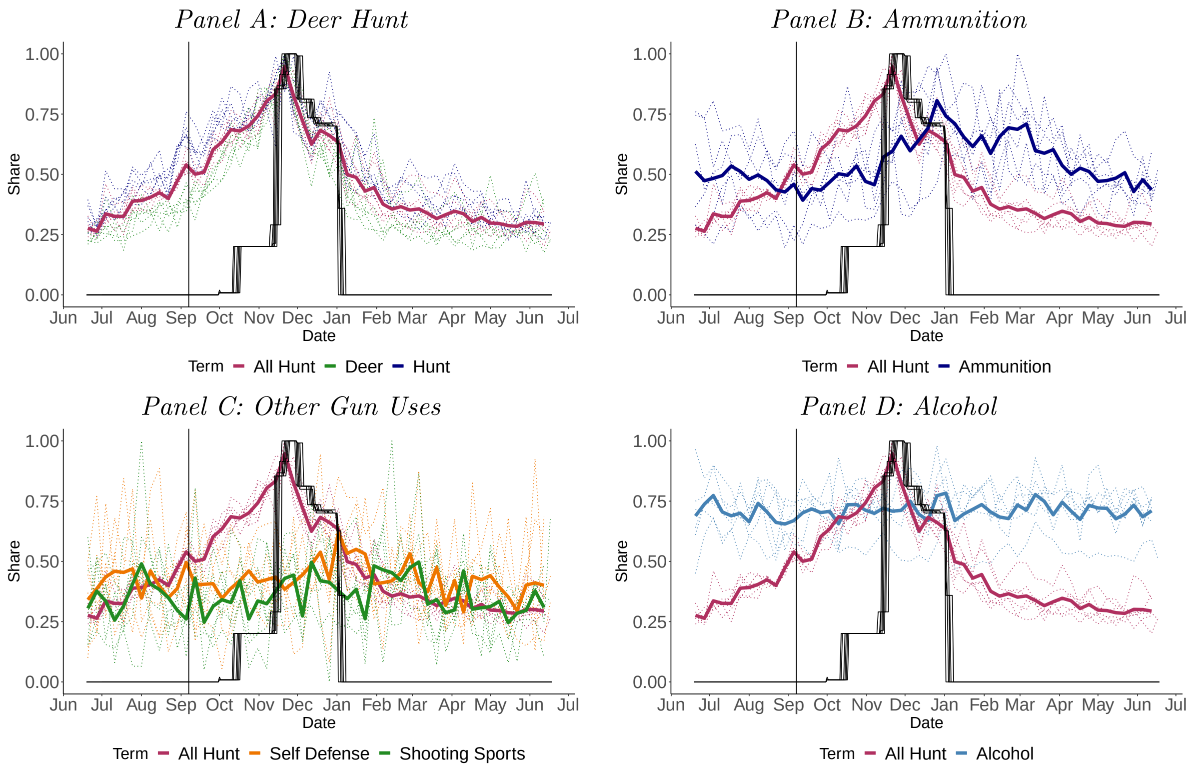

Figure 4: Dynamics of Deer Season and Google Search Interest

Figure shows weekly relationship between deer season and Google Trends results queried for North Carolina and Virginia during the last six years of the primary dataset. Panel A presents results for hunting and deer. Panel B presents results for ammunition. Panel C presents results for sport shooting and self defense. Panel D presents results for alcoholic drinks. Horizontal axis is aggregated to the week level. Vertical axis is both the weekly value of Google Trends search interest as a share of the maximal search for the query year or the share of counties currently in deer season. Dotted lines are yearly time series for the last six years of the primary dataset averaged across states. Solid lines are averages of the year-specific dotted lines, the maroon line is constant in all panels. Averaging assigns equal weight to each state and year. Solid black lines represent the share of counties in the gun deer season during each week. The vertical black line is the earliest start of the bow deer hunting season.

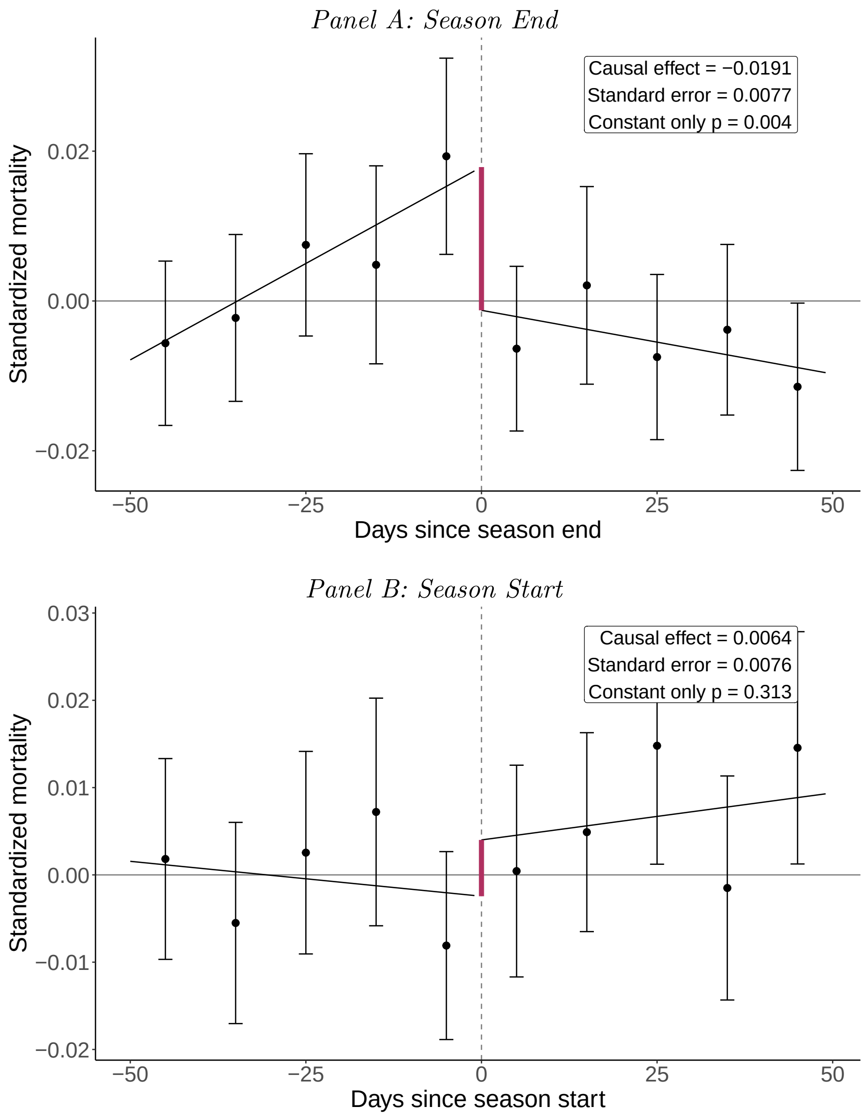

Figure 5: Change in Firearm Homicides before and after Deer Season

Figure shows expected gun fatalities expressed in county standard deviations and estimated from Equation (1). Causal effect is the estimate of τ, summarizing the average change in gun mortality at the season's change. Standard error is the standard error of $\hat{\tau}$, accounting for clustering by state and season end date. Constant only is the p-value from a cluster-robust test of whether the estimated regression is statistically different from the null intercept-only model. Panel A presents results for the end of hunting season. Panel B presents results for the beginning of hunting season. Lines are estimated conditional expectations from Equation (1) with a bandwidth of 50 days. Circular black points are means of observed outcomes aggregated across county-years and 10-day bins, with 95 percent confidence intervals for the means based on the normal approximation without accounting for clustering or serial correlation.

