Guns, Privacy, and Crime*

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Abstract

Anecdotal evidence suggests that online information about potential victims is being exploited to plan and conduct offline crimes. For instance, Twitter feeds, Facebook status updates, and online foreclosure listings have been linked to alleged changes in burglars' behavior. We investigate the effect of the online publication of personal information of handgun carry permits holders on criminals' propensity to commit crimes. In December 2008, a Memphis, TN newspaper published a searchable online database of names, zip codes, and ages of Tennessee handgun carry permit holders. We use detailed crime and handgun carry permit data for Memphis to estimate the impact of publicity about the database on different types of crime. We find that crimes more likely to be affected by knowledge of gun ownership - such as burglaries - increased more significantly, after the database was publicized, in zip codes with fewer gun permits, and decreased in those with more gun permits. We find no comparable effect for crimes that are usually not premeditated, like assaults or shootings, or in nearby areas and comparable cities that were not covered by the published database. Our findings provide suggestive evidence of criminals' usage of online tools for offline crimes.

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1 Introduction

A small but growing body of anecdotal evidence is emerging, suggesting that online information about potential victims is being exploited to plan and conduct *offline* crimes. Twitter feeds,[1] Facebook status updates,[2] and online foreclosure listings,[3] have, allegedly, been used to target vacant dwellings and precisely time burglaries based on the victims' schedules and locations. Websites such as `www.icanstalku.com` and `www.pleaserobme.com` have alerted users of online social networks of the offline dangers of online oversharing. For instance, photos captured with most smart-phone cameras contain geolocation meta-data that reveals the exact location where each photo was taken; once uploaded to - say - a Twitter feed, they may be used to infer whether the uploader is currently away from home. So far, evidence for such Internet-driven crimes is merely episodic and hardly significant: for instance, the alleged Facebook "burglary ring" prying on victims through their status updates[4] turned out to be an isolated case of two individuals accused of "burglarizing the house of [a] Facebook 'friend' after she posted a message [that] she would be out at a concert that night."[5] However, according to a British insurance company's recent study, 12 per cent of surveyed former criminals claimed to have "used social networking sites to do their research" before committing a crime.[6] In this manuscript, we exploit a natural experiment which took place in Memphis, Tennessee between 2008 and 2009 to investigate the likelihood that traditional, offline crimes can be significantly affected by information that criminals may find online.

In December 2008, the Commercial Appeal (a newspaper in Memphis, Tennessee) made available on its website a searchable database of names, addresses, and ages of all Tennessee handgun carry permit holders.[7] Two months after its publication, after a shooting incident near a Memphis shopping mall, the database came under public scrutiny. The National Rifle Association (NRA)'s Institute for Legislative Action, alerted of its content, orchestrated a campaign against the newspaper, drawing even more publicity to its database. The Commercial Appeal was flooded "with calls and e-mails demanding the database be removed on the

[1] "'Bling ring' on trial for Hollywood celebrity burglaries," *The Observer*, Paul Harris, January 17, 2010.

[2] "Hoover Police officers arrest Facebook burglary suspects," *NBC13*, Shannon Delcambre, July 31, 2009; "Burglars said to have picked houses based on Facebook updates," *New York Times Bits blog*, Nick Bilton, September 12, 2010.

[3] "Homes in tax foreclosure property listings attract crime," *Real Estate Pro Articles*, John Cutts, January 11, 2010.

[4] "NH burglary ring found victims on Facebook," *FoxNews.com*, September 10, 2010.

[5] "Second in 'Facebook burglary' case arrested," *NewsAndTribune.com*, Matt Thacker, September 27, 2010.

[6] "Burglars using Twitter and Facebook to 'case the joint'," *The Telegraph*, Harry Wallop, July 20, 2010.

[7] At the time of writing, the database is still accessible at `http://www.commercialappeal.com/data/gunpermits/`.

grounds that it [was] an invasion of privacy."[8] The NRA argued that the newspaper's decision to publish the list of permit holders had put law-abiding gun owners at risk. A lobbyist for the NRA said, "[W]hat they've done is give criminals a lighted pathway to [burglarize] the homes of gun owners."[9] Using the words of the late Charlton Heston, the NRA claimed that the risks extended to non-permit-holders: "[T]he essence of Right-to-Carry is that in a world where wolves cannot distinguish between lions and lambs, the whole flock is safer."[10] The Tennessee newspaper that released the data responded by rhetorically asking, in an editorial, whether criminals checking the permit-to-carry list before picking a target "would [be] likely [to] choose a house where they know the owner could be carrying a gun, or would they more likely steer away from that house to avoid a possible confrontation?"[11] Invoking the First Amendment of the US Constitution, the Commercial Appeal noted that the publication of the database had drawn attention to who, in the community, carries "concealed weapons."[12]

Did the online publication of gun permit holders' information deter, or increase, certain types of crimes? Or did it simply displace crime from one area to another? We investigate this question using detailed crime and handgun carry permit data for Memphis and nearby areas, from before and after the newspaper's publication of the permits. We evaluate how incidences of different kinds of crime changed before and after the database was published and publicized, as a function of the number of guns in a zip code. Our analysis suggests a post-publicization relative decrease - both in absolute and in percentage terms - in the types of crimes likely to be affected by knowledge of the data publicized (in particular, burglaries) in zip codes with higher numbers of gun permits, relative to zip codes with median numbers of permits, and a post-publicization relative increase in zip codes with fewer gun permits.

We model the relationship between gun permits and crimes using both continuous and non-parametric specifications, and we test numerous variations of our primary specification. Across all specifications, in order to control for the spatial dependency that may exists for crimes across zip codes, we use cross-sectional averages (for each time period) of the moment conditions and rely on asymptotics in the time dimension to yield consistent estimators for

[8]'Tennessee bills focus on gun owners,' *The Commercial Appeal*, Richard Locker, February 13, 2009.

[9]'Armed and dangerous: Dozens with violent histories received handgun carry permits,' *The Commercial Appeal*, Marc Perrusquia, March 12, 2009.

[10]NRA News Release, February 10, 2009, at `http://www.nraila.org/News/Read/NewsReleases.aspx?id=12123`, accessed on May 21, 2010.

[11]'Inside the Newsroom: Case for gun-permit listings trumps emotional opposition,' *The Commercial Appeal*, Chris Peck, February 15, 2009.

[12]Under Tennessee law, handgun carry permits do not actually require citizens to conceal their firearms. The Commercial Appeal was probably implying that many permit holders carry their handguns concealed.

the spatial covariance structure, as suggested in Driscoll and Kraay (1998). We test our hypotheses using absolute counts of permits and crimes, and then normalizing dependent and independent variables by the number of dwellings in a zip code. We test both OLS and negative binomial specifications, to account for our use of count data. We test log-normalized versions of our model, to account for the fact that, while the number of crimes may change more dramatically in zip codes with more guns, the percentage changes in crimes may not differ across zip codes with different numbers of guns. Our main results are consistent across all specifications. Furthermore, to control for the complex patterns of crime raises and falls across zip codes, we add zip code-specific cubic time fixed effects; our results remain consistent also under this specification.

In order to exclude alternative explanations for the relationship we observed, we ran multiple robustness checks. We find no significant changes for the types of crimes that are usually not premeditated, such as assaults or shootings. We find no comparable changes in border counties in states neighboring Memphis that were not covered in the published database (such as DeSoto County, MS, and West Memphis, AR), or in similar southern metropolitan cities (such as Jackson, MS, and St Louis, MO). We also find no strong evidence of a link between the number of burglaries and the number of issued permits that were too new to be included in the online database, and that therefore were not visible to the newspaper's readers.

Our finding that the publicization of gun permit records was linked to localized changes in the number of crimes provides suggestive evidence of criminals' usage of online tools for offline crimes. It also carries direct implications for a heated political debate. In the United States, some of the strongest regulatory protections of privacy are those afforded to gun owners and dealers.[13] As the Electronic Privacy Information Center (EPIC)'s page on "Gun Owners Privacy" notes, "[w]hile it is possible for a person to legally purchase a firearm on the secondary market without revealing personal information, it is not possible for the same individual to open a U.S. bank account without providing personally identifying information, including name, address, date of birth, and often social security number, which is retained indefinitely for later verification purposes."[14] Due to this particular controversy over the gun permit database, four bills were filed in the state of Tennessee to protect the identities of gun-carry permit holders, and to make it a crime for anyone to publish their names.[15] At the time of writing, 19 states allow gun permit information to be made public, and 21 states

[13]See, for instance, *Bureau of Alcohol, Tobacco, and Firearms v. City of Chicago.*

[14]At `http://epic.org/privacy/firearms/`, accessed on May 21, 2010.

[15]'Tennessee bills focus on gun owners,' *The Commercial Appeal*, Richard Locker, February 13, 2009.

keep that information confidential.[16] As noted by EPIC, "[g]un ownership organizations [...] argue that the release of information about licensed concealed handgun holders may create a larger illegal secondary market for gun resale, which in turn would create a more dangerous society."[17] Our results suggest that, despite activism on the part of gun owners against the publication of such databases, it may actually be gun permit holders who benefited from publicization, relative to people who do not hold gun permits, or relative to people living in areas with a lower number, or density, of gun permits. Furthermore, we found no evidence that publishing the identities of gun permit holders led to an increase in crimes aimed at stealing their weapons, relative to other forms of theft or burglaries.

Our findings link two different streams of academic literature.

First, they contribute to the economics and criminology literature on the role of guns in either preventing or spurring crime (and, in particular, burglaries: Kopel (2001); Cook and Ludwig (2003)). This literature has been marked by conflicting results. Lott and Mustard (1997), and then Lott (2000), found a relationship between a reduction in violent crime and a concealed weapons law. As Ayres and Donohue (2003) noted, Lott and Mustard's work triggered "an unusually large set of academic responses, with talented scholars lining up on both sides of the debate." The theoretical underpinnings of a "more guns, less crime" argument rely on a deterrence effect, whereas unobservable precautions by ordinary citizens (such as carrying concealed weapons) should make criminals more cautious about engaging in crime. The counterarguments focus on the possibility that "shall-issue" laws (under which the authority granting permits to carry concealed guns has no discretion in the awarding of said permits) may increase both the number of criminals carrying weapons, and the speed at which they decide to use them on potential victims (Ayres and Donohue, 2003). Furthermore, the presence of guns may escalate otherwise resolvable conflicts, and also may increase the likelihood that guns may fall into the hands of criminals. Lott and Mustard (1997)'s results have since been disputed in research by Black and Nagin (1998), Ayres and Donohue (2003), and Levitt (2004), who found little evidence to support the hypothesis that right-to-carry laws reduce violent crime. Duggan (2001), using gun magazine subscription rates as a proxy for gun ownership, found that an increase in gun ownership was associated with an increase in homicides. On the other hand, Bronars and Lott (1998) presented evidence of a displacement effect of crime in counties bordering states that enacted shall-issue, concealed-carry licensing laws.

[16]'Gun Database Ignites Debate in Tennessee,' *The Associated Press*, March 1, 2009.

[17]At `http://epic.org/privacy/firearms/`, accessed on May 21, 2010.

This debate has remained contentious. In 2004, a report by the Committee on Law and Justice of the National Academy of Science concluded that, despite the wealth of research in the area, "no credible evidence [was found] that the passage of right-to-carry laws decreases or increases violent crime" (see Wellford et al. (2005), which also contains an overview of the literature). The committee also concluded that "the data available on these questions are too weak to support unambiguous conclusions or strong policy statements." Our contribution to this literature consists in estimating the impact of *information* about the location and numbers of gun permit holders being made publicly available. The informational shock represented by the publication and publicization of gun permit holders' data allows us to address one of the challenges faced by previous studies, which have argued that changes in gun ownership rates deter criminals, but have not been able to specify the mechanism by which criminals themselves were aware of the changing gun rates that the researchers study. Our study focuses on the very events of publication and publicization of gun permit ownership, so we directly study one mechanism for potential offenders to be aware of gun ownership rates, and therefore for guns to affect crime. Criminals may infer from the published data the probability of encountering armed resistance when committing certain crimes in a given location; this, in turn, should influence their propensity to commit the crime in that location. In this respect, our manuscript is also related to the economics literature on crime and criminals' decision making (Becker, 1968).

Second, our manuscript is related to the literature and debate over the boundaries and connections between privacy and security, and, specifically, the stream of research on the so-called economics of privacy (Stigler, 1980; Posner, 1981). Real or alleged trade-offs between privacy and security have been highlighted in disciplines as diverse as computer science (Demchak and Fenstermacher, 2004), law (Harris, 2006), and public policy (Kleiman, 2002). This debate has often intersected with the discussion of the private costs (in terms of loss of confidentiality) versus the public benefits of the dissemination of governmental data (Duncan et al., 1993). Some argue that privacy and security are part of a "zero-sum game."[18] According to this view, for certain crimes to be prevented (for instance, acts of terrorism), it is necessary to monitor individuals' activities closely. On the opposite side, it is argued that a society does not have to "accept less of one to get more of the other [..] Security affects privacy only when it is based on identity."[19] Frequently, in this debate, *personal* privacy is

[18]Ed Giorgio, an NSA security consultant, as quoted in a New Yorker's article in January 2008: `http://www.newyorker.com/reporting/2008/01/21/080121fa_fact_wright?currentPage=all`.

