Does School Gun Violence Return?

Geographical Recurrence Across Educational and Non-Educational Settings

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Abstract. I examine the geographical recurrence of school gun violence, focusing on whether schools, colleges, universities, and surrounding communities experience repeated shootings. The study investigates patterns of recurrence across educational institutions and other public settings in the United States and internationally, distinguishing mass shootings from broader incidents of firearm violence. The findings reveal that repeated mass shootings at the same educational institutions are uncommon, although less severe firearm incidents recur more frequently. Similar patterns emerge across educational settings internationally, while broader public mass shootings exhibit greater geographical recurrence. However, the absence of repeated mass shootings does not necessarily indicate a reduction in future risk, particularly given the rarity of such events. The findings contribute to understanding the geographical distribution and recurrence of gun violence, offering new insights into the economics of crime, public safety, and the potential implications for institutional security and community safety.

Keywords: mass shootings; gun violence; schools; universities; geographical recurrence; spatial concentration; rare events; economics of crime.
JEL: D74; H52; I21; R23; C21.

1. Introduction

Does firearm violence return to an educational institution after a devastating shooting? The question is relevant to families weighing school quality and safety, administrators responsible for large campuses, and economists studying the geographical distribution of rare but consequential shocks. A first attack is observable; the risk that would have existed afterward in the absence of that event is not. A location might never experience a second mass shooting simply because such shootings are exceedingly rare, even when its underlying risk is unchanged. Conversely, one community may have several ordinary firearm incidents despite never registering another mass-fatality event.

In this paper, I ask three empirical questions. First, how frequently do recorded school shootings recur at the same named school, city, or county? Second, how does recurrence change when the outcome moves from any on-campus gunfire to fatal firearm incidents and then to events involving at least four victim deaths? Third, do observed recurrence patterns differ between K–12 institutions, colleges and universities, other public settings, and school incidents outside the United States? These questions are examined using six US and international incident databases, preserving each source's event definition rather than pooling incompatible incident totals.

The distinction between place and event definition is essential. A K–12 school is a much smaller potential target than a county containing dozens or hundreds of schools; a university campus can host a large, changing population and different forms of interpersonal violence. Meanwhile, a database that records all school-day gunfire will generate different recurrence statistics from a source limited to fatalities or public mass shootings. Accordingly, repeat-location shares are reported conditional on a location entering a particular database, with the number of locations and incidents specified for each estimate; these shares are not treated as unconditional future risks.

The empirical strategy is deliberately transparent. I deduplicate one same-school, same-date K–12 record and consolidate perpetrator-level observations in the public mass-shooting database into unique incidents. Repeated locations are matched at nested geographical levels, separate comparisons are calculated for fatal and nonfatal events, and an exact finite-sample benchmark is constructed for the frequency of no geographical duplication among a small set of severe attacks. This last exercise is useful because it reveals whether a zero-repeat result is remarkable once the full observed geographical distribution is considered. It does not estimate a treatment effect, predict an individual school's safety, or substitute for a risk model with an exposure population.

The economic significance extends beyond fatalities. Gun violence is a locally salient shock to human capital, household welfare, expectations, housing values, and the demand for public safety. Existing research reports effects on county earnings, labor markets, school expenditures, and political preferences [2, 15, 16]. The present analysis makes a more basic contribution to this literature: it establishes what the incident histories themselves say about within-location recurrence, and what they do not establish. A distinction between descriptive absence and causal protection matters when interpreting the social costs of violence and observed differences among places.

Four principal findings emerge. In the Washington Post school-shooting dataset, repeated events occur at 11 schools but 82 counties, reflecting a steep increase with geographical aggregation. No two of the 11 four-plus-fatality incidents repeat at the same school, city, or county, although less severe school shootings are observed subsequently in some affected cities and counties. In the Violence Project public mass-shooting source, 24 educational incidents have no repeated county among themselves, but 30 counties have repeats when all public settings are considered. Separate

school-homicide datasets show that repeated fatal firearm incidents occur at higher-education institutions more often within the affected-institution sample than at US K–12 schools. Finally, the exact benchmark indicates that zero duplication among such small groups does not constitute strong evidence of an unusually low underlying recurrence hazard. These results motivate a cautious interpretation of geographical safety, rather than a claim that tragedy renders a place less likely to experience another shooting.

2. Related Research and Economic Motivation

2.1. Mass-shooting dynamics, contagion, and geographical dependence

The economics of rare acts of violence faces a familiar inference problem: locations can appear free of repeated extreme events even when the underlying event hazard has not changed. The expected number of observed repeats depends on the baseline arrival rate, time at risk, number of schools or residents exposed, changes in reporting, and the width of the geographical unit. A school is not interchangeable with the city or county containing it.

