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Who Watches the Watchmen?

Local News and Police Behavior in the United States*

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Abstract

Do U.S. municipal police departments respond to news coverage of local crime? We address this question exploiting an exogenous shock to local crime reporting induced by acquisitions of local TV stations by a large broadcast group, Sinclair. Using a unique dataset of 8.5 million news stories and a triple differences design, we document that Sinclair acquisitions decrease news coverage of local crime. This matters for policing: municipalities that experience the change in news coverage have lower violent crime clearance rates relative to municipalities that do not. The result is consistent with a decrease of crime salience in the public opinion.

JEL Codes: K42, D73

Keywords: Police, Local News, Clearance Rates, Sinclair

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1 Introduction

Law enforcement is one of the most important functions of U.S. local governments, yet we have a limited understanding of what factors shape the incentive structure of police departments (Owens (2020)). Recent years have seen an increased debate on the extent to which civil society is able to influence the behavior of police officers. In this paper, we investigate a force that might have a role to play in this respect: local media. Focusing on local TV news, we find that the police are responsive to changes in news coverage of local crime.

Local media, and local news in particular, might influence the behavior of public officials through two main channels. First, by providing information to the public, the news facilitates monitoring (Ferraz and Finan (2011), Lim et al. (2015), Snyder Jr and Strömberg (2010)). This is especially true at the local level, where the news garners high levels of trust (Knight Foundation (2018)) and serves as one of the few democratic watchdogs (Rolnik et al. (2019)). Second, what news the media cover influences perceptions of topics that are salient in the political debate (DellaVigna and Kaplan (2007), Martin and Yurukoglu (2017), Mastrorocco and Minale (2018)), potentially affecting the demand for specific policies (Galletta and Ash (2019)).

Studying the relationship between local news and the police is especially interesting for two reasons. The first has to do with media content and the fact that local news focuses on a topic closely intertwined with policing: crime. In local TV news—the focus of our study—crime is the most popular topic, appearing in almost 25% of all local stories. This makes local news uniquely positioned to influence how the public perceives police behavior. The second has to do with police departments themselves. Because of protections coming from strong union contracts and civil service laws, the organizational structure of police departments creates barriers to their responsiveness to the public. As a result, how the police respond to external incentives is an open question.

The specific research question that we ask is how a decline in TV news coverage of local crime impacts the behavior of police officers. Our proxy for police behavior are clearance rates, i.e. crimes

cleared over total crimes.[1] To get exogenous variation in the probability that local crime is covered by local TV news, we exploit the fact that, in the last ten years, the local TV market has seen a large increase in concentration driven by broadcast groups acquiring high numbers of TV stations, and that acquisitions are likely to affect content (Stahl (2016)). We focus in particular on the most active group in this sense: Sinclair.

Sinclair acquisitions affect content in two ways. First, Sinclair reduces local news in favor of a national focus (Martin and McCrain (2019)). This gives us variation in news coverage of local crime, which is the change in content we are interested in studying. In addition to this, however, Sinclair—a right-leaning media group—also makes content more conservative. This means that a differences-in-differences design exploiting the staggered timing of Sinclair entry across media markets would not be appropriate to answer our research question, as it would not allow us to separately identify the effect of the two changes in content.

To address this challenge, we rely on the fact that the relevant geography for local TV stations is that of a media market, which by definition is a region where all households receive the same TV station offerings. This means that once Sinclair acquires a station, all municipalities in that station's media market experience its conservative messaging. However, not all municipalities are equally exposed to the shock in news coverage of local crime.

The proxy for exposure that we use is the baseline probability that a municipality is covered in the news.[2] The intuition for this is that, if Sinclair acquisitions decrease local news coverage as we hypothesize, municipalities often in the news at baseline (covered municipalities) should bear the brunt of the decline. Instead, municipalities that were never in the news in the first place (non-covered municipalities) are also not going to be covered after Sinclair acquires a station: they do not experience any change in their news coverage of local crime.

[1] More precisely, clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. A crime is considered cleared if at least one person has been arrested, charged, and turned over for prosecution or if the offender has been identified, but external circumstances prevent an arrest. Clearance rates are highly sensitive to what resources are allocated to investigations and have often been used by economists to study police behavior (see, among others, Mas (2006), Shi (2009), and Premkumar (2020)).

[2] More precisely, we define covered municipalities as municipalities mentioned in the news more than the median municipality in 2010.

Our empirical strategy is a triple differences design that combines variation from the staggered timing of Sinclair acquisitions with cross-sectional variation across municipalities in whether they are covered by the news at baseline. In other words, we estimate the effect of a decline in the probability that a municipality appears in the news with a crime story on the violent crime clearance rate by focusing on the relative effect of the Sinclair entry on covered municipalities, that experience *both* Sinclair's conservative slant *and* a large decline in the probability that their local crime events appear in the news, and non-covered municipalities, that also experience Sinclair's conservative slant *but* no change in the probability that their crime events appear in the news.

For this to identify a causal effect, covered and non-covered municipalities must be on parallel trends leading up to Sinclair's entry. We provide evidence supporting this assumption using an event study specification that allows the relative effect of Sinclair in covered and non-covered municipalities to vary in time since and to treatment. In addition, Sinclair's acquisitions must not be driven by differential trends in the two types of municipalities. We provide suggestive evidence that this is the case by looking at cases in which Sinclair acquires stations in a bundle (namely, by acquiring a smaller broadcast group), where entry is less likely to be endogenous to a specific media market's condition.

We begin by characterizing in detail how Sinclair acquisitions affect news coverage of local crime. We do so using a novel dataset containing the transcripts of almost 8.5 millions stories in 300,000 newscasts. These data allow us track news coverage of 325 stations weekly from 2010 to 2017, which represents a significantly larger time and geographic coverage with respect to previous studies of local TV news content (see, for example, Moskowitz (2021)).

We use these data to quantify the change in coverage of local crime induced by Sinclair acquisitions. To do so, we identify crime stories using a pattern-based sequence-classification method that labels a story as being about crime if it contains a "crime bigram." That is, if it contains two word combinations (i.e. bigrams) that are much more likely to appear in crime-related stories of the Metropolitan Desk Section of the New York Times than in non-crime related ones. In addition, we assign stories to municipalities based on mentions of the municipality's name.

We find that ownership matters for content: once acquired by Sinclair, local TV stations decrease news coverage of local crime. In particular, covered municipalities are 2.1 percentage points less likely to be mentioned in a crime story after a station gets acquired by Sinclair compared to non-covered municipalities. The effect is significant at the 1% level and economically important, corresponding to almost 25% of the baseline outcome mean. Examining the timing of content changes, we find a reduction in local crime coverage in the year that immediately follows the acquisition, with the effect increasing over time. Other stations in the same media market do not change their crime coverage after Sinclair entry: the main result is explained by an editorial decision of Sinclair.

How does the change in news coverage of local crime impact clearance rates? We estimate that after Sinclair enters a media market, covered municipalities experience 3.4 percentage points lower violent crime clearance rates relative to non-covered municipalities. The effect is significant at the 5% level, and corresponds to 7.5% of the baseline mean. This shows that there is scope for external forces to exert an influence on police behavior, despite the strong protections that police officers get from strong union contracts and civil service laws.

Using an event study specification, we find no difference between covered and non-covered municipalities in the four years before Sinclair enters the media market. The effect appears within the first year after treatment and becomes smaller over time, which is potentially consistent with a rational learning model in which viewers learn that the signal on local crime that they receive from Sinclair is biased over time, and adjust for it based on their own observation or other media sources (DellaVigna and Kaplan (2007)).[3]

In contrast, property crime clearance rates do not experience a similar decline. This heterogeneity can be explained by the fact that local TV news has a clear violent crime focus. We document this in our data by training a classifier model to identify whether local crime stories are about a violent or a property crime. We show that 91% of the stories are about a violent crime and only 17% are

[3]We also provide evidence of the robustness of our estimates when taking into account concerns of heterogeneous treatment effect with two way fixed effects estimators (de Chaisemartin and D'Haultfœuille (2020)).

about a property crime (8% are about both), a difference which is even starker if we consider that property crimes are more common by orders of magnitude. Our unique content data underpin one of the most novel contributions of this paper: the ability to characterize in detail the content shock and precisely map content into the real-word outcomes we are interested in studying.

The effect on the violent crime clearance rate is not explained by changes in violent crime rates. However, we find that, after Sinclair entry, covered municipalities have higher property crime rates relative to non-covered municipalities. This can be explained by a decreased incapacitation or deterrence effect due to the lower clearance rates. Finally, we do not find evidence of the decrease in crime coverage affecting police violence, although we cannot draw strong conclusions because of the imprecision of our estimates.