[19]'Security vs. Privacy,' *Schneier on Security*, Bruce Schneier, January 29, 2008, `http://www.schneier.com/blog/archives/2008/01/security_vs_pri.html`.

contrasted to *collective* security. For instance, the personal (i.e. private) benefit an individual enjoys when her privacy is protected may come at the collective (i.e. societal) cost of less security: Consider, as an example, the case of public databases of convicted sex offenders.[20] However, nuanced trade-offs also arise in the context of personal privacy and personal security: When an individual's personal data is intruded upon (for instance, her bank account balance is exposed), that individual may become more vulnerable to crime, because criminals gain information that can motivate or direct an attack. In other situations, the opposite may happen: criminals may use personal data to choose which potential victims to *avoid*. Other times, the very lack of data may favor, or damage, the individual. The debate gets, if anything, thornier when – instead of trading off privacy for security – a more ethereal call for "transparency" pits against each other the privacy rights of the individual and the collective, societal right to know. Consider, for instance, databases with information about salaries of public officials,[21] or databases listing personal finances of congressional members,[22] or the above-mentioned case of databases of sex offenders.

Our results bear witness to the nuances of this debate. When personal data listing gun permit holders' names and locations are made public, potential criminals might be deterred from initiating criminal acts against gun permit holders, knowing beforehand that a person is likely to be armed (the argument the Commercial Appeal invoked in defending its decision to publish the TN permits database). On the other hand, criminals may also use that information to identify individuals to steal guns from (the argument adopted by the NRA to attack that decision), or, more indirectly, to target individuals whose personal information did *not* appear in the database (and who were therefore less likely to own, and be ready to use, a gun).[23] In a seminal article, Posner (1981) argued that privacy, in an economics sense, should be interpreted as the concealment of personal information, and that such concealment, even when intended to protect the subject, "is surely an inefficient method of insurance; rather than spread costs widely, it shifts them from one small group to another." In the scenario we investigate, however, a related but different dynamic seems to be occurring: It is the *revelation* of personal information (the identity and location of permit holders) that may affect how costs (various types of crimes) are spread, and the individual may suffer or benefit even if (or precisely because) her identity has not been revealed.

[20] See, for instance, `http://www.nsopr.gov/`.

[21] See, for instance, `http://db.lsj.com/community/dc/som/index.php`.

[22] See, for instance, `http://www.opensecrets.org/pfds/index.php`.

[23] In Section 3, we discuss how a growing body of anecdotal evidence suggests that criminals - and, in particular, burglars - have started using the Internet and online social media tools to target potential victims.

In short, the Commercial Appeal controversy highlights issues arising from the interaction of First and Second Amendments, as well as the challenges raised by databases of personal information already available under old information technologies,[24] but now "too" available (Varian, 1996) in our Internet age.

2 Institutional Details and Data

2.1 The Publishing of Tennessee's Handgun Carry Permit Database

In October of 1996, the Tennessee Department of Safety began issuing "shall-issue" handgun carry permits pursuant to Public Chapter 905. Prior to this change, handgun carry permits were issued by local sheriff's offices. Since then, the Department of Safety has issued more than 339,000 handgun carry permits.

Under Tennessee law, handgun carry permits do not require citizens to conceal their firearms. In fact, the number of permits does not represent the total number of guns owned by Tennesseeans, since no permit is necessary to purchase or hold an handgun at home, and also because not every permit holder can be presumed to own a gun. Furthermore, the identities of those obtaining such permits are not considered confidential information: The Tennessee Department of Safety (TDS) makes the list available to anyone who wants it for a very reasonable charge.[25]

The Commercial Appeal – Memphis's highest-circulation daily newspaper – obtained the handgun data from the TDS to include it in its online 'Data Center.' In conversations with staff of the Commercial Appeal, we established that the publication of the gun permits database was not endogenous: it was not motivated by any particular or novel crime trend in the Memphis area. Rather, the Data Center was created by the newspaper in a bid to establish itself as a data provider and data clearinghouse for the Memphis area, as the Internet had been threatening their previous print subscription business model. It contains numerous databases, including ones for missing IRS refund checks, nursing home reports, health and safety scores of local restaurants and school test scores. However, the database that has received the most attention is the listing of all residents in Tennessee who have a handgun carry permit.

The database was first made available online on December 12, 2008.[26] The newspaper did

[24]Handgun carry permit information was public in TN even before the Commercial Appeal published the permit holders' list on its website.

[25]We obtained the list ourselves and paid $80, including postage. However, it did take some weeks of 'phone tag' to identify the correct person from whom to get the list.

[26]This was not the first time a Tennessee newspaper had published the database, but it was the first time that the database stayed up. In 2007, the Nashville Tennessean had published the handgun per-

edit the TDS's publicly available list: it removed street addresses and birth dates to lessen the chance that somebody might use information on the list for identity theft. The primary pieces of information available to website users once the database came under public scrutiny were, therefore, the first names and last names of gun permit holders, and the 5-digit zip code for their address.[27] The fact that the geographical information available was presented at the zip code level motivates our use of zip codes as the primary unit of analysis.

Although the information was made available to online readers of the Commercial Appeal in mid-December 2008, the database only started receiving attention around February 10, 2009. Its traffic increased sharply after a reader linked the database in an online comment to the February 9, 2009 report of a shooting at a Memphis shopping center.[28] Web-searchers wanted to find out whether the shooter had a gun permit - and soon, the database itself became the story. The database page was inundated with comments, often, but not exclusively, critical of its content. The Tennessee Firearms Association and other pro-gun organizations orchestrated a campaign where the Commercial Appeal executives were sent as many as 600 e-mails a day, along with dozens of phone calls at home, at work and on their cell phones. Soon, stories about the Commercial Appeal database started appearing in local, then national news sources. By March, Fox News, CBS News, and The New York Times had all run stories about the Commercial Appeal database. According to the statistics we have received from the newspaper, after receiving an average of only 5 pageviews per day since its December 2008 inception, the database suddenly attracted 589,697 page views in February 2009 and 250,520 page views in March; after that, the number of pages viewed settled to 40,000 per month. By December 2009, the database had received more than a million page views.[29]

2.2 Data

In order to estimate whether and how the number of crimes changed in the Memphis area after the Commercial Appeal's publication of the gun permits list, we used two sources of data: 1) the gun database itself, and 2) crime statistics.

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mit database on its website, before shutting the information down within hours of making it public: see http://news.tennesseeanytime.org/node/958.

[27]The other pieces of information consisted of the holder's year of birth, the date when their permit was issued, and the date when the permit would expire.

[28]'Attorney: Accused shooter in Cordova parking-lot killing regretful,' *The Commercial Appeal*, Hank Dudding, February 9, 2009.

[29]We mined data from `Alexa.com`'s AWIS service in order to estimate the number of users visiting the *entire* Commercial Appeal website between December 2008 and May 2009. These estimates confirm a spike in traffic in February, corresponding to the initial publicity the database received (February 10, 2009), and following the Commercial Appeal op-ed about the controversy (February 16, 2009).

2.2.1 Gun Database

We used original data obtained by the Commercial Appeal in December 2008 from the Tennessee Department of Safety to measure the actual number of gun permit holders in each zip code. This database held information on permits that were issued up to July 2008. We then obtained a second, updated database of gun permit holders from the Tennessee Department of Safety, which covers the period until December 2009. In the time period we study, it appears that the newspaper requested new data twice, once around February 19th, 2009, and once around May 1st, 2009. We exploit as a robustness check the fact that, due to these lags, the displayed number of gun permits on the webpage did *not* always reflect the true extent of gun permits in a zip code.

Although the database contains information on guns in all Tennessee zip codes, we focused our analysis on Memphis zip codes, because the newspaper that published the database was targeted at Memphis-area readers, and consequently much of the publicity around the database focused on that city. Figure 1 shows the relative distribution of gun permits across zip codes at the time the database was publicized. More than 80 percent of zip codes had at least one permit. In communications with the authors, the Commercial Appeal confirmed that it believes the data to be as accurate as government data can be. Naturally, the database cannot provide any information on whether the gun permit holder continues to own a gun or not at his or her home address, or indeed whether they ever did. However, and importantly, our research focuses on the fact that holding a gun permit creates the impression that the person in question currently owns, and may regularly carry, a gun. Furthermore, we focus on permitted guns only, meaning that we ignore any potential effects of black-market guns. As pointed out by Jacobs and Potter (1995), black-market guns are often linked to the commission of crimes.

However, not only is the *actual* spatial correlation between propensity to own a gun and issuance of right-to-carry permits likely to be positive; more importantly, because the actual distribution of gun ownership across zip codes could not be precisely estimated by criminals before the publication of the database, individuals perusing the database would infer the likely distribution of gun ownership from the distribution of gun permits. In other words, lacking a reliable prior, potential criminals' *expected* spatial correlation between guns and gun permits is likely to be high; our results focus, precisely, on the deterrent effects of such an exogenous information shock about gun permits and gun ownership where no reliable information about either gun ownership or gun permits previously existed.

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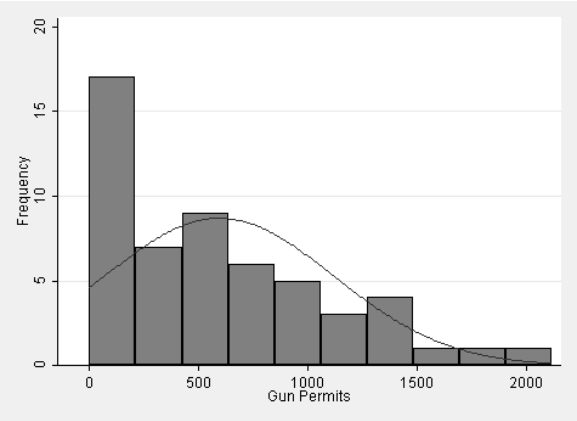
Figure 1: Distribution of Gun Permits



Distribution of Gun Permits by Zip Codes at the Time the Database Was Publicized.

2.2.2 Crime Data

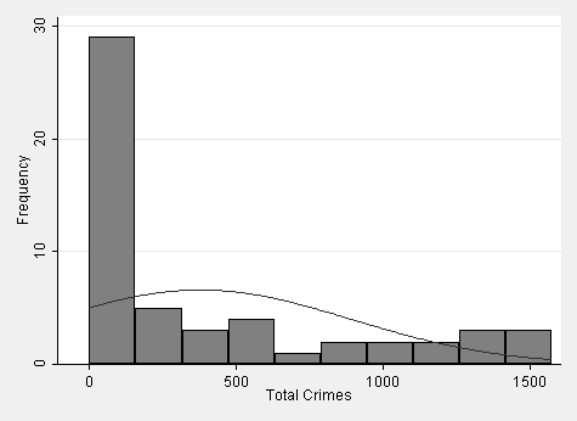
We gathered daily data on Memphis criminal activity from the website `http://spotcrime.com/`. As described by Smillie (2010), SpotCrime was set up to help home-owners map areas of high crime. We focused on weekly reports of crimes starting from October 28, 2008 through May 21, 2009, which formed a 30-week window around the publicization of the database. These data are largely based on information released electronically by police departments,[30] but are sometimes augmented by media reports. (Since the role of the media could be viewed as somewhat endogenous, we test, below, the robustness of our results to the exclusion of these press-reported crimes.[31]) Figure 2 shows the relative distribution of total crimes summed across weeks in our data. The distribution is skewed, but more than 70% of zip codes experienced at least one of the types of crime we investigate (assaults, burglaries, robberies, shootings, and thefts) during the period of observation (the correlation between zip codes with zero permits and zip codes with zero crimes is low: 0.1942).

Typically, police blotters report crimes on the basis of intersections or redacted street addresses, for example, 'Shooting 20XX Brooks Rd' or 'Shooting, North Hollywood and Hunter.' Therefore, we queried Google Maps API in order to match each crime record's redacted location to a specific zip code, in order to get a zip code identifier which could be associated with the zip code presentation of the hand gun permit information used by the

[30]We requested crime records from the Memphis Police Department in order to verify SpotCrime data, but the MPD refused to participate in this study.

[31]We interviewed the founder and owner of SpotCrime, who verified that he believed that the information was representative and accurate. Our analysis of Memphis data contained in the `spotcrime.com` database suggests that the overwhelming majority of those crimes come, in fact, from police blotters.

Figure 2: Distribution of Crimes



Distribution of Total Crimes Across Zip Codes.

Commercial Appeal.[32]

2.3 Initial Analysis

Table 1 presents an overview of the data in our possession. We analyzed data for 54 Memphis zip codes, as identified by the Census (including rural areas and densely populated zip codes), and 30 weeks. These were all zip codes that lay in or within 20km of the Memphis metropolitan statistical area, but within Tennessee state lines. The weeks spanned the period from October 28, 2008 through May 21, 2009 (15 weeks before the publicity surrounding the database and 15 weeks after). According to TDS data, four percent of Memphis residents own handgun permits. On a per-dwelling basis this translates to one gun permit for every three dwellings (as identified by the 2000 Census, "dwellings" include large multi-family housing blocks).

Figure 3 shows mean trends, over time, for different types of crimes in the period from October 2008 to May 2009. In each sub-figure, vertical bars indicate the time of publication (December 12th, 2008) and the time (February 10th, 2009) when publicity increased awareness of the database. The values on the y axis are the weekly averages, by zip code, of the total number of various crimes across three types of zip codes: Those lying in the top third of the distribution of the number of gun permits, those in the middle third, and those in the bottom third. In Figure 6 in the Appendix, we repeat the exercise, but the values on the y axis are the mean logarithms of the total number of various crimes in each week.[33]

[32]When Google Maps API returned two zip codes for a given address (which may be the case when the start and end of a block lie in two different zip codes), we repeated the analysis presented below using the alternative zip code. Our results did not change.

[33]Unless otherwise specified, in all tables, figures, and discussions that follow, the term 'guns' concisely

The top left quadrant of Figure 3 combines different types of crime. In general, zip codes with higher numbers of gun permits also have higher numbers of crimes, including burglaries (Cook and Ludwig (2003) also find that residential burglary rates are correlated with gun prevalence). However, the figure also suggests an upward trend in crimes across all zip codes in December, spiking around Christmas, followed by a downward trend in January. After the publicity around the database started intensifying in early February (solid vertical line), the downward trend seems to intensify. The reduction seems particularly dramatic for certain types of crimes - such as burglaries - and in zip codes with more gun permits. By late February, an overall upward trend emerges. Note that the values reported in the Figure are weekly means per single zip code: in reality, even across the bottom zip codes, more than 8 burglaries per week would take place before the publicization of the database.