Towers et al. [12] identify short-term temporal contagion in some US datasets of mass killings and school shootings. Such nationwide time clustering need not imply clustering at the same campus. Using spatial and temporal models at the US state level, Schweikert et al. [10] identify positive spatial and temporal dependence. These findings raise the possibility of processes operating at different geographic and temporal scales rather than a universal deterrence mechanism. Separately, Pappa et al. [9] question causal claims linking unemployment to school shootings and emphasize omitted-variable concerns, reinforcing the need to distinguish geographically patterned correlates from causal effects.

There is also an institutional comparison question. Universities serve different populations, operate larger and more open campuses, and exhibit different types of interpersonal conflict than elementary and secondary schools. A repeated homicide at a university does not necessarily indicate a repeated public mass attack. In fact, without preserving event definitions, any comparison of campuses will conflate the frequency of serious but isolated interpersonal assaults with extreme multiple-victim attacks.

2.2. Economic consequences and the value of geographical risk measurement

Brodeur and Yousaf [2] estimate that mass shootings negatively affect county earnings and employment, economic expectations, and housing prices. Their working-paper estimates indicate an approximately 2.4% earnings-per-capita decline and a 1.6% house-price decline under specified comparisons; these values are findings from that study, not from the incident datasets analyzed here. Yousaf [16] finds that mass shootings raise the salience of gun policy and alter electoral outcomes, highlighting the possibility that a geographically localized violent event affects political economy even when rare. Yang and Gopalan [15] finds a positive effect of school shootings on total district spending, estimated near $248 per pupil, although this is not a separately measured security budget.

Related work addresses market and welfare implications of firearm violence and the ability of interventions to modify violent-crime outcomes [4–6]. These studies show why exposure, location, and event definitions matter economically. A county's exposure to mass shootings is not equivalent

to a campus's exposure to all fatal firearm events. Nor can changes in perceived safety or in local housing values be inferred mechanically from the fraction of attacked schools with repeated incidents. Correctly measuring recurrence is a necessary descriptive foundation for discussing the spatial distribution of gun violence's social and economic burden.

3. Research Questions and Measurement Framework

Let i denote a school or university, g a geographical unit (campus, city, or county), and t the date of a recorded incident. Define N_g as the number of distinct locations in a given source and M_g as locations registering incidents on two or more distinct dates. The measure M_g/N_g is a *repeat-location share among recorded affected places*. It is not a hazard, incidence rate, or conditional probability of another attack during a fixed future interval. Location histories have unequal follow-up time, and the underlying sources do not cover every school-year in the risk population.

Three nested outcome types are distinguished: (i) any qualifying firearm discharge recorded by a school gunfire source, whether fatal or not; (ii) fatal incidents involving a firearm in a school-homicide source; and (iii) four-plus victim-fatality firearm incidents. The third is an operational severity screen, not a replacement for the Violence Project's separate public mass-shooting inclusion criteria. Schools, cities, and counties are analyzed separately. The contrast between K–12 and university recurrence is treated as a within-source description of incidents and geography rather than a controlled comparison of institutional hazards.

An additional question concerns the concentration of rare events by chance. If severe incidents were assigned randomly to the set of already-recorded geographical events, how often would a subset exhibit no repeated school, city, or county? That question is addressed using an exact combinatorial benchmark in Section 4.4. A high probability of no duplication in that benchmark does not prove independence of attacks or explain why shootings occur; it limits what can be inferred from a zero count alone.

4. Data and Construction

4.1. Sources and observational units

Table 1 summarizes the six incident databases. The Washington Post database tracks K–12 school gunfire in a restricted school-day setting from the 1999 Columbine attack forward, including incidents with no fatalities [14]. The Violence Project's K–12, higher-education, and international homicide databases cover fatal incidents and include non-firearm weapons; the US public mass-shooter database has a narrower mass-public-shooting definition and uses perpetrators as its original observation unit [13]. Mother Jones is a selective historical mass-shooting compilation with a setting classification. The datasets represent dated archival extracts and do not share a common endpoint or ascertainment method.

4.2. Definitions and deduplication

The primary analysis labels an event a *four-plus fatal firearm incident* if its reported firearm-event victim death count is at least four; the perpetrator is excluded to the extent supported by the source. A subset in the Violence Project public mass-shooting database additionally meets that project's

