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News coverage and mass shootings in the US[☆]

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ABSTRACT

We study the potential effect of mass-shooting-related television news in the US on subsequent mass shootings from 2006–2017. To circumvent endogeneity, our identification strategy relies on unpredictable disasters in countries home to substantial numbers of US emigrants crowding out shooting news. Instrumental variable and reduced form regressions consistently suggest a positive and statistically significant effect. This result remains consistent throughout a battery of robustness checks. In terms of magnitude, a one standard deviation increase in shooting news raises mass shootings by approximately 73% of a standard deviation. We then explore potential mechanisms, broadly delineating (*i*) the ideation of murder, (*ii*) fame seeking, and (*iii*) behavioral contagion. The number of murders in general remains orthogonal to shooting news, and mass shootings are not more likely on days with predictable news pressure (e.g., during the Olympics or the Super Bowl). However, mass shootings are more likely after anniversaries of the most deadly historical mass shootings. Taken together, these results lend support to a behavioral contagion mechanism following the public salience of mass shootings.

1. Introduction

Mass shootings have become a gruesome regularity in the US. Columbine (1999), Virginia Tech (2007), Sandy Hook (2012), Orlando (2016), and Las Vegas (2017) constitute just some of the deadliest examples. Mass shootings were listed as the most common source of significant stress among 71% of respondents in a 2019 US survey (American Psychological Association, 2019; also see Rossin-Slater et al., 2019).

How can we explain these tragedies? One hypothesis put forth by criminologists, psychologists, and popular commentators emphasizes the extensive media coverage mass shootings receive. According to criminologist Adam Lankford, "there is no doubt that there is an association between media coverage that these offenders [mass shooters] get and the likelihood that they will act" (Christensen, 2017; also see Larkin, 2007, 2009; Towers et al., 2015, and Lankford and Madfis, 2018). The underlying behavioral hypotheses to link shooting news to subsequent mass shootings can broadly be grouped in three categories (Section 2.2 provides details). First, shooting news may generally render gun violence and murder salient. Second, a fame-seeking motivation has been suggested (e.g., see Larkin, 2009, p.1318). For example, Omaha gunman Robert Hawkins' suicide note read "[j]ust think tho [sic]

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I'm gonna be (expletive) famous" (Gun, 2007).[1] Third, a threshold-type model (e.g., see Granovetter, 1978) implies a 'contagion effect' by which increased observation of others behaving in a certain way (i.e., committing a mass shooting) can move that behavior into the realm of feasibility for an individual. While cleanly delineating these behavioral mechanisms remains difficult, what they share is the idea that shooting news could lead to more mass shootings.

However, despite these theoretical priors and descriptive evidence, the US media has largely refused to report more prudently on mass shootings. Notably, this journalistic pattern contrasts with those for other sensitive topics, such as suicides, where the media has found ways to limit coverage.[2] In a recent survey, journalists agreed covering mass shootings has become routine and sensational — yet most of them remain strongly supportive of reporting on shootings (Dahmen et al., 2018). In fact, the Omaha shooter mentioned above received five complete segments on the *ABC Evening News* for a total of 16 min and 10 s of television news attention in front of millions of people.

What has remained elusive is an empirical test to explore potentially causal links between shooting news and subsequent mass shootings — an analysis that goes beyond the existing anecdotal, descriptive, and theoretical body of research. The following pages aim to make such a contribution by connecting television news dedicated to mass shootings (as a proxy for the overall extent of shooting news) to the occurrence of subsequent mass shootings. To overcome latent endogeneity concerns owed to measurement error and omitted variables, our identification strategy draws on unexpected disasters in countries home to numerous US emigrants. Specifically, we focus on earthquakes, epidemics, and volcanic activity that have been shown to be virtually unpredictable and have traditionally drawn substantial media attention (Section 3.3 provides details).[3] Indeed, we find significantly fewer shooting-related news segments on such disaster days, even when controlling for weekday-, month-, and year-fixed effects, as well as linear and squared time trends.[4] In turn, we find no evidence to suggest shooters are able to anticipate these disasters (and the associated news congestion) to perhaps time their acts. In the spirit of Eisensee and Strömberg's (2007) concept of news pressure, this identification strategy allows us to test for a causal effect of shooting news on the number of subsequent mass shootings.

Accessing the *USA Today* database for mass shootings and the *Vanderbilt Television News Archive* for information from *ABC World News Tonight* (the most-watched prime-time television news program), we analyze daily data from January 1, 2006 to December 31, 2017. The corresponding results suggest a positive, statistically significant, and quantitatively meaningful effect. Although we advise caution not to over-interpret magnitudes (since we rely on a *local* average treatment effect induced by disasters abroad), our results connect a one standard deviation increase in shooting news to an increase in mass shootings by 73% of a standard deviation within the subsequent 14 days.

We then explore the statistical sensitivity of this result. Findings are consistent when (i) splitting instruments; (ii) predicting *deaths* from mass shootings; (iii) employing alternative databases from the *Gun Violence Archive* or the *Brady Campaign*; (iv) considering alternative measures and definitions of shooting news; (v) incorporating shooting-related coverage from the *CBS Evening News*, the *NBC Nightly News*, and *CNN*; and (vi) accounting for lagged shootings and potential autocorrelation. Overall, the derived effect prevails for 2–3 weeks before reverting back to zero.

For the paper's final contribution, we explore potential mechanisms related to ideation, behavioral contagion, and fame seeking. First, we find no evidence of murders in general increasing because of shooting news; rather, studying mass shootings categorized as public or family shootings both produces positive and statistically significant estimates. Thus, shooting news are predictive of different types of mass shootings but not other murders. Second, we consider anniversaries of the most infamous mass shootings in US history as natural primers of such events. Indeed, we find systematically more shootings after such anniversaries, but again identify no statistically meaningful relationship to general murders. Finally, we study days on which the news are predictably congested, e.g., if major sporting events, the Super Bowl, or the Academy Awards are scheduled. If fame was a shooter's primary motivation, we would expect to see a statistically significant decrease in the occurrence of mass shootings on such days. However, we identify no such effects. Taken together, while one should interpret the results of these additional specifications carefully and in context, the evidence is consistent with a behavioral contagion model as a possible mechanism linking shooting news to the incidence of subsequent mass shootings.

Overall, we hope this paper can contribute to three areas of research. First, we extend the empirical evidence studying the causal effects of mass media in policy-relevant outcomes. Adding to the suggested consequences from various types of television coverage (see Section 2.1), we believe this paper is the first to provide causal evidence for detrimental consequences from covering mass shootings. Second, we contribute to the policy debate surrounding the causes of and potential avenues to prevent mass shootings. To our knowledge, our study provides the first piece of systematic empirical evidence to test the theoretical predictions made by scholars from the criminology and psychology disciplines. We discuss potential policy implications and limitations of our study in Sections 5 and 6. Third and final, we refine the methodological tools available to measure causal effects of particular types of news

[1] In another example, the perpetrator responsible for the Parkland shooting predicted in a self-recorded video that "[w]hen you see me on the news, you'll know who I am" (Cooper, 2018).

[2] The contagion phenomenon pertaining to suicide has long been suspected, at least since Goethe's 1774 publication of *The Sorrows of Young Werther*, which was thought to have inspired numerous suicides (Niederkrotenthaler et al., 2007; also see De Tarde, 1903, for early work on the phenomenon of imitation). Gould et al. (2014) explore the role of newspapers in suicide clusters, while (Corbo and Zweifel, 2013) discuss how the media should report suicides, referring to contagion (also see Pirkis et al., 2006).

[3] For example, Strömberg (2007, p.215) discusses natural disasters and points out that "[e]arthquakes and volcanoes are more often covered by the news than equally severe disasters of other types".

[4] To ensure we do not confound our analysis with false positives, such as 'shooting' a movie, we carefully check each news segment and remove segments unrelated to mass shootings.

coverage. While some studies have used exogenous variation from disasters (e.g., see Jetter, 2017, 2019; Garz and Pagels, 2018, and Aparicio and Jetter, 2021), we (*i*) focus on disasters in countries of particular interest to the domestic audience and (*ii*) employ disasters that have been shown to be statistically unpredictable (earthquakes, epidemics, and volcanic activity), as opposed to those that can be anticipated (e.g., because they tend to follow seasonal patterns; see Section 3.3).