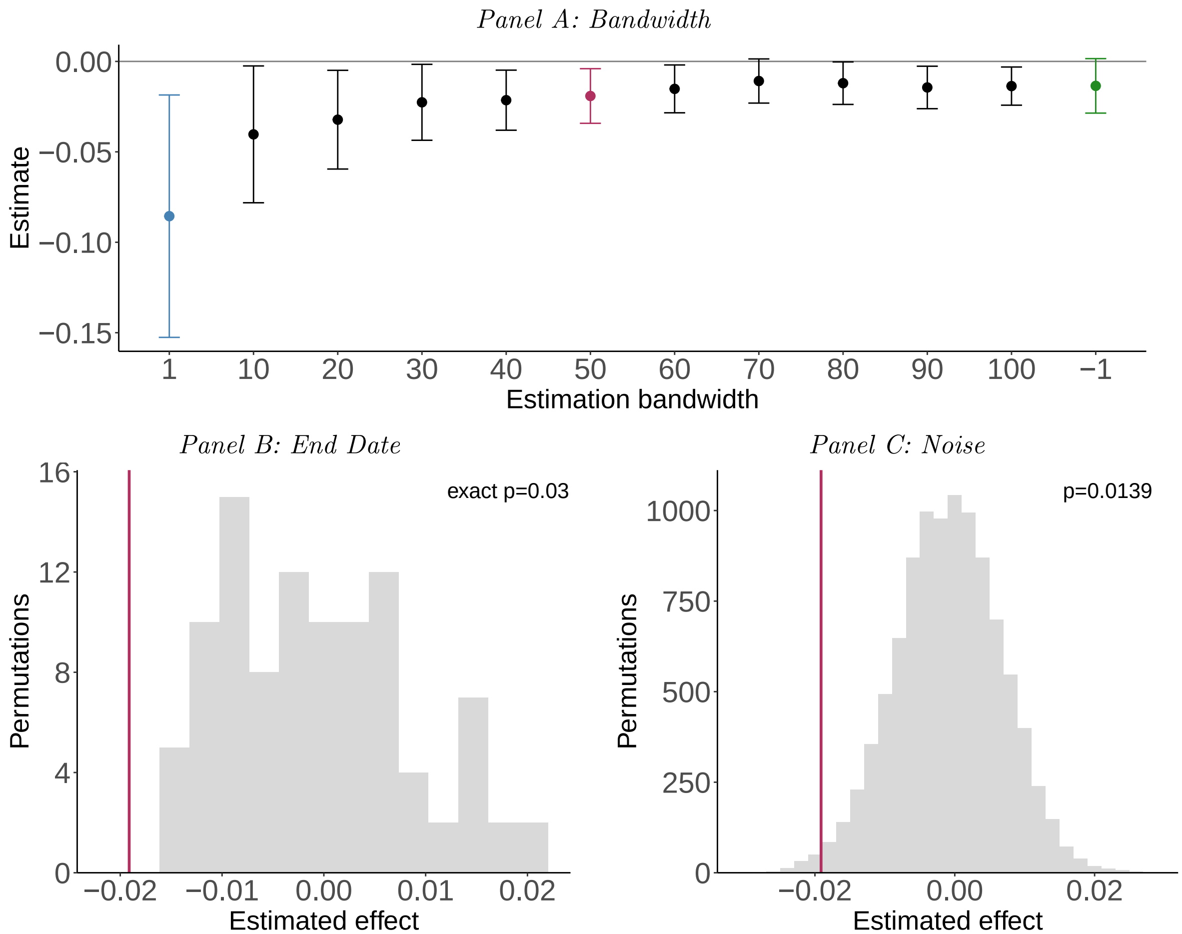

Figure 6: Robustness of Deer Season End on Gun Assault Mortality

Figure shows robustness of the drop in mortality from gun-related assaults at the conclusion of hunting season in Column 3 of Table 2. Panel A estimates the effect of the conclusion of deer season on gun accidents under different estimation bandwidths around the end of deer season. The bandwidth of -1 estimates a donut regression, where the estimation sample is formed from the preferred bandwidth of 50 days, excluding the last day of deer season and the first day after the season. 95-percent confidence intervals account for clustering by the interaction of state, year, and hunting season end-date. Panel B simulates the distribution of estimates $\hat{\tau}$ for alternative deer season end dates according to the first permutation test in Section 4.2. Panel C simulates the distribution of estimates of $\hat{\tau}$ when mortality is uncorrelated the change in deer season according to the second permutation test in Section 4.2. In both tests, the red vertical line is the estimate on real data, the gray histogram is the distribution of the estimator on fake data in which there is no effect under the null hypothesis, and the estimation bandwidth is $b = 50$ days.