We propose the following explanation for our results. When stories about a municipality's violent crimes are less frequent, crime loses salience in the eyes of local citizens and the police find themselves operating in a political environment where there is less pressure to clear violent crimes.[4] As a result, they might reallocate their resources away from clearing these crimes in favor of other policing activities. Three pieces of evidence are consistent with this explanation. First, we use data on monthly Google searches containing the terms "crime" and "police" to show that indeed, after Sinclair enters a media market, the attention given to these issues decreases. Second, exploiting survey data from Gallup, we find that, after Sinclair entry, it is less likely that individuals report crime to be the most important problem facing the country in covered relative to non-covered municipalities. Third, we note that the key audience of local news, individuals over 55 years of age, are also an important interest group for local politics and law enforcement in particular (Goldstein, 2019). Consistent with this, we find that the effect is driven precisely by those municipalities where individuals over 55 years of age constitute a larger share of the population. We interpret this evidence as supporting the idea of a feedback mechanism from salience to police behavior through

[4]Crime news are one of the most important determinants of salience of crime, more so than actual crime rates (see Ramırez-Alvarez (2021), Shi et al. (2020) and Velásquez et al. (2020)). In addition, Mastrorocco and Minale (2018) show using data from Italy that, when exposed to less crime related news, individuals become less concerned about crime.

citizens' and politicians' pressure.

Alternatively, it is possible that the effect might be explained by explicit monitoring of the police. If police officers anticipate a lower probability of appearing in the news if they fail to solve a crime, they might shirk. We find this explanation to be less convincing because the decline in crime reporting is almost entirely driven by stories about crime incidents as opposed to stories that are arrest-related, thus not changing the probability of delays in solving a crime being the subject of a story. The same result also suggests that it is unlikely that perceptions of police are negatively affected by the content change, which makes it unclear why community cooperation with the police should be affected by Sinclair entry. In addition, we argue that the magnitude of the effect on violent crime clearance rates is not consistent with a decline in tips being the main driver of the effect. Finally, we consider whether the pattern we observe could be explained by Sinclair's conservative slant rather than by the change in news coverage of local crime, but we show several pieces of evidence that are not in line with this interpretation.

A long tradition in the economics of media shows that the media influence the behavior of public officials. By providing information on current events, the media performs a monitoring function (Ferraz and Finan (2011), Lim et al. (2015), Snyder Jr and Strömberg (2010)). In addition, media content impacts individuals' beliefs and voting decisions (DellaVigna and Kaplan (2007), Durante et al. (2019), Durante and Knight (2012), Martin and Yurukoglu (2017), Spenkuch and Toniatti (2018)). We contribute to this literature in three ways. First, our extensive content data, which span multiple years and include a large share of TV stations, allow us to precisely document and quantify the content changes and their timing following acquisitions. As a result we can exactly map how content influences policy. Second, by focusing on the relationship between the media and the police, we show that media content has the potential to influence even an institution not generally considered to be responsive to external forces. Third, in the discussion of the mechanisms, we provide evidence on how media-induced changes in perceptions may feed back into the behavior of public officials. The two papers that are closest to ours in this respect are Galletta and Ash (2019) and Ash and Poyker (2019), which study how FOX News influences local

government spending and judges' sentencing decisions; they also show that the way in which the media influence preferences might have a policy impact. We add to these papers by studying the role played by crime perceptions in influencing police behavior.

One of our most policy-relevant findings is that ownership of local TV stations affects content in a way that is consequential for public officials: the trend of increasing concentration, which currently characterizes not only the local TV industry but also other media types such as newspapers (Hendrickson (2019)), might have tangible externalities (Prat (2018), Stahl (2016), Angelucci et al. (2020)). This questions the use of standard criteria in competition and antitrust regulation of media industries (Rolnik et al. (2019)). Consistent with Martin and McCrain (2019), we confirm that Sinclair acquisitions lead to a crowding out of local news in favor of national stories. We add to this paper by investigating the consequences of this shift for the behavior of police officers.

Finally, we contribute to the growing literature aimed at understanding the determinants of police behavior (see, among others, Ba (2018), Chalfin and Goncalves (2020), Dharmapala et al. (Forthcoming), Grosjean et al. (2020), Mas (2006), McCrary (2007), Stashko (2020)) and the role played by institutional level incentives in particular (Goldstein et al. (2020), Harvey (2020), Makowsky and Stratmann (2009)). To the best of our knowledge, ours is one of the first studies to provide systematic causal evidence on how crime news influences the police. It is particularly interesting to contrast our finding that a reduction in news coverage of local crime decreases clearance rates with the evidence that increases in monitoring following scandals can sometimes have the same effect (Ba and Rivera (2019), Premkumar (2020), Devi and Fryer Jr (2020)). The two results can be rationalized by the attention change being of a very different nature: negative outside pressure following scandals is likely to be very different than increases in crime salience driven by media coverage of crime incidents.

The remainder of the paper proceeds as follows. In the next section we present the background, in Section 3 the data, and in Section 4 the empirical strategy. The main results of the effect of Sinclair on local news are in Section 5, and the results of the effect of Sinclair on police behavior are in Section 6. Section 7 discusses potential mechanisms. Finally, we conclude in Section 8.

2 Background

2.1 Institutional Setting

A media market, also known as designated market area (or DMA), is a region where the population receives the same television and radio station offerings. Media markets are defined by Nielsen based on households' viewing patterns: a county is assigned to the media market if that media market's stations achieve the highest viewership share.[5] As a result, media markets are non-overlapping geographies. In each market, we focus on stations that are affiliated to one of the big-four networks (ABC, CBS, FOX, and NBC) as they they tend to take up most of the viewership and be the ones producing local newscasts.[6] In fact, 85% of local TV stations that do so belong to this category (Papper, 2017).

2.2 Local TV News

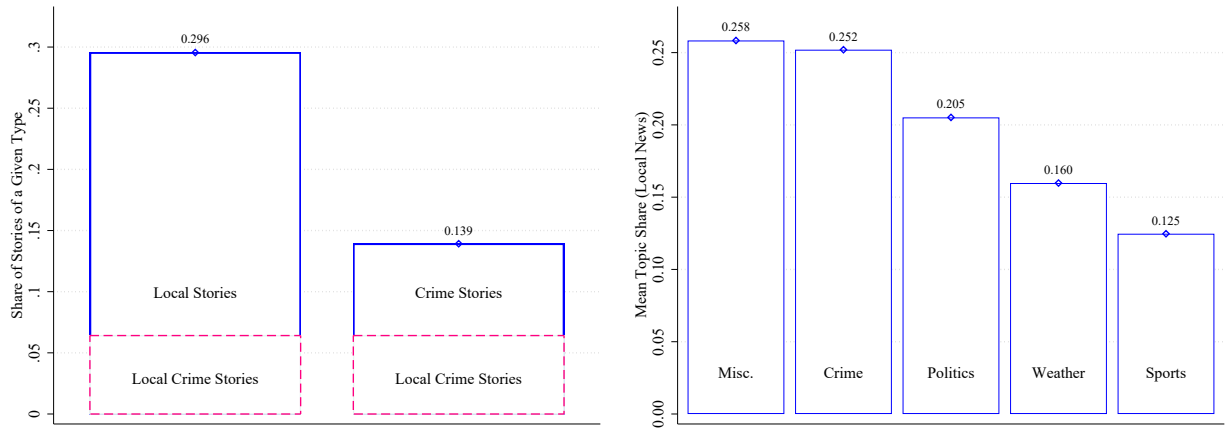
Although its popularity has been declining in recent years, local TV news remains a central source of information for many Americans. In a 2017 Pew Research Center report, 50% of U.S. adults mentioned often getting their news from television, a higher share than those turning to online sources (43%), the radio (25%), or print newspapers (18%) (Gottfried and Shearer, 2017). Among TV sources, news stories airing on local TV stations have larger audiences than those on cable or on national networks (Matsa, 2018).

In addition, the overarching narrative regarding the decline in TV news masks substantial heterogeneity. First, the decrease in viewership has been limited outside top-25 media markets (Wenger and Papper, 2018). In fact, local TV news still plays an important role in small and medium sized markets, both in terms of viewership and because there tend to be fewer outlets such as newspapers producing original news focusing on the area (Wenger and Papper, 2018).

[5]Counties can be split across media markets, but this happens rarely in practice. As noted by Moskowitz (2021), only 16 counties out of 3130 are split across media markets. Similarly, while media markets are redefined by Nielsen every year, only 30 counties changed their media market affiliation between 2008 and 2016.

[6]Networks are publishers that distribute branded content. Affiliated stations, although under separate ownership, carry the television lineup offered by the network while also producing original content. With few exceptions, each network has a single affiliate by media market.

Figure I: Local TV News Content



(a) Types of News Stories

(b) Local News Topics

Notes: This figure describes local TV news content. Panel (a) shows the share of stories that are local, that are about crime, and both local and about crime. A story is local if it mentions at least one of the municipalities with more than 10,000 people in the media market. A story is about crime if it contains a "crime bigram" (i.e. a bigram that is much more likely to appear in crime-related stories than in non-crime related ones of the Metropolitan Desk Section of the New York Times). For more details, see Section 3. Panel (b) shows the mean topic share from an unsupervised LDA topic model trained on local stories. In both graphs, the sample is restricted to media markets that never experienced Sinclair entry.