2. Theoretical background and relevant literature

2.1. Policy-relevant mass media effects

Mass media in general and television in particular have been connected to a range of policy-relevant outcomes. Although the corresponding hypotheses were often introduced much earlier, recent works present empirical identification strategies related to voting behavior (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Campante and Hojman, 2013; DellaVigna et al., 2014; Schroeder and Stone, 2015; Peisakhin and Rozenas, 2018; Philippe and Ouss, 2018), policymakers (Arceneaux et al., 2016), disaster relief efforts (Eisensee and Strömberg, 2007), politicians' resignation decisions (Garz and Sörensen, 2017), capturing criminals (Webbink et al., 2016), terrorism (Jetter, 2017, 2019; Durante and Zhuravskaya, 2018), genocide (Yanagizawa-Drott, 2014), fertility decisions (Kearney and Levine, 2015), divorce rates (Chong and Ferrara, 2009), as well as economic and social developments more generally (La Ferrara, 2016; also see Kleemans and Vettehen, 2009; for an overview of the causes and effects of television sensationalism).

However, causal evidence exploring news coverage and subsequent mass shootings has, to our knowledge, remained elusive. Perhaps one of the closest related studies comes from Dahl and DellaVigna (2009) who test the 'copycat' effect of violent movies. Interestingly, they find "violent crime *decreases* on days with larger theater audiences for violent movies" (emphasis in original; Dahl and DellaVigna, 2009, p.677). Further estimations suggest a substitution effect away from pursuing violent crimes to watching violence in movies. Nevertheless, potential effects from news coverage of actual mass shootings have not been explored through an explicit identification strategy.

2.2. Media attention and mass shootings

In general, a link between shooting news and mass shootings has been suggested by several scholars and popular commentators.[5] In 2007, the American Psychiatric Association issued an open letter to the news media, highlighting that "[t]he media have an important role to play in limiting the power of such tragedies [mass shootings] by choosing not to sensationalize them" (American Psychiatric Association, 2007). The underlying mechanisms can broadly be grouped into three categories. First, observing coverage of a mass shooting may generally make violence salient. Prominent analogies can be found in the suggested link between violent video games and violent crime (e.g., see Cunningham et al., 2016).

Second, an often-discussed connection between shooting news and mass shootings relates to fame seeking. Lankford and Silver (2020, p.43) report that, between 2010 and 2019, more than half of all mass shooters had "explicit evidence of fame- or attention-seeking". For example, in 2015, a shooter who murdered two people live on air stated: "[s]eems like the more people you kill, the more you're in the limelight" (Perez et al., 2015). Further, Stanglin (2013) writes "[t]he police believe that Lanza [the Sandy Hook shooter] aimed to be a glory killer". Several other shooters explicitly claimed to be seeking media attention (e.g., see Larkin, 2009, p.1318).

Third, shooting news could produce 'contagion' or 'ripple' effects, whereby observing others behaving in a certain way (in this case committing a mass shooting) moves that behavior into the realm of feasibility, at least for some individuals. Gladwell (2015) proposes that view, referring to Granovetter's (1978) theory of behavioral threshold models (also see Wheeler, 1966). Studying the psychology of mass murderers, Auxemery (2015) notes that "[t]he media appears to play a crucial role in preventing the occurrence of imitation or copycat tragedies" (also see Rogers, 2016a,b for recent popular discussions). Gould and Olivares (2017) summarize the existing evidence related to the contagion hypothesis, while Madfis (2017) provides an overview of the descriptive and theoretical literature on school shootings. Cantor et al. (1999) propose a 'ripple effect' related to the media coverage of seven mass homicide events in Australia, New Zealand, and the UK (also see Paton, 2012 for shooters' emulating behavior of prior events).

Naturally, these behavioral mechanisms remain difficult to delineate, and other (yet to be proposed) mechanisms may be at work. Nevertheless, Section 4.4 will seek to explore which, if any, of these narratives receives empirical support. Primarily, however, our identification strategy is able to explore whether the *quantity* of shooting news per se can potentially alter the likelihood of subsequent mass shootings.

[5] Concerning empirical studies on gun ownership and regulations, as well as economic and political consequences of mass shootings, we refer to Duggan (2001), Dube et al. (2013), Edwards et al. (2018) and Yousaf (2018), and Brodeur and Yousaf (2019).

Table 1
Summary statistics of main variables for all 4383 days between January 1, 2006, and December 31, 2017. Summary statistics of additional variables are available in Table A.2.

Variable (Source)	Mean (Std Dev)	Min (Max)	Description
Main variables			
Shootings (*USA Today*)	0.063 (0.250)	0 (2)	# of shootings today, where at least 4 people are killed
Shooting news (*VTNA*)	0.112 (0.389)	0 (5)	# of *ABC news* segments including *shoot* (case insensitive) in headline[a]
Earthquakes, epidemics, & volcanic eruptions (*EM-DAT*)	0.240 (0.483)	0 (3)	# of earthquakes, epidemics, and volcanic eruptions in countries of interest[b]
Additional variables			
Earthquake, epidemic, & volcano news (*EM-DAT*)	0.132 (0.391)	0 (4)	# of *ABC news* segments including *earthquake*, *epidemic*, *flu*, or *volcano* today[a]
First disaster day (*EM-DAT*)	0.023 (0.151)	0 (1)	= 1 if earthquake, epidemic, or volcanic eruption in country of interest[b] today but not yesterday
Shooting victims (*USA Today*)	0.341 (1.856)	0 (58)	# of victims from shootings today
Shootings[c] (*Gun Violence Archive*)	0.863 (1.104)	0 (6)	# of shootings today, where at least 4 people are injured
Shootings[d] (*Brady Campaign*)	0.067 (0.268)	0 (3)	# of shootings today, where at least 4 people are killed

[a]We manually check and remove false positives (see Section 3.2).
[b]Includes the countries that count at least 50000 US emigrants, as well as Afghanistan and Iraq. The 19 countries include (ranked by number of US emigrants): Mexico, Canada, India, Germany, Philippines, Israel, United Kingdom, Puerto Rico, Costa Rica, South Korea, France, China, Brazil, Colombia, Australia, Japan, Pakistan, Italy, and the United Arab Emirates.
[c]Counts 1826 observations from January 1, 2013 to December 31, 2017.
[d]Counts 2588 observations from January 1, 2006 to January 31, 2013.

3. Data and empirical strategy

3.1. Data on mass shootings

We identify mass shootings in the *USA Today*'s *Behind the Bloodshed* database (*USAT*, Overberg et al., 2019). Defining a mass shooting by four or more victims and deriving data from the FBI, local police records, and media reports, the *USAT* database ranges from January 1, 2006 until December 31, 2017. Our results are consistent when employing data from the *Brady Campaign* for shootings in which four or more people are killed (ranging from January 2006 to January 2013; Towers et al., 2015) or the *Gun Violence Archive*, which includes all shootings in which four or more people are *injured* (data available from 2013 to 2017; GVA, 2018; see Table 4). Results are also consistent if we raise the threshold to only consider shootings that killed five or more people (see Table A.10). Additional specifications explore the number of deaths from mass shootings (see Table 4).

Table 1 reports the corresponding summary statistics, whereas Table A.1 lists all mass shootings chronologically, and Table A.2 presents summary statistics for all additional variables. According to the *USAT*, 265 (or six percent) of the 4383 sample days from 2006 to 2017 experienced at least one mass shooting, whereas nine days saw two mass shootings. To simplify notation, we use the terms mass shooting and shooting interchangeably throughout the paper.

3.2. Data on television news

For data on prime-time television news, we access the *Vanderbilt Television News Archive* (*VTNA, 2019*), following other studies on media effects in the US (e.g., see Eisensee and Strömberg, 2007; Durante and Zhuravskaya, 2018, or Jetter, 2019). As a representative measure of news coverage, our main estimation considers the headline of each news segment from *ABC World News Tonight*, the 30-minute flagship evening television news format of *ABC News*. Averaging an audience of eight million people, the program ranks first among evening news programs in the US (Joyella, 2018). We focus on television news – as opposed to newspapers, for example – since prime-time news draws a viewership that comfortably exceeds the readership of major newspapers by a factor of five or more (US Securities and Exchange Commission, 2017, p.2).