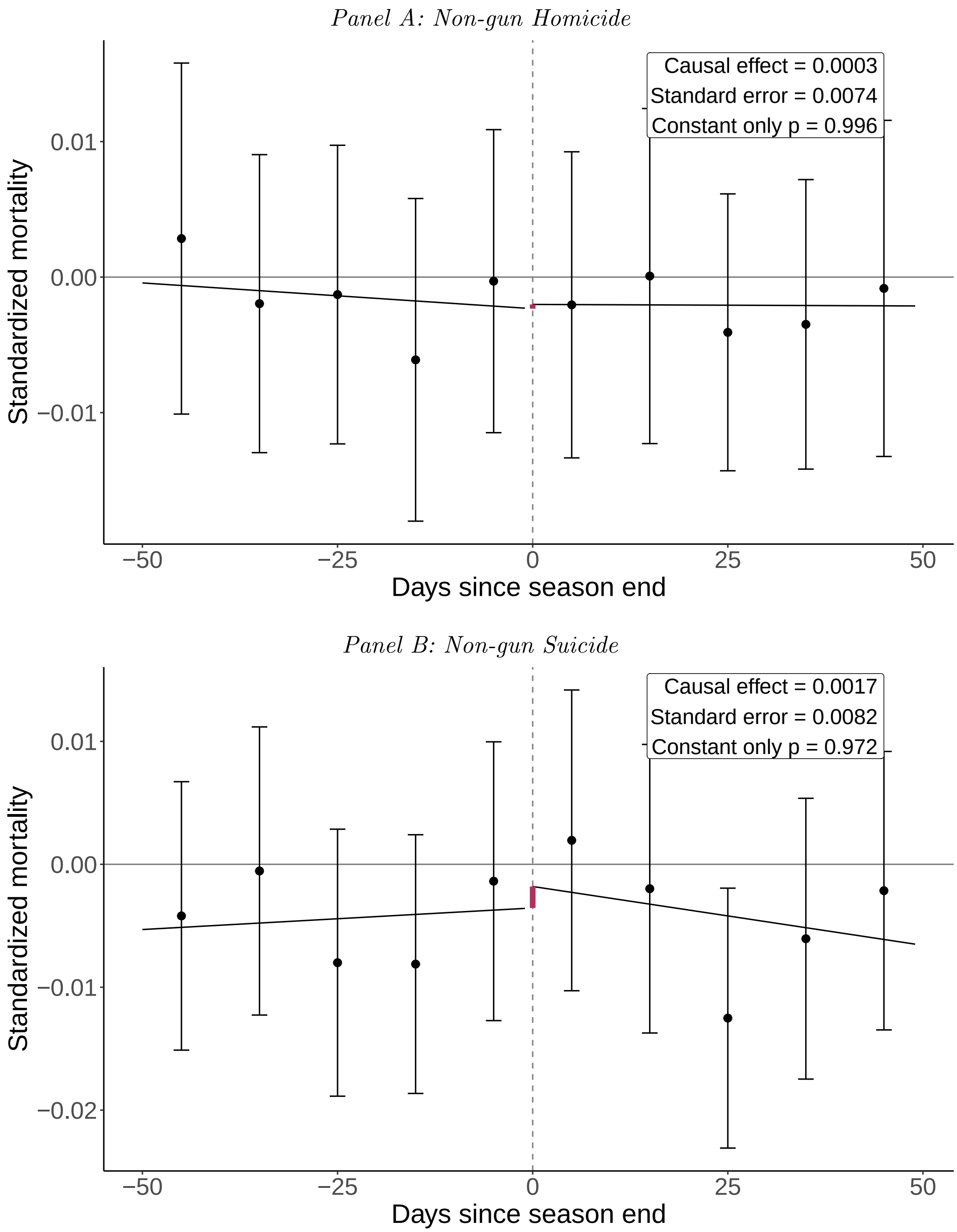

Figure 7: Change in Non-gun Fatalities at the end of Deer Season

Figure shows expected gun fatalities expressed in county standard deviations and estimated from Equation (1). Causal effect is the estimate of τ, summarizing the average change in non-gun mortality at the season's change. Standard error is the standard error of $\hat{\tau}$, accounting for clustering by state and season end date. Constant only is the p-value from a cluster-robust test of whether the estimated regression is statistically different from the null intercept-only model. Panel A presents results for the end of hunting season. Panel B presents results for the beginning of hunting season. Lines are estimated conditional expectations from Equation (1) with a bandwidth of 50 days. Circular black points are means of observed outcomes aggregated across county-years and 10-day bins, with 95 percent confidence intervals for the means based on the normal approximation without accounting for clustering or serial correlation.

Online Appendix to

Regulating Local Markets for Consumer Firearms: Estimates of Preferences and Externalities in California

Adam M. Rosenberg, *Stanford University*[12]

January 26, 2024

OA.1 SUTVA, deer hunter behavior, and cross-county spillovers

As described in Section 4, the research design requires a Stable Unit Treatment Value Assumption ("SUTVA") that separates mortality in a county-day from the deer season status of all other counties. This is a mild assumption that accommodates many plausible behaviors from hunters on the ground. Individual hunters may travel across counties to hunt without violating this assumption because their decisions are aggregated into each county-day's overall outcomes. The assumption thus accommodates for the possibility that individuals travel from urban to rural areas to hunt, travel across rural areas to hunt in a favorite location, or even spend different periods hunting in different counties with different deer seasons. My estimator can also accommodate certain violations of SUTVA, since groups of counties tend to enter and exit deer seasons simultaneously, as in Figure 1, and I cluster my standard errors to adjust for potential spillovers across these groups.

Violations of SUTVA thus require a link between a county's deer season schedule, its mortality outcomes, and the deer season schedules of counties in a different state-season cluster. Such a violation would arise in the following setting. Suppose county A's deer season ends before county B's, and that after the end of A's season, all its local hunters travel to B to continue hunting. Rather than having only two treatments—pre and post deer season's end—this type of interdependence creates three treatments. These are A and B both in season, A out of season but B in season, and A and B both out of season.

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In the above setting, the observed change in mortality at the end of deer season in county A may not represent the full decrease in mortality from ending hunting season altogether. This is because A's hunter's remain active even after the end of their own deer season. The label "post" mis-measures deer season status on the ground. If having a local population participate in hunting activities increases mortality, as my estimates and discussion of mechanisms suggest, then this mis-measurement will lead my estimate to understate the full effect of hunting season ending altogether.

The converse effect may hold in county B, leading me to overestimate the effect of the end of deer season in this location. This is because A's hunter's participate in the end of county B's season. If having these additional hunters increases mortality in B beyond what would be created by their local hunters, then B's mortality at the end of deer season will be inflated. Comparing B's mortality just before and after the end of deer season would then overstate my estimates of the effect of the season's end.

Violations of SUTVA thus create spillovers across county seasons, and these spillovers' net effect is an empirical question. I evaluate the potential for the type of spillover in the example above in Figure OA.7. I find that earlier-ending deer season's tend to have decreases in firearm homicide fatalities at their season ends similar to deer seasons ending later. This suggests that my estimates are driven by the end of deer season *per se*, not by spillovers linking earlier- and later-ending deer seasons.

Of course, SUTVA is an assumption on the joint assignment deer seasons and mortality outcomes, not just the setting discussed above. This section provides evidence in favor of SUTVA in a context in which its violation is intuitive to consider. Although it is not possible to test every potential violation of this assumption, the evidence in this sections suggests that spillover generated through violations of SUTVA are likely to induce little bias in my estimates.

OA.2 Appendix Tables

Table OA.1: Google Trends Query terms

String	Type
Deer	Animal
Hunting	Topic
Ammunition	Topic
Self defense	Topic
Shooting	Sport
Alcoholic drink	Topic

Table presents exact string and string types used to query Google Trends.