We advise caution against reading too much into absolute numbers such as the ones presented in these figures. From the perspective of our analysis, what matters is whether the trends in zip codes with more gun permits differ more from the trends in zip codes with lower numbers of gun permits *after* the publicization of the database, than they differed *before*. By inspecting the figures, it is hard to establish whether specific crimes did experience a steeper decline in higher-gun-permit zip codes following the February 2009 events. Our econometric analysis below can help us compare relative trends while controlling for factors such as seasonal trends common across zip codes, as well as the size and population of a given zip code. Controlling for those factors, in turn, helps us determine the statistical significance of the variations in crimes that Figure 3 seems to suggest.

3 Theoretical background

Did the publication and publicization of the gun permit database give burglars a "lighted pathway" to the homes of gun owners, as Chris Cox, executive director of the lobbying arm of the NRA, put it, or did it make criminals less likely to target households they knew to be protected by weapons, as the Commercial Appeal argued? Or, perhaps, did the publication and publicization of the database have no effect on crime?

Consider a potential offender who is contemplating engaging in a crime. The offender may rationally choose whether to commit crimes by trading off the expected benefits of doing so and the probability, and cost, of being apprehended and punished (Becker, 1968). In fact, criminals may make strategic use of information about the likelihood of successfully completing the criminal offense (Ayres and Levitt, 1998; Vollaard and Ours, 2010). A sim-

refers to the number of *handgun carry permits* issued and displayed, at a given time and for a given zip code, on the Commercial Appeal database.

Table 1: Summary Statistics for Memphis Sample (by Week and Zip Code)

	(1) Mean	Std Dev	Min	Max	Observations
No. Assault	3.94	6.57	0	35	1620
No. Burglary	4.24	7.13	0	50	1620
No. Robbery	1.15	2.34	0	17	1620
No. Shooting	0.080	0.41	0	6	1620
No. Theft	3.12	4.91	0	30	1620
Gun stolen	0.029	0.24	0	3	1620
Jewelry stolen	0.12	0.49	0	5	1620
Computer stolen	0.39	1.10	0	8	1620
Currency stolen	0.19	0.69	0	7	1620
Outside stolen	0.044	0.29	0	4	1620
Lowvalue stolen	0.080	0.46	0	10	1620
Guns	654.6	579.2	0	2637	1620
Guns per Dwelling	0.39	0.34	0	1.97	1620
Bottom Third gun permits zipcode: Number of Guns	89.0	82.3	0	330	540
Middle Third gun permits zipcode: Number of Guns	536.8	142.9	273	898	540
Top Third gun permits zipcode: Number of Guns	1338.0	422.2	744	2637	540
Post-Publication	0.77	0.42	0	1	1620
Post-Publicity	0.53	0.50	0	1	1620
Undisplayed Guns	61.9	83.6	0	453	1620
Dwellings	1768.6	900.5	193	4971	1620
Observations	1620				

ilar account would be posited by the routine activity theory of crime of Cohen and Felson (1979), according to which variations in crime rates over space are affected by the perceived availability of "suitable targets" and the absence of "capable guardians." The expected punishment may *not* act as deterrent if the offender had a short time horizon (McCrary and Lee (2009) find only a 2 percent decline in the probability of offense when a juvenile offender becomes of legal age - which represents an increase of roughly 230 percent in the expected period of detention). In our case, however, the exogenous shock represented by the Commercial Appeal's publication of the gun permits database offered information with immediate relevance to offenders: The likelihood that a potential victim in a certain location might protect herself with a gun is analogous to a probability of "immediate" punishment for the attempted crime. Both the economics and the sociology literature on crime, therefore, would predict a link between the publication and publicization of the database, and localized changes in crime patterns in the Memphis area.

The opposing argument - suggesting that the publication and publicization of the database could *not* produce any discernible effect on crime - would, instead, rely on one or more of the following claims: Maybe criminals did not know about the database; or, they were not interested enough to check it; or, they did check it, but did not find a way to extract useful information from it, were not sophisticated enough to use it, or simply did not find its information sufficiently compelling to affect their established crime patterns. Each of

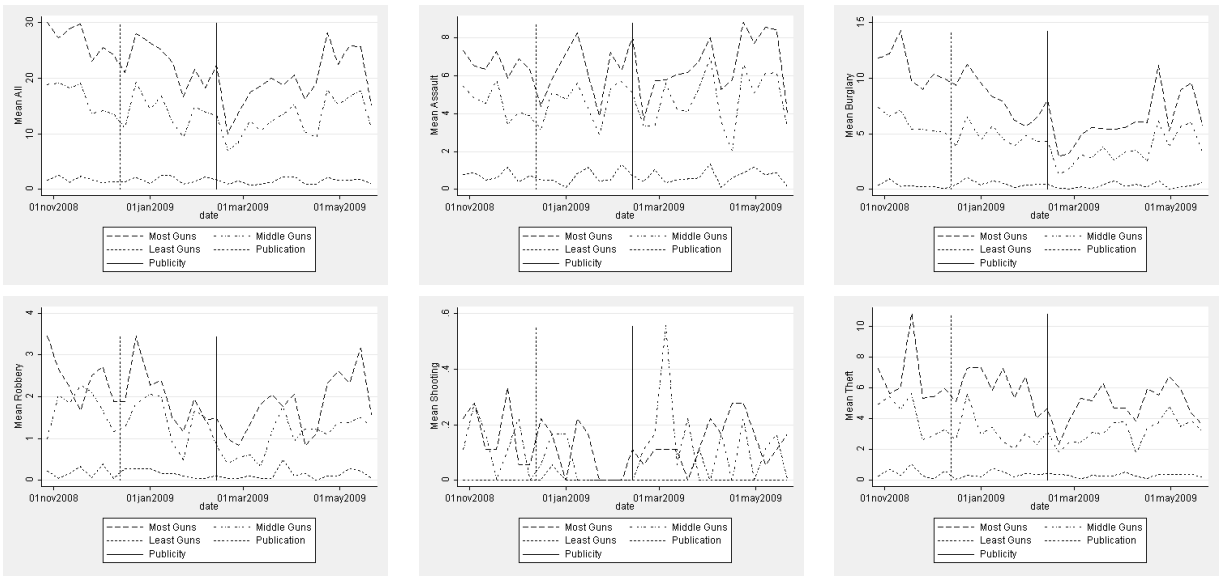


Figure 3: Variation of Mean Weekly Crimes by Gun Permits.

these alternative explanations, however, seems unlikely in the case of the Commercial Appeal database. First, burglars are known to carefully select their targets (Cromwell et al., 1991) (for instance, burglaries planned on information retrieved from obituaries are recurrent phenomena across the United States).[34] Second, scholars have found that participants in criminal activities comprise both unsophisticated offenders (unlikely to make strategic use of the available information) and elite, professional criminals (Clarke and Felson, 1993). While the former are the vast majority, the latter are more likely to use information for tactic and strategic planning (Miethe and McCorkle, 1998), and even to recruit less sophisticated colleagues to direct them towards targets.[35] Third, as noted in the Introduction, a growing body of anecdotal evidence suggests that criminals - and in particular burglars - have started using the Internet and online social media to plan their crimes: from Twitter feeds to Facebook status updates and online foreclosure listings. Fourth, the Commercial Appeal is Memphis's highest-circulation daily newspaper, and the database controversy attracted significant attention both nationally and locally. The Commercial Appeal confirmed to us that the gun permit database received more than half a million page views in February 2009,

[34]See "'Funeral Day Burglar' found guilty in Mo.", *The Associated Press*, May 9, 2008; or: "Should Terry Lee Alexander, the 'Obituary Burglar,' Be Given a Second Chance?, *The Seattle Weekly*, Caleb Hannan, November 6, 2009.

[35]For the specific case of burglaries, see, for instance, "'Fence' in burglary receives sentence," *The Times Herald*, Carl Hessler Jr., March 28, 2010.

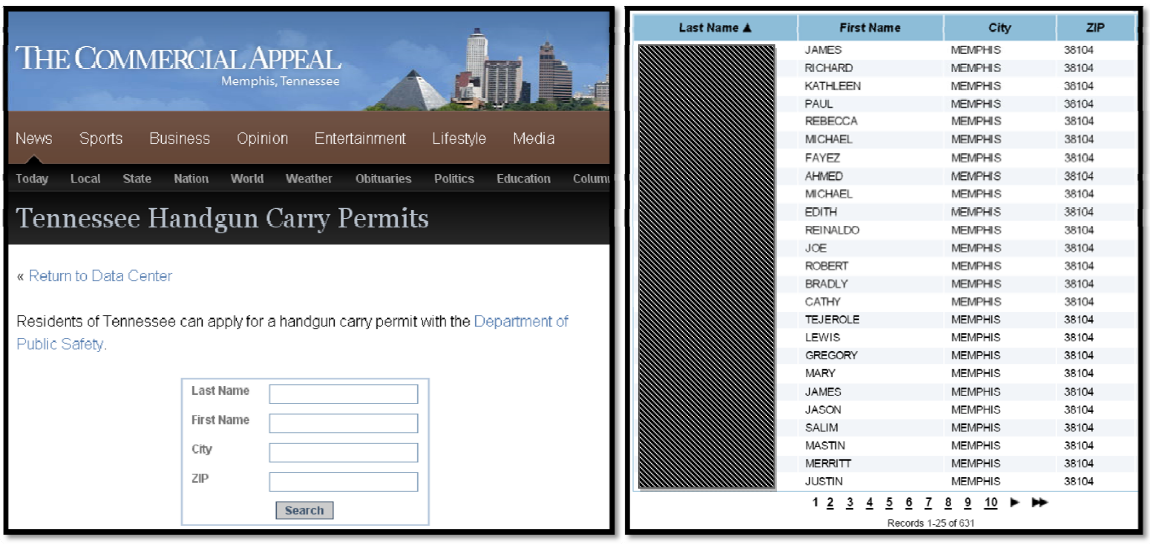


Figure 4: The Commercial Appeal Gun Permits Database

Left quadrant: The search interface. Right quadrant: An example of results following a query for all permits in zip code 38104 (last names have been obscured by the authors). Note the total count of permit holders near the bottom.

immediately following the shopping mall incident; the ostensibly rising popularity among potential criminals of Internet tools suggests a high likelihood that, among the hundreds of thousands of visitors to the database over those days, there were also potential offenders motivated by more than simple curiosity. Fifth, the search interface for the Commercial Appeal database is very intuitive: It allows a visitor to search for an individual's name (and find whether he or she has a permit), as well as to obtain a count of all gun permit holders in a given zip code (with a list of their names); this makes it fast and simple for any visitor to estimate the number of permit holders across zip codes (see Figure 4).[36] Furthermore, several sites can be used to precisely map the boundaries of a zip code.[37]

Research in economics and criminology suggests that burglars, in particular, are likely to engage in advance planning before attempting a crime. A majority of felons interviewed on the impact of target victims' firearms on burglars' behavior agreed that a reason burglars avoid houses when people are at home is the fear of being shot (Wright and Rossi, 1986). The availability of guns increases the likelihood that a victim will defend herself against assault, especially in the case of so-called "hot" burglaries of occupied dwellings (Kopel,

[36]To our knowledge, even if the database attracted considerable attention in February 2009, the interface depicted in Figure 4 remained the only way - aside from requesting the original data from the TDS - to get information about the prevalence of gun permits across Memphis zip codes.

[37]For Memphis, see for instance `http://www.city-data.com/zipmaps/Memphis-Tennessee.html`.

2001).[38] When burglars lack the knowledge of which households are armed, households that hold gun permits for self-defense may generate positive externalities for households that do not (Kopel, 2001). On the other hand, the NRA's argument that publishing information about gun (permit) owners may put the latter at risk is not without merit. Burglars value guns highly, as "items that are easy to carry, easily concealed, and have a high 'pound for pound value' " (Cook and Ludwig, 2003, p. 78). Estimates of the actual frequency of gun use in self-defense against burglaries, however, vary significantly across studies (Cook and Ludwig, 2003); and the issue of whether available empirical evidence links guns to a net increase or decrease in burglaries is still hotly debated (see Kopel (2001), Cook and Ludwig (2003), and Kopel's commentary to Cook and Ludwig (2003)). To address this issue, Cook and Ludwig (2003) used the proportion of suicides that involved firearms as a proxy for local gun ownership prevalence. They found no support for a net deterrence effect of guns on burglaries; however, in absence of an exogenous shock such as the publication of gun permit owners' information, the precision of Cook and Ludwig (2003)'s results was reduced by the difficulty of establishing causal relationships between burglaries and guns.

In short, both Becker (1968)'s and Cohen and Felson (1979)'s theories of crime, and the specifics surrounding the Commercial Appeal database publicization, suggest an high likelihood that potential offenders in the Memphis area became aware of the database and had reasons to peruse it. The streams of economics and criminology literature that focus on burglars, furthermore, suggest that said offenders would be more likely to focus on the uncertainty of being confronted by gun holders, than on the possibility of stealing a gun from a household which held it. This is because the Commercial Appeal published zip codes, but expunged the actual street addresses, of gun permit holders, making it costlier (albeit by no means impossible) to identify specific houses holding guns. In other words, the publication of the database did not readily offer all the information to easily target specific households, but provided a simple way to infer which zip codes, having a larger number of carry-gun permits, might also be rich in gun-holding households. This information may have been used by potential offenders either to avoid areas with higher concentration of gun permits, or to target areas with low numbers of permits, or both.