Table 1: Dataset inventory and scope

Source / setting	Records	Observation dates	Unit and definition
Washington Post, US K–12	428 raw; 427 unique	Apr. 1999–Jan. 2025	On-campus school-time gunfire incidents; fatal and nonfatal
Mother Jones, US mass shootings	162	1982–2026	Compiled mass shootings across settings; case-level source classifications
Violence Project, US K–12	398	2000–2025	Fatal events at schools, firearm and non-firearm
Violence Project, US higher education	280	2000–2025	Fatal events associated with colleges/universities, firearm and non-firearm
Violence Project, international schools	335	2000–2026	Fatal school events in other countries, multiple weapon types
Violence Project, US public mass shootings	202 perpetrators; 197 events	1966–2025	Public mass shootings with four or more victims murdered using firearms; exclusions apply

intentional public-mass-shooting scope. Thus, the same numerical casualty threshold need not generate the same sample across databases. The secondary category, *any firearm incident*, includes fatal firearm incidents in the Violence Project homicide datasets and fatal or nonfatal gunfire in the Washington Post dataset. Knife, blunt-force, arson, vehicle, and other non-firearm events are retained for reporting data coverage but excluded from firearm outcomes.

In the Washington Post dataset, incidents are deduplicated by incident date, state, city and NCES school identifier when available, with a normalized school-name fallback. This removes one same-date school duplicate, reducing 428 entries to 427 events. NCES identifiers are prioritized for school recurrence; city and county recurrence are matched using normalized city–state and five-digit county FIPS identifiers. The event is counted more than once at a *location* only if distinct incident dates are observed. Consequently, multiple shooters in the same attack never count as recurrence.

For the Violence Project homicide datasets, the Event worksheet is used rather than the perpetrator or victim worksheets. Institution names and city labels are normalized, and repeat events must have distinct dates. The international dataset has substantial missing province/state information (57 of 132 firearm records), so the comparison is based on country, city and normalized institution rather than an unreliable subnational province crosswalk. One international firearm event lacks a usable school name. Transliteration and changed campus identities remain possible matching errors.

Finally, the Mother Jones database is retained as corroborating source coverage, not an additive event sample. Its two fields named "location" must be disambiguated when read from raw CSV: one denotes a geographic label and the other denotes a venue category. Mother Jones identifies 26 school-setting cases, but that classification alone does not distinguish K–12 from universities. A source-independent six-database grand total would double count overlapping events and would be analytically invalid.

4.3. Geography, recurrence metrics and exposure

For a level $\ell \in \{\text{school,city,county}\}$, let N_ℓ be the number of distinct affected locations recorded in source data and M_ℓ the number with events on at least two distinct dates. The reported measure is M_ℓ/N_ℓ. This is a proportion *among affected locations in the selected source*, not a probability that an arbitrary US school or county will experience a shooting. The numerator also cannot distinguish early initial attacks with decades of follow-up from recent first events with almost no observation time. The analysis also identifies locations in which *another broad firearm incident* occurred after a four-plus fatal event; this is not the same as the recurrence of a second four-plus fatal incident.

Identification of absolute or relative risk requires a universe of school-years, campus-years or county-years and an explicit exposure denominator. For example, the relevant school-level outcome might be attacks per 100,000 student-years, not M_{school}/N_{school}. Distinctions between geography levels reflect very different exposures: a county contains many potential schools, commercial venues, and individuals. Similarly, an apparent decline following a first event could result from changes in school openings/closures, reporting practices, or regression to the mean.

4.4. Finite-sample random-label benchmark

The frequency of zero repetition is assessed using a transparent combinatorial benchmark. Consider N observed incidents grouped into G counties, with county incident frequencies $n_1, \ldots, n_G$ summing to N. If m incidents are selected uniformly without replacement, the probability that the selected incidents belong to m distinct counties equals

$$\Pr(\text{distinct} \mid m, \{n_g\}) = \frac{[z^m] \prod_{g=1}^{G} (1 + n_g z)}{\binom{N}{m}}, \tag{1}$$

where $[z^m]$ extracts the coefficient of z^m. The numerator is computed exactly by elementary-symmetric-polynomial dynamic programming. This baseline preserves each source's recorded distribution of events over locations but *does not* preserve timing, treatment assignment, exposure, event severity, reporting probabilities, or characteristics of mass shooting locations. Accordingly, it is only a descriptive "how surprising is zero duplication?" exercise and cannot yield a causal test of deterrence or institutional protection.

5. Results

5.1. Scope of firearm and non-firearm events

The three homicide datasets contain 398 fatal incidents in US K–12 schools, 280 in higher education, and 335 internationally. In the K–12 source 319 records are classified as firearm incidents and 79 as other weapons; the corresponding counts are 178 and 102 for US higher education, and 132 and 203 internationally. Figure 1 emphasizes why pooled counts of all homicides should not be relabeled as mass shootings. Sources differ in the events they are designed to capture, so the height of a bar is not a comparable incident rate.

Only 10 US K–12 homicide incidents, four higher-education events, and 14 events in the international dataset pass the four-or-more-victim-fatalities screen with a recorded firearm weapon.