Second, the decline has been concentrated in younger demographics, while the core audience of local TV news – those above 50, who constitute 73% of the viewership – has not been been affected (Wenger and Papper, 2018). Considering that local TV news also tends to garner the highest levels of trust from the public (Mitchell et al., 2016), it constitutes an important source that has the potential to shape public information and perceptions.

What is local TV news about? Our novel content data allow us to provide a precise answer to the question. Newscasts of local TV stations include both national and media market-specific stories. As we show in Figure I Panel (a), approximately 30% of stories are specific to the media market, in that they mention at least one same media market municipality with more than 10,000 people. Crime is a prime subject of local TV news: 22% of local stories are crime-related (13% overall).[7]

To have a more complete picture of the breakdown of topics covered in local TV news, we also train an unsupervised LDA model with five topics on the 2 million local stories in our content data.[8] In Figure I Panel (b), we show the average topic shares across all local news stories. Again, apart from

[7] We discuss in detail the content data and the methodology we use to identify local stories and crime stories in the following section.

[8] Appendix Figure I shows word clouds with the 50 words that have the highest weight for the five topics. Four of the five topics can be easily identified to be related to crime, politics, weather, and sports. The last topic appears to be a miscellaneous topic with no clear meaning.

a miscellaneous topic with no clear meaning, the most covered topic is crime (with a topic share of 25%), followed by politics (20.5%), weather (16%), and sports (12.5%). Given the crime focus of TV newscasts, studying the relationship between local news and police departments appears to be first order.

2.3 The Sinclair Broadcast Group

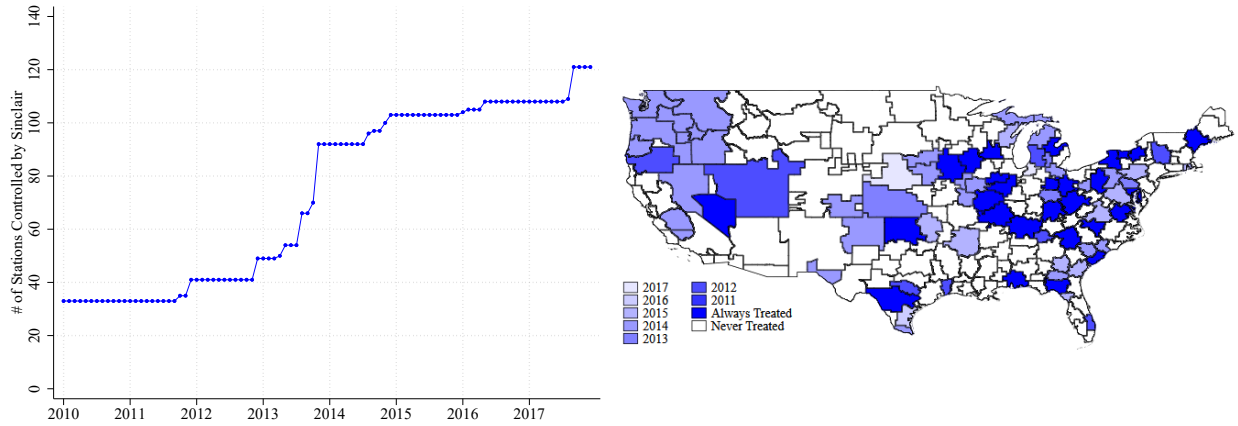
Since 2010, the local TV market has seen the emergence of large broadcast groups owning a significant share of local TV stations (Matsa, 2017). We focus on one of the most active players in the local TV market: the Sinclair Broadcast Group. Figure II Panel (a) shows the number of local TV stations under Sinclair control monthly from 2010 to 2017. Sinclair expanded from 33 stations in January 2010 to 121 stations in December 2017, which corresponds to about 14.5% of all big-four affiliates. As shown in Figure II Panel (b), there have been acquisitions in media markets across the United States, although Sinclair was particularly active in medium-sized media markets. Given that the Federal Communications Commission restricts the number of stations that a single entity can control in each media market, Sinclair acquisitions generally correspond to Sinclair owning one out of the four stations we consider.[9]

With respect to other broadcast groups, Sinclair holds a right-leaning political orientation (see Miho (2020) for a detailed discussion) and it appears to be particularly interested in controlling the messaging of its stations (Fortin and Bromwich (2018)). Importantly, after acquisitions, stations maintain their call sign, network affiliation, and news anchors: it might take time for viewers to realize that content has changed.

Existing research supports the anecdotal evidence. Martin and McCrain (2019) show using a differences-in-differences design that when Sinclair acquired the Bonten Media Group in 2017, the ideological slant of Bonten stations moved to the right. Miho (2020) shows that Sinclair's conservative leaning might have real word effects, with exposure to Sinclair-owned stations increasing the Republican vote share in presidential elections. In addition, Martin and McCrain (2019) also show

[9] A single entity can control at most two stations in a media market provided that the signal areas of the stations do not overlap, or at least one of the stations is not among the four with the highest audience share.

Figure II: Sinclair Acquisitions over Time and Space



(a) Number of Stations Controlled by Sinclair

(b) Media Markets Experiencing Sinclair Entry

Notes: Panel (a) shows the number of big-four affiliate stations controlled by Sinclair in each month from January 2010 to December 2017. A station is considered controlled by Sinclair if it is owned and operated by the Sinclair Broadcast Group, if it is owned and operated by Cunningham Broadcasting, or if Sinclair controls programming through a local marketing agreement. Panel (b) shows year of Sinclair entry across media markets in the United States. Lighter colors correspond to later entry. Never treated are media markets that never experience Sinclair entry; always treated are media markets that have at least one station controlled by Sinclair at the beginning of the period of interest (January 2010). There were no additional stations that were acquired in 2010.

that Sinclair acquisitions increase national coverage mostly at the expense of local stories. These content changes have limited negative effects on viewership, at least in the short run.

2.4 Municipal Police Departments

Law enforcement in the United States is highly decentralized. Municipal police departments are the primary law enforcement agency in incorporated municipalities: they are responsible for responding to calls for service, investigating crimes, and engaging in patrol within the municipality's boundaries. Municipal police departments are lead by a commissioner or chief that is generally appointed (and removed at will) by the head of the local government. For more details on the functioning of law enforcement agencies in the United States see Appendix A.

3 Data and Measurement

This paper combines multiple data sources.

Station Data. Our starting sample are 835 full-powered commercial TV stations that are affiliated to one of the big four networks (ABC, CBS, FOX, and NBC).[10] Information on the market served

[10] As discussed in Section 2.1, this choice is motivated by the fact that these stations tend to have the largest viewer shares and produce their own newscasts.

by each station and yearly network affiliation 2010-2017 is from from BIA/Kelsey, an advisory firm focusing on the media industry.

Sinclair Ownership and Control. Information on Sinclair control is from the group's annual reports to shareholders. In particular, we collect information on the date on which Sinclair took control over the station's programming. When the annual reports do not allow us to determine the exact date of take-over, we recover this information from the BIA/Kelsey data, which include the full transaction history of all stations in the sample.[11] We consider stations to be controlled by Sinclair if they are owned and operated by the Sinclair Broadcast Group, if they are owned and operated by Cunningham Broadcasting, or if Sinclair controls the station's programming through a local marketing agreement.[12] We use Sinclair acquisitions to refer to Sinclair control over the station's content determined by any of these instances, unless otherwise specified.[13]

Newscast Transcripts. To study how Sinclair acquisitions affect content, we use transcripts of local TV newscasts from ShadowTV, a media monitoring company. For each station, we have the closed caption transcripts of all evening newscasts (5-9pm) for a randomly selected day per week. The data cover 325 (39%) stations in 113 media markets from 2010 to 2017, for a total of 293,045 newscasts. We segment each transcript into separate stories using an automated procedure based on content similarity across sentences described in detail in Appendix B, which gives us 8.5m separate stories.

We use the segmented transcripts to measure whether a municipality appears in a crime story. We identify crime stories about a municipality using the following procedure:

[11] We use annual reports as our primary source because we are interested in Sinclair control of a station's programming in addition to outright ownership, which the BIA/Kelsey data is limited to. In particular, the BIA/Kelsey data does not report information on local marketing agreements under which Sinclair effectively operates the stations while not owning it.

[12] Sinclair has a controlling interest in Cunningham Broadcasting, although it does not have a majority of voting rights. The strong ties between Sinclair and Cunningham are also evidenced by the fact that as of the end of 2017, the estate of Carolyn C. Smith owned all of the voting stock of the Cunningham Stations. She is the mother of the two controlling shareholders of Sinclair. Under a local marketing agreement, Sinclair operates the station therefore controlling its programming.

[13] The large majority of stations under Sinclair control are owned and operated by Sinclair directly. Allowing for a more comprehensive definition of control sets a different treatment date for around 10 stations out of the 121 that are ever controlled by Sinclair (Appendix Table I, column (1)).