Therefore, we would expect to detect an impact of the publicization of the database on crime trends, because - lacking a reliable prior about actual gun ownership - potential criminals' may have taken the distribution of gun permits as a proxy foro the distribution of

[38]Our crime data for Memphis area does not distinguish between burglaries to occupied or unoccupied homes, and therefore does not allow us to control for hot burglaries.

actual gun ownership. Specifically, we would expect to detect an impact of the publicization of the database on crime trends measured at the *zip* code level, as this was the information available to potential criminals who perused the Commercial Appeal database. Furthermore, and unlike the impact of shall-issue concealed gun permit laws (which increase the overall uncertainty about who may or may not be carrying a gun in public across an entire state), we would expect a more significant effect of the permits database publication for crimes likely to be premeditated and/or associated with households (such as thefts and burglaries), compared to non-premeditated crimes (such as assaults) or crimes not confined to households (such as shootings). Finally, while we would expect this effect to be statistically significant, we would also hypothesize it to be associated with the activities of elite, professional burglars (rather than the majority of amateur criminals), and therefore circumscribed to a minority portion of thefts and burglaries.

4 Results

We estimated a panel fixed-effects specification to establish whether the publication of the gun permit database had an impact on crimes in the Memphis area. As noted above, visitors to the database could straightforwardly infer not just whether a given individual held a permit, but also the total count of individuals holding permits in a given zip code. Hence, in our initial analysis, we use the absolute number of guns permits in a zip code. Before moving on, we alert the reader that the results for our absolute counts model remain robust when we examine a per-dwelling specification, which focuses on normalized crime trends, and a logged specification, which measures the percentage changes in crimes across zip codes (see Section 5).

We model $Crimes$ in week t in zip code z, such that:

$$Crimes_t^z = \alpha_1 Guns_t^z + \beta_1 Postpublicity_t \times Guns_t^z + \gamma^z + \delta_t + \epsilon_z \tag{1}$$

Since crimes rise and fall repeatedly in complex patterns across zip codes, γ - a series of fixed effects at the zip code level - captures characteristics of a zip code which may affect the number of crimes but are likely to remain constant during our period of observation (such as number of residents, racial composition, income distribution, stock of guns, and so forth);[39] while δ_t - a series of fixed effects for each week - captures time-specific trends in crime that are constant across Memphis zip codes.

[39] On the effects of neighborhood on crime propensity, see Kling et al. (2005).

$Guns_t^z$ represents the number of gun permits displayed in the Commercial Appeal database for a given zip code z in week t (the number of permits displayed in the database changed, albeit very slightly, over time: old permits expired and were removed, while new permits were added to the database. We exploit this variation in our panel fixed-effects specification.). $Postpublicity_t$ is an indicator variable representing weeks from February 10, 2009 onward, when the publicity surrounding the database first started (= 1), or from before February 10, 2009 (= 0). The inclusion of the week fixed effects means that we omit the collinear main effect of $Postpublicity_t$. Our key variable of interest is $Postpublicity_t \times Guns_t^z$, which captures how crimes differed in the period after the database was publicized for zip codes as a function of their number of gun permits. We use weekly data for the period from October 28, 2008 to May 21, 2009. Our panel is balanced since it covers the 15 weeks before the publicity surrounding the database and 15 weeks after.

We estimate this specification using ordinary least squares (we present other specifications further below). Standard errors are clustered at the zip code level, in accordance with the simulation results presented by Bertrand et al. (2004). Furthermore, we use the approach presented in Driscoll and Kraay (1998) to consistently estimate standard errors taking into account the spatial dependency across zip codes. Driscoll and Kraay (1998)'s approach avoids estimating the spatial covariances by distance bands in latitudes and longitudes by using cross-sectional averages (for each time period) of the moment conditions, and by relying on asymptotics in the time dimension to yield an estimator for the spatial covariance structure.

Table 2 presents the results of our basic model. Column (1) presents the results of the specification that examines the correlation between publicity surrounding the database and all types of crime. The fact that $Postpublicity_t \times Guns_t^z$ is negative suggests that zip codes with more gun permits were more likely, after the database was publicized, to experience a drop in overall crime. However, the result is only marginally significant. Column (2) reports the results for burglaries. We find a significant drop in the number of burglaries, after the database was publicized, in zip codes with more gun permit holders. This suggests that, even though handgun permits are primarily for the use of guns outside the home, potential criminals may still use the issuance of a permit as an indicator of the presence of a gun within a home. For every 1,000 more gun permits in a zip code than the average zip code, there were 1.62 fewer burglaries per week in the mid-February 2009 through May 2009 period. Column (3) reports the results for assaults. The estimates suggest that there was no appreciable effect on assaults. Column (4) reports the results for robberies. Ayres and Donohue (2003)

Table 2: Results for Memphis: Continuous specification

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.00269***	-0.00162**	0.000132	-0.000281	-0.00000669	-0.000910***
	(0.000879)	(0.000642)	(0.000264)	(0.000176)	(0.0000372)	(0.000182)
Guns	0.00661	0.00378	-0.00152	0.00169	-0.000126	0.00279*
	(0.00586)	(0.00376)	(0.00177)	(0.00102)	(0.000165)	(0.00153)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area.

argue that concealed-carry permits should be associated with a drop in robberies.[40] The sign is indeed negative, but we are not able to estimate such an effect precisely. This could be because criminals who commit robberies find it difficult to connect a person in a database to the set of people who may be present in a store or bank at any one time. Column (5) reports results for shootings. Again, we see no significant effect of publicization. Last, column (6) reports the correlation between publicization in areas with many gun permits and thefts. As for burglaries, there appears to be a negative and significant correlation. Because the majority of these thefts were car thefts, this result suggests that car thieves were less likely to target zip codes where more people had handgun carry permits.

The results in Table 2 suggest that zip codes with more gun permits experienced a larger decrease in burglaries relative to zip codes with fewer gun permits. However, they do not tell us whether this relative effect was primarily driven by crime going down in areas with more gun permits, or by crime increasing in areas with fewer gun permits. This is important, as it is this distinction that illuminates whether publicization led to a relative deterrence of crimes or merely a relative displacement. The 2004 NAS Committee's Report found that intensive, localized crime prevention initiatives in high gun density areas did not seem to generate crime displacement to other areas (Wellford et al., 2005). Guerette and Bowers (2009) analyzed a plethora of evaluations of situationally-focused crime-prevention projects, and found that negative displacement was observed in 26 percent of cases (diffusion of benefits was observed in 27 percent of cases). To investigate this question, we estimate Equation (2) - a non-parametric version of Equation (1) in which we separate and consider three types of zip codes.

[40] TN law does not require permit holders to conceal their arms. However, permit holders may still carry their weapons concealed.

$$Crimes_t^z = \alpha_1 TopGuns_t^z + \alpha_2 BottomGuns_t^z + \beta_1 Postpublicity_t \times TopGuns_t^z + \beta_2 Postpublicity_t \times BottomGuns_t^z + \gamma^z + \delta_t + \epsilon_z \quad (2)$$

We define a zip code to be a top-gun-permit zip code if it lies in the top third of the distribution of number of gun permits in the gun database. We define a zip code to be a bottom-gun-permit zip code if it lies in the bottom third of the distribution for the number of gun permits. This means that the results associated with Equation (2) should be interpreted relatively to the middle tier of gun permit holding. Note that the difference in the number of gun permit holders in each zip code during our period of observation was quite dramatic. On average, zip codes in the bottom third had a mean of 71 gun permits, in the middle third they had a mean of 533 gun permits, and in the top third they had a mean of 1330 gun permits. Again, we determine top, middle and bottom gun permit zip codes by the absolute number of gun permits in a zip code.

Our key variables of interest are $Postpublicity_t \times TopGuns_t^z$ and $Postpublicity_t \times BottomGuns_t^z$, which capture how crimes differed in the period after the database was publicized for zip codes in the top and bottom third of gun permit distribution. Again, the signs and coefficients of $Postpublicity_t \times TopGuns_t^z$ and $Postpublicity_t \times BottomGuns_t^z$ should be interpreted relative to the changes in the middle third of zip codes in terms of gun permits. So, for instance, when the overall seasonal trend suggests a rise in crime across all zip codes, a negative sign for $Postpublicity_t \times TopGuns_t^z$ would imply that crimes in zip codes with larger numbers of guns decreased, or increased less dramatically, after the publication of the database, relative to the corresponding change in the number of crimes in zip codes in the middle tier of gun permit holding.

Table 3 reports the results for this specification. They confirm that for zip codes with many gun permits, burglaries and thefts decreased relatively to median gun permits zip codes, but burglaries and (to a lesser extent) robberies increased. That is, burglars may have been deterred from burglarizing houses in higher-gun-permit zip codes, their crimes being displaced to zip codes with fewer guns. Relative to zip codes with the middle number of permits, zip codes with the highest concentration of permits experienced roughly 1.7 fewer burglaries per week/per zip code in the 15 weeks following the publicization of the database, and those with the lowest concentration experienced on average 1.5 more burglaries. Given that, on average, there were 9.7 burglaries per week in each of the top zip codes, our results

Table 3: Results for Memphis: Non-Parametric Specification

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Guns	-2.974***	-1.719***	-0.185	-0.0111	-0.0259	-1.033***
	(0.714)	(0.594)	(0.288)	(0.223)	(0.0544)	(0.367)
Post-Publicity*Bottom Third Guns	2.052	1.548***	-0.370	0.470**	-0.0185	0.422
	(1.290)	(0.564)	(0.306)	(0.184)	(0.0420)	(0.412)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area.

imply an 18% relative decrease of burglaries in those zip codes. This finding supports the hypothesis of a relatively small but significant group of burglars following the publication of the database and being affected by it. Once more, however, we stress that our results should be considered in relative terms: in absolute terms, as Figure 3 suggests, overall crimes fell in both top and middle zip codes, and remained more or less stable in bottom zip. However, the usage of zip code and week fixed effects allows us to at least partially disentangle the effect of time and spatial trends from that of the publicization of the database.

Note that the results for the non-parametric specification are more precisely estimated than those for the continuous specification. A similar trend can also be observed for the additional specifications we present in the rest of the paper. This may reflect that the assumption of a linear-functional form leads to worse predictions, and is compatible with the hypothesis that potential offenders merely roughly evaluated the permit count difference across zip codes, rather than precisely sorting in a linear manner the number of permits in each zip code.

The results in Table 3 are not yet conclusive: the negative sign associated with the $Postpublicity_t \times TopGuns_t^z$ coefficient could represent an actual decrease, or merely *smaller increases*, of burglaries and robberies, relative to the trends in the zip codes with a median number of gun permits. Furthermore, we do not know about the trends in the middle zip codes. Therefore, we cannot yet distinguish between the relative potency of possible deterrence and displacement effects. In order to further disentangle this issue, we estimated a simpler specification of Equation (2). Equation (3), below, represents a two-period comparison without fixed effects, 15 weeks before and 15 weeks after the database was publicized. The interaction terms, similarly to Equation (2), non-parametrically evaluate how the number of crimes for zip code z varies with the relative number of gun permits in that zip code:

Table 4: Results for Memphis: Before and After Comparison

	(1) Total All	(2) Total Burglary	(3) Total Assault	(4) Total Robbery	(5) Total Shooting	(6) Total Theft
Post-Publicity*Top Third Guns	-44.61*	-25.78*	-2.778	-0.167	-0.389	-15.50*
	(25.11)	(15.34)	(7.672)	(5.175)	(0.748)	(8.688)
Post-Publicity*Bottom Third Guns	30.78**	23.22***	-5.556	7.056	-0.278	6.333
	(14.15)	(7.869)	(6.333)	(4.236)	(0.585)	(4.619)
Post-Publicity	-33.56**	-25.39***	7.222	-7.944*	0.278	-7.722*
	(13.98)	(7.578)	(6.053)	(4.174)	(0.579)	(4.394)
Bottom Third Guns	-201.2***	-73.89***	-56.67***	-21.00***	-1.389***	-48.22***
	(61.49)	(21.85)	(20.82)	(7.562)	(0.391)	(14.24)
Top Third Guns	148.0	64.67	30.39	10.44	0.500	42.00*
	(102.9)	(40.35)	(32.03)	(11.57)	(0.833)	(23.17)
Constant	227.1***	80.83***	66.28***	23.89***	1.500***	54.61***
	(59.16)	(21.34)	(19.84)	(7.322)	(0.383)	(13.60)
Observations	108	108	108	108	108	108
Log-Likelihood	-738.4	-629.0	-627.3	-496.3	-235.9	-579.3

Standard errors clustered by zip code. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is observations of crimes in the Memphis area, pre- and post-publicization of database.

$$Crimes_t^z = \alpha_1 TopGuns_t^z + \alpha_2 BottomGuns_t^z + \beta_1 Postpublicity_t \times TopGuns_t^z + \beta_2 Postpublicity_t \times BottomGuns_t^z + \beta_3 Postpublicity_t + \epsilon_z \quad (3)$$

Since we use only two data periods, spanning October 28, 2008 to May 21, 2009, $Postpublicity_t$ is again the indicator variable representing the weeks following February 10, 2009, when the publicity surrounding the database first started (= 1), or the weeks before February 10, 2009 (= 0). Once again, this implies that the signs and coefficients of $Postpublicity_t \times TopGuns_t^z$ and $Postpublicity_t \times BottomGuns_t^z$ should be interpreted relative to the changes in the bottom third of zip codes. However, the exclusion of the week fixed effects means that we no longer need to omit the main effect of $Postpublicity_t$, which now captures the impact of publicization on the zip codes with the middle number of gun permits. Table 4 reports the results of this specification.

The coefficient for $Postpublicity_t$ shows that, in zip codes in the middle third of the distribution of gun permits, burglaries significantly decreased in the overall period following the publicization of the database. Interestingly, it appears that in this simple comparison, the displacement effect is measured more precisely than the deterrence effect.