Table OA.2: Main Results, More Controls

	Firearm Fatalitiy			
	Homicide	Accident	Suicide	All
	(1)	(2)	(3)	(4)
Days β	0.0004	0.0004	-0.0003	0.0001
	(0.0002)	(0.0003)	(0.0001)	(0.0001)
Post τ	-0.015	-0.032	0.014	-0.001
	(0.008)	(0.013)	(0.006)	(0.007)
Days×Post γ	-0.001	-0.001	0.0001	-0.0003
	(0.0002)	(0.0004)	(0.0002)	(0.0002)
Constant-only (p)	0.00865	0.00134	0.132	0.132
County×Year FE	Y	Y	Y	Y
Day of Week FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Clusters	101	101	101	101
Observations	279,392	198,717	289,502	289,502

Table presents estimates of an extension to Equation 1 with more controls, summarizing the change in standardized firearm fatalities at the end of deer season under the default estimation bandwidth of $b = 50$ days around the season's end. Additional controls are fixed effects for county×year, fixed effects for day of week, and fixed effects for holidays (Christmas Eve, Christmas Day, New Year's Eve, and New Year's Day). Observations are the number of county-days used in estimation. Standard errors in parentheses are adjusted for clustering by state and date of season change. Constant-only presents the p-value from a cluster-robust Wald test of the joint hypothesis that all coefficients in a column are equal to zero.

Table OA.3: Season start, main results

	Firearm Fatalitiy			
	Homicide	Accident	Suicide	All
	(1)	(2)	(3)	(4)
Intercept α	-0.002	0.008	-0.002	-0.0004
	(0.007)	(0.006)	(0.006)	(0.005)
Days β	-0.0001	0.0003	-0.00000	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Post τ	0.006	0.013	-0.005	0.0003
	(0.008)	(0.010)	(0.007)	(0.007)
Days$\times$Post γ	0.0002	-0.001	-0.0001	-0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Constant-only (p)	0.313	0.000686	0.143	0.952
Clusters	79	79	79	79
Observations	284,061	202,562	294,285	294,285

Table presents estimates of Equation 1, summarizing the change in standardized firearm fatalities at the start of deer season under the default estimation bandwidth of $b = 50$ days around the season's end. Observations are the number of county-days used in estimation. Standard errors in parentheses are adjusted for clustering by state and date of season change. Constant-only presents the p-value from a cluster-robust Wald test of the joint hypothesis that all coefficients in a column, other than the intercept, are equal to zero.

Table OA.4: Magnitude of Decrease in Gun Mortality at Deer Season End

Gun Fatality	Point Estimate	Lives Saved	Average Percent Decrease
Homicide	-0.019	0.33	45
	(0.008)	(0.13)	(18)
Accident	-0.037	0.14	121
	(0.012)	(0.05)	(39)
All	-0.006	0.2	7
	(0.007)	(0.21)	(7)

Table shows magnitude of decrease in gun mortality at the end of deer season. The leftmost columns specifies the cause of gun mortality. The point estimate is τ from model (1). Standard errors for the point estimate are clustered by the interaction of state, year, and hunting season end-date. Standard errors in subsequent columns are computed using the continuous mapping theorem. The lives saved columns estimates the single-day count of lives saved by the end deer season for all counties in North Carolina and Virginia in the average year

$$\textit{Lives Saved} = \sum_i \tau \sigma_i^l.$$

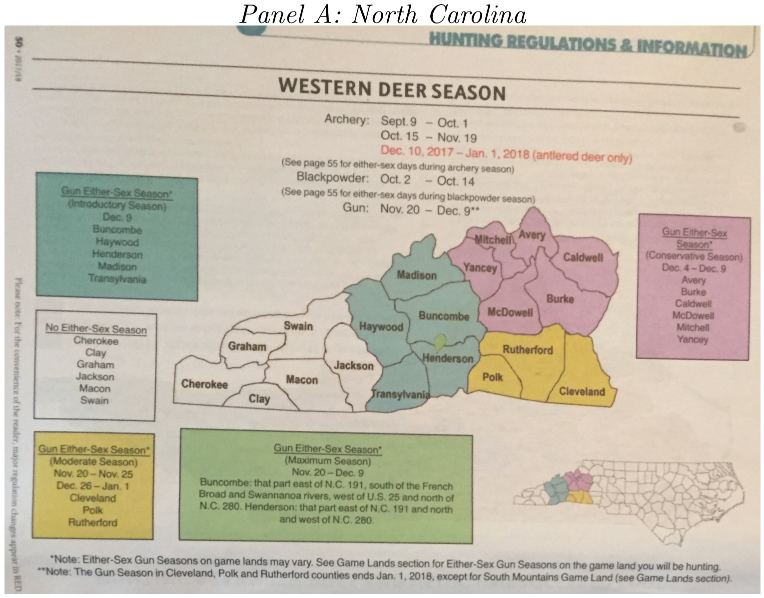
The average percent decrease estimates the decrease in gun fatalities in a county as a percent of the county's daily average gun fatalities, and averages this quantity over counties

$$\textit{Average Percent Decrease} = 100 \times \frac{1}{\sum_i 1} \sum_i \tau \frac{\sigma_i^l}{\mu_i^l},$$

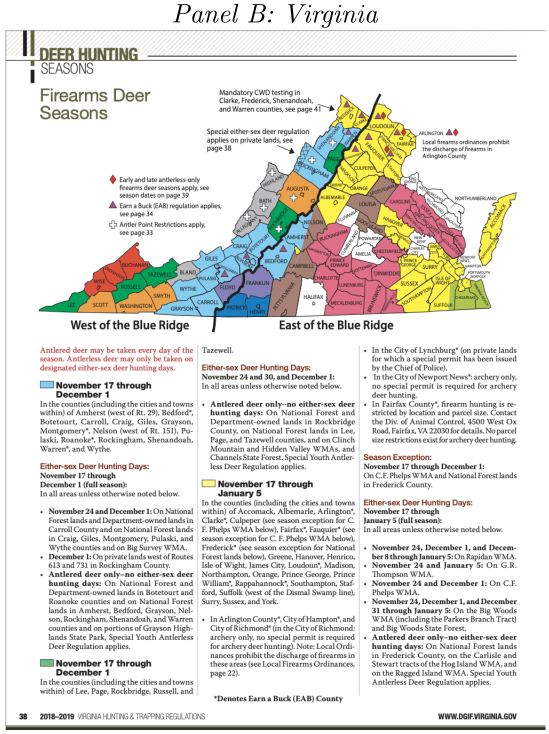
where $\frac{\sigma_i^l}{\mu_i^l}$ is the coefficient of variation of the observed daily level of fatalities for county i. I impute this quantity as zero when the estimated ratio is undefined.