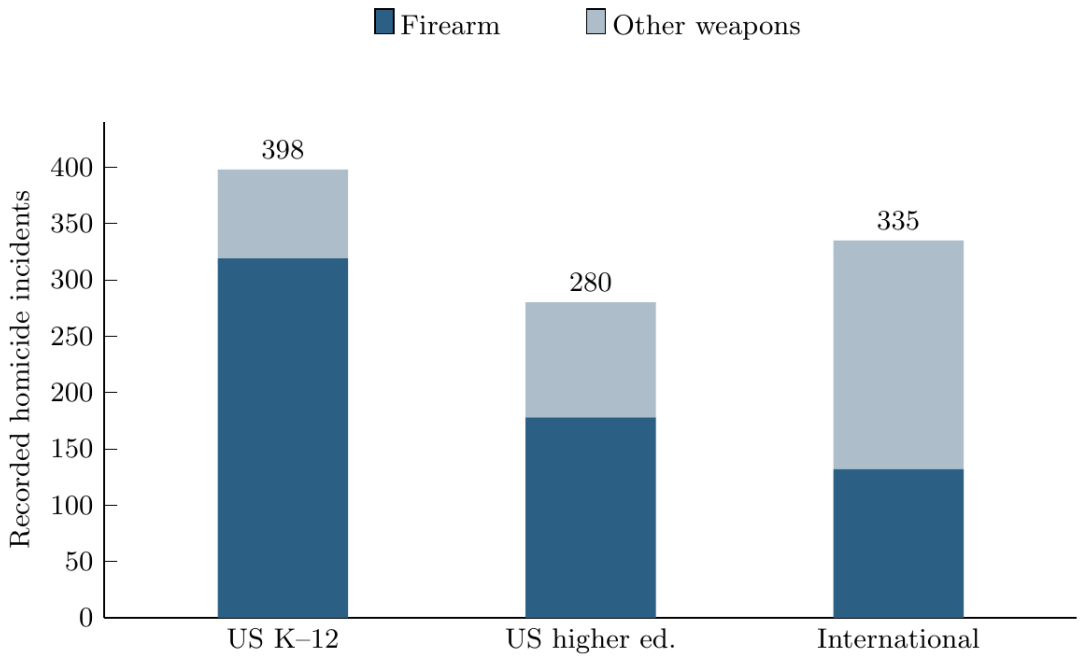


Figure 1: Composition of fatal incidents in the three Violence Project education databases

One higher-education entry classified by the source as associated with a college is the 2016 Dallas police shooting, which was not a conventional mass attack on students at an educational campus. For this reason, the simple four-event higher-education screen is reported as source-coded rather than asserted to represent four unambiguous college-targeting attacks. Removing that record would leave three cases, again underscoring the importance of human adjudication.

5.2. Within-source recurrence across geographic levels

Table 2 provides the primary descriptive results. Among 427 unique Washington Post incidents, there are 416 affected schools, of which 11 have at least two recorded incidents, or 2.6%. Moving to city and county levels, the corresponding repeat-location shares rise to 16.1% and 31.7%, respectively. This ordering reflects expanded geographic exposure as well as any genuinely different clustering process. It is neither surprising nor directly interpretable as a within-place post-event risk gradient.

Table 2: Recorded recurrence in the Washington Post K–12 sample

Level	Affected places	Places with ≥ 2 dates	Share (%)	Extra dated incidents
Individual school	416	11	2.6	11
City–state	316	51	16.1	111
County FIPS	259	82	31.7	168

Observation window April 1999–January 2025. School identifiers use NCES ID where present. Each ratio conditions on at least one incident recorded at that level.

The recurrence pattern changes sharply when the outcome is restricted to mass fatalities. Among