The *VTNA* contains *ABC News* data for 4123 of the 4383 days, and a manual search of the *ABC News* online archive fills the remaining gaps. The *VTNA* reports less data for the *CBS Evening News*, the *NBC Nightly News*, and *CNN* with 2885 days, 3310 days, and 3371 days, i.e., 23–35 percent of all days are missing for those outlets. Nevertheless, shooting-related news coverage (explained shortly) correlates well between programs, as the number of shooting news on the *CBS*, *NBC*, and *CNN* programs show correlation coefficients of 0.37, 0.43, and 0.46 with the number of shooting news on *ABC World News Tonight*. Our results are consistent when incorporating these news outlets and deriving an average of shooting-related coverage (see Table A.11).

To capture shooting-related news, we first count the number of news segments that include the term *shoot* (case insensitive), such as in *shooter* or *shooting*. Manually browsing the days of and immediately following some particularly deadly shootings (e.g., Dylan

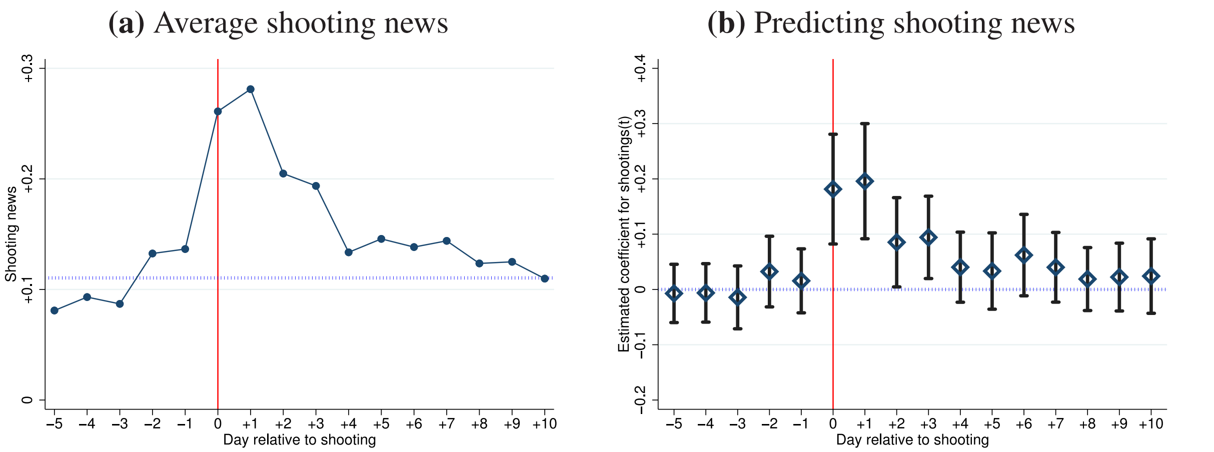


Fig. 1. *Left*: Shooting news around shooting days, where the dashed horizontal line indicates the sample average of 0.11. *Right*: Estimates from regressing shooting news on day t on 16 variables for the number of shootings on days $t-5$ until $t+10$, while controlling for a linear and squared time trend, as well as fixed effects for each weekday, month, and year.

Roof's church shooting on June 17, 2015) shows that either the word *shooting* or *shooter* consistently appears in the respective headlines. Typical headlines read *School Shootings/Kansas Plot* (on April 20, 2006) or *Bailey, Colorado/School Shooting* (on September 28, 2006). We then manually inspect each headline and abstract to filter out false positives, such as shooting of a movie or police shootings.[6]

This leaves 490 news segments or 0.11 news segments per day, on average (see Table 1; Table A.3 lists all segments). The maximum number of five segments was reached once — on December 14, 2012, the day of the Sandy Hook Elementary School shooting. Days with four segments occurred three times: on (*i*) January 8, 2011 (Tucson shooting), (*ii*) June 13, 2016 (Orlando nightclub shooting), and (*iii*) October 2, 2017 (the day after the Las Vegas shooting). In general, we identify at least one shooting-related news segment on 408 of the 4383 sample days.

Fig. 1**(a)** visualizes the average number of shooting news segments around shooting days, documenting a significant rise on shooting days and the days thereafter. Fig. 1**(b)** displays estimates from regressing shooting news on day t against 16 variables that capture the five days before until the ten days after shootings. In addition, we account for fixed effects at the weekday, month, and year level, as well as a linear and quadratic time trend. These covariates constitute the full set of our time-specific regressors that aim to filter out time-specific developments and will be introduced more formally in Section 3.4. Overall, as we would expect, Fig. 1 illustrates how shooting news rise systematically on the day of and the day after a mass shooting.

Importantly, that variable is likely unable to capture *all* television news related to shootings. However, using the term *shoot* and carefully checking the corresponding news headlines and abstracts for false positives provides a consistent and objectively defined identification strategy. Nevertheless, there is likely to be measurement error that further complicates the identification of statistically meaningful relationships. In further specifications, we aim to alleviate these concerns by adding other shooting-related keywords and mechanically incorporating all mentions of *shoot* in the headline or abstract (see Section 4.2).

3.3. Data on disasters

Our empirical analysis employs an identification strategy based on events that are (*i*) unpredictable for potential shooters and news agencies, as well as (*ii*) able to capture US television news attention, thereby potentially crowding out shooting news. We access the International Disasters Database (*EM-DAT*) for the day-to-day number of earthquakes, epidemics, and incidents of volcanic activity in countries that host at least 50000 US emigrants, in addition to Afghanistan and Iraq, the two major war zones that hosted hundreds of thousands of US troops in the early 21st century.[7] Results are consistent if we lower that threshold to 40000, 30000, 25000, or 10000 US emigrants with the corresponding findings referred to Table A.7.[8] In total, we capture 158 such disasters

[6] Examples of these include *Cheney shooting*, *Reagan shooting*, and *CIA Plane Shootdown*.

[7] For a disaster to be included in *EM-DAT*, one of the following three conditions needs to be met: (*i*) 10 or more deaths; (*ii*) 100 or more people affected/injured/homeless; (*iii*) declaration by the country of a state of emergency and/or an appeal for international assistance (see Guha-Sapir et al., 2014). The 19 countries with at least 50000 US emigrants are (with the respective number of US emigrants in parentheses as of January 2020): Mexico (738100–1000000), Canada (700000–1000000), India (700000–1000000), Germany (324000), Philippines (220000–600000), Israel (200000), United Kingdom (139000–197143), Puerto Rico (189000), Costa Rica (130000), South Korea (120000–158000), France (100000), China (71493), Brazil (70000), Colombia (60000), Australia (56276), Japan (55173), Pakistan (52486), Italy (50000–54000), and the United Arab Emirates (50000). We derive that list from several individual sources (see Wikipedia, 2019).

[8] The additional countries are, with the respective number of US emigrants listed in parentheses: Haiti (45000) and Saudi Arabia (40000); Argentina (37000), Spain (34638), Norway (33509), the Bahamas (30000), and Russia (30000); Lebanon (25000) and Panama (25000); El Salvador (19000), New Zealand (17751), Ireland (17552), Honduras (15000), Chile (12000), Taiwan (10645), and Austria (10175).

Table 2
Predicting *ABC News* coverage of particular disaster types. All estimations incorporate the full set of time-specific covariates.[a]

Dependent variable:	Earthquake news$_t$ (1)	Epidemic news$_t$ (2)	Volcano news$_t$ (3)	Fire news$_t$ (4)	Storm news$_t$ (5)	Flood news$_t$ (6)	Earthquake, epidemic, & volcano news$_t$ (7)
Earthquakes$_t$	0.209***						
	(0.039)						
Epidemics$_t$		0.030**					
		(0.013)					
Volcano activity$_t$			0.035*				
			(0.020)				
Fires$_t$				−0.021			
				(0.030)			
Storms$_t$					0.003		
					(0.010)		
Floods$_t$						0.004	
						(0.005)	
Earthquakes, epidemics, and volcanic eruptions$_t$							0.126***
							(0.020)
N	4376	4376	4376	4376	4376	4376	4376
R^2	0.081	0.113	0.026	0.068	0.044	0.063	0.086

Notes: Newey–West standard errors are displayed in parentheses (lag of one day). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ indicate conventional levels of statistical significance at the ten, five, and one percent level.
[a]Includes a linear and quadratic time trend, as well as fixed effects for each weekday, month, and year.