OA.3 Appendix Figures

Panel A: North Carolina



Panel B: Virginia


DEER HUNTING SEASONS

Firearms Deer Seasons

Antlered deer may be taken every day of the season. Antlerless deer may only be taken on designated either-sex deer hunting days.

November 17 through December 1

In the counties (including the cities and towns within) of Amherst (west of Rt. 29), Bedford*, Botetourt, Carroll, Craig, Giles, Grayson, Montgomery*, Nelson (west of Rt. 151), Pulaski, Roanoke*, Rockingham, Shenandoah, Warren*, and Wythe.

Either-sex Deer Hunting Days:
November 17 through December 1 (full season):
In all areas unless otherwise noted below.

- **November 24 and December 1:** On National Forest lands and Department-owned lands in Carroll County and on National Forest lands in Craig, Giles, Montgomery, Pulaski, and Wythe counties and on Big Survey WMA.
- **December 1:** On private lands west of Routes 613 and 731 in Rockingham County.
- **Antlered deer only–no either-sex deer hunting days:** On National Forest and Department-owned lands in Botetourt and Roanoke counties and on National Forest lands in Amherst, Bedford, Grayson, Nelson, Rockingham, Shenandoah, and Warren counties and on portions of Grayson Highlands State Park, Special Youth Antlerless Deer Regulation applies.

November 17 through December 1

In the counties (including the cities and towns within) of Lee, Page, Rockbridge, Russell, and Tazewell.

Either-sex Deer Hunting Days:
November 24 and 30, and December 1:
In all areas unless otherwise noted below.

- **Antlered deer only–no either-sex deer hunting days:** On National Forest and Department-owned lands in Rockbridge County, on National Forest lands in Lee, Page, and Tazewell counties, and on Clinch Mountain and Hidden Valley WMAs, and Channels State Forest. Special Youth Antlerless Deer Regulation applies.

November 17 through January 5

In the counties (including the cities and towns within) of Accomack, Albemarle, Arlington*, Clarke*, Culpeper (see season exception for C. F. Phelps WMA below), Fairfax*, Fauquier* (see season exception for C. F. Phelps WMA below), Frederick* (see season exception for National Forest lands below), Greene, Hanover, Henrico, Isle of Wight, James City, Loudoun*, Madison, Northampton, Orange, Prince George, Prince William*, Rappahannock*, Southampton, Stafford, Suffolk (west of the Dismal Swamp line), Surry, Sussex, and York.

- In Arlington County*, City of Hampton*, and City of Richmond* (in the City of Richmond: archery only, no special permit is required for archery deer hunting). Note: Local Ordinances prohibit the discharge of firearms in these areas (see Local Firearms Ordinances, page 22).
- In the City of Lynchburg* (on private lands for which a special permit has been issued by the Chief of Police).
- In the City of Newport News*: archery only, no special permit is required for archery deer hunting.
- In Fairfax County*, firearm hunting is restricted by location and parcel size. Contact the Div. of Animal Control, 4500 West Ox Road, Fairfax, VA 22030 for details. No parcel size restrictions exist for archery deer hunting.

Season Exception:
November 17 through December 1:
On C.F. Phelps WMA and National Forest lands in Frederick County.

Either-sex Deer Hunting Days:
November 17 through January 5 (full season):
In all areas unless otherwise noted below.

- **November 24, December 1, and December 8 through January 5:** On Rapidan WMA.
- **November 24 and January 5:** On G.R. Thompson WMA.
- **November 24 and December 1:** On C.F. Phelps WMA.
- **November 24, December 1, and December 31 through January 5:** On the Big Woods WMA (including the Parkers Branch Tract) and Big Woods State Forest.
- **Antlered deer only–no either-sex deer hunting days:** On National Forest lands in Frederick County, on the Carlisle and Stewart tracts of the Hog Island WMA, and on the Ragged Island WMA. Special Youth Antlerless Deer Regulation applies.

*Denotes Earn a Buck (EAB) County

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Figure OA.1: Raw Input Data on Deer Season

Figure shows an example of raw input data for on deer hunting season dates for North Carolina and Virginia.