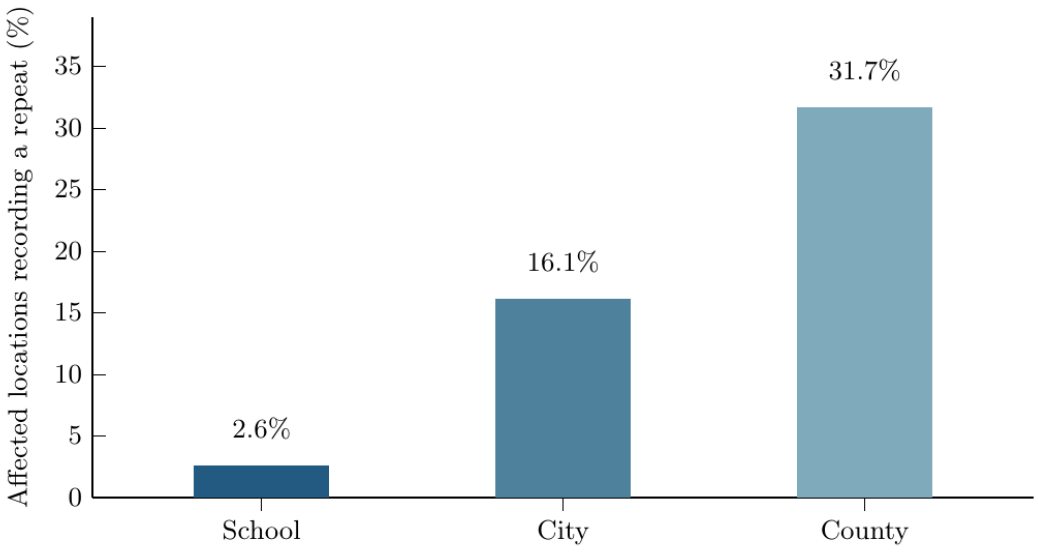


Figure 2: Repeat share among locations already present in the Washington Post incident sample

the 11 Washington Post incidents with four or more victim deaths, no two have the same matched school, city, or county. However, after those 11 shootings, another *school firearm incident of any severity* appears later in the same city for two index events and later in the same county for five index events. No post-event event is recorded at the same school in this particular dataset. Thus, "no second mass-fatality event" and "no subsequent firearm violence" are empirically distinct claims. The latter is falsified at wider geographic scales in the broader school-shootings sample.

The broader Violence Project homicide samples also reveal heterogeneity across educational settings. Among 319 firearm homicide records in US K–12 schools, 317 normalized institutions appear, and two are recorded on multiple dates (0.6% of affected institutions). In higher education, 178 firearm homicide records cover 160 institutions and 13 have multiple dates (8.1%). In the international school sample, 132 firearm homicide records cover 128 normalized schools with one repeated site, or 0.8%. These ratios cannot establish a difference in relative hazard because universities and K–12 schools have different enrollments, exposure, selection and definitions; the international sample adds heterogeneity in reporting and country-level coverage.

Table 3: Source-specific firearm homicide recurrence at educational institutions

Source	Firearm	Institutions	Repeats	Share
US K–12 homicide records	319	317	2	0.6%
US higher-education homicide records	178	160	13	8.1%
International school homicide records	132	128	1	0.8%

Same-named institutions are matched by normalized campus/school name and city, with state used for US campuses and country used internationally. Distinct dates are required. Same-location IDs should be manually reviewed. International missing place names and transliteration may bias counts downward or upward.

The difference between the university and K–12 distributions supports a cautious conclusion: repeat *fatal firearm incidents* are demonstrably possible at the same institution, even if repeat *mass-fatality shootings* are absent from the small subsets studied. In particular, the higher-education dataset records the 2007 Virginia Tech mass shooting as well as later separate fatal violence at that university, including another shooting in 2011. The later firearm incident does not satisfy the four-death threshold. The finding illustrates why safety cannot be summarized by the mass-shooting threshold alone.

5.3. Public mass shootings beyond educational settings

The public mass-shooter database provides a source-internal comparison across settings. Its 202 valid perpetrator records correspond to 197 unique incidents after same-event consolidation; five incidents involve two perpetrators. Among the 197, 15 are coded K–12, nine higher education, and 173 other public settings. When the 24 educational cases are analyzed on their own, there are no repeated city–state or state–county pairs. For the full public mass-shooting dataset, 15 of 180 affected cities and 30 of 154 affected counties have multiple events. Among the 173 non-educational public events, 15 of 156 cities and 27 of 137 counties recur.

Table 4: Public mass shootings: educational versus other public settings

Category	Incidents	City repeats	County repeats
K–12 and higher education combined	24	0 of 24	0 of 24
Other public settings	173	15 of 156	27 of 137
All public mass shootings	197	15 of 180	30 of 154

Source: Violence Project public mass-shooter database, Version 10, January 2026 snapshot. Perpetrators are deduplicated by date, city, state and county. A multi-location incident may require further adjudication of county assignment. Setting types are the source's coded locations (0=K–12; 1=higher education), not inferred from casualty counts alone.

This comparison is informative descriptively but weak as a direct hypothesis test. The educational group contains only 24 events and is distributed over a much smaller and different set of possible target locations than the 173 other incidents. Differences in the time span, population size, and probability of entering the sample could account for much of the contrast. Accordingly, no school–non-school hazard ratio is estimated from these raw counts.

5.4. The scale effect in non-educational public settings

The public mass-shooting source permits a particularly useful internal comparison because its K–12, higher-education, and other public records share a database-level definition. There are 173 other-setting incidents after event deduplication, 156 affected city–state units, and 137 affected county units. Fifteen cities (9.6% of other-setting cities) and 27 counties (19.7% of other-setting counties) have events on multiple dates. The corresponding educational set includes 24 incidents at 24 distinct cities and counties, with zero repeated units under either definition. Figure 3 displays these conditional location shares.