1. We define a story to be local to a given municipality if the name of the municipality appears in it. If multiple municipalities' names appear in the same story, we define the story to be local to all of them.[14] For each station, we search the name of all municipalities with at least 10,000 people according to the 2010 Census that are located in the media market the station belongs to. We exclude smaller municipalities as they are likely to receive a negligible share of overall coverage.

2. We identify whether a story is about crime using a pattern-based sequence-classification method. The method defines a story to be about crime if it contains a bigram that is much more likely to appear in an external crime-related library, as opposed to a non crime-related one, and is similar to the one used by Hassan et al. (2019) to identify firms' exposure to political risk from quarterly earnings calls.

   The crime-related training library we consider are articles from the Metropolitan Desk of the New York Times with the tags Crime Statistics, Criminal Offenses, or Law Enforcement 2010-2012, that we download from Factiva. The non crime-related training library is composed of all Metropolitan Desk articles without those tags over the same period. Each library is composed of all adjacent two word combinations (i.e. bigrams) contained in the articles. We focus on bigrams because they tend to convey more information than single words. We remove punctuation and stop words and lemmatize the remaining words using WordNet's lemmatizer. We use articles from the New York Times as they are a readily available, previously tagged corpus, but focus on the Metropolitan Desk to capture language that is appropriate to local news stories.

   We define a bigram to be about crime if it is ten times more likely to appear in the crime-related library versus the non crime-related one. Focusing on the relatively frequency of bigrams between the two libraries allows us to filter out common use bigrams (e.g. "New York", "last year") that are likely to appear in the corpus but are not specific to crime. We additionally

[14] 76.5% of local crime stories mention a single media market municipality, 18.5% mention two municipalities, and the remaining 4% mention three or more.

filter out uncommonly used bigrams that might show up only because of noise by selecting bigrams that appear at least 50 times in the crime library.

We identify 179 crime bigrams following this procedure. Appendix Figure II shows word clouds for the selected bigrams, where the size of the word is proportional to its relative frequency (Panel (a)) or its overall frequency in the crime-related library (Panel (b)). The bigrams we identify to be about crime are quite general, and make intuitive sense: e.g. "police said", "police officer", "law enforcement". In addition, they do not display an ideologically driven view of crime, which lowers the concern of measurement error systematically varying with Sinclair acquisitions.

We validate the procedure by comparing the classification of local stories (i.e. stories that mention at least one of the municipalities with more than 10,000 people in the media market) that we obtain following this methodology and a content characterization that results from training an unsupervised LDA model with five topics on the same stories (see Section 2.2). First, going back to Figure I, we see that the share of local stories about crime that we identify with our methodology (22%) is very similar to the overall weight of the crime topic (25%). Second, Appendix Figure III shows that stories about crime display significantly higher crime topic shares than non-crime stories. Overall, these results indicate that the procedure we follow successfully identifies crime stories.

3. We combine the definitions to create an indicator variable equal to one if a given municipality was mentioned in a crime story by a given station in a given week.

Our starting sample is composed by stations that are continuously present in the content data 2010-2017, and municipalities that have more than 10,000 people. We only include municipality-station pairs where the station and the municipality belong to the same media market. In order to maximize sample size in the presence of short gaps in the content data, we replace missing observations in spells shorter than two consecutive months using linear interpolation (see Appendix B for more details), but we show that our findings are robust to leaving these observations as missing in Section 5.4. The resulting sample includes 325 stations and 2253 municipalities in 113 media

markets.

Crime and Clearance Data. Crime and clearance data are from the Uniform Crime Reports (UCRs) published by the Federal Bureau of Investigation (FBI) 2010-2017.[15] UCRs are compiled from returns voluntarily submitted to the FBI by police departments. They report monthly counts of offenses known to the police and counts of offenses cleared for three property crimes (burglary, larceny-theft, and motor vehicle theft) and four violent crimes (murder, rape, robbery, and aggravated assault). We use UCRs to study crime rates, defined as crimes per 1,000 people under the inverse hyperbolic sine (IHS) transformation, and clearance rates, defined as cleared crimes over total crimes.[16,17]

We aggregate the data at the year level for two reasons. The first has to do with the definition of clearance rates. When there are no offenses over the time period considered, the denominator is zero and the clearance rate is undefined. Aggregating the data at the yearly level allows us to create a balanced sample without sacrificing sample size. Second, there is no perfect correspondence between the crimes that are reported as being cleared in a certain month and the offenses taking place in that month, although the vast majority of arrests happen relatively close to the date of the incident. Using the yearly data minimizes this mismatch.

UCR data may contain record errors and need extensive cleaning, as shown by Evans and Owens (2007) and Maltz and Weiss (2006). Following the state of the art in the crime literature (see, among others, Chalfin and McCrary (2018), Mello (2019), Premkumar (2020)), we use a regression-based method to identify and correct record errors, and define crime rates using a smoothed version of the population reported in the UCRs. We describe the data cleaning procedure in detail in Appendix B. Finally, we winsorize crime and clearance rates at the 99% level to minimize the influence of outliers.

[15] UCR data 2020-2016 are from NACJD 2017. UCR data for 2017 are from Kaplan (2019*b*).

[16] A crime is considered cleared if at least one person has been arrested, charged, and turned over for prosecution or if the offender has been identified, but external circumstances prevent an arrest.

[17] The UCR data offer the best coverage of police departments across time and space, but only allow us to run quite coarse heterogeneity analysis. For this reason, we also considered using incident-level data from the National Incident Based Reporting System (NIBRS). Out of the 1792 municipalities in our sample, only 453 report their data through the NIBRS system. In addition, more than 70% of these municipalities are covered, which is concerning in terms of power. Overall, this suggest that the NIBRS data are unfortunately not apt to implement the analysis proposed in this paper.

Nonetheless, we show that our results are robust to the data cleaning procedure in Section 6.5.

Our starting sample is composed by municipalities with more than 10,000 people with a municipal police department. To create a balanced sample, we exclude municipalities that do not continuously report crime data to the FBI and do not have at least one violent and one property crime in every year. In addition, the empirical strategy requires restricting the sample to municipalities located in media markets included in the content data. Our final sample includes 1792 municipalities (see Appendix B for more details).[18]

Municipality Characteristics. Municipality characteristics are from the 2006-2010 American Community Survey (Manson et al., 2019). Since municipal election results are not available at a sufficiently large scale, we focus on presidential elections and construct the Republican vote share in the 2008 presidential election aggregating precinct level returns to the municipal level. Precinct level returns are from the Harvard Election Data archive (Ansolabehere et al., 2014). When these are not available (approximately 10% of the sample), we assign to the municipality the share who voted Republican in the county the municipality is located in. County level returns are from the MIT Election Data and Science Lab (2017).

Media Market Characteristics. Media market characteristics from 2010-2017 are from the Census Bureau (demographics), the Bureau of Labor Statistics (unemployment), and the Bureau of Economic Advisers (income per capita). Turnout and Republican vote share in presidential elections are from the MIT Election Data and Science Lab (2017). In all cases, we start from county level data and aggregate them to the media market level.

Police Violence. Data on police-involved fatalities are from Fatal Encounters. Fatal Encounters is a crowd-sourced dataset that aims to document all deaths where police are present or involved.[19] We

[18]The sample for the content analysis includes 461 municipalities not in the police behavior analysis. These are municipalities with more than 10,000 people in media markets for which we have content data, but that do not satisfy the conditions to be included in the police behavior analysis (for example, because they might continuously report data to the UCR). We include them in order to maximize power, but show in Section 5.4 that this does not affect our results.

[19]While the data is notoriously challenging to collect and verify, Fatal Encounters aims to provide a comprehensive account of these incidents through "Freedom of Information Act requests to police departments, web-scraping of news sources, paid researchers to run additional searches and data checks from public sources, and aggregation from multiple other sources" (Premkumar (2020)). It is considered to be the most comprehensive dataset of police-involved fatalities.

use the data to define an indicator variable equal to one if the police department was involved in at least one death involving intentional use of force in a given year.

Police Expenditures and Employment. Data on police departments' employment are from the UCR's Law Enforcement Officers Killed in Action (LEOKA) files, which report the number of sworn officers and civilian employees as of October of each year (Kaplan, 2019*a*). We supplement these data with expenditures and employment from the Annual Survey of State and Local Government Finances and the Census of Governments 2010-2016, which are published by the Census Bureau.

Google Trends. To study the effect of Sinclair on salience of crime, we collect data on monthly Google searches containing the terms "crime", "police", "youtube", and "weather" at the media market level using the Google Trends API (see Appendix B for more details).

Gallup. We use data from the Gallup Poll Social Series, a set of public opinion surveys, to define an indicator variable equal to one if at least one respondent living in the municipality reports crime as being the most important problem. More details on how we create this variable are reported in Appendix B.

3.1 Descriptive Statistics

Appendix Table II columns (1) to (3) show descriptive statistics for the main variables considered in the analysis. Panel A shows that the average municipality was mentioned in 27% of newscasts in 2010, and appeared with a local crime story in 10% of them. Panel B reports the average property and violent crime and clearance rates for the same year, and Panel C reports socio-economic characteristics of these municipalities.