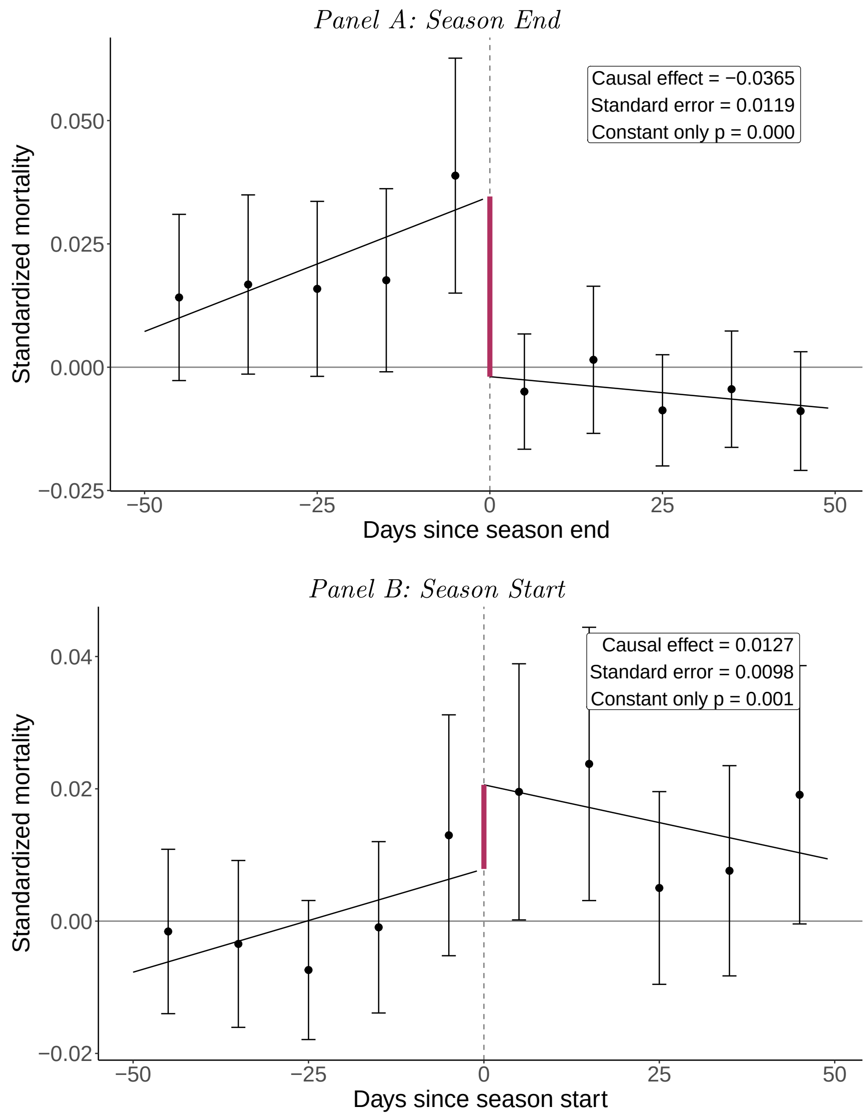

Figure OA.2: Change in Firearm Accident Fatalities before and after Deer Season

Figure shows expected gun fatalities expressed in county standard deviations and estimated from Equation (1). Causal effect is the estimate of τ, summarizing the average change in gun mortality at the season's change. Standard error is the standard error of $\hat{\tau}$, accounting for clustering by state and season end date. Constant only is the p-value from a cluster-robust test of whether the estimated regression is statistically different from the null intercept-only model. Panel A presents results for the end of hunting season. Panel B presents results for the beginning of hunting season. Lines are estimated conditional expectations from Equation (1) with a bandwidth of 50 days. Circular black points are means of observed outcomes aggregated across county-years and 10-day bins, with 95 percent confidence intervals for the means based on the normal approximation without accounting for clustering or serial correlation.

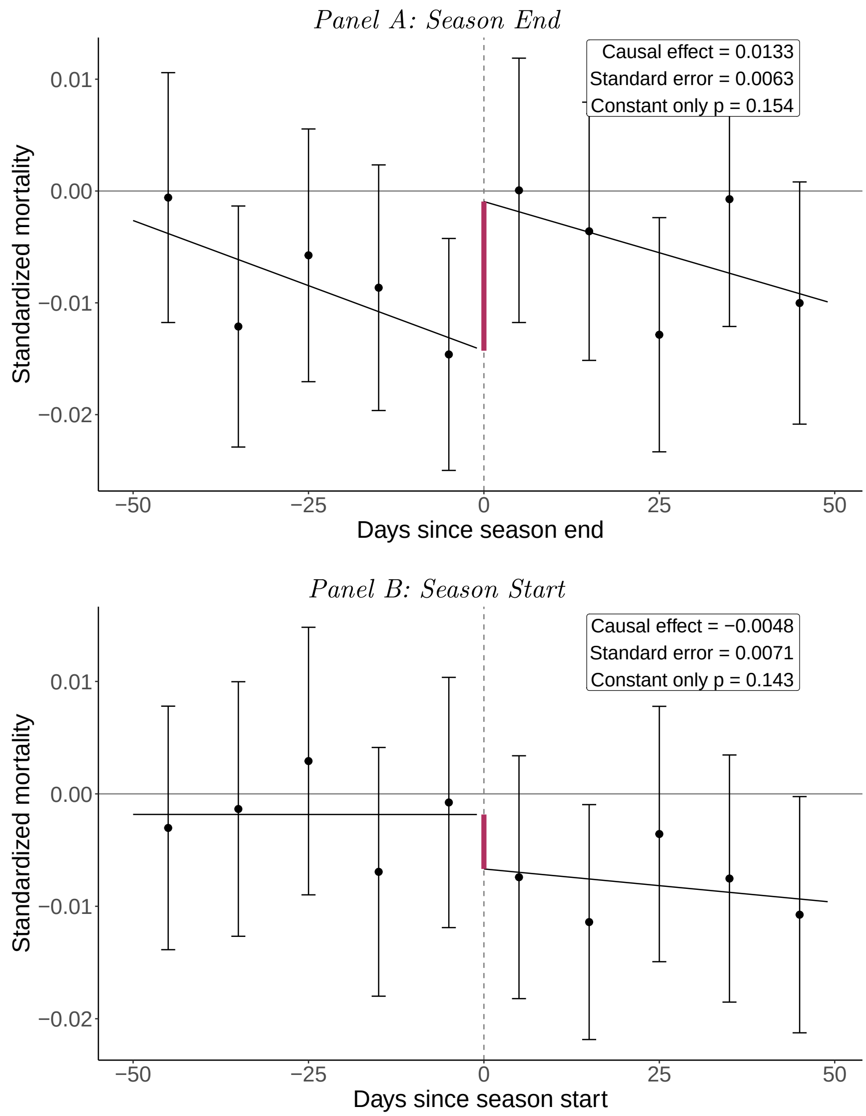

Figure OA.3: Change in Firearm Suicides before and after Deer Season

Figure shows expected gun fatalities expressed in county standard deviations and estimated from Equation (1). Causal effect is the estimate of τ, summarizing the average change in gun mortality at the season's change. Standard error is the standard error of $\hat{\tau}$, accounting for clustering by state and season end date. Constant only is the p-value from a cluster-robust test of whether the estimated regression is statistically different from the null intercept-only model. Panel A presents results for the end of hunting season. Panel B presents results for the beginning of hunting season. Lines are estimated conditional expectations from Equation (1) with a bandwidth of 50 days. Circular black points are means of observed outcomes aggregated across county-years and 10-day bins, with 95 percent confidence intervals for the means based on the normal approximation without accounting for clustering or serial correlation.