The zero educational count is compatible with an important alternative explanation: a 24-incident subset contains many fewer opportunities to land twice in the same county than a 173-incident

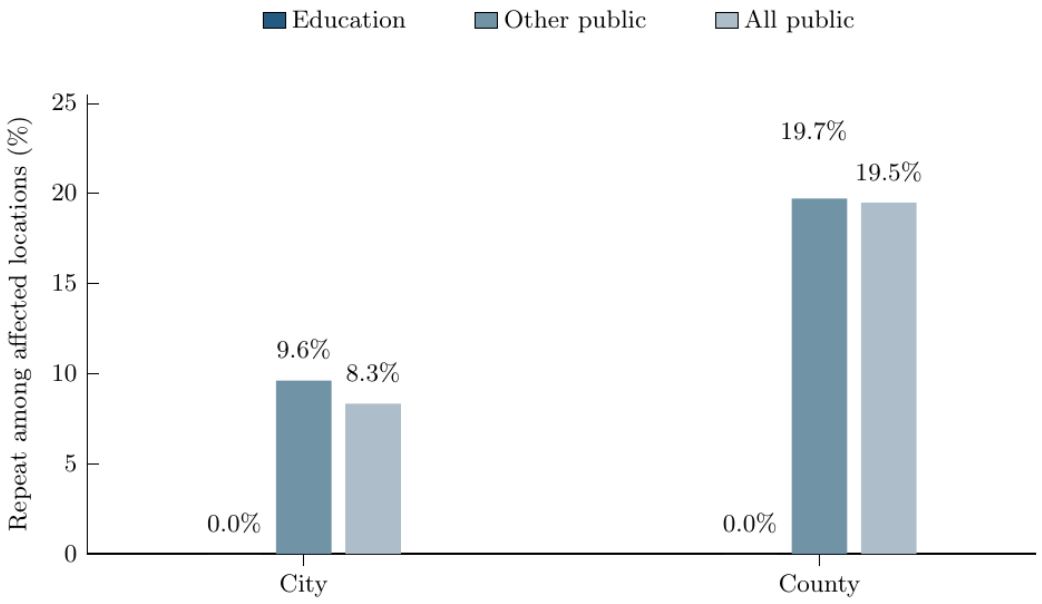


Figure 3: Within-source repeated public mass shootings by setting and geographical unit

subset. Raw repeated-site proportions therefore confound setting, group size and geography. An informal comparison of 0 of 24 with 27 of 137, by itself, cannot separate school-specific risk from the sampling process. The exact benchmark below addresses one piece of this finite-sample issue, although it is not a substitute for adjusting underlying institutional populations.

A further descriptive distinction concerns cross-setting events. Within the 197-incident public mass-shooting source, the number of repeated counties for *all* settings is 30, compared with 27 when the same calculation is restricted to other public settings. This does not mean that exactly three counties have a public mass shooting followed by an educational shooting. Removing one setting changes both the incident sequence and the set of locations counted as affected. Cross-setting recurrence requires its own dated county event history, not subtraction between these two totals.

5.5. How surprising is zero duplication?

Table 5 reports the exact random-label calculations defined by Equation (1). Conditional on the geographic concentration of all 427 Washington Post school incidents, selecting 11 uniformly without replacement gives a 99.3% probability of distinct school IDs, 86.1% of distinct cities, and 79.6% of distinct counties. In the 197-event public mass-shooting dataset, the corresponding probability that 24 randomly labeled events have distinct counties is 42.9%; for cities it is 76.1%. Thus, observing no repeated counties among 24 source-defined school-setting public mass shootings is not a rare result of this deliberately limited benchmark. In particular, it cannot be interpreted as statistically persuasive evidence that a past shooting reduced the chance of a future one.

Figure 4 places the five exact benchmarks on a common probability scale. In the Washington Post data, the probability of drawing 11 events from different schools is 99.3%, from different

Table 5: Exact random-label probabilities of no repeated location

Reference population	Geographic key	Selected events	Pr(all distinct)
Washington Post, 427 incidents	Same school	11	0.993
Washington Post, 427 incidents	Same city	11	0.861
Washington Post, 427 incidents	Same county	11	0.796
Violence Project, 197 public mass incidents	Same city	24	0.761
Violence Project, 197 public mass incidents	Same county	24	0.429

These are *not* posterior probabilities, causal p-values, or risk forecasts. They condition on observed source-specific geographic event frequencies and random assignment of a subset label. The algorithm is implemented with exact coefficient extraction from $\prod_g (1 + n_g z)$ rather than independent Bernoulli sampling.

cities 86.1%, and from different counties 79.6%. For 24 of 197 public mass-shooting events, the corresponding city and county probabilities are 76.1% and 42.9%. Such probabilities quantify what the observed spatial dispersion implies for small randomly chosen groups; they do not measure preventive efficacy or the conditional probability of a second attack after a first attack.