The sample is restricted to municipalities for which we have coverage information, which might raise concerns related to the external validity of our findings. However, Appendix Figure IV shows that the content sample has good geographic coverage. In addition, Appendix Table II columns (4) to (6) report descriptive statistics for all municipalities with more than 10,000 people that satisfy the conditions to be included in the police behavior analysis for comparison. The municipalities

The database can be accessed here.

included in our sample appear to be highly comparable to other municipalities, as is confirmed by the *p*-values reported in column (7).

4 Empirical Strategy

4.1 Why a triple differences design?

The objective of this paper is to study how TV news coverage of a municipality's crime impacts police behavior, that we proxy using clearance rates. The major challenge to answering this question is finding a shock to news coverage of local crime that is exogenous to clearance rates. We address this issue by exploiting a supply driven change in local TV news coverage. That is, we exploit a change in content that is explained by acquisitions of local TV stations by a large broadcast group, Sinclair.

Figure II shows that Sinclair acquisitions are staggered across space and time, which suggests we could use a difference-in-differences design to study their effect. However, this would not allow us to identify the treatment of interest. This is because the shock to news content induced by Sinclair is twofold. First, when Sinclair acquires a station, newscasts increase their national focus to the detriment of local coverage (*effect #1*). This gives us variation in news coverage of local crime, which is the change in content we are interested in identifying. But in addition to this, because Sinclair is a right-leaning media group, acquisitions make content more conservative (*effect #2*), which might also affect the way in which crime and police are discussed. For example, Sinclair is notorious for imposing on its stations must-run segments that include law and order features such as the "Terrorism Alert Desk," which provides frequent updates on terrorism-related news.

To disentangle the two effects on content, we make use of the fact that media markets are regions where households receive the same TV station offerings. This means that all municipalities in media markets where Sinclair enters experience its conservative messaging. However, not all municipalities are exposed to a change in the probability of appearing in the news with a crime story. Our empirical strategy is a triple differences design that combines variation from the staggered timing of Sinclair acquisitions with cross-sectional variation across municipalities in whether they

are covered by the news at baseline, our proxy for exposure to the local news shock.[20] This design allows us to capture solely the effect of variation in news coverage of local crime and control for any changes in content that all municipalities in the media market are exposed to, including *effect #2*.

The intuition for using whether a municipality is covered by the news at baseline as a proxy for exposure to the local news shock is the following. If Sinclair acquisitions decrease local news coverage, municipalities often in the news at baseline (i.e. covered municipalities) would bear the brunt of the decline. Instead, municipalities that are never in the news in the first place (i.e. non-covered municipalities) are also not going to be covered after Sinclair acquires a stations. They do not experience any change, and therefore function as our control group.

Appendix Figure V provides a visual representation of our intuition, based on the fact that crime reporting is principally a function of a municipality's violent crime rate. The graphs are unconditional binned scatter plots of the relationship between a municipality's violent crime rate and the share of weeks in a year in which the same municipality is in the news with a local crime story, separately for years before and after the Sinclair acquisition. The sample is restricted to stations ever acquired by Sinclair. Panel (a) shows the relationship for non-covered municipalities: the probability of being in the news with a crime story is at very low levels both before and after the acquisition. For covered municipalities (Panel (b)), higher violent crime rates are always correlated with a higher probability of being in the news with a crime story, but for every level of violent crime, crime reporting is lower after Sinclair acquires the station.

We define a municipality as covered in the following way. First, we calculate the share of weeks a municipality is mentioned in the news in our baseline year, 2010. If we have data for multiple stations in the same media market, we assign to each municipality the median share of weeks a municipality is mentioned in the news across the different stations. Finally, we define an indicator variable equal to one if the municipality is in the news more than the median municipality in 2010,

[20] Nonetheless, we also always estimate separate differences-in-differences designs for covered and non-covered municipalities to understand what effect is driving the result. It is especially interesting to do so when we are considering clearance rates, as the effect of Sinclair acquisitions on non-covered municipalities is informative on how conservative content affects police behavior.

and zero otherwise. As Appendix Figure VI shows, using data from media markets that never experience Sinclair entry, the measure is persistent across years, showing that the likelihood of being in the news can be seen as a fixed characteristic of a municipality and mean reversion is unlikely to explain our results.

As Appendix Figure VII shows, covered and non-covered municipalities differ on a number of characteristics. To ensure that the effect is not confounded by other municipality attributes but is truly driven by exposure, our baseline specification includes interactions between Sinclair acquisitions and baseline socio-economic characteristics of the municipalities. This implies that the effect is going to be driven by those idiosyncratic traits other than the observable ones that make one municipality more likely to be in the news than another. Given that covered and non-covered municipalities are especially different in population size, we check whether our results survive restricting the analysis to medium sized municipalities between 10,000 and 50,000 people.

4.2 Identification

Identification in our triple differences design relies on covered and non-covered municipalities being on parallel trends. As a starting point, we provide supporting evidence for the parallel trend assumption by estimating event study specifications in which the treatment effect varies in time since/to the Sinclair acquisition. The event studies allow us to test empirically whether the outcome in covered and non-covered municipalities begins evolving differently prior to Sinclair entering a media market.

However, even if event studies show convincing patterns, we might still be concerned about contemporaneous shocks influencing both Sinclair's decision to enter a media market and the evolution of the outcome. Specifically, we might worry about Sinclair entry being endogenous to an area's demographic or economic trends. Our triple differences specification allows us to explicitly control for any shock at the media market level that equally affects covered and non-covered municipalities.[21] This means that we should only be concerned about trends that differentially affect

[21] While Appendix Table III shows no change in media markets' socio-economic characteristics following Sinclair

covered and non-covered municipalities.

To take this into account, we note that the timing of Sinclair entry is less likely to be endogenous to a specific media market's conditions when Sinclair acquires a station by buying a smaller broadcast group spanning multiple media markets (in other words, when multiple stations are bought as a bundle).[22] Then, if our results are robust to focusing on group acquisitions only, we can be reassured that the effects are not going to be driven by the endogenous timing of Sinclair's entry decisions. Importantly, the qualitative evidence is very much in line with the no endogenous timing hypothesis, with Sinclair looking to expand and taking advantage of opportunities to acquire stations as these present themselves.[23]

5 Effect of Sinclair Control on Coverage of Local Crime

5.1 Specification

We estimate the effect of a Sinclair acquisition on the probability that covered municipalities are mentioned in a crime story compared to non-covered municipalities using the following baseline specification:

$$y_{mst} = \beta Sinclair_{st} * Covered_m + Sinclair_{st} * X'_{m2010}\gamma + \delta_{st} + \delta_{ct} + \delta_{ms} + \epsilon_{mst} \qquad (1)$$

where y_{mst} is an indicator variable equal to one if municipality m was mentioned in a crime story by station s in week t, $Sinclair_{st}$ is an indicator variable equal to one after a station is acquired by Sinclair, $Covered_m$ is an indicator variable equal to one if a municipality is likely to be in the news at baseline, X_{m2010} are baseline municipality characteristics, δ_{st} are station by week fixed effects, δ_{ct} are covered status by week fixed effects, and δ_{sm} are municipality by station fixed effects.[24]

entry, the fact that our design allows us to control for observable and unobservable trends strengthens the credibility of the results.

[22] Such group acquisitions are behind 75% of Sinclair's entries in new media markets over the time period of interest.

[23] For example, when Barrington's stations went on the market in 2012, both Sinclair and Nexstar (another large broadcast group) got to final talks for the acquisitions. Moreover, Allbritton's decision to put its stations on the market was mainly driven by the company's decision to focus its resources on Politico.

[24] In particular, X_{m2010} includes the following variables: population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share below the poverty line, and Republican vote share in the 2008 presidential election. Population is in logs.

Each municipality is associated with one media market, but there can be multiple stations that belong to the media market covering the municipality. Given that the outcome is station and municipality specific, the cross-sectional unit of analysis is the municipality-station pair. More precisely, we estimate the regression on a municipality-station pair by week balanced panel that only includes pairs where the station and the municipality belong to the same media market. Standard errors are clustered at the media market level.

The station by week fixed effects (δ_{st}) control non-parametrically for station specific shocks in content that are common to all municipalities, while covered status by week fixed effects (δ_{ct}) allow the two different types of municipalities to be on different trends. Finally, municipality by station (δ_{sm}) fixed effects control for station specific level differences across municipalities, including level differences explained by non-time-varying measurement error due to how stories are assigned to municipalities.[25]

We provide evidence supporting the parallel trends assumption by estimating an event study version of the baseline specification that allows the effect to vary over time. In particular, we estimate the following specification:

$$y_{mst} = \sum_{y=1}^{T_{min}} \beta_y * Pre_{t-y,s} * Covered_m + \sum_{y=0}^{T_{max}} \gamma_y * Post_{t+y,s} * Covered_m + \delta_{st} + \delta_{ct} + \delta_{ms} + \epsilon_{mdt} \quad (2)$$

where variables are defined as above. To reduce noise, we constrain the effect to be constant by year since treatment.[26]

[25] We assign a story to a municipality if the municipality's name is mentioned in the story. This might give rise both to false positives (e.g. mentions of "Paris, France" might be counted for "Paris, TX") and false negatives (e.g. neighborhoods might be mentioned instead of municipalities, or unusual municipality names might be more likely to be misspelled in the close captioned text). We can account for both types of measurement error using the municipality by station fixed effects, as long as the error is stable over time. We believe this to be a reasonable assumption in this setting. For example, we might worry that Sinclair's increased focus on national news might increase the probability of false positives for municipalities that have the same name as nationally relevant places. However, to the extent that these municipalities are more likely to be covered in the first place, the effect should go in the opposite direction to our findings.