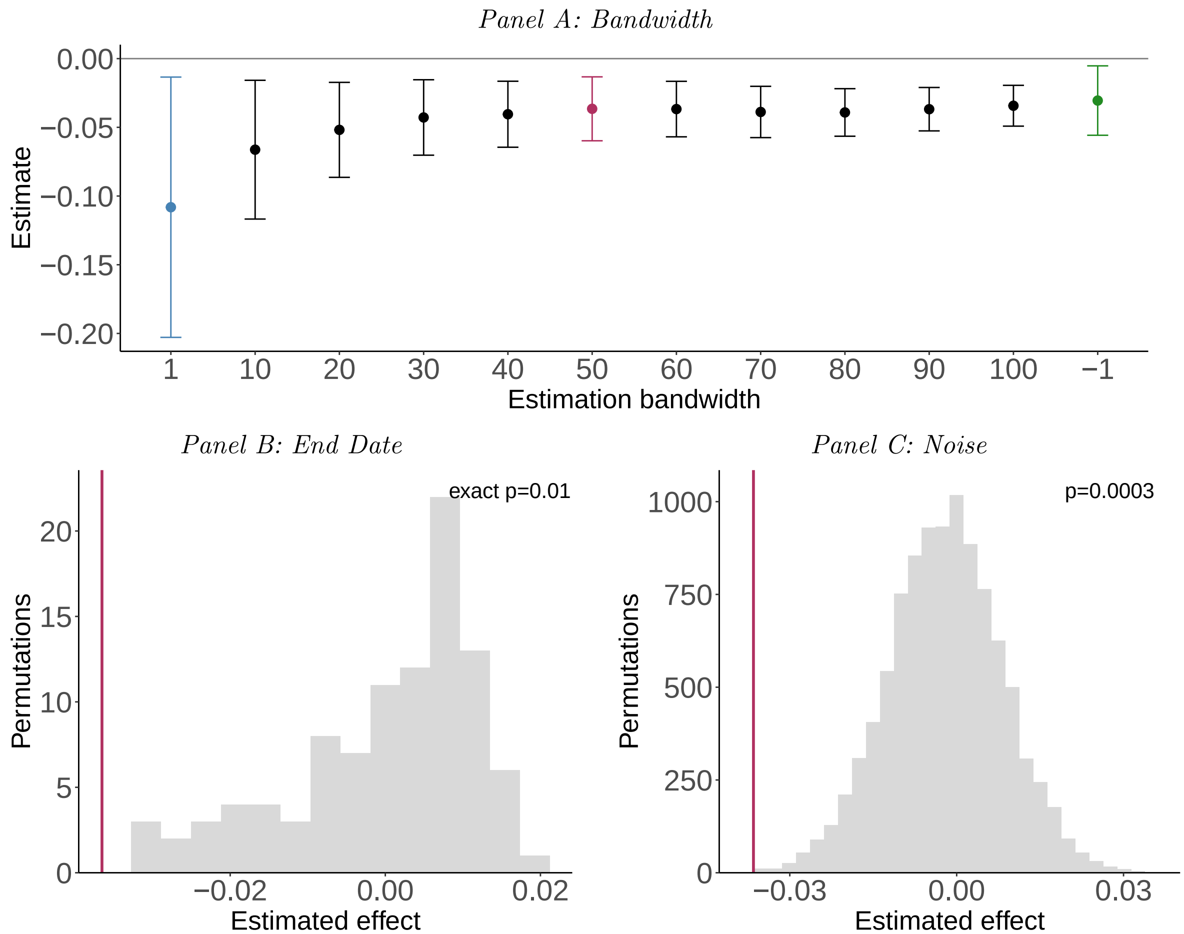

Figure OA.4: Robustness of Deer Season End on Gun Accident Mortality

Figure shows robustness of the drop in mortality from gun-related accidents at the conclusion of hunting season in Column 2 of Table 2. Panel A estimates the effect of the conclusion of deer season on gun accidents under different estimation bandwidths around the end of deer season. The bandwidth of -1 estimates a donut regression, where the estimation sample is formed from the preferred bandwidth of 50 days, excluding the last day of deer season and the first day after the season. 95-percent confidence intervals account for clustering by the interaction of state, year, and hunting season end-date. Panel B simulates the distribution of estimates $\hat{\tau}$ for alternative deer season end dates according to the first permutation test in Section 4.2. Panel C simulates the distribution of estimates of $\hat{\tau}$ when mortality is uncorrelated the change in deer season according to the second permutation test in Section 4.2. In both tests, the red vertical line is the estimate on real data, the gray histogram is the distribution of the estimator on fake data in which there is no effect under the null hypothesis, and the estimation bandwidth is $b = 50$ days.