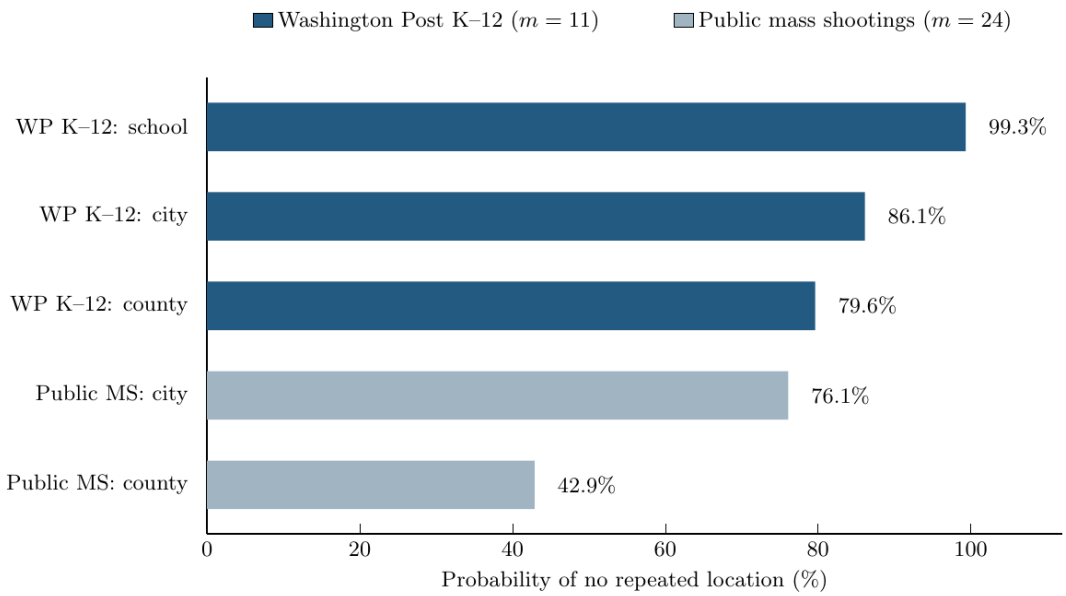


Figure 4: Exact random-label probability of no repeat location in a small incident subset

5.6. International comparison

The international homicide dataset includes 335 fatal incidents, of which 132 use firearms and 14 have at least four victim fatalities. None of those 14 mass-fatality cases shares the same normalized school or country–city combination with another such case. This is qualitatively similar to the US education-setting subset. Yet there is one repeated international institution among the wider 132 fatal firearm incidents, and seven country–city combinations appear on multiple dates. Non-firearm

violence is also a considerably larger share of the recorded international sample than of the US K–12 fatal-incident sample.

These findings are *not* a formal international replication of risk rates. The multicountry dataset does not represent an exhaustive common-risk set of schools or identical national reporting systems, the observation windows differ, and institutions are not identified by comparable administrative IDs. International similarities in small-sample non-recurrence therefore cannot establish geographical universality. A defensible replication would need country-specific school universes, school population at risk, time-varying coverage, and adjusted within-country comparisons.

5.7. Source agreement and boundary cases

The sources are deliberately not added together. The Washington Post contains nonfatal school-day gunfire and reports 11 four-plus victim fatalities; the separate Violence Project K–12 fatal-incident dataset reports ten four-plus firearm events over its own window. Both include several of the same national tragedies but can differ in casualty counts, location scope and the treatment of off-campus casualties. The public-mass-shooter database does not use the same campus homicide inclusion rule, and the Mother Jones school classification includes both K–12 and university settings. For this reason, apparent disagreement in totals is partly a *definition problem*, not necessarily a measurement error to repair by averaging.

A second issue is data provenance. The international dataset includes records dated in 2026, while the Violence Project's mass-shooter Version 10 ends in 2025, and the Washington Post dataset analyzed here ends in January 2025. One must not interpret a difference in terminal-year counts as a trend. Named dates and locations should be checked individually before circulation of definitive event-level lists. The estimates are reported as aggregates and do not identify perpetrators.

6. Economic Interpretation of the Observed Patterns

6.1. Concentration of violent events and the economics of exposure

The contrast between institutions and counties is not merely a statistical curiosity. It reflects different units of economic exposure. A school can be considered a single service provider, whereas a county contains multiple educational institutions, workplaces, businesses, neighborhoods and transportation sites. A higher county-level repeat proportion is therefore mechanically compatible with low school-level repetition without demonstrating either displacement or resilience. The Washington Post sample illustrates this clearly: only 2.6% of affected schools have multiple dated incidents, compared with 31.7% of affected counties. Since a county may contain many schools, the measures are not equivalent and must not be entered as if they were competing estimates of the same hazard.