(132 earthquakes, 10 epidemics, and 16 events of volcanic activity) that span 950 days out of our 4383 sample days. Epidemics include outbreaks of the avian influenza (in Iraq), cholera (Iraq and the Philippines), dengue fever (Australia and Pakistan), diarrhea (Philippines), vibrio cholera (Iraq), and yellow fever (Brazil). Table A.4 provides the corresponding list of earthquakes, epidemics, and volcanic events.[9]

Our instrumental variable (IV) choice involves two definitions to strengthen its predictive power and address the exclusion restriction: disaster types and reference countries. First, we focus on earthquakes, epidemics, and volcanic activity as opposed to other disasters, such as storms, floods, or wildfires. We take this step because the latter types of disasters can often be anticipated or follow seasonal patterns (Chen et al., 2002; Riaño et al., 2007; National Oceanic and Atmospheric Administration, 2019), whereas the former types have been shown to be virtually unpredictable (Ogbunu et al., 2019; United States Geological Survey, 2019). If a disaster and the accompanying news pressure could be predicted by potential shooters or news agencies, the exclusion restriction may be violated.

Another relevant aspect is the US media attention commonly devoted to particular disaster types. Strömberg (2007, p.215) remarks that "[e]arthquakes and volcanoes are more often covered by the news than equally severe disasters of other types" (also see Table VIII of Eisensee and Strömberg, 2007). Similarly, Yan and Bissell (2018, p.872) find that "[t]he average amount and length of reports was highest for earthquakes" in the major US newspapers, when compared to hurricanes and flooding. To explore this hypothesis in our sample, we count the number of *ABC News* segments that mention the respective keywords and stems, i.e., ***earthquake***, ***epidemic*** or ***flu***, ***volc***, ***fire***, ***storm***, and ***flood*** (all case insensitive).[10] For all news segments captured by these keywords, we manually check for and remove false positives.[11] Table 2 displays regression results from predicting the corresponding news with the respective disasters in our reference countries. In all regressions, we account for time trends (linear and quadratic), as well as fixed effects for each weekday, month, and year. We find earthquakes, epidemics, and volcanic activity to be statistically relevant predictors of the corresponding news coverage, while we identify no statistically discernible evidence for the other disasters. Thus, we focus on earthquakes, epidemics, and volcanic activity in our main estimations.[12]

Second, to increase the likelihood of media attention, we focus on disasters in countries for which the US media likely shows particular interest (Adams, 1986, 2003). In our main specification, we select those nations that host at least 50000 US emigrants, in addition to Afghanistan and Iraq where hundreds of thousands of US troops were stationed throughout our sample period (Hanusch, 2008; Department of Defense, 2019). We choose these countries because, everything else equal, we assume US television news outlets are more likely to cover a disaster if it affects places that are home to more US citizens.[13] Including countries that host fewer US

9 We also explore the possibility of such disasters directly affecting the US public, e.g., by spreading fear of the corresponding epidemic (see Campante et al., 2020 for the Ebola scare in 2014). However, we find no indication for that to be the case, largely because none of the epidemics in our sample are likely to pose a significant threat to the US.

10 We include the term *flu* to capture news on the avian influenza, commonly referred to as the bird flu by US news outlets.

11 For example, related to the flu we remove the segments entitled *Flutie* (referring to the football player Doug Flutie on January 1, 2006), *The Money Trail (Politics: Corporate Influence)* (September 4, 2008), and *Law: "affluenza" Defense* (October 16, 2015). Table A.5 documents the corresponding news segments identified as false positives.

12 Results are consistent if we exclude volcanic activities, given the statistical evidence from column (3) of Table 2 is weaker than for the other two disaster types (see Table A.11).

13 Incorporating disasters from *all* countries worldwide or from countries that send the most migrants to the US does not produce a statistically significant relationship for our first stage (see Table A.8).

emigrants (thresholds of 40000, 30000, 25000, or 10000), as well as adjusting disasters by the country's respective population size, produces consistent results (see Tables A.7 and A.11).

3.4. Baseline empirical specification

We begin with a standard linear regression analysis, predicting the number of mass shootings per day on days $t+1$ until $t+7$ with a measure for shooting-related news on day t[14]:

$$\frac{Shootings_{(t+1),\dots,(t+7)}}{7} = \beta_0 + \beta_1 \left(Shooting\ news\right)_t + \boldsymbol{X}_t' \beta_2 + \epsilon_t, \tag{1}$$

where $\frac{Shootings_{(t+1),\dots,(t+7)}}{7}$ measures the number of shootings on days $t+1$ until $t+7$ divided by seven. To facilitate the interpretation of coefficients and comparison to alternative time frames for the outcome variable, we predict shootings *per day*. $(Shooting\ news)_t$ captures the number of *ABC news* segments related to mass shootings. The vector $\boldsymbol{X}_t'$ contains linear and quadratic time trends (number of days since January 1, 2006), and fixed effects for each weekday, month, and year.[15]

These covariates aim to isolate our estimation of β_1 from confounding time-specifics. For example, shootings and shooting news may be more frequent on particular weekdays (e.g., because school is in session), months of the year (perhaps because of holiday seasons), and particular years (e.g., see BBC News, 2019). Time trends capture broad developments that may independently influence the occurrence of shootings and media coverage. Finally, ϵ_t denotes the conventional error term, and we employ robust, heteroskedastic-, and autocorrelation-consistent (HAC) Newey–West standard errors accounting for a lag of one day. Additional estimations show that autocorrelation is not a concern (see Table A.6). For all three time series of interest (shootings, shooting news, and disasters), augmented Dickey–Fuller unit root tests (Dickey and Fuller, 1979) comfortably reject the null hypothesis of a unit root at all conventional significance levels.

3.5. Addressing endogeneity

If media coverage of shootings indeed encouraged future shootings, then β_1 should be positive, statistically significant, and quantitatively meaningful. However, endogeneity concerns pertaining to (i) measurement error and (ii) omitted variables can substantially bias β_1. First, given the difficulty of sharply capturing the degree to which people (and especially potential shooters) are exposed to shooting news, there is substantial concern for measurement error. Note that this is in addition to the direct issues related to measurement of television news laid out in Section 3.2. Econometrically, measurement error can introduce substantial attenuation bias, i.e., skewing OLS estimates towards zero (e.g., see Aydemir and Borjas, 2011 and their Fig. 1 or Wooldridge, 2015 more generally).

Second, societal developments may independently affect shooting news and the likelihood of subsequent mass shootings. In fact, omitted variables could bias β_1 positively or negatively. For instance, assume a violent video game was released: This could both spike the public debate about mass shootings (as a potential consequence of the video game) *and* independently lead to mass shootings, thereby biasing β_1 upwards.

However, there is also substantial concern for omitted variables biasing β_1 downwards. For example, localized efforts to prevent mass shootings may both make news headlines (thereby raising shooting-related news) *and* decrease the likelihood of subsequent mass shootings. Large initiatives, such as "Everytown for Gun Safety" or the "Sandy Hook Promise", can attract substantial media attention. In addition, numerous local measures have been put in place to prevent mass shootings around the country (e.g., see Kristof, 2017; Follman, 2022; Riddle, 2022; Kowalski, 2022, or Manjoo, 2022 for related discussions). In turn, there is evidence of shootings being prevented by people who have become alert to warning signs.[16] Thus, local, regional, and national prevention measures could both increase shooting-related news coverage *and* decrease the number of subsequent mass shootings.

As another example of an omitted variable, consider the debate pertaining to gun regulations. Local and regional activists often refer to mass shootings to advocate more restrictive gun regulations, i.e., when gun control enters public debate, shooting-related news may increase (e.g., see Fox, 2016; Oremus, 2017, or Timm, 2017). Yet, at the same time, mass shootings may be prevented *because* of local activists' actions and calls to be careful with guns.[17] Thus, societal efforts to promote (or oppose, for that matter) gun control can both increase shooting-related news *and* decrease the occurrence of subsequent mass shootings (e.g., see Iyengar and Westwood, 2015).

As another omitted variable candidate, consider the *Black Lives Matter* or the *Blue Lives Matter* protests. Such events often draw heavy news coverage — news attention that may otherwise be devoted to discussing mass shootings. At the same time, these protests

[14] Results are consistent when employing an IV Poisson or GMM format (see Table A.9).

[15] In additional specifications, we include two variables measuring the number of shootings on days $t-1$ and t, which produces consistent results (see Table A.9). Accounting for shootings today and yesterday captures potential short-run dynamics, i.e., recent shootings may encourage further shootings, independent of news coverage.