[26] Our event study specifications do not include the interaction between the municipality's baseline characteristics and Sinclair entry. This is justified because we only need the fixed effects for identification. Instead, we control for the baseline characteristics to check that our effects are indeed driven by and not some other characteristics, which means that they should not affect parallel trends. Nonetheless, the event study graphs look virtually unchanged when

Table I: Effect of Sinclair Control on the Probability of Having a Local Crime Story

Dependent Variable	Had Local Crime Story			
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.024***	-0.021***	-0.014***	-0.022***
	(0.007)	(0.007)	(0.005)	(0.007)
Non-Sinclair Stations in Sinclair Media Market * Covered				-0.007
				(0.006)
Observations	3143360	3143360	2398902	3143360
Clusters	113	113	111	113
Municipalities	2253	2253	1715	2253
Stations	325	325	323	325
Outcome Mean in 2010	0.092	0.092	0.050	0.092
P-value Sinclair = Other				.055
Station by Week FE	X	X	X	X
Covered by Week FE	X	X	X	X
Station by Municipality FE	X	X	X	X
Sinclair * Controls		X	X	X
Restricts Sample 10k-50k			X	

Notes: This table shows the effect of Sinclair control on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities. We regress an indicator variable for the station reporting a local crime story about the municipality on the interaction between an indicator variable for the station being under Sinclair control and an indicator variable for whether the municipality is covered at baseline, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects. Column (2) additionally includes the interaction between an indicator variable for the station being under Sinclair control and baseline municipality characteristics (equation (1)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Column (3) restricts the sample to municipalities with fewer than 50,000 people. Finally, column (4) also includes the interaction between an indicator variable for being in the same media market as a station under Sinclair control and an indicator variable for whether the municipality is covered at baseline. The *p*-value reported in column (4) is from a test of the difference between the effect of Sinclair entry on the station controlled by Sinclair and other stations in the same media market. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level. Covered municipalities are mentioned in the news more than the median municipality in 2010.

5.2 Main Results

Table I shows the effect of Sinclair acquiring a station on its local crime coverage of covered versus non-covered municipalities. In particular, the table reports the coefficient on the interaction between an indicator variable for the station being under Sinclair control and an indicator variable for the municipality being covered at baseline, estimated from equation (1). Column (1) reports the estimates from a specification that only controls for the fixed effects, while column (2) additionally includes the interaction between Sinclair and socio-economic characteristics of the municipality at baseline (equation (1)).

We find that a Sinclair acquisition decreases the probability that the station reports a local crime

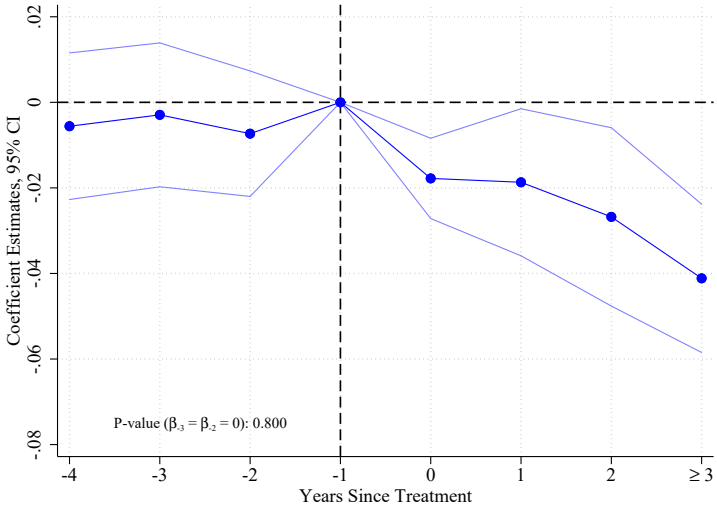
the interaction is added to the specification.

story about covered municipalities by 2.1 percentage points compared to municipalities that were not likely to be in the news at baseline. The effect is significant at the 1% level. The magnitude of the effect is large, corresponding to almost 25% of the baseline mean. The coefficient is smaller in size but similar in magnitude, corresponding to 28% of the baseline mean, if we exclude municipalities with more than 50,000 people to increase the comparability of the sample (column (3)). This is an important test as one of the main differences between covered and non-covered municipalities is precisely population.

Event Study. The identification assumption is that, absent treatment, the probability of covered municipalities being in the news with a local crime story would have evolved similarly to that of non-covered municipalities. We provide evidence supporting this assumption by estimating an event study specification that allows the effect of Sinclair control to vary by time since treatment. Figure III reports the β_y and γ_y coefficient estimates from equation (2), together with 95% confidence intervals. The figure shows no difference between covered and non-covered municipalities in the four years leading up to the station coming under Sinclair control. Immediately after Sinclair acquires the station, covered municipalities become less likely than non-covered municipalities to appear in the news with a crime story. The effect in the first year is large in magnitude and almost comparable to the point estimate from the triple differences specification. After this, the effect becomes larger over time, almost doubling by year three.

Same Media Market Stations. Our result might still reflect an underlying change in a municipality's crime prevalence or demand for crime stories. To examine this, we replicate our baseline model but focus our attention on the local crime coverage of stations that are in the same media market as stations that are acquired by Sinclair, but are not themselves bought by the group. In Appendix Figure VIII, we report the same β_y and γ_y coefficient estimates from equation (2), together with similarly defined leads and lags for same media market stations that are not directly controlled by Sinclair. In the four years leading up to Sinclair entry, there is no difference in how Sinclair and non-Sinclair stations report about crime in covered relative to non-covered municipalities. Once Sinclair enters the media market, we only see a decrease in local crime coverage by

Figure III: Effect of Sinclair Control on the Probability of Having a Local Crime Story, by Year since Treatment



Notes: This figure shows the effect of Sinclair control on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities, by year since treatment. We report coefficient estimates and 95% confidence intervals from a regression of an indicator variable for the station reporting a local crime story about the municipality on the interaction between indicator variables for years since Sinclair control and an indicator variable for whether the municipality is covered at baseline, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects (equation (2)). The omitted category is T-1. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level, but the effect is constrained to be the same by year since treatment. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Sinclair stations. Table I column (4) confirms the result: a test of equality of the effect of Sinclair entry on Sinclair and non-Sinclair stations shows that the two effects are indeed statistically different (p-value = 0.055).

This evidence supports the interpretation that decreasing local crime coverage is an editorial decision on the part of Sinclair stations. It is also interesting to note that this shows limited spillovers of Sinclair's change in content to other outlets in the media market: other stations do not appear to be responding to what Sinclair is doing, at least as far local crime coverage is concerned. This signals that there might be demand for local news stories, which is in line with stations acquired by Sinclair potentially experiencing a decline in viewership (Martin and McCrain (2019)). Nonetheless, decreasing local news might still be an optimal strategy for Sinclair if economies of scale from jointly operating a large number of stations outweigh the potential decline in advertising revenues due to smaller viewership.

Differences-in-Differences Decomposition. We justify the triple differences design using the

intuition that municipalities with a low baseline probability of being in the news should not experience a change in their local crime coverage, while covered municipalities should bear the brunt of the decline. To explore whether this is the case, we estimate a differences-in-differences specification that only exploits variation coming from the staggered timing of Sinclair acquisitions, separately for non-covered and covered municipalities. As we hypothesize, Appendix Table IV shows that after Sinclair acquires a station, there is no change in the probability that non-covered municipalities appear in the news with a crime story (columns (1) and (2)): they do not appear in the news before the acquisition, and they still do not appear in the news after it. Instead, Sinclair entry implies a large decline in the probability of being mentioned in the news with a crime story for covered municipalities (columns (3) and (4)).

5.3 Additional Findings

Other Types of Local News. In light of the results in Table I, it is natural to ask to what extent the decline in local coverage is specific to crime news. In Appendix Table V, we show that local news decreases across the board, but the effect is larger for stories about crime. Sinclair acquisitions lower the probability that a station reports a story about covered municipalities relative to non-covered municipalities by 3.2 percentage points or 13% of the baseline mean (column (1)). However, the effect is much larger in magnitude for crime compared to non-crime stories more generally (23% versus 10%). We interpret this result as providing supporting evidence that the effects on police behavior that we identify are going to be related to the change in local coverage of crime, and not result from decreased coverage of other non-crime events.