We then explored what kinds of burglaries were affected most by the publicization of the database. In around 70% of burglaries, SpotCrime detailed what was stolen. We used these data to split out five categories of stolen items: guns, jewelry, cash, computers, items that were most likely to be stolen from the exterior of the property (predominantly tools and window air conditioners), and items that were stolen but had little value (dog food,

Table 5: Results for Memphis: Crimes Involving Guns

	(1) Gun stolen	(2) Television stolen	(3) Jewelry stolen	(4) Computer stolen	(5) Currency stolen	(6) Outside stolen	(7) Lowvalue stolen
Post-Publicity*Top Third Guns	-0.0296	-0.852***	-0.159***	-0.463**	-0.126	-0.0667*	-0.0296
	(0.0340)	(0.222)	(0.0402)	(0.185)	(0.0867)	(0.0346)	(0.0519)
Post-Publicity*Bottom Third Guns	0.0407*	0.404***	0.167***	0.526***	0.252***	0.0296	0.115**
	(0.0204)	(0.113)	(0.0356)	(0.116)	(0.0569)	(0.0356)	(0.0435)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620	1620
Log-Likelihood							

	(1) Gun stolen	(2) Television stolen	(3) Jewelry stolen	(4) Computer stolen	(5) Currency stolen	(6) Outside stolen	(7) Lowvalue stolen
Post-Publicity*Guns	-0.0000235	-0.000711**	-0.000202***	-0.000427**	-0.000237***	-0.00000284	-0.0000755
	(0.0000220)	(0.000306)	(0.0000684)	(0.000210)	(0.0000844)	(0.0000228)	(0.0000471)
Guns	-0.0000393	0.00178	0.000774*	0.000806	0.000845**	-0.000251	0.000124
	(0.0000827)	(0.00173)	(0.000432)	(0.00112)	(0.000338)	(0.000162)	(0.000295)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620	1620
Log-Likelihood							

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area.

cigarettes, lottery tickets, hair accessories, or drinks). Table 5 reports the results (again, for both the non-parametric and the continuous specifications). They suggest that burglaries involving jewelry, currency, televisions, and computers appeared to experience the largest effect from publication. There was less or no effect for low-value and external goods. This makes sense, because these are more likely to be crimes of opportunity, rather than crimes that are premeditated in a manner such that the burglar would examine a database in advance. Furthermore, we find that the effect of the publicization of the database on gun theft was similar to other categories. This is of interest, because one of the main concerns of gun rights protestors (and, indeed, one of the arguments in support of gun owners' privacy) was that the publication of the database would make it easier for burglars to steal their guns. This should have been reflected in an increase in guns stolen during burglaries in zip codes with more gun permits. However, we do not find such an effect, perhaps because many of the guns stolen were rifles, and rifle owners did not need to apply for a handgun permit.

5 Robustness

We ran a battery of tests to verify the robustness of our results to different specifications of the model, as well as to investigate further the mechanisms underlying our results.

A concern may stem from the fact that our main specification is couched in absolute numbers both for the dependent variable (crimes) and the explanatory variable of interest

(gun permits). As noted, this specification reflects the most probable behavior of potential offenders. Mining the database for actionable information, offenders would have searched either for specific names of gun permit holders, or - more easily, and more likely - for all gun permit holders in a given zip code. The output to a query of the latter type would have been a list of such holders, which provides the total number of permits in a given zip code. However, this specification could be biased by trends in zip codes with particularly many permits. The specification may inaccurately report as significant changes which, in percentage terms, are not significant, and may ignore the fact that, with some extra effort, would-be offenders could have 'normalized' the total number of permits per zip code by its population. We ran five additional specifications to rule out these possibilities.

First, we examined whether our results held if we used a logged dependent variable. This is a useful robustness check: it studies the effect of policy shifts on the percentage change in crime, rather than the absolute number of crimes. Even if potential offenders used an absolute count of permits, we should expect the effect of the law to have an impact not just on the level but on the percentage of crimes.[41] One issue with using a logged specification, however, is that - as noted in Section 2.2.2 and Figure 2 - many of our dependent variables have a value of zero (close to 30 percent of zip codes did not experience any of the crimes we investigate in this study). Dropping zero-crime observations would not be a desirable option: Bartley and Cohen (1998) have shown that results previously reported in the literature on the impact of guns on crimes were biased by the exclusion of counties with zero-crime rates from regressions. To keep all zip codes and avoid this bias - which would naturally arise in any log-distribution where the log of zero is not defined - we transpose all observations by adding 1 to each week/zip code observation. Since we are interested in the relative direction of coefficients, this would not bias our results. Table 6 reports our results. The results for the deterrence and displacement effects are similar to what we have reported above. (Table A-1 in the Appendix presents the results of the logged dependent variable specification without such 'plus-one' transformation. The results are directionally consistent with those presented in this section, albeit not significant.)

Second, Table 7 reports our results for a negative binomial specification that reflects our use of count data (Plassmann and Tideman, 2001). The estimates for the displacement effect in the non-parametric specification and for the interaction between number of permits and publicization in the continuous specification are consistent with those reported previously.

[41] Our main specifications already included zip code fixed effects, and therefore took into account stock of crimes and gun permits by zip code. A log-type specification of our model, however, tests directly for differences in the percentage changes in crime across different zip codes.

Table 6: Results for Memphis: Logged Dependent Variable

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Guns	-0.0602*	-0.107***	-0.0163	-0.0223	-0.00285	-0.0606
	(0.0356)	(0.0365)	(0.0381)	(0.0572)	(0.0233)	(0.0606)
Post-Publicity*Bottom Third Guns	0.119**	0.181**	-0.0292	0.0907	0.00185	0.109**
	(0.0578)	(0.0715)	(0.0475)	(0.0608)	(0.0194)	(0.0489)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.000144***	-0.000171***	-0.00000926	-0.0000589*	-0.00000127	-0.000119***
	(0.0000289)	(0.0000401)	(0.0000343)	(0.0000344)	(0.0000194)	(0.0000325)
Guns	0.000617**	0.000624**	0.000131	0.000335	-0.0000751	0.000405**
	(0.000292)	(0.000265)	(0.000310)	(0.000253)	(0.0000913)	(0.000191)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is logged (plus one) weekly observations of different crimes in the Memphis area.

We employed a negative binomial specification because a likelihood ratio test of the natural log of the over-dispersion coefficient, α, strongly rejected the hypothesis that it is zero (for example, $\chi = 202.08$ for column (1)). Over-dispersion in our dependent variable implies that the negative binomial distribution fits our data better than a simple poisson. Note, however, that our distribution of crimes displays not only over-dispersion but also - as noted above - a concentration of zeros. As discussed by Winkelmann (2003), the negative binomial distribution can address either a concentration of zeros or over-dispersion for the dependent variable, but attempting to use its functional form assumptions to address both often leads to imprecision and lack of convergence. This explains why a specification that does not use a 'plus-one' transformation (see in Table A-2 in the Appendix) is not precisely estimated.[42]

Third, we looked at the relationship between per-dwelling crimes and per-dwelling gun permit holding. We normalize crimes and permits using dwellings, rather than population, because our results focus on property crimes. Table 8 reports the results. The results are consistent with our general findings, especially in the case of the displacement effect in the non-parametric specification and the negative and significant interaction in the continuous

[42]As noted in Section 2.2.1 and shown by Figure 1, slightly more than 15 percent of all zip codes also recorded no *gun permits*. This would not affect the specification represented by Equation (2), since the bottom third of zip codes includes those without permits. However, we also tested Equation (1) again, focusing only on zip codes with at least one permit. The results - available on request from the authors - remain robust to these specifications, albeit less precise (after removal of zip codes without gun permits, the interaction $Postpublicity_t \times TopGuns_t^z$ is still negative and significant at the 10% level).

Table 7: Results for Memphis: Negative Binomial Regression

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Guns	-0.0843	-0.0779	-0.0466	0.0474	-0.0233	-0.102
	(0.0591)	(0.0680)	(0.0763)	(0.107)	(0.0433)	(0.0910)
Post-Publicity*Bottom Third Guns	0.0804	0.206**	-0.0258	0.175*	-0.0167	0.0710
	(0.0556)	(0.0936)	(0.0854)	(0.101)	(0.0341)	(0.0810)
Top Third Guns	-2.227***	-1.589***	-1.040***	-0.418***	-0.146***	-1.140***
	(0.0263)	(0.0436)	(0.0439)	(0.0460)	(0.0172)	(0.0380)
Bottom Third Guns	-1.619***	-1.252***	-0.875***	-0.263***	-0.142***	-0.878***
	(0.0273)	(0.0286)	(0.0391)	(0.0479)	(0.0217)	(0.0411)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood	-3491.8	-2893.5	-2845.7	-2373.4	-1718.0	-2891.8

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.000188**	-0.000205**	-0.0000370	-0.0000981	-0.00000440	-0.000181
	(0.0000791)	(0.0000837)	(0.0000934)	(0.000113)	(0.0000357)	(0.000148)
Guns	0.000733	0.000825*	-0.0000643	0.000935	-0.000138	0.000676
	(0.000592)	(0.000487)	(0.000644)	(0.000782)	(0.000227)	(0.000966)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood	-3492.2	-2896.8	-2845.7	-2374.4	-1718.0	-2892.6

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes (plus one) in the Memphis area.

specification. Echoing earlier robustness checks, it appears that, relative to the middle zip codes, the displacement effect is more precisely estimated than the deterrence effect.

Fourth, we reran the basic specification described by Equation (1) multiple times, omitting one zip code and then one week in each run, in order to investigate whether a specific zip code or week drove our results. The results suggest that our findings are robust and not driven by outliers. The standard deviation on $Postpublicity_t \times Guns_t^z$ when we omit different weeks is 0.00016; when we omit different zip codes, it is 0.00012. Figure A-2, in the Appendix, displays box-plots for coefficients and their t-values. It reassures us that our results do not change substantially and do remain significant when omitting specific weeks or zip codes.

Fifth, we tested a specification that focuses only on zip codes with fewer than 200 residents. The results can be found in the Appendix (Table A-3) and are, again, consistent with the results presented here.

Overall, our nonparametric specifications tend to be more precisely estimated than the continuous ones, and the specifications with absolute and logged variables seem to be more precisely estimated than specifications based on per-dwelling values; all this is compatible with the observation that potential offenders had access to aggregate, absolute data.

Table 8: Results for Memphis: Crimes per Dwelling as a Function of Guns per Dwelling

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Guns per Dwelling	-0.000383	0.000123	-0.000491**	0.000254**	0.0000149	-0.000283
	(0.000447)	(0.000220)	(0.000227)	(0.000120)	(0.0000337)	(0.000255)
Post-Publicity*Bottom Third Guns per Dwelling	0.00197*	0.00157***	-0.000598*	0.000357**	-0.00000310	0.000650**
	(0.00101)	(0.000433)	(0.000333)	(0.000135)	(0.0000256)	(0.000282)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns per Dwelling	-0.00173**	-0.00131**	0.000490	-0.000285	0.00000568	-0.000626***
	(0.000671)	(0.000546)	(0.000633)	(0.000179)	(0.0000305)	(0.000193)
Guns per Dwelling	0.00290	0.00540	-0.00424	0.00204	-0.0000100	-0.000297
	(0.00471)	(0.00374)	(0.00474)	(0.00125)	(0.000193)	(0.00189)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area.

As noted above, crimes rise and fall repeatedly in complex patterns across zip codes. While our original specification already included zip and week specific fixed effects, we tested a version with zip code-specific cubic time fixed effects, which more closely captures the changes in crime rates over time. This, however, comes at a cost: we can present only results for the continuous specification, since the combination of a non-parametric model, together with time trends fixed effects and spatial correlation, creates nonsymmetric variance matrix. For the continuous specification (Table A-4 in the Appendix), we do find that, even after controlling for possible time trends across zip codes, there is a significant negative relation between permits and overall crimes, which supports our main findings (burglaries are not significant likely due to multi-collinearity and the fact that we may be overfitting the model).

One assumption in our regressions is that the effective "publication date" was really the date of publicization: The date associated with the publicity in the local press about the availability of the database. However, the data were made available without fanfare two months earlier, and could have already been used by potential offenders. To check robustness to this earlier publication date, we reran our basic specification including interactions for the actual publication date. Table 9 reports the results. The point estimates suggests that the effect was both larger and more significant for all crimes, and only significant for certain crimes, in the post-publicization period compared to the post-publication period. This seems

Table 9: Results for Memphis: Publication Relative to Publicization

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publication*Top Third Guns	-1.506**	-1.580*	-0.659**	-0.130	0.0417	0.821
	(0.704)	(0.839)	(0.322)	(0.299)	(0.0356)	(0.504)
Post-Publication*Bottom Third Guns	2.110*	1.186***	-0.179	0.244	0.0843**	0.775
	(1.250)	(0.283)	(0.292)	(0.211)	(0.0382)	(0.591)
Post-Publicity*Top Third Guns	-3.677***	-2.456***	-0.493	-0.0718	-0.00648	-0.650
	(0.705)	(0.367)	(0.352)	(0.324)	(0.0580)	(0.406)
Post-Publicity*Bottom Third Guns	3.037**	2.101***	-0.454	0.584***	0.0208	0.784
	(1.341)	(0.524)	(0.344)	(0.195)	(0.0423)	(0.469)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.00308***	-0.00196***	0.0000777	-0.000341**	-0.0000159	-0.000840***
	(0.000670)	(0.000418)	(0.000286)	(0.000148)	(0.0000378)	(0.000209)
Post-Publication*Guns	-0.00116**	-0.00100**	-0.000164	-0.000178*	-0.0000274	0.000207
	(0.000497)	(0.000402)	(0.000194)	(0.000105)	(0.0000167)	(0.000143)
Guns	0.00601	0.00327	-0.00160	0.00160*	-0.000140	0.00289*
	(0.00520)	(0.00323)	(0.00177)	(0.000925)	(0.000161)	(0.00163)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area.

to confirm that the effect of the publication of the database was sharply increased by the publicity surrounding it. In other words, as noted above, our results can be taken to reflect the *combined* effect of the publication and the publicization of the database, rather than only the effect of publication.