[16] For example, a hotel worker may have prevented a mass shooting by reporting a disgruntled colleague's threats to shoot staff and guests (BBC News, 2020). In another example, the police arrested three middle school students who showed signs of plotting an attack (USA Today, 2019).

[17] This likely matters because many mass shooters have been shown to "obtain guns as gifts from the parents – or borrow or steal weapons from their house" (Thrush, 2022). Peterson and Desnley (2022) write: "In cases involving K-12 school shootings, over 80% of individuals who engaged in shootings stole guns from family members".

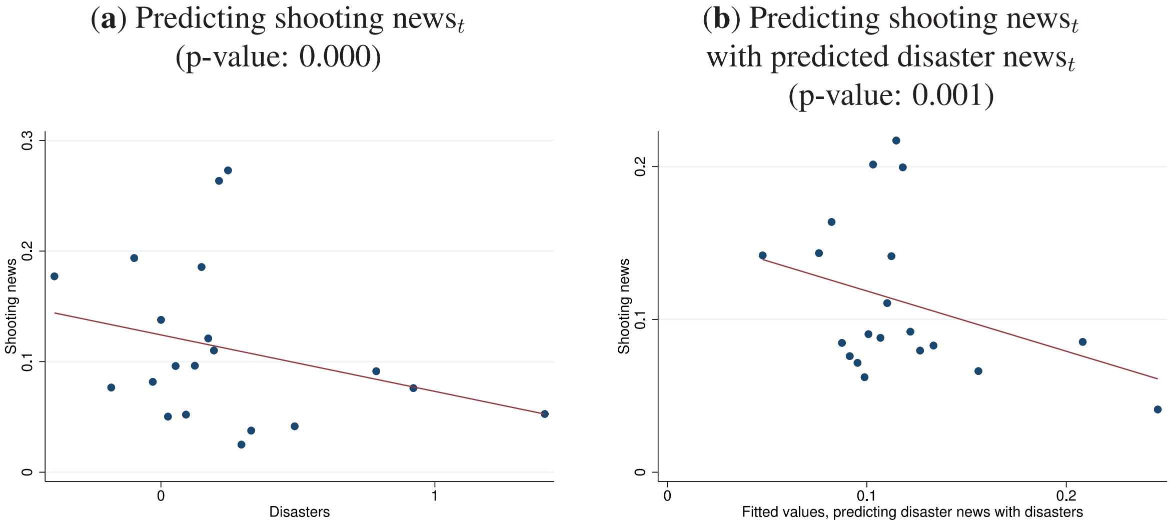


Fig. 2. (**a**) displays a binned scatterplot controlling for all covariates from Eq. (1); (**b**) displays a binned scatterplot, where we first predict disaster news coverage with disasters in the countries of interest, conditional on all covariates from Eq. (1).

can stoke social tensions and the propensity to violence, which could increase the likelihood of mass shootings. In fact, some mass shootings have been linked to hate crimes (e.g., Dylan Roof's church shooting in South Carolina in 2015). Thus, protests may decrease shooting-related news *and* raise the incidence of mass shootings, thereby introducing a downward bias into β_1.

To circumvent these problems, we propose an identification strategy in which $Shooting\ news_t$ are first predicted by the number of disasters discussed in Section 3.3. In general, IV analyses are particularly powerful in their ability to overcome attenuation bias (e.g., see Griliches and Mason, 1972; also see Pischke, 2007). Formally, the first stage becomes

$$\left(Shooting\ news\right)_t = \alpha_0 + \alpha_1 \left(Disasters\right)_t + \boldsymbol{X}_t' \alpha_2 + \delta_t. \tag{2}$$

The predicted $(\widehat{Shooting\ news})_t$ values are then used in the second stage to predict mass shootings on days $t+1$ until $t+7$, following Eq. (1).

3.6. Graphical evidence

3.6.1. First stage

Fig. 2 visualizes two graphs related to the first stage, i.e., the link between disasters abroad and shooting news. Fig. 2(**a**) displays a binscatter graph, where we predict shooting news today with disasters today, conditional on all covariates from Eq. (1). We identify a firm negative relationship that is statistically significant at the one percent level (p-value of 0.000). That result remains consistent if we exclude days with more than one disaster (98 days; p-value of 0.000).[18]

For Fig. 2(**b**), we first predict disaster news (i.e., news segments discussing earthquakes, epidemics, or volcanic activity) with the occurrence of disasters and then plot shooting news as a function of that predicted value, while accounting for all covariates in both regressions. Indeed, we identify diminished shooting news as disaster news coverage (as predicted by disasters abroad) becomes more frequent (p-value of 0.001). Results are consistent if we ignore days with predicted disaster news above the 90th percentile (p-value of 0.001).[19]

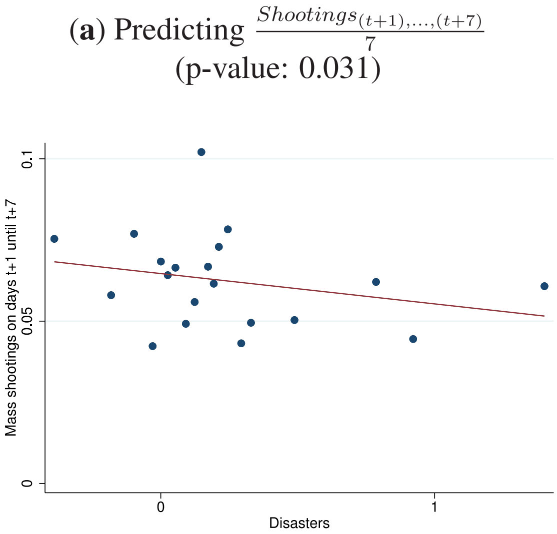
3.6.2. Reduced form

Fig. 3 turns to visual evidence for the reduced form, i.e., the direct link between disasters and subsequent mass shootings. Figs. 3(**a**) and 3(**b**) show binscatter plots that predict the average daily number of mass shootings in the subsequent seven days with the number of disasters today and predicted disaster news today. In both graphs, we account for the full set of control variables from Eq. (1).

Consistent with our hypothesis, we identify a negative relationship that is statistically significant at the one percent level. Here again, removing those days with more than one disaster and days with predicted disaster news above the 90th percentile produces

[18] The coefficient (standard error) associated with the disaster variable in the full specification is −0.051 (0.012). Excluding days with more than one disaster yields a coefficient (standard error) of −0.055 (0.014).

[19] The coefficient (standard error) associated with the disaster news variable in the full specification is −0.394 (0.113). Ignoring days with predicted disaster news above the 90th percentile produces a coefficient (standard error) of −0.418 (0.121).



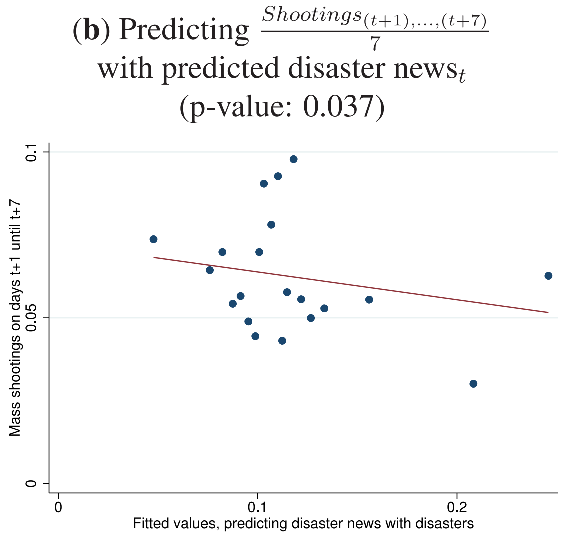


Fig. 3. (**a**) displays a binned scatterplot controlling for all covariates from Eq. (1); (**b**) displays a binned scatterplot, where we first predict the number of disasters with disasters in the countries of interest, conditional on all covariates from Eq. (1).

consistent findings (p-values of 0.000 and 0.001).[20] Thus, days with multiple disasters or extraordinary disaster news are not driving the negative relationship with subsequent shootings.

3.7. Exclusion restriction

In addition to instrument strength, the exclusion restriction constitutes an important assumption of our identification strategy. For example, if shooters were able to predict the disasters we measure here, they may be less likely to strike on such days. Or, if these disasters somehow affected shooters through channels other than a potential suppression of shooting news, the exclusion restriction may be violated.