Overall Crime Coverage. How is non-local crime coverage affected by Sinclair acquisitions? We address this question in Appendix Table VI, where we estimate a differences-in-differences specification at the station level. The main outcome is the share of stories that are about crime in a month (column (1)), which we further decompose into stories about crime that are local (column (2)) or non-local (column (3)). The table shows a negative effect of Sinclair acquisitions on the overall share of stories about crime, which is entirely explained by a decline in local crime stories. Coverage

of non-local crime stories does not appear to be affected by Sinclair: non-covered municipalities are exposed to the same level of non-local crime news both before and after acquisitions. Given that Sinclair is a conservative media group, it might be surprising to not see an increase in the volume of non-local crime stories. However, we show in Appendix Table VII that while the volume of non-local crime coverage is constant, the way in which crime and police are covered is not: after Sinclair acquires a station, the station is less likely to talk about police misconduct and more likely to talk about crime and drugs, and crime and immigrants.

Heterogeneity by Political Leaning of the Municipality. Since Sinclair is a conservative media group, we might worry that the decline in coverage could be influenced by political considerations. Ideally, we would like to test this possibility using election results for municipal level races. Unfortunately, these data are not only not widely available, but they are especially hard to collect for smaller municipalities (de Benedictis-Kessner and Warshaw (2016)), that comprise a large share of our sample. We get around this problem by using electoral results in presidential election as a proxy for a municipality's partisanship. In particular, we split the sample by whether the municipality's Republican vote share was above the median (column (1)) or below the median (column (2)) in the 2008 presidential election.[27] Appendix Table VIII shows that the effect is very similar for Democratic- and Republican-leaning municipalities (p-value=0.635), which suggests a limited scope for strategic coverage decisions based on political considerations.[28]

5.4 Robustness of the Effect of Sinclair on Coverage of Local Crime

Appendix Table IX shows that the effect of Sinclair acquisitions on news coverage of local crime is robust to a number of concerns. Column (1) reports the baseline estimates for reference.

Robustness to Data Cleaning and Sample. We begin by showing that the choices we make when cleaning the content data and defining the outcome do not matter for the effect on the probability that a municipality appears in the news with a crime story. First, columns (2) and (3) show that the result

[27] As described in the data section, we construct the vote shares in the 2008 presidential election aggregating precinct level returns to the municipal level.

[28] In Appendix Figure IX we additionally show that the change in coverage of local crime is not heterogeneous based on municipality characteristics.

is not affected if we identify crime stories using bigrams that are less (more) distinctively about crime, i.e. bigrams that are five (twenty) times more likely to appear in the crime-related versus the non crime-related library. In addition, not replacing missing observations using linear interpolation as described in Appendix B (column (4)) or segmenting newscasts using a fixed number of words (column (5)) leaves the result unchanged. Similarly, restricting the sample to the same set of municipalities included in the analysis of clearance rates does not impact the result (column (6)).

Robustness to Treatment Definition. Columns (7) to (9) show robustness to using alternative definitions of Sinclair control. In the baseline analysis, we consider a station to be controlled by Sinclair in all months after acquisition, independently of whether Sinclair retains ownership of the station or not. Column (7) shows that dropping the three stations that were divested by Sinclair in the 2010 to 2017 period does not make a difference. Focusing on stations directly owned and operated by Sinclair also does not affect the result (column (8)). Finally, we show that the result is unchanged if we only include markets that Sinclair entered as part of a group acquisition (column (9)), where endogenous acquisitions are less likely to be a concern.

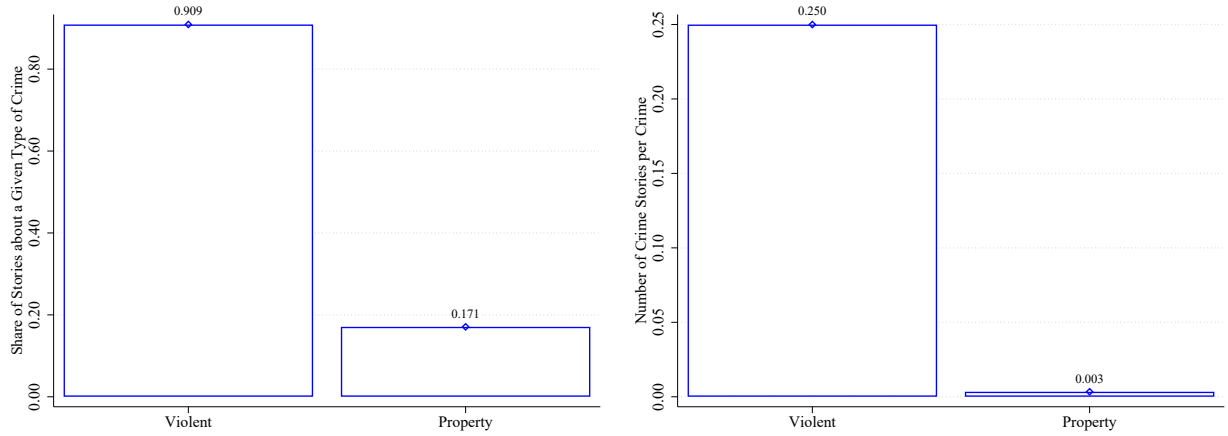
6 Effect of Sinclair Control on Clearance Rates

6.1 How Should the Decline in News Coverage of Local Crime Influence Clearance Rates?

In the previous section, we documented that when a local TV station is acquired by Sinclair, covered municipalities become less likely to appear in the news with a local crime story compared to non-covered municipalities. While from Sinclair's point of view cutting local coverage may simply be a way to lower costs, this decline may have tangible implications. Specifically, we are interested in understanding the effect of the decline in news coverage of local crime on clearance rates.

Crime clearances are highly sensitive to what resources are allocated to investigations. For example, Blanes i Vidal and Kirchmaier (2017) show that increases in the response time to crime calls have a negative effect on the probability that a crime is cleared. In addition, Cook et al. (2019) show that the involvement of a specialized detective squad also increases the probability that a crime

Figure IV: Local Crime News of Violent and Property Crimes



(a) Share of Crime Stories

(b) Crime Stories per Offense

Notes: This figure shows what crimes are covered in local TV news. Panel (a) shows the average share of a municipality's crime stories that are about violent crimes (i.e. murder, assault, rape, and robbery) and property crimes (i.e. burglary, theft, and motor vehicle theft). Panel (b) shows the average number of crime stories per reported offense across municipalities. 8% of stories are about both a violent and a property crime. Note that this does not exactly correspond to the probability that a crime of a given type appears in the news because we have information on news coverage only for one randomly selected day per week. In both graphs, the sample is restricted to 2010 and to media market that never experience Sinclair entry.

is cleared in the medium run. As a result, clearance rates have often been used by economists to study police behavior (see, among others, Mas (2006), Shi (2009), and Premkumar (2020)). They are especially interesting in our setting as they allow us to consider whether the types of crimes that get prioritized by police departments are affected by news coverage.

To understand how news coverage of local crime is likely to affect clearance rates, it is important to note that not all crime types are equally likely to be reported in the news. This is relevant to the extent that we should expect clearance rates of different crimes to respond differently, depending on how important news coverage is for them. We explore this heterogeneity in our content data by training a classifier model to identify whether local crime stories are about a violent crime or a property crime, which we describe in detail in Appendix C.

Figure IV Panel (a) reports the share of crime stories that are about violent crimes (i.e. murder, assault, rape, and robbery) and the share of stories that are about property crimes (i.e. burglary, theft, and motor vehicle theft). Local crime news has a clear violent crime focus: 91% of local crime stories are about a violent crime, while only 17% of crimes stories are about a property crime (8% of stories are about both). The difference in reporting across crime types is even sharper if we

consider the fact that violent crimes are relatively rare, while property crimes are more common by orders of magnitude. In Figure IV Panel (b) we normalize the number of crime stories of a given type that were reported about a municipality in 2010 by the number of offenses of the same type for the same municipality. There are approximately 0.25 stories for each violent crime. Instead, property crimes, at 0.003 stories per offense, receive negligible news coverage.[29] Overall, property crimes appear to be less important than violent crimes for local news.

In addition to this, we can use our classifier model to test whether Sinclair acquisitions have differential effects on local news coverage of violent and property crimes. In Appendix Table X, we show that after Sinclair acquires a station, covered municipalities are 2 percentage points (23% of the baseline mean) less likely to appear in the news with a story about a violent crime. Instead, they are not significantly less likely to appear in the news with a story about a property crime. Taken together, the evidence suggests that we should expect an effect on violent rather than property crimes: the main outcome of interest for our analysis is the violent crime clearance rate.