We also checked that the results were not influenced by the nature of the crime data we used. The SpotCrime website used both police blotter and press reports. A danger exists that press reports may be influenced by the very policy that we study. For example, newspapers keen to emphasize the benefits of free speech may have been less keen to report crimes that could have resulted from the publication of the database. To check for such bias, we reran our regressions using only the crimes that came from the Memphis police blotter. Table A-5, in the Appendix, report the results for both non-parametric and continuous specifications, which are reassuringly similar to those in Tables 3 and 2.

Finally, we found that our results are also robust to a different specification in which we estimate a *monthly* rather than a weekly panel model, suggesting that any potential

variations in the blotter's daily reporting patterns is not driving our results.[43]

5.1 Falsification Checks

The previous section presented evidence suggesting that our results are robust to multiple specifications and alternative definitions of the variables. However, there is still the concern that unobserved heterogeneity may drive the findings, providing an alternative explanation of why burglaries in high-gun areas fell after the publicization of the database and rose in low-gun areas. To rule out this possibility we conducted two sets of falsification tests. The first set looked at actually registered guns that, because of time delays, were not reported in the database. The second set looked at whether similar time trends in crimes could be observed within comparable communities which were not covered by the publication of the database.

As a first falsification check, we checked whether a correlation could be found between the gun permits that were displayed by the Commercial Appeal database over time, and gun permits that had recently been approved but were not yet included in the database published online, and therefore could *not* motivate the actions of potential offenders who consulted the database. Table 10 presents the results of this robustness check. In the non-parametric specification, the interaction terms between zip codes and number of undisplayed gun permits are, as we would hypothesize, not significant. In the continuous specification, the interaction between undisplayed gun permits and post-publicity is positive and significant (although, in terms of economic significance, it is smaller than the effect of interest, because there was only one-tenth as many undisplayed guns as displayed guns). This may be because the new guns (that were too new to be displayed) in the database were obtained in response to an increase in burglaries in that zip code. Overall, these results do not suggest a strong linkage between guns that had not yet been added to the database and crimes: Table 10 suggests that crimes were more strongly linked to publicized guns.

We then tested whether comparable cities in neighboring states (Jackson, MS and St Louis, MO) had a similar pattern of crime. Jackson and St Louis are reasonably similar to Memphis in terms of population size and crime rates. Crime data available for these cities from SpotCrime are also based on a similar feeds mechanism (from electronic police blotters) as the one we used for our Memphis specifications.[44] However, Mississippi and Missouri do not publish gun records, so we had to predict how many gun permits each zip code had using

[43] Results for this specification are available on request from the authors.

[44] The SpotCrime data experienced a data-processing issue when mining data from the police blotters for both St Louis and Jackson in November 2008. We drop the problematic weeks from our data.

Table 10: Results for Memphis: Falsification Check Specification

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Undisplayed Guns	1.637	0.838	-0.210	0.440*	-0.0121	0.581
	(1.395)	(0.651)	(0.388)	(0.245)	(0.0276)	(0.402)
Post-Publicity*Bottom Third Undisplayed Guns	0.188	0.421	-0.102	0.189	0.119**	-0.439
	(1.268)	(0.627)	(0.392)	(0.180)	(0.0520)	(0.268)
Post-Publicity*Top Third Guns	-3.764**	-2.084**	-0.0929	-0.207	-0.00587	-1.374***
	(1.489)	(0.869)	(0.457)	(0.334)	(0.0537)	(0.433)
Post-Publicity*Bottom Third Guns	2.157	1.407	-0.337	0.416	-0.0914	0.763
	(1.962)	(0.854)	(0.518)	(0.253)	(0.0639)	(0.502)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Undisplayed Guns	0.0471*	0.0378***	-0.00276	0.00514	0.0000921	0.00683
	(0.0269)	(0.0136)	(0.00577)	(0.00518)	(0.000991)	(0.00652)
Post-Publicity*Guns	-0.0107*	-0.00809***	0.000802	-0.00116	-0.000000952	-0.00226
	(0.00560)	(0.00286)	(0.00113)	(0.00109)	(0.000211)	(0.00137)
Guns	0.0549	0.0440*	-0.0137	0.00726	-0.00103	0.0184
	(0.0521)	(0.0260)	(0.00865)	(0.00949)	(0.00179)	(0.0130)
Undisplayed Guns	0.00105	0.00241	-0.00993*	0.000434	-0.00105	0.00919
	(0.0259)	(0.0135)	(0.00496)	(0.00455)	(0.000931)	(0.00625)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area.

demographic data. Following Glaeser and Glendon (1998), who examined the correlates of gun ownership using national survey data, we used the 2000 Census data information on each zip code's land area and demographic measures, and we used political donations data from the website *OpenSecrets.org*, projecting the number of gun permits in these other states based on the actual gun permit information that is available in Tennessee. Table 11 presents the results of this prediction exercise. The r-squared, at 0.89, is relatively high, suggesting that we are able to predict the number of guns using these data relatively well.

One disadvantage of not having gun data, and having to predict gun permit numbers from external data, is that we are less able to predict changes in the time trend of gun registrations. Therefore, for these falsification checks, we focus on a simple 'two-period' model before and after the change in policy. Since this is a two-period model, Table 4 should be used as a comparison. The results of the falsification checks for St Louis and for Jackson are presented in Tables 12 and 13 respectively. They suggest that, unlike Memphis, neither St Louis nor Jackson exhibited a significant larger decrease in burglaries (or in all other crimes) in gun-permit-heavy zip codes in the period following February 10, 2009. These

Table 11: First Stage Results for Predicting Guns

	(1) No. Guns	
Republican Donations (000)	-0.685***	(0.0683)
Democrat Donations (000)	0.727***	(0.0722)
Land Area	0.0335	(0.0793)
Water Area	17.81***	(1.667)
Pop 18 years and over: Total	0.182***	(0.0146)
Pop 25 years and over: Total	-0.217***	(0.0155)
Households: Total	0.282***	(0.0174)
Total Pop: Total	-0.451***	(0.0219)
Households: 2-or-more-person household; Married-couple family	0.888***	(0.0391)
Total Pop: Native; Born in state of residence	-0.0983***	(0.00640)
Workers 16 years and over: Private vehicle occupancy; Car; truck; or van	-0.141***	(0.0110)
Total Pop: Urban; Inside urbanized areas	0.0530***	(0.00235)
Total Pop: Rural	0.138***	(0.00376)
Total Pop: Rural; Farm	-1.691***	(0.0656)
Total Pop: White alone	0.192***	(0.0130)
Total Pop: Black or African American alone	0.234***	(0.0129)
Households: Median household income in 1999	0.000609***	(0.000110)
Civilian veterans 18 years and over: Total	0.410***	(0.0184)
Total Pop: Male	0.406***	(0.0269)
Pop 25 years and over: Male; High school graduate (includes equivalency)	-0.868***	(0.0367)
Pop 25 years and over: Female; Bachelor's degree	-0.316***	(0.0355)
Pop 25 years and over: Female; High school graduate (includes equivalency)	-0.0292	(0.0323)
Pop 25 years and over: Male; Bachelor's degree	-0.933***	(0.0503)
Pop 18 years and over: Male; 18 to 64 years; In Armed Forces	-1.967***	(0.0628)
o.Pop 18 years and over: Female; 65 years and over; In Armed Forces	0	(.)
postpublicity	133.9***	(5.322)
Constant	-78.37***	(10.47)
Observations	6156	
R-Squared	0.889	
Adjusted R-Squared	0.889	
Log-Likelihood	-41587.6	

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly number of guns in a zip code.

coefficients remain similarly not significant when estimated on a per-dwelling basis (such results are available from the authors on request).

Finally, we looked at two Memphis suburbs that actually lie in a different state, and for which crime data was available: DeSoto County (in Mississippi) and West Memphis (in Arkansas). We employed the same approach described above, first estimating the number of gun permits by zip codes, and then estimating the relationship between crimes and (predicted) gun permits before and after February 10, 2010. Because of the scarcity of crimes in DeSoto County and West Memphis compared to Memphis, TN, our analysis aggregates the two suburbs. Even so, there are so few crimes that the results should only be considered suggestive, rather than conclusive. Furthermore, the crime data for these border counties is based on media reports rather than official police records. This is less than ideal, but it should provide some form of robustness check, providing there is no reason to think that there would be different time trends in official versus media-reported crimes. The results are presented Table A-6 in the Appendix. The table suggests that DeSoto County and West Memphis did *not* exhibit a significant fall in burglaries. There would appear to be a significant fall in thefts and shootings - but given that there were only one instance of a shooting and ten instances of a theft in our data, these results should be considered with extreme caution.

Table 12: Results for St Louis

	(1) Total All	(2) Total Burglary	(3) Total Assault	(4) Total Robbery	(5) Total Shooting	(6) Total Theft
Post-Publicity*Top Third Guns	-26.40	-3.468	-8.149*	-1.894	-0.0851	-12.81
	(17.72)	(3.123)	(4.791)	(1.772)	(0.0909)	(8.461)
Post-Publicity*Bottom Third Guns	-11.38	-1.362	-4.979	-1.149	7.56e-17	-3.894
	(20.01)	(3.495)	(5.186)	(1.833)	(0.0680)	(10.12)
Post-Publicity	43.11***	6.638***	12.36***	3.766***	0.0213	20.32***
	(14.22)	(2.351)	(3.865)	(1.271)	(0.0569)	(7.152)
Bottom Third Guns	-8.915	-1.979	-1.851	-0.660	-1.09e-16	-4.426
	(13.95)	(2.711)	(3.020)	(1.397)	(0.0422)	(7.424)
Top Third Guns	-21.94*	-4.021*	-4.298	-2.043*	0.0638	-11.64*
	(12.53)	(2.366)	(2.689)	(1.136)	(0.0694)	(6.851)
Constant	32.81***	5.596***	6.532***	2.915***	0.0426	17.72***
	(10.48)	(2.117)	(2.188)	(1.010)	(0.0298)	(5.691)
Observations	282	282	282	282	282	282
Log-Likelihood	-1734.7	-1254.4	-1331.1	-1070.8	-51.73	-1544.1

	(1) Total All	(2) Total Burglary	(3) Total Assault	(4) Total Robbery	(5) Total Shooting	(6) Total Theft
Post-Publicity*Guns	-0.0207	-0.00323	-0.00499	-0.00143	-0.0000639	-0.0110
	(0.0239)	(0.00436)	(0.00573)	(0.00227)	(0.0000614)	(0.0122)
Post-Publicity	44.69**	7.252**	11.30**	3.800**	0.0260	22.30**
	(18.44)	(3.358)	(4.417)	(1.750)	(0.0473)	(9.372)
Guns	-0.0157	-0.00256	-0.00311	-0.00160	0.0000303	-0.00849
	(0.0169)	(0.00308)	(0.00405)	(0.00161)	(0.0000434)	(0.00860)
Constant	29.58**	4.742**	5.878**	2.732**	0.0503	16.17***
	(12.26)	(2.232)	(2.936)	(1.163)	(0.0314)	(6.230)
Observations	282	282	282	282	282	282
Log-Likelihood	-1734.5	-1254.2	-1331.6	-1070.5	-52.02	-1543.7

Standard errors clustered by zip code. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is pre- and post-publicization of database observations of different crimes in the St Louis, MO area.

Table 13: Results for Jackson

	(1) Total All	(2) Total Burglary	(3) Total Assault	(4) Total Robbery	(5) Total Shooting	(6) Total Theft
Post-Publicity*Top Third Guns	-43.10	-26.17	-3.467	-1.967	-0.367	-11.13
	(36.49)	(24.98)	(2.635)	(2.809)	(0.258)	(6.971)
Post-Publicity*Bottom Third Guns	-51.24*	-31.83	-3.522	-3.067	-0.356	-12.47*
	(29.16)	(19.43)	(2.446)	(1.953)	(0.263)	(6.301)
Post-Publicity	67.80**	43.67**	4.467*	4.067**	0.467*	15.13**
	(27.95)	(18.51)	(2.392)	(1.890)	(0.238)	(6.069)
Bottom Third Guns	-37.72*	-25.67*	-1.267	-2.100	-0.400**	-8.289*
	(19.54)	(13.20)	(0.901)	(1.273)	(0.165)	(4.564)
Top Third Guns	-33.57	-22.83	-0.600	-2.300*	-0.400**	-7.433
	(22.53)	(14.97)	(1.247)	(1.278)	(0.165)	(5.491)
Constant	45.67**	30.33**	1.600*	2.600**	0.400**	10.73**
	(19.15)	(13.00)	(0.878)	(1.243)	(0.165)	(4.415)
Observations	86	86	86	86	86	86
Log-Likelihood	-509.4	-475.0	-267.3	-271.2	-86.92	-378.8

	(1) Total All	(2) Total Burglary	(3) Total Assault	(4) Total Robbery	(5) Total Shooting	(6) Total Theft
Post-Publicity*Guns	0.0209	0.0146	0.000810	0.00227	0.0000715	0.00315
	(0.0410)	(0.0274)	(0.00242)	(0.00254)	(0.000313)	(0.00907)
Post-Publicity	25.21	16.57	1.662	1.263	0.198	5.509
	(26.44)	(17.66)	(1.557)	(1.639)	(0.202)	(5.845)
Guns	0.0120	0.00775	0.00116	0.000204	0.0000161	0.00285
	(0.0290)	(0.0194)	(0.00171)	(0.00180)	(0.000222)	(0.00641)
Constant	18.22	11.79	0.557	1.121	0.134	4.619
	(17.49)	(11.68)	(1.030)	(1.084)	(0.134)	(3.866)
Observations	86	86	86	86	86	86
Log-Likelihood	-513.5	-478.8	-269.9	-274.3	-94.29	-383.7

Standard errors clustered by zip code. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is pre- and post-publicization of database observations of different crimes in the Jackson, MS area.

5.2 Time Trends in Gun Ownership

Last, we considered the potential endogeneity of gun permits to crimes. Memphis residents may have reacted to the publication of the database (and the discovery of the number of their neighbors owning a permit) by applying for gun permits and/or purchasing guns; this, in turn, may have affected the number of crimes, especially if residents in zip codes with higher density of gun permits (as reported in the database) were more likely to apply for new permits. Even so, we can rule out that any such endogeneity biased our results. First, we must consider that there exists a rather significant lag between the decision to apply for a permit and the time when a permit is issued. That lag is due to the frequency and schedule of the course that any TN aspiring permit holder must undertake (see, e.g., `http://www.rattlesnakeridge.org/Handgunpermitinfo.html`), and the time that the DMV requires to process the application (around 6 weeks[45]). In other words, 1) Any effect of the permits database publication on new permits would not have taken place for at least 7 weeks from the time of the publication; 2) Furthermore, since the newspaper updated the online permit database only twice (around February 19, 2009 and around May 1, 2009),

[45]See, for instance, `http://www.lesjones.com/posts/000191.shtml`.

Table 14: New Gun Permits

	(1) 8 Week Lag	(2) 12 Week Lag	(3) 16 Week Lag	(4) 20 Week Lag	(5) m16	(6) m
Post-Publicity* 8 Week Lagged Guns	0.0000955					-0.00243
	(0.000109)					(0.0258)
Post-Publicity* 10 Week Lagged Guns		0.000188				-0.0123
		(0.000114)				(0.0359)
Post-Publicity* 12 Week Lagged Guns			0.000348***			-0.0228
			(0.000117)			(0.0216)
Post-Publicity* 14 Week Lagged Guns				0.000152		0.0640*
				(0.000122)		(0.0361)
Post-Publicity* 16 Week Lagged Guns					0.000295**	-0.0259
					(0.000113)	(0.0299)
8 Week Lagged Guns	0.00440***					0.00511
	(0.000440)					(0.0255)
10 Week Lagged Guns		0.00563***				0.0155
		(0.000839)				(0.0355)
12 Week Lagged Guns			0.00622***			0.0263
			(0.00185)			(0.0211)
14 Week Lagged Guns				-0.00333		-0.0567**
				(0.00436)		(0.0218)
16 Week Lagged Guns					0.000394	0.0334*
					(0.00375)	(0.0190)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood	-2177.7	-2168.5	-2176.3	-2201.4	-2201.8	-2150.8

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is the weekly number of new gun permits in Memphis, by zip code. Explanatory variable is the lagged stock of gun permits (with various different lags) in that week, by zip code.

any published evidence of newer gun permits would have only affected the very final weeks of the period we considered.

In any case, we tested whether the number of guns in a given zip code was correlated with a larger number of newly issued permits following the publication of the database, with lags of 8, 10, 12, 14, and 16 weeks. The results are presented in Table 14, and suggest a positive but insignificant relation (for three out of five possible lags) between the number of newly issued permits and the number of permits at the time of publication. The few significant effects disappear when we consider the permits issued with various lags from publication: See Column (6).

This makes sense. The gun rights debate is so polarized that, arguably, most Memphis residents already fell into two opposite categories at the time of the database publication: "Wants and has guns" or "Does not want and does not have guns." Only a few 'marginal' individuals, in the middle between those positions, would have been moved to apply for a permit they did not hold before by the publicity surrounding the Commercial Appeal database.

6 Discussion

While a growing body of anecdotes suggests that online information is being exploited by potential criminals to plan and conduct *offline* crimes, the evidence for such Internet-driven crimes has been, so far, merely episodic and hardly significant. In this manuscript, we offer some evidence of a specific a mechanism by which criminals may have became aware, and taken advantage of, online information about potential victims. Furthermore, due to an exogenous information shock (the gun permits database publication and its subsequent publicization) we can establish a firmer causal relationship between crimes (and, in particular, burglaries) and guns than what had been possible so far in the literature. Our results suggest that potential offenders (and in particular, burglars) in the Memphis area did pay attention to the publication of the gun permits database. Specifically, we found evidence of a larger decrease in burglaries in zip codes with higher levels of permit issuances and their displacement to zip codes with lower number of gun permits. Relative to zip codes with the middle number of permits, zip codes with the highest concentration of permits experienced roughly 1.7 fewer burglaries per week in the 15 weeks following the publicization of the database, and those with the lowest concentration experienced on average 1.5 more burglaries.

With 19 states allowing gun permit holders information to be made public, 21 states keeping that information confidential, and four bills criminalizing the publication of gun owners' names filed in Tennessee after the Commercial Appeal controversy, the privacy of gun owners is a topic as hotly debated as the actual impact of guns on crimes. In principle, the publication of the names of gun permit holders might have produced a host of different outcomes, thus fueling both sides of the debate on the privacy and security trade-offs of gun holders' privacy. Opposing yet equally reasonable theoretical arguments could be put forward, to suggest that the availability of this information could carry both positive and negative consequences for permit holders, non-holders, and their respective neighbors. In practice, in the Memphis case, revealing identities and zip codes of gun permit holders seemed to lead to a decrease in violent crimes in the areas more likely to host them, and a relative increase in areas with fewer guns. However, despite activism on the part of gun owners against the publication of such databases, we found no evidence that publishing the identities of gun permit holders led to an increase in crimes aimed at stealing their weapons, relative to other forms of theft or burglaries. If anything, the loss of privacy seemed to carry a positive, but short-lived, externality for both those whose identities were published on the database, and for some of those living near them.

It is appropriate to point at a number of limitations to qualify our current results. First

of all, it is worth noting that the changes in crime we detected came as a result of the *publicization* of the database. This implies two things. First, our results do not distinguish between the effect of information about gun permit holders *per se*, and the combined effect of that information and its publicization. Second, they suggest that information alone may not be sufficient to influence criminal behavior: The decrease in crime was much more significant after its publicization. As we wrote in the Introduction, Varian (1996) once wrote that public information becomes "too" public under new information technologies that lower the cost of access. Our results suggest that, even in presence of such lowered costs, catalytic events are needed to raise the public's awareness of those data. Furthermore, seasonality (as well as countless other factors) may have affected the general crime trends in the Memphis area.

Second, and more pertinently to our analysis, we should stress that our econometric approach was aimed at teasing out differential patterns in the number of crimes across zip codes with different numbers of guns. Therefore, it should not be concluded, based only on these data, that an overall increase in gun permits will correspond to overall net decreases in crime, since - in an arms-race dynamic - even zip codes with an absolutely high number of permits may still be perceived as less dangerous by criminals, *relative* to zip codes with even higher numbers of permits.

Third, it is open to debate whether a privacy cost had to be actually paid for the decrease in crimes following the publicization of the database. On the one hand, one may conjecture that an anonymous publication of gun permits statistics - such as the number of permits in a given zip code - may have achieved similar results, without invading individual holders' privacy. On the other hand, part of the appeal of the database, and the reason why it drew significant traffic, was arguably the fact that it included individual permit holders' names.

Fourth, the kind of crime data that we have access to does not contain information about the specifics of the crime. Therefore, we cannot distinguish between, say, burglaries where many things were stolen and burglaries where little was stolen.

Finally, the location where the gun permits publication occurred has several state-specific features that may make the findings non-generalizable. Unlike in most states, in Tennessee there is no requirement to conceal a weapon. This, in theory, should mute the effects that we study, and may explain why we observe a reduction in burglaries but not in person-to-person crimes. Tennessee, and Memphis in particular, also have many gun permits (as high as one gun permit for every three Memphis dwellings). It is not clear whether criminals could be deterred by publicly available information on gun permits if there were fewer gun permit holders. The Commercial Appeal redacted street address information, making their

information available at the zip code level. It is not clear whether the pattern of a zip-code-wide reduction in crimes would hold if gun permit street addresses were also available.

On the one hand, a gun permit holder in Memphis may have felt violated even if the publication of the database led to a statistical decrease of certain crimes in her zip code. On the other hand, members of the Memphis community may still invoke a right to know who and how many, among them, are carrying arms, independently of the positive or negative effects on certain types of crime associated with the publication of that information. Our findings, however, provide new evidence for this debate, and bear witness to the power that the revelation of personal information can have on complex societal dynamics.

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Table A-1: Results for Memphis: Logged Dependent Variable

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Guns	-0.0760	0.0125	-0.103	0.0131	-0.187	-0.139
	(0.0507)	(0.0573)	(0.0655)	(0.109)	(0.309)	(0.0881)
Post-Publicity*Bottom Third Guns	0.0583	-0.00826	-0.0157	0.0247	-0.0956	0.0789
	(0.0698)	(0.181)	(0.0905)	(0.125)	(0.365)	(0.174)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	893	744	701	509	81	758
Log-Likelihood						

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.000118	-0.000103	-0.0000336	0.0000185	0.000536	-0.000188**
	(0.0000880)	(0.000119)	(0.000107)	(0.000136)	(0.000670)	(0.0000791)
Guns	0.000689	0.00150**	-0.000526	0.000507	-0.00545	0.000570
	(0.000608)	(0.000676)	(0.000723)	(0.000743)	(0.00602)	(0.000664)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	893	744	701	509	81	758
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is logged weekly observations of different crimes in the Memphis area. These results should be compared to Table 6, where the dependent variable is transformed by adding one to account for the over-dispersion of zeroes.

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Table A-2: Results for Memphis: Negative Binomial Regression

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Guns	-0.0860	-0.0589	-0.0617	0.0881	0.00764	-0.0818
	(0.0649)	(0.0804)	(0.0982)	(0.175)	(0.376)	(0.127)
Post-Publicity*Bottom Third Guns	0.0477	0.0111	0.0343	0.000000817	0.0364	0.00962
	(0.0651)	(0.259)	(0.153)	(0.197)	(0.775)	(0.160)
Top Third Guns	-17.60***	-18.52***	-17.23***	-16.21***	-17.15***	-19.45***
	(1.010)	(1.015)	(1.013)	(1.014)	(1.118)	(1.012)
Bottom Third Guns	-2.387***	-2.674***	-2.386***	-1.436***	-16.15***	-2.278***
	(0.0297)	(0.0319)	(0.0502)	(0.0816)	(1.055)	(0.0532)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood	-2891.9	-2108.7	-2051.3	-1410.3	-295.6	-2117.8

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.000217**	-0.000239*	-0.0000935	0.000000758	0.00235	-0.000213
	(0.0000869)	(0.000144)	(0.000146)	(0.000294)	(0.00153)	(0.000179)
Guns	0.000955	0.00159	0.000112	0.00151	-0.0242**	0.000997
	(0.000589)	(0.000993)	(0.000991)	(0.00202)	(0.0116)	(0.00119)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood	-2891.1	-2107.6	-2051.3	-1409.5	-292.7	-2117.5

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area. These results should be compared to Table 7, where the dependent variable is transformed by adding one to account for the over-dispersion of zeroes.

Table A-3: Results for Memphis: Zip codes with < 200 population

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Guns	-2.974***	-1.719***	-0.185	-0.0111	-0.0259	-1.033***
	(0.714)	(0.594)	(0.288)	(0.223)	(0.0544)	(0.367)
Post-Publicity*Bottom Third Guns	1.980	1.512***	-0.334	0.444**	-0.0185	0.377
	(1.280)	(0.561)	(0.299)	(0.182)	(0.0424)	(0.414)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1500	1500	1500	1500	1500	1500
Log-Likelihood						

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.00252***	-0.00150**	0.0000607	-0.000236	0.00000110	-0.000844***
	(0.000872)	(0.000630)	(0.000293)	(0.000189)	(0.0000390)	(0.000183)
Post-Publicity	10.49***	2.960	5.805***	-0.127	0.176*	1.673*
	(3.219)	(1.932)	(1.068)	(0.588)	(0.0901)	(0.975)
Guns	0.00704	0.00406	-0.00141	0.00181	-0.000213	0.00279*
	(0.00576)	(0.00356)	(0.00192)	(0.00109)	(0.000171)	(0.00165)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1500	1500	1500	1500	1500	1500
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area.