Table 3 documents results from several regressions to explore that possibility. While these specifications cannot comprehensively prove the exclusion restriction, the corresponding insights may help understand whether disaster days are different along several relevant dimensions. Panel A explores our key independent variable with actual disasters, whereas Panel B turns to disaster news. Column (1) directly predicts the number of mass shootings on day t to see whether shooters can systematically avoid days on which news pressure might be elevated because of disasters. However, we find no evidence for that hypothesis, deriving precisely estimated null relationships in Panel A and Panel B.

In columns (2)–(5), we access *Google Trends* for a variety of online search topics that could potentially be affected by disasters and disaster news. *Google Trends* is increasingly employed to measure society-level interests and how they develop over short time intervals in the absence of social censoring.[21] The service allows downloading day-to-day data on relative search volumes for particular topics, whereby individual searches are grouped under a particular theme.[22]

First, we explore whether disasters abroad affect immigration-related online interest in the subsequent seven days. For example, disasters could trigger anti-immigration sentiment and xenophobia (e.g., see Andrighetto et al., 2014), thereby motivating potential shooters. To investigate that possibility, we predict topic searches for *immigration* and *Stormfront*, the first major racial hate site in the US (e.g., see Levin, 2002). However, we find no statistically meaningful evidence to support that hypothesis when considering disasters in Panel A. For disaster news in Panel B, we only derive a marginally significant coefficient when predicting online searches of *Stormfront* (p-value of 0.082).

Second, it is possible that US Americans become more compassionate when disasters hit, which could also influence potential mass shooters. For example, Eisensee and Strömberg (2007) show that US governmental relief efforts dedicated to disaster victims abroad rise substantially if these disasters are in the news. To explore that possibility, we check whether searches related to *charitable*

[20] The corresponding coefficients (standard errors) in Fig. 3 are −0.009 (0.004) and −0.084 (0.040). Excluding the discussed extreme values (days with more than one disaster and disaster news above the 90th percentile) produces coefficients (standard errors) of −0.012 (0.004) and −0.108 (0.038), respectively.

[21] Examples come from Stephens-Davidowitz (2014) who studies racism in Obama's electoral outcomes or (Jetter and Molina, 2020) who explore political agenda setting in the Philippines.

[22] Data are adjusted to assign the value of 100 to the day with the highest search volume relative to all other *Google* searches on the same day within a given 6-month period. We control for lagged values from yesterday's search interest, as well as fixed effects for each 6-month time period, to account for general time trends and differences across 6-month time periods.

Table 3
Exclusion restriction, predicting shootings on day t (column 1) and various *Google* search aggregates on day t (columns 2–5) with disasters (Panel A) and disaster news (Panel B). All estimations incorporate the full set of covariates.[a]

Dependent variable:	$Shootings_t$	*Google Trends*$_{t+1,\ldots,t+7}$ data for...			
		...immigration	...Stormfront	...charitable organizations	...mental health
	(1)	(2)	(3)	(4)	(5)
Panel A: $Disasters_t$					
Earthquakes, epidemics, and volcanic $eruptions_t$	−0.001	−0.333	0.438	0.020	−0.811***
	(0.009)	(0.358)	(0.463)	(0.342)	(0.233)
Panel B: Disaster $news_t$					
Earthquake, epidemic, and volcano $news_t$	−0.004	1.152	6.704*	4.118	1.799
	(0.087)	(3.098)	(3.855)	(2.931)	(2.175)
N	4376	4369	4369	4369	4369
R^2 (Panel A)	0.004	0.841	0.374	0.591	0.610
R^2 (Panel B)	0.004	0.876	0.562	0.688	0.719

Notes: Newey–West standard errors are displayed in parentheses (lag of one day). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ indicate conventional levels of statistical significance at the ten, five, and one percent level.

[a]Includes a linear and quadratic time trend, as well as fixed effects for each weekday, month, and year. Columns (2)–(5) also incorporate fixed effects for every 6-month interval to accommodate the fact that *Google Trends* only reports daily data for 6-month intervals, as well as the lagged dependent variable.

organizations rise significantly on disaster days. Importantly, that includes all searches *Google* would categorize as being related to *any* charitable organization, such as the *Red Cross*. However, we find no evidence to support that hypothesis.

Finally, column (5) seeks to understand whether online interest in mental health topics is altered on disaster days. For instance, being faced with the tragedy of a disaster might diminish the mental health of some people (e.g., see Fergusson et al., 2014). In this case, we actually identify a *negative* coefficient, indicating mental health-related searches drop significantly after disasters abroad. This result is consistent with the idea of people actually searching online for topics related to the respective disaster, which leads to a relative drop in online interest of mental health-related issues. For disaster news in Panel B, however, we derive a statistically insignificant coefficient. Overall, these specifications from Table 3 cannot fully dissolve concerns about the exclusion restriction, of course, and should be viewed as basic checks only.

4. Empirical findings

4.1. Main results

Table 4 presents our main findings that include the full set of covariates, beginning with a conventional OLS estimation of Eq. (1), followed by various IV specifications. Panel A displays the estimates associated with shooting news in predicting mass shootings in the subsequent seven days. Panel B documents first-stage coefficients, and Panel C reports various relevant test statistics.

The OLS regression produces a precisely estimated null coefficient, indicating no meaningful correlation between shooting news and subsequent mass shootings. Not only is the coefficient statistically indistinguishable from zero (p-value of 0.912), the implied magnitude of −0.000 also indicates no quantitative relevance. Were we to stop here, the hypothesis put forward by criminologists, psychologists, and numerous commentators linking media attention to mass shootings would receive no empirical support.[23]

Column (2) displays our benchmark IV result where shooting news are predicted by the discussed disasters in the first stage. Shooting news now become a positive, statistically significant, and quantitatively powerful predictor of mass shootings. The corresponding magnitude indicates a one standard deviation increase in shooting news (0.389) translates to an increase in the number of shootings by approximately 73% of a standard deviation. The corresponding statistics in Panels B and C suggest a powerful first stage with the expected negative coefficient associated with disasters as a predictor for shooting news. The corresponding F-statistic satisfies a number of the corresponding test thresholds for IV strength (but only remains statistically significant at the 90% level when adjusting standard errors, following Lee et al., 2021). Finally, shooting news are indeed endogenous from a statistical perspective, confirming our intuitive concerns outlined earlier.

In column (3), we split the IV into its components of earthquakes, epidemics, and incidents of volcanic activity. This allows us to test for overidentification (see Panel C) and understand whether each disaster type is indeed able to crowd out shooting news on its own. The results pertaining to shooting news in the second stage closely follow those from column (2).

Considering the intensity of mass shootings, column (4) predicts the number of casualties. Although our baseline results suggest the *frequency* of shootings increases, policymakers and members of society ultimately also care about the deadliness of such events.

[23] As an aside, if we descriptively explore the number of shootings in the week right after the most deadly mass shootings in US history (see Wikipedia, 2022 and the associated discussion in Section 4.4), we derive more such events than in an average week across our sample. Specifically, we find a mean of 0.117 mass shootings per day in that week, which constitutes almost double the sample average of 0.063. The corresponding difference in means is not quite statistically significant at the five percent level, however, with a p-value of 0.059.