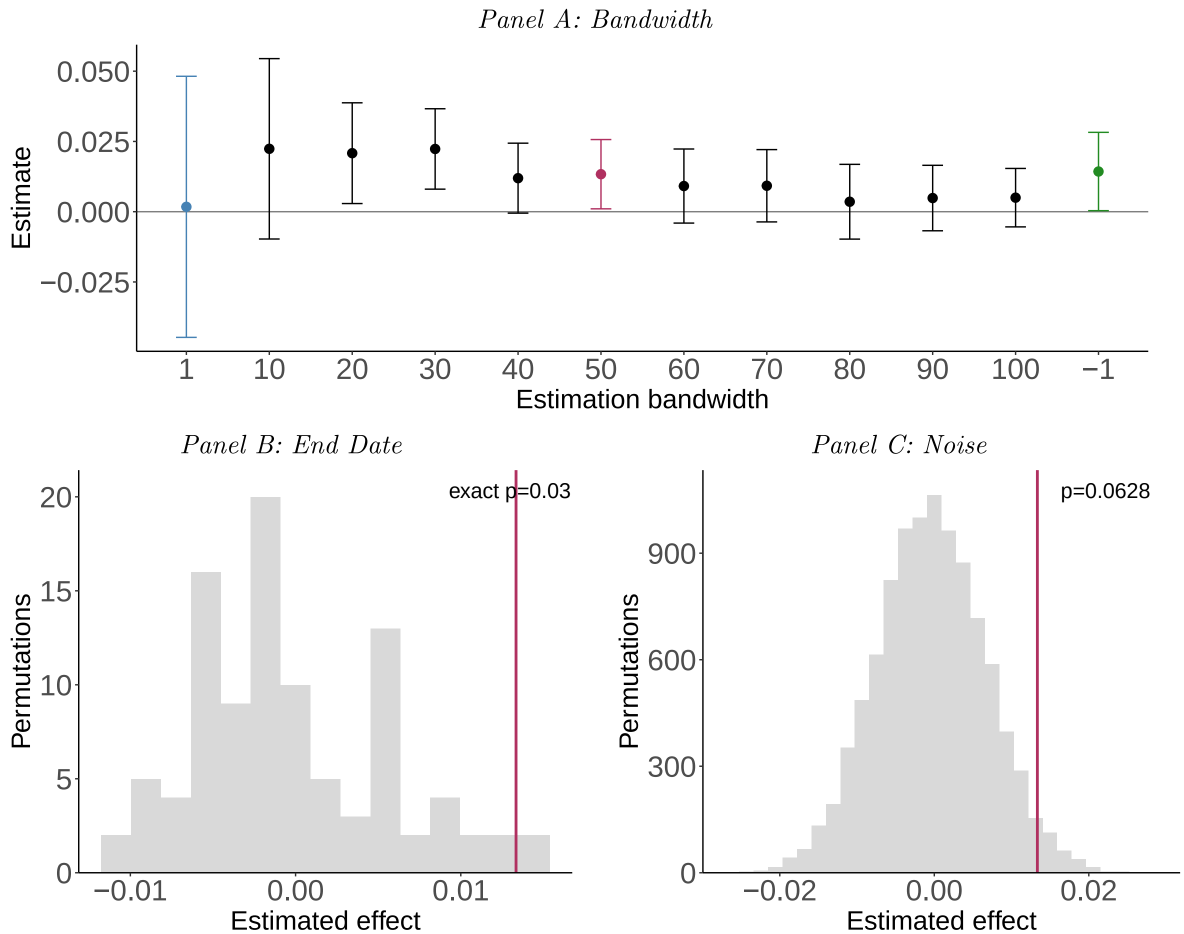

Figure OA.5: Robustness of Deer Season End on Gun Suicide Mortality

Figure shows robustness of the drop in mortality from gun-related accidents at the conclusion of hunting season in Column 3 of Table 2. Panel A estimates the effect of the conclusion of deer season on gun accidents under different estimation bandwidths around the end of deer season. The bandwidth of -1 estimates a donut regression, where the estimation sample is formed from the preferred bandwidth of 50 days, excluding the last day of deer season and the first day after the season. 95-percent confidence intervals account for clustering by the interaction of state, year, and hunting season end-date. Panel B simulates the distribution of estimates $\hat{\tau}$ for alternative deer season end dates according to the first permutation test in Section 4.2. Panel C simulates the distribution of estimates of $\hat{\tau}$ when mortality is uncorrelated the change in deer season according to the second permutation test in Section 4.2. In both tests, the red vertical line is the estimate on real data, the gray histogram is the distribution of the estimator on fake data in which there is no effect under the null hypothesis, and the estimation bandwidth is $b = 50$ days.

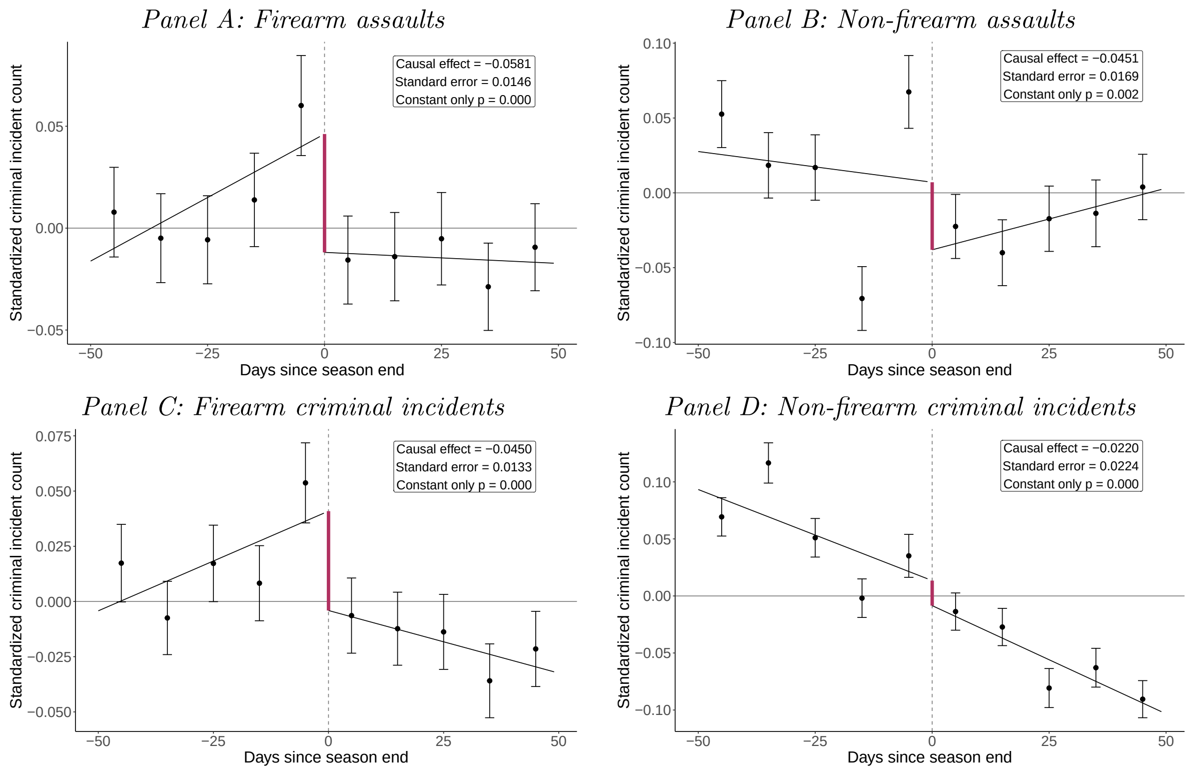

Figure OA.6: Change in Criminal Incidents after Deer Season

Figure shows criminal incidents reported to the police at the conclusion of hunting season expressed in county standard deviations. Causal effect is the estimate of τ, summarizing the average change in criminal incidents at the season's change. Standard error is the standard error of $\hat{\tau}$, accounting for clustering by state and season end date. Constant only is the p-value from a cluster-robust test of whether the estimated regression is statistically different from the null intercept-only model. Panel A considers firearm-related assaults. Panel B considers non-firearm assaults Panels C and D consider all firearm and non-firearm criminal incidents. Lines are estimated conditional expectations from Equation (1) with a bandwidth of 50 days. Circular black points are means of observed outcomes aggregated across county-years and distinct days, with 95 percent confidence intervals for the means based on the normal approximation without accounting for clustering or serial correlation.

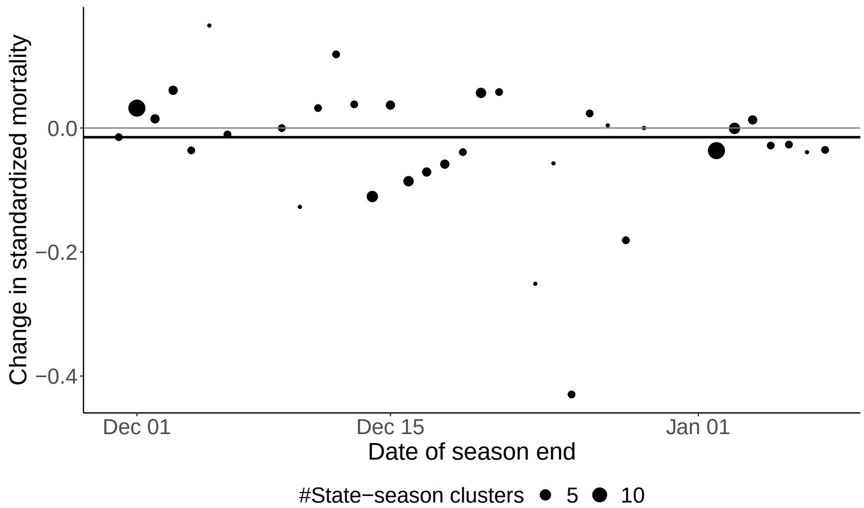

Figure OA.7: Decrease in Firearm Homicide Fatalities at End of Deer Season by Exact End Date

Figure shows expected gun fatalities expressed in county standard deviations and estimated as in the final column of Table OA.2 for groups of season clusters ending on the same day and month. Points are the estimated effect within each group. Dark horizontal line is the full sample estimate.