Table A-4: Results for Memphis: Zip code cubic time fixed effects

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.00218***	-0.000741	-0.000193	-0.0000955	-0.0000183	-0.00113***
	(0.000613)	(0.000481)	(0.000242)	(0.0000828)	(0.0000149)	(0.000186)
Guns	-0.00532	-0.00346***	-0.00264	0.000225	-0.000326***	0.000876
	(0.00357)	(0.00113)	(0.00164)	(0.000525)	(0.000102)	(0.00135)
d_zip_1_timetrend	-0.0958**	-0.0283	-0.0141	-0.0146***	0.00121***	-0.0399***
	(0.0431)	(0.0196)	(0.0117)	(0.00417)	(0.000408)	(0.0126)
d_zip_1_timetrend2	-0.000141	0.0000108	-0.00000738	-0.0000548**	0.0000105***	-0.0000999
	(0.000213)	(0.0000984)	(0.0000581)	(0.0000217)	(0.00000293)	(0.0000625)
d_zip_2_timetrend	-0.0713**	-0.0187	-0.0177***	-0.00824**	0.00127*	-0.0279***
	(0.0281)	(0.0132)	(0.00530)	(0.00398)	(0.000651)	(0.00847)
d_zip_2_timetrend2	-0.000176	-0.00000600	-0.0000563*	-0.0000239	0.00000714**	-0.0000965**
	(0.000135)	(0.0000641)	(0.0000290)	(0.0000197)	(0.00000333)	(0.0000373)
d_zip_3_timetrend	-0.0734***	-0.0197	-0.0201***	-0.00821**	0.00126*	-0.0267***
	(0.0264)	(0.0122)	(0.00504)	(0.00348)	(0.000671)	(0.00792)
d_zip_3_timetrend2	-0.000201	-0.0000164	-0.0000685**	-0.0000265	0.00000697**	-0.0000965***
	(0.000125)	(0.0000576)	(0.0000271)	(0.0000179)	(0.00000341)	(0.0000347)
d_zip_4_timetrend	-0.0661***	-0.0162	-0.0194***	-0.00791**	0.00133*	-0.0240***
	(0.0243)	(0.0119)	(0.00454)	(0.00387)	(0.000782)	(0.00683)
d_zip_4_timetrend2	-0.000202*	-0.0000144	-0.0000731***	-0.0000236	0.00000644*	-0.0000969***
	(0.000113)	(0.0000550)	(0.0000240)	(0.0000189)	(0.00000375)	(0.0000291)
d_zip_5_timetrend	-0.0897*	-0.0268	-0.0199**	-0.00848	0.00104**	-0.0355**
	(0.0471)	(0.0202)	(0.00835)	(0.00617)	(0.000411)	(0.0171)
d_zip_5_timetrend2	-0.0000718	0.0000387	-0.0000269	-0.0000265	0.0000101***	-0.0000671
	(0.000221)	(0.0000917)	(0.0000442)	(0.0000267)	(0.00000289)	(0.0000752)
d_zip_6_timetrend	0.0166	-0.00392	-0.0109	-0.0131**	0.00126***	0.0432**
	(0.0408)	(0.0145)	(0.0114)	(0.00611)	(0.000464)	(0.0198)
d_zip_6_timetrend2	0.000263	0.000132*	-0.00000521	-0.0000392	0.00000951***	0.000166*
	(0.000198)	(0.0000688)	(0.0000565)	(0.0000314)	(0.00000300)	(0.0000903)
d_zip_7_timetrend	-0.0346	-0.00530	-0.00342	-0.00511	0.00126*	-0.0220***
	(0.0255)	(0.0104)	(0.00832)	(0.00314)	(0.000633)	(0.00780)
d_zip_7_timetrend2	0.00000617	0.0000765	-0.00000468	-0.00000447	0.00000733**	-0.0000685*
	(0.000125)	(0.0000525)	(0.0000392)	(0.0000174)	(0.00000331)	(0.0000403)
d_zip_8_timetrend	-0.0684**	-0.0172	-0.0196***	-0.00798**	0.00128*	-0.0249***
	(0.0259)	(0.0121)	(0.00576)	(0.00389)	(0.000742)	(0.00718)
d_zip_8_timetrend2	-0.000198	-0.0000135	-0.0000707**	-0.0000236	0.00000650*	-0.0000966***
	(0.000121)	(0.0000573)	(0.0000293)	(0.0000191)	(0.00000362)	(0.0000309)
d_zip_9_timetrend	-0.0519*	-0.00665	-0.0147***	-0.00715**	0.00124*	-0.0246***
	(0.0276)	(0.0136)	(0.00534)	(0.00351)	(0.000664)	(0.00829)
d_zip_9_timetrend2	-0.000103	0.0000430	-0.0000470	-0.0000215	0.00000681**	-0.0000847**
	(0.000134)	(0.0000657)	(0.0000291)	(0.0000185)	(0.00000336)	(0.0000368)
d_zip_10_timetrend	-0.0621***	-0.0157	-0.0171***	-0.00780**	0.00132	-0.0228***
	(0.0229)	(0.0116)	(0.00453)	(0.00384)	(0.000823)	(0.00651)
d_zip_10_timetrend2	-0.000200*	-0.0000196	-0.0000656***	-0.0000234	0.00000601	-0.0000971***
	(0.000104)	(0.0000525)	(0.0000233)	(0.0000187)	(0.00000387)	(0.0000274)
d_zip_11_timetrend	-0.0627***	-0.0159	-0.0171***	-0.00783**	0.00132	-0.0232***
	(0.0232)	(0.0117)	(0.00457)	(0.00385)	(0.000808)	(0.00660)
d_zip_11_timetrend2	-0.000197*	-0.0000181	-0.0000645***	-0.0000235	0.00000614	-0.0000971***
	(0.000106)	(0.0000533)	(0.0000237)	(0.0000187)	(0.00000383)	(0.0000279)
d_zip_12_timetrend	-0.0633***	-0.0162	-0.0173***	-0.00783**	0.00130	-0.0233***
	(0.0234)	(0.0117)	(0.00459)	(0.00385)	(0.000798)	(0.00663)
d_zip_12_timetrend2	-0.000197*	-0.0000181	-0.0000645***	-0.0000235	0.00000613	-0.0000969***
	(0.000107)	(0.0000538)	(0.0000240)	(0.0000187)	(0.00000379)	(0.0000280)
d_zip_13_timetrend	-0.0629***	-0.0160	-0.0172***	-0.00782**	0.00131	-0.0231***
	(0.0231)	(0.0117)	(0.00453)	(0.00384)	(0.000813)	(0.00658)
d_zip_13_timetrend2	-0.000200*	-0.0000197	-0.0000656***	-0.0000234	0.00000600	-0.0000970***
	(0.000106)	(0.0000531)	(0.0000234)	(0.0000187)	(0.00000384)	(0.0000277)
d_zip_14_timetrend	-0.0621***	-0.0157	-0.0171***	-0.00780**	0.00133	-0.0229***
	(0.0230)	(0.0116)	(0.00454)	(0.00384)	(0.000822)	(0.00651)
d_zip_14_timetrend2	-0.000199*	-0.0000193	-0.0000653***	-0.0000235	0.00000604	-0.0000971***
	(0.000105)	(0.0000526)	(0.0000234)	(0.0000187)	(0.00000387)	(0.0000274)
d_zip_15_timetrend	-0.0631***	-0.0146	-0.0167***	-0.00793**	0.00132*	-0.0252***
	(0.0221)	(0.0110)	(0.00486)	(0.00388)	(0.000778)	(0.00667)
d_zip_15_timetrend2	-0.000192*	-0.00000914	-0.0000624**	-0.0000236	0.00000644*	-0.000103***
	(0.000104)	(0.0000526)	(0.0000254)	(0.0000189)	(0.00000374)	(0.0000277)

Table A-5: Results for Memphis: Blotter Crimes

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Top Third Guns	-2.804***	-1.644***	-0.193	0.0370	0.0333*	-1.037***
	(0.687)	(0.598)	(0.299)	(0.226)	(0.0168)	(0.376)
Post-Publicity*Bottom Third Guns	2.226*	1.567***	-0.370	0.563***	0.0333	0.433
	(1.285)	(0.554)	(0.310)	(0.175)	(0.0210)	(0.415)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

	(1) No. All	(2) No. Burglary	(3) No. Assault	(4) No. Robbery	(5) No. Shooting	(6) No. Theft
Post-Publicity*Guns	-0.00272***	-0.00158**	0.000115	-0.000297	0.00000621	-0.000964***
	(0.000871)	(0.000629)	(0.000288)	(0.000184)	(0.00000870)	(0.000183)
Guns	0.00712	0.00369	-0.00144	0.00170	-0.0000327	0.00320**
	(0.00586)	(0.00365)	(0.00183)	(0.00111)	(0.0000511)	(0.00153)
Zipcode Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1620	1620	1620	1620	1620	1620
Log-Likelihood						

Standard errors adjusted for spatial correlation clustered at zip code level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is weekly observations of different crimes in the Memphis area.

Table A-6: Results for Border Counties

	(1) Total All	(2) Total Burglary	(3) Total Assault	(4) Total Robbery	(5) Total Shooting	(6) Total Theft
Post-Publicity*Top Third Guns	-0.726*	-0.0902	-0.162	-0.105	-0.0526	-0.316
	(0.393)	(0.174)	(0.235)	(0.0734)	(0.0534)	(0.233)
Post-Publicity*Bottom Third Guns	-0.357	-0.143	-0.214	-9.88e-17	5.34e-18	5.69e-16***
	(0.226)	(0.0974)	(0.215)	(1.30e-16)	(4.39e-17)	(1.92e-16)
Post-Publicity	0.357	0.143	0.214	4.65e-17	-1.86e-17	-5.25e-16*
	(0.226)	(0.0974)	(0.215)	(1.44e-16)	(6.80e-17)	(2.94e-16)
Bottom Third Guns	-0.0714	-0.0714	1.86e-17	1.35e-16***	5.02e-17	-7.51e-16***
	(0.0717)	(0.0717)	(.)	(3.93e-17)	(.)	(1.36e-16)
Top Third Guns	0.823**	0.0865	0.105	0.158*	0.0526	0.421
	(0.401)	(0.137)	(0.0734)	(0.0872)	(0.0534)	(0.272)
Constant	0.0714	0.0714	4.16e-17	-8.33e-17***	-3.64e-17	5.13e-16***
	(0.0717)	(0.0717)	(.)	(2.22e-25)	(2.78e-17)	(7.85e-17)
Observations	88	88	88	88	88	88
Log-Likelihood	-120.1	-33.25	-39.60	17.35	74.52	-71.93

	(1) Total All	(2) Total Burglary	(3) Total Assault	(4) Total Robbery	(5) Total Shooting	(6) Total Theft
Post-Publicity*Guns	-0.000487***	-0.0000547	-0.0000808	-0.0000649	-0.0000502**	-0.000237**
	(0.000178)	(0.0000707)	(0.0000782)	(0.0000411)	(0.0000202)	(0.000104)
Post-Publicity	0.258	0.0953	0.146	-0.00258	0.0108	0.00898
	(0.230)	(0.0917)	(0.101)	(0.0532)	(0.0261)	(0.134)
Guns	0.000653***	0.000125**	0.0000753	0.0000689**	0.0000502***	0.000333***
	(0.000126)	(0.0000500)	(0.0000553)	(0.0000290)	(0.0000143)	(0.0000732)
Constant	-0.0271	0.00722	-0.00490	0.0221	-0.0108	-0.0407
	(0.158)	(0.0629)	(0.0695)	(0.0365)	(0.0179)	(0.0920)
Observations	88	88	88	88	88	88
Log-Likelihood	-112.3	-31.29	-40.06	16.58	79.21	-64.78

Standard errors clustered by zip code. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Dependent variable is pre- and post-publicization of database observations of different crimes in the border counties area.

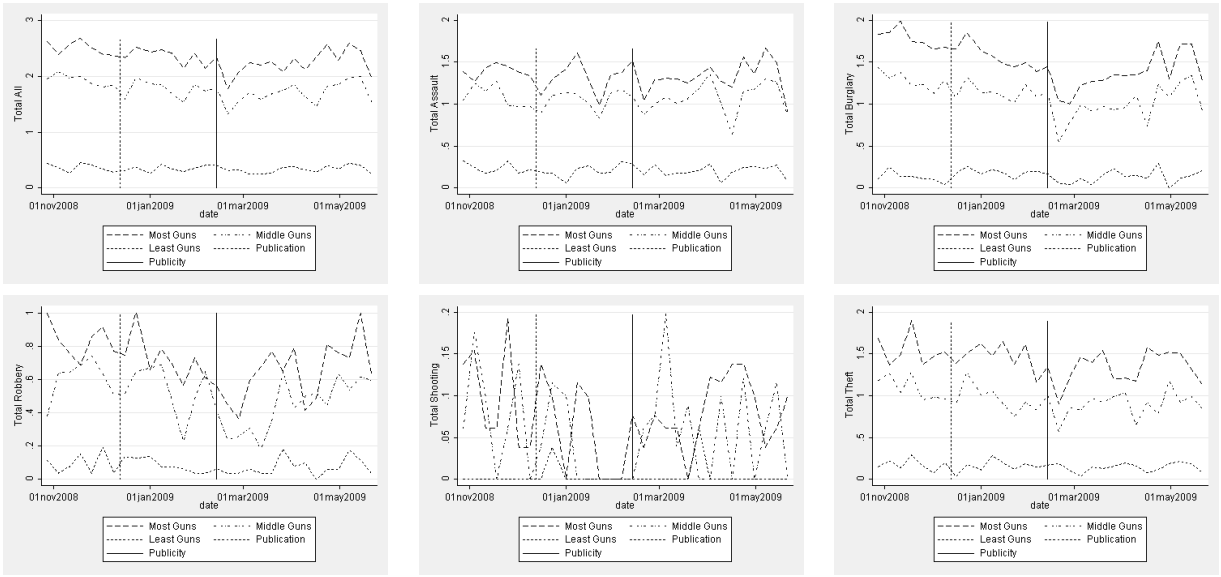

Figure A-1: Variation of Logged Weekly Crimes by Gun Permits.

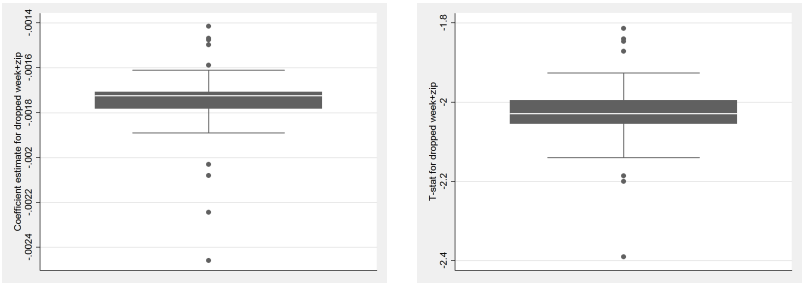

Figure A-2: Robustness of results to dropped variables.