Table 4
Main results, predicting the number of mass shootings per day in the subsequent 7 days (except column 4, where we predict the number of deaths from mass shootings). All estimations incorporate the full set of covariates.[a]

Estimation method:	OLS	IV				
		Main estimation	Splitting instruments	Predicting deaths	*GVA* data[i]	*Brady* data
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of dependent variable:	0.062	0.062	0.062	0.341	0.863	0.067
Panel A: Predicting shootings per day on days $t+1$ until $t+7$; exception: column (4)						
Shooting news$_t$	0.000	0.183**	0.183**	1.235***	3.021**	0.289***
	(0.004)	(0.076)	(0.076)	(0.457)	(1.220)	(0.111)
Panel B: 1st stage results, predicting shooting news on day t						
Earthquakes, epidemics, and volcanic eruptions$_t$		−0.051***		−0.051***	−0.039*	−0.045***
		(0.011)		(0.011)	(0.022)	(0.011)
Earthquakes$_t$			−0.049**			
			(0.021)			
Epidemics$_t$			−0.051***			
			(0.016)			
Volcanic eruptions$_t$			−0.054**			
			(0.023)			
Panel C: Statistical properties						
F-test insignificance of IV		22.130***	7.414***	22.130***	7.183**	18.173***
Weak IV test (Wald, p-value)[b]		0.017**	0.016**	0.007***	0.013**	0.009***
Effective F-statistic[c]		22.130	7.567	22.130	6.084	16.407
Stock–Wright S statistics (p-value)[d]		0.004***	0.005***	0.000***	0.000***	0.001***
Kleibergen–Paap rk LM statistic (p-value)[e]		0.000***	0.000***	0.000***	0.008***	0.000***
Adjusted t-statistic of shooting news$_t$[f]		1.865*	1.127	2.093**	1.129	1.903*
Endogeneity test (p-value)[g]		0.005***	0.005***	0.001***	0.000***	0.002***
Hansen J-statistic (p-value)[h]			0.225			
N	4376	4376	4376	4376	1820	2581
R^2 (OLS/2nd stage)	0.012	0.014	0.014	0.023	0.320	0.241
R^2 (1st stage)		0.051	0.051	0.051	0.063	0.059

Notes: Column (1): Newey–West standard errors are displayed in parentheses (lag of one day). Columns (2)–(6): IV estimations are conducted using robust, heteroskedastic-, and autocorrelation-consistent (HAC) standard errors (option $r\,bw(1)$ in Stata). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ indicate conventional levels of statistical significance at the ten, five, and one percent level.
[a]Includes a linear and quadratic time trend, fixed effects for each weekday, month, and year, as well as variables measuring the number of shootings on days t and $t-1$.
[b]Following Magnusson (2010), we apply the *weakiv* command in Stata to test for weak instruments.
[c]The 1st stage effective F-statistic is computed following Olea and Pflueger (2013).
[d]Following Stock and Wright (2000).
[e]Presenting results from under-identification tests, following Kleibergen and Paap (2006).
[f]Calculating *tF* confidence intervals according to Table 3 of Lee et al. (2021).
[g]Following Hayashi (2000) and Wooldridge (2015), we test for whether endogenous regressors are in fact exogenous. Statistical significance indicates variables must be treated as endogenous.
[h]Presenting results from over-identification tests.
[i]Excludes month-fixed effects (also see footnote 24 in text).

The results again produce a robust positive coefficient, indicating significantly more deaths from mass shootings when shooting news are prevalent. If anything, the corresponding results are more precise in statistical terms.

In columns (5) and (6), we turn to alternative databases related to mass shootings with the *GVA* and the *Brady Campaign*. Although sample sizes diminish in both estimations due to data availability, we again derive positive and statistically significant coefficients in the second stage in both cases (p-values of 0.013 and 0.009).[24] These results are reassuring, illustrating that our main insights are not exclusive to employing data from the *USAT*.

4.2. Robustness checks and extensions

In the appendix, Table A.11 reports results from additional estimations, where we consider alternative measures for the IV, mass shootings, and shooting news. The relevant summary statistics are referred to Table A.2. In these specifications, we (i) adjust disasters by the population size of the respective country (i.e., measuring disasters per capita) and then (ii) exclude volcano activity from

[24] Note that our estimation using data from the *GVA* excludes month-fixed effects because the sample period is much shorter (from 2013 to 2017), and the corresponding IV estimation that includes month-fixed effects does not produce a statistically significant first stage (F-statistic of 3.31). Nevertheless, that estimation still produces a positive coefficient associated with shooting news in the second stage that is close to reaching conventional levels of statistical significance (p-value of 0.107).

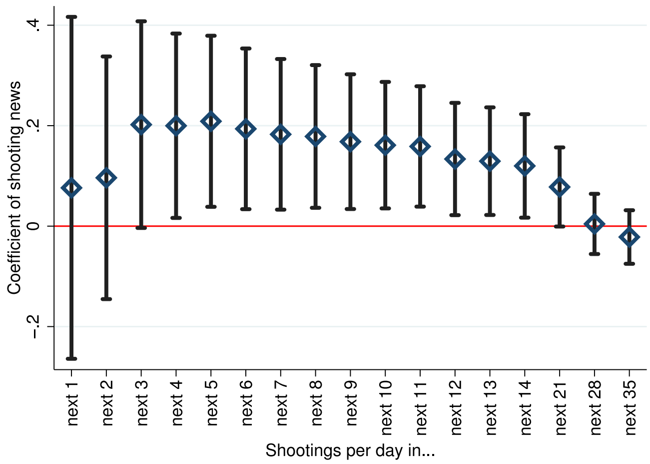


Fig. 4. Predicted additional shootings for various time frames of the dependent variable. Each point represents one IV regression outcome, and all regressions include the control variables presented in Eq. (1) and Table 4. Two-sided 95 percent confidence intervals are displayed.

the list of relevant disasters (as this constitutes the weakest predictor of diminished shooting news). These specifications explore whether results are sensitive to the initial disaster definitions.

Further, we vary the definition of the dependent variable by (iii) capturing a binary indicator for any mass shooting in the subsequent seven days. This estimation ensures we are not over-emphasizing particular 'shooting-heavy' time periods that could perhaps drive results. Next, we consider various alternative measures of shooting news by (iv) adding various keywords to first identify news segments that are potentially related to shootings (before manually removing false positives); (v) employing an alternative measure for shooting coverage by calculating the natural logarithm of the total seconds of shooting-related news that day[25]; (vi) extending our news measure to include the *CBS Evening News*, *NBC Nightly News*, and *CNN* to form an average daily coverage across all four programs; (vii) not removing any news segments to ensure our manual removal of news segments is not crucial to results.

Finally, rather than counting news *segments*, we $(viii)$ count how many times *shoot* is mentioned in abstract and headline; (ix) count the news segments mentioning *shoot* in the headline *or* abstract; and (x) predict a binary independent variable to capture seven-day periods that feature at least one shooting-related news segment. The corresponding results from all these regressions produce positive and statistically significant coefficients, suggesting (i) our results are robust to a variety of alternative news measures and (ii) both the intensive and extensive margins of shooting news appear to matter for potential mass shooters.

4.3. Time frame of subsequent shootings

Next, Fig. 4 returns to our benchmark specification and focuses on the time frame of subsequent shootings. We visualize second-stage coefficients from 17 unique 2SLS estimations that only differ in the horizon of the outcome variable. We conduct this exercise because our initial choice of a seven-day time window for subsequent shootings may well be considered arbitrary, as it remains difficult to theorize *how long* a potential shooter would need to move from observing shooting news to engaging in a shooting themselves. The corresponding descriptive studies suggest substantial differences in planning horizons (e.g., see Vossekuil, 2004; Kissner, 2016; Gill et al., 2017; Langman, 2017; Capellan and Gomez, 2018, and Silver et al., 2018). Varying the time horizon also allows us to explore whether our findings from Table 4 are specific to a seven-day time window.

The results displayed in Fig. 4 suggest a consistent effect with the derived coefficient ranging from 0.12 to 0.21 when moving the time window of subsequent shootings between three to fourteen days after the initial coverage. We then extend the time horizon further up to 21, 28, and 35 days. The derived coefficients decrease in magnitude and eventually turn statistically indistinguishable from zero after 3–4 weeks.

4.4. Mechanisms

With those results in mind, we now aim to delineate potential mechanisms that would connect shooting news to subsequent shootings. In particular, the associated literature suggests (i) ideation, (ii) fame seeking, and (iii) a behavioral threshold model based on contagion in this context (see Section 2.2). Table 5 documents results from several specifications along those lines.

Column (1) draws on the ideation-based narrative: If mass shootings generally rendered murder salient, we should see some relationship between shooting news and murders in general. Thus, we predict the average number of murders in the subsequent

[25] To preserve days with zero coverage, we set this to Ln(1+ shooting-related seconds).