For higher education, 13 of 160 institutions with recorded fatal firearm incidents recur, compared with two of 317 K–12 institutions. The distinction is economically meaningful as a description of settings with different size, use and exposure. University institutions tend to encompass multiple buildings, open campus areas, activities beyond the school day, and overlapping populations; the homicide dataset also includes incidents whose educational connection may be indirect. Without enrolled-student-years, campus boundaries and consistent reporting rules, there is no justified

conclusion that higher-education campuses are intrinsically more dangerous.

6.2. Severity thresholds change the apparent recurrence process

Four-plus fatal firearm events are a much thinner tail of school firearm violence than fatal and nonfatal gunfire in general. The Washington Post data contain 11 above-threshold events in 427 unique shootings, compared with 11 repeated schools in the full incident dataset. These are different subsets and should not be confused. The ten US K–12 four-plus-fatality firearm records in the Violence Project homicide data similarly occupy a very small share of that source's 319 fatal firearm records. A zero severe-event repeat must therefore be interpreted alongside the substantially larger risk of other firearm incidents.

Of the 11 Washington Post four-plus-fatality index shootings, the source contains later school gunfire in two of the same cities and in five of the same counties. These comparisons are source-window specific, and several index shootings have little remaining follow-up by January 2025. Nevertheless, they demonstrate that a community can experience another documented school shooting without experiencing another mass-fatality school attack. The monetary and welfare burden of a nonfatal shooting can remain significant, including school disruption and fear, even though it falls below a conventional threshold for a public mass shooting.

6.3. Implications for the economic interpretation of safety

Economic studies document that firearm violence affects earnings, housing markets and political salience [2, 16]. The present figures do not estimate any of those outcomes. They instead show that a location's event history is highly sensitive to the geographical boundary and severity definition selected. That sensitivity matters whenever incident histories are used to characterize places. A zero repeat count is an empirical fact about a sample and outcome, but it should not be reported as proof that additional expenditures, community vigilance, or security policies caused a subsequent reduction in risk.

The exact random-label calculations further indicate that the lack of duplicate counties among 11 severe school shootings is not an unusual configuration given the geographical distribution of all recorded school shootings. An economic explanation involving local institutional adaptation remains possible, as do ordinary rare-event variation, different observation windows, school closure, migration, and reporting differences. The data analyzed here do not adjudicate among those processes. The contribution is therefore interpreted as measured geographical concentration, variation by educational setting, and a boundary on causal conclusions, rather than an estimate of the monetary return to school security.

7. Limitations

Four limitations govern interpretation. First, these data are collections of observed incidents, not exhaustive school-year risk registers, so repeat shares among affected locations are not unconditional probabilities. Second, observation windows and inclusion criteria differ: firearm versus non-firearm events, K–12 school-day gunfire, public mass shootings, and university-related fatal incidents cannot be pooled without harmonizing definitions. Third, institutional matching may fail when campuses

change names or locations; reporting of geographical identifiers and international transliteration may be incomplete. Finally, a small sample of events crossing a high fatality threshold has little power to distinguish ordinary rarity from a reduction in recurrence. The benchmark holds incident geography fixed while randomizing a severe-event label; it is neither a treatment-effect estimator nor a longitudinal hazard model. The data do not support ranking named schools, counties or cities as safer relocation choices.

8. Conclusion

I find that recurrent school gun violence depends strongly on the definition of an incident and the size of the location considered. In the Washington Post K–12 gunfire records, 11 of 416 affected schools experienced another dated shooting, while 82 of 259 counties experienced repeated incidents. Among 11 school shootings exceeding a four-victim fatality threshold, there is no same-school, same-city, or same-county recurrence of another threshold event. Yet less severe shootings subsequently occur in some affected cities and counties. In a separate public mass-shooting source, no educational-setting county repeats among 24 school or university incidents, whereas all public settings combined include 30 counties with multiple incidents. Source-specific homicide collections show that repeated fatal firearm violence is observed at both K–12 and higher-education institutions, with substantially different observed repeat proportions. The international school records yield a similarly small set of severe events without a repeated school or city, but they are not a country-adjusted test of risk.

The central inferential result is equally important. Exact random-label benchmarks demonstrate that observing zero duplicated locations among a small group of severe events is compatible with the underlying spatial distribution of incident records. Historical absence of a second mass shooting should not be interpreted as evidence that a first shooting made a location safer. This empirical distinction is important for the economics of risk, crime, education, and local welfare: event histories are informative, but event rarity and differing exposure must remain part of their interpretation.

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