6.2 Specification

We estimate the relative effect of Sinclair entry on violent crime clearance rates of covered municipalities with respect to non-covered municipalities using the following baseline specification:

$$y_{mdt} = \beta Sinclair_{dt} * Covered_m + Sinclair_{dt} * X'_{m2010}\gamma + \delta_{dt} + \delta_{ct} + \delta_m + \epsilon_{mdt} \qquad (3)$$

where y_{mdt} is the violent crime clearance rate in municipality m in media market d in year t, $Sinclair_{dt}$ is an indicator variable equal to one after a media market experiences Sinclair entry, $Covered_m$ is an indicator variable equal to one if a municipality is likely to be in the news at baseline, X_{m2010} are baseline municipality characteristics, δ_{dt} are media market by year fixed effects, δ_{ct} are covered status by year fixed effects, and δ_m are municipality fixed effects.[30] The regression is estimated on a yearly balanced panel 2010-2017 that includes 1792 municipalities. Standard errors

[29] It is important to note that, given that we only have transcripts for a random sample of days and multiple stories can cover the same crime, these numbers do not precisely correspond to the probability that a given crime appears in the news, although they are likely to be positively related.

[30] Because of restrictions on ownership imposed by the Federal Communications Commission, each owner generally controls one station by media market. Acquiring a new station usually implies entering a new media market.

are clustered at the media market level.

The media market by year fixed effects (δ_{dt}) control non-parametrically for media market level shocks. This includes any non municipality-specific change in content that is associated with Sinclair entering a media market, such as increased conservative slant. In addition, these fixed effects allow us to take into account media market specific trends in demographics that might correlate with Sinclair entry. Covered status by year fixed effects (δ_{ct}) allow covered and non-covered municipalities to be affected by different shocks over time, while municipalities fixed effects (δ_m) allow for level differences across municipalities.[31]

We consider a media market to be treated in a given year if Sinclair owns one of the media market's stations in January of that year. This implies that the year of treatment is the first year in which Sinclair is continuously present in the media market. This is reasonable because 87% of the stations in our sample are acquired by Sinclair in the second half of the year (58% in the last trimester), which means that partially treated years only see a Sinclair presence for a couple of months.

As before, we also estimate an event study specification that allows the relative effect of Sinclair entry to vary over time. In particular, we estimate the following specification:

$$y_{mdt} = \sum_{y=1}^{T_{min}} \beta_y * Pre_{t-y,d} * Covered_m + \sum_{y=0}^{T_{max}} \gamma_y * Post_{t+y,d} * Covered_m + \delta_{dt} + \delta_{ct} + \delta_m + \epsilon_{mdt} \quad (4)$$

where all variables are defined as above.

6.3 Main Results

Table II shows the effect of Sinclair entry into a media market on the violent crime clearance rate of covered versus non-covered municipalities. The table reports the coefficient on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline. Column (1) reports the estimates from a

[31] Given that each municipality is associated with one media market, the inclusion of municipality fixed effects makes controlling for covered status by media market fixed effects, as is customary in triple differences specification, redundant.

Table II: Effect of Sinclair Entry on the Violent Crime Clearance Rate

Dependent Variable	Violent Crime Clearance Rate			
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.032**	-0.034**	-0.032*	-0.032*
	(0.015)	(0.016)	(0.016)	(0.019)
Observations	14336	14336	14336	10640
Clusters	112	112	112	108
Municipalies	1792	1792	1792	1330
Outcome Mean in 2010	0.461	0.461	0.461	0.466
Media Market by Year FE	X	X	X	X
Covered by Year FE	X	X	X	X
Municipality FE	X	X	X	X
Sinclair * Controls		X	X	X
Restricts Sample 10k-50k			X	
Controls for Crime Rates and Population				X

Notes: This table shows the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities. We regress the municipality's violent crime clearance rate on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects. Column (2) additionally includes the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Column (3) restricts the sample to municipalities with fewer than 50,000 people. Column (4) additionally controls for the property crime rate, the violent crime rate, and log population. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. Crime rates are IHS crimes per 1,000 people. Both clearance rates and crime rates are winsorized at the 99% level.

specification that only controls for the fixed effects, while column (2) additionally includes the interaction between Sinclair and socio-economic characteristics of the municipality at baseline (equation (3)).

After Sinclair enters a media market, the violent crime clearance rate is 3.4 percentage points lower in covered than in non-covered municipalities.[32] The effect is significant at the 5% level, and sizable in economic magnitude, corresponding to 7.5% of the baseline mean. To put this number if prospective, the median municipality in our sample experiences 69 violent crimes in a year and 32 violent crime clearances: a 7.5% decline in the violent crime clearance rate corresponds to approximately 2.4 fewer clearances per year.[33]

[32] An earlier version of the paper reported a different point estimate (namely, a reduction of the violent crime clearance rate of 4.6 percentage points), due to a coding error in the assignment of municipalities to media markets. The error has now been corrected throughout the paper, which explains why some of the estimates reported in this version are slightly different. This being said, none of the results change in a substantive way.

[33] For a detailed discussion of how to reconcile the magnitude of this effect with the 25% decline in local crime coverage of stations acquired by Sinclair, and why we believe this is informative of the mechanisms at play, please refer to Section 7.

When violent crime is less covered by local news, fewer violent crimes get cleared: news coverage of local crime matters for policing. This shows that there is scope for external forces to exert an influence on police behavior, despite the strong protections that police officers get from strong union contracts and civil service laws. Unfortunately, we are unable to follow clearances through the criminal justice system, and know whether they lead to a conviction or an acquittal. As a result, we cannot make inference relative to the quality of the clearances themselves, which limits our ability to draw efficiency or welfare conclusions from our analysis.[34]

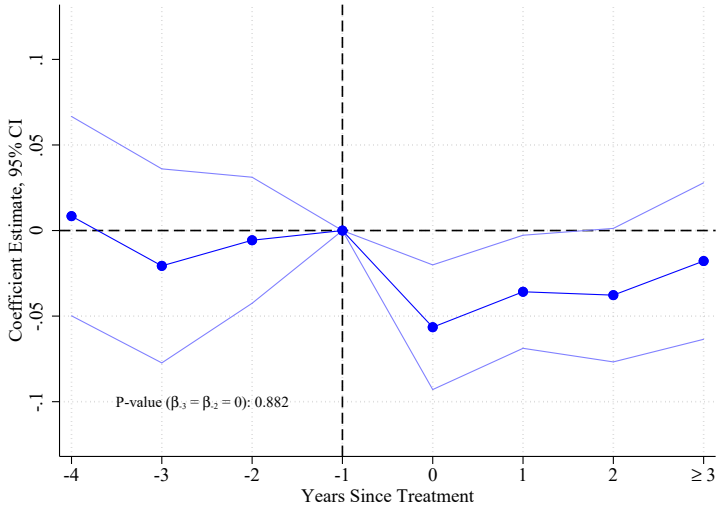
The point estimate is almost the same whether we control for the interaction between Sinclair and observable characteristics of the municipality at baseline (column (2)) or not (column (1)). This suggests that the main effect is unlikely to be explained by Sinclair having a differential effect based on some other characteristic of the municipality, that just happens to be correlated with coverage. In addition, restricting the sample to municipalities with fewer than 50,000 people minimally affects the result (column (3)), as does controlling for crime rates and population (column (4)), two factors that we might worry influence violent crime clearance rates but that we do not include in the main specification because they are potentially endogenous to the treatment.

Event Study. The identifying assumption is that, had Sinclair not entered the media market, the violent crime clearance rate of covered and non-covered municipalities would have evolved similarly. We provide evidence supporting this assumption by estimating an event study specification that allows the effect of Sinclair entry in a media market to vary by time since treatment. Figure V reports the β_y and γ_y coefficient estimates from equation (4), together with 95% confidence intervals. The figure shows no difference between covered and non-covered municipalities in the four years leading up to Sinclair entry into the media market.[35]

[34] According to theories of "de-policing" (Owens (2019)), decreasing arrest rates might be socially optimal.

[35] The paper focuses on the 2010-2017 period because it is the period for which we have collected the content data. Given that only a handful of municipalities are treated after 2015, the maximum number of pre-periods we can estimate is four as we do not sufficient observations to identify periods before than. However, UCR data is easily available before 2010. As a result we also estimate the event study specification on 2009-2017 data, which allows us to both include one additional pre-period and to estimate the other pre-period dummies using a larger sample of municipalities. Appendix Figure X, which shows the resulting event study graph, confirms the evidence in support of the identification assumption: covered and non-covered municipalities appear to be on comparable trajectories in the five years preceding Sinclair entry.

Figure V: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by Year since Treatment



Notes: This figure shows the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities, by year since treatment. We report coefficient estimates and 95% confidence intervals from a regression of the municipality's violent crime clearance rate on the interaction between indicator variables for years since Sinclair entry and an indicator variable for whether the municipality is covered at baseline, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (5)). The omitted category is T-1. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Consistent with the time pattern of the effect on news coverage of local crime, which showed a large effect immediately in the first year after treatment (see Figure III), covered municipalities have a lower violent crime clearance rate than non-covered municipalities already in the first year in which Sinclair is fully present in the media market. However, the gap between covered and non-covered municipalities seems to be shrinking after that. This is consistent with viewers learning that the signal on local crime that they receive from Sinclair is biased, and adjusting for it based on their own observation or other media sources. To the extent that the change in content is driven by a supply-side shock that might be opaque to viewers (DellaVigna