Table 5
Exploring mechanisms. All estimations incorporate the full set of time-specific covariates.[a]

Estimation method:	IV			OLS		
Dependent variable:	Murders/day on days $t+1$ until $t+7$	Public shootings/day on days $t+1$ until $t+7$	Family shootings/day on days $t+1$ until $t+7$	Shootings/day on days $t+1$ until $t+7$	Murders/day on days $t+1$ until $t+7$	Shootings/day$_t$
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of dependent variable:	10.768	0.016	0.042	0.062	10.768	0.063
Shooting news$_t$	−0.505	0.104***	0.240***	0.000	0.033	
	(0.997)	(0.040)	(0.070)	(0.004)	(0.069)	
Shooting anniversary$_t$[c]				0.017**	−0.125	
				(0.008)	(0.109)	
Major event$_t$[b]						−0.000
						(0.019)
N	4376	4376	4376	4376	4376	4383
R^2 (OLS/2nd stage)	0.655	0.044	0.035	0.014	0.655	0.004
R^2 (1st stage)	0.050	0.050	0.050			

Notes: Columns (1)–(3) display robust, heteroskedastic-, and autocorrelation-consistent (HAC) standard errors in parentheses (option $r\ bw(1)$ in Stata). Columns (4)–(6) report Newey–West standard errors in parentheses (lag of one day). * $p<0.10$, ** $p<0.05$, *** $p<0.01$ indicate conventional levels of statistical significance at the ten, five, and one percent level.

[a] Includes a linear and quadratic time trend, as well as fixed effects for each weekday, month, and year.

[b] Binary indicator equal to one if any of the following events is ongoing: The Super Bowl, the FIFA World Cup, the Olympic Games (summer or winter), the Academy Awards, or a G7/G8 Meeting.

[c] Binary indicator for anniversaries of the 18 deadliest mass shootings in US history (see Wikipedia, 2022).

seven days, employing the same 2SLS method as before. However, we identify a statistically insignificant coefficient, yielding no support for a purely ideation-based mechanism. This is also the case in an OLS regression (see column 5).

Columns (2) and (3) then distinguish types of mass shootings between those that are primarily categorized as public from those categorized as family (see Overberg et al., 2019). Interestingly, both types appear to be affected by shooting news, which further emphasizes the generality of the link between shooting news and mass shootings.

In column (4), we return to the benchmark dependent variable of mass shootings but incorporate a binary indicator for anniversaries of infamous mass shootings in a basic OLS structure.[26] We posit that these events serve as powerful primers of mass shootings to the general population, as they may be mentioned in memorials, local, regional, or national news, as well as in private communications, online forums, or social media outlets, for instance. Thus, the corresponding anniversaries possibly render mass shootings salient in the minds of the general populace and potential shooters. The results reported in column (4) show that variable to be a positive and statistically precise predictor of further mass shootings. That evidence would be consistent with a mechanism based on fame but also with a narrative that supports a behavioral threshold model by which the salience of mass shootings may produce a contagious effect (see Granovetter, 1978). Here also, that variable remains statistically insignificant in predicting subsequent murders (see column 5), yielding no support for a pure ideation-based mechanism.

Finally, column (6) turns to predictable events that would commonly produce substantial news pressure, which would in turn leave less airtime to potential shootings. Specifically, we code binary indicators for those days on which we would predict the news to be full, capturing the Super Bowl, the FIFA World Cup, the Olympics (summer and winter), the Academy Awards, and G7/G8 Meetings (similar to Jetter, 2019). If fame was a primary motivator of shooters, we would expect to see fewer shootings on such days. However, the corresponding estimate produces no statistical or quantitative evidence along these lines, which speaks against fame being a main driver of our results.

Taken together, the results from Table 5 are consistent with a behavioral contagion model along the lines of Granovetter (1978) and Gladwell (2015) but provide less support for ideation or fame seeking as primary drivers. Nevertheless, these specifications should be seen as basic steps towards delineating mechanisms but should not be taken as conclusive evidence; while the quantity of shooting news does seem to matter, sharply identifying mechanisms remains an important question for future research.

5. Caveats and avenues for future research

Of course, our study is not without caveats, and we want to briefly discuss what we believe to be the main ones. First, we capture (and manually check) those news segments whose headline contains keywords that are used to describe mass shootings. Thus, we may still miss some news segments that discuss shooters (false negatives), and measurement error remains a concern. Nevertheless, our IV approach should be able to alleviate much of the attenuation bias we may expect in the corresponding OLS estimates.

Second, our study focuses on *ABC World News Tonight* as a representative television news program, with additional estimations incorporating the *CBS Evening News*, *NBC Nightly News*, and *CNN*. Thus, we do not capture the *entire* universe of news communications

[26] We draw on Wikipedia (2022) for an objective source to define the deadliest mass shootings in the US since 1949. That list includes 18 mass shootings before or during our sample period, and we code a binary indicator for their anniversaries, beginning with the first year after the respective event.

in the US and the associated news attention dedicated to shootings, let alone dynamics associated with social media that have likely become more relevant in recent years. This, again, would suggest attenuation bias in OLS coefficients but is something that our IV structure would address.

Third, our IV results produce a *local* average treatment effect, i.e., our analysis relies on the assumption that unexpected disasters in countries with a substantial number of US emigrants crowd out at least some shooting-related news. While we find robust empirical evidence for this assumption, it is likely that not *all* shooting news would follow that dynamic. For example, breaking news about the Sandy Hook massacre are unlikely to be removed from television news because of an earthquake in Costa Rica. In fact, in additional estimations we find that these disasters abroad are not able to crowd out shooting news *if a mass shooting also occurred that day* (see Fig. A.1). Thus, our IV likely operates for marginal shooting news segments but not the most urgent ones.

Fourth, we study the quantity of shooting news, but our IV strategy is unable to tease out causal relationships regarding the *quality* or *content* of shooting news. An important question relates to whether perpetrators are named and pictured on television or whether coverage focuses on victims, for example. Unfortunately, this is where our identification strategy reaches its limits. Anecdotal and descriptive accounts suggest shooting news should limit attention devoted to the shooter in order to deny them the fame they presumably seek (Lankford, 2016; Langman, 2018). We hope future research can delineate between such content-specific attributes.

6. Conclusions

Although theories nested in criminology and psychology have long suggested shooting news to encourage subsequent mass shootings, we believe this paper presents the first empirical approach to systematically test for a causal relationship. Our findings from using unpredictable disasters in countries closely associated with the US as an exogenous variation suggest shooting news may indeed cause future mass shootings. Estimates differ from those derived by a simple correlational analysis, in which we identify a null effect. News coverage seems to systematically predict mass shootings for up to 3–4 weeks after which the effect reverts back to statistical and quantitative insignificance.

What can policymakers and media representatives take away from this study? First, our results advise journalists to report less on mass shootings. Self-imposed reporting guidelines are employed for other incidents to prevent unintended consequences, such as suicides (e.g., see Pirkis et al., 2006, King, 2010, or Reporting on suicide, 2017). Perhaps the associated informal rules could serve as a starting point to discuss media coverage of mass shootings. Nevertheless, such measures have to be carefully weighed against potential benefits from news coverage that lay beyond our study here, such as public awareness or drawing political attention to mass shootings. Second, our results also explain (at least in part) why shootings sometimes cluster in short intervals (e.g., see Towers et al., 2015). Thus, police and other security forces may be well advised to be alert on and after days of heightened media coverage of mass shootings. Overall, we hope this study helps shed light on the unintended externalities of covering mass shootings, in addition to stimulating further research into identifying the consequences of mass shooting-related news coverage.

Appendix figures

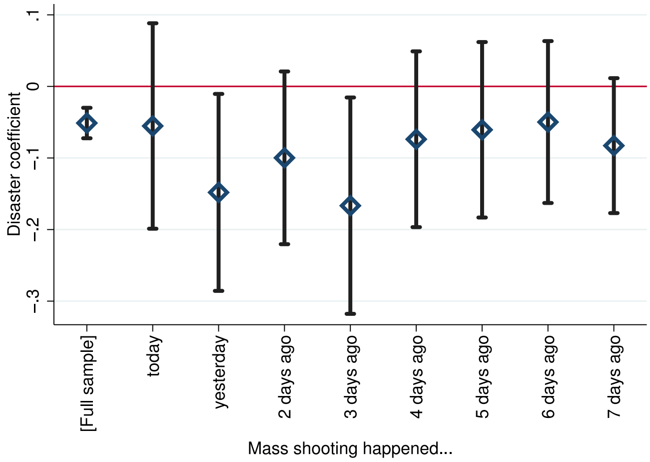


Fig. A.1. Displaying regression results from predicting shooting news$_t$. Each coefficient comes from an independent OLS regression that accounts for the full set of time-specific control variables (a linear and quadratic time trend and fixed effects for each weekday, month, and year). Two-sided 95 percent confidence intervals are displayed.

Appendix B. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.euroecorev.2022.104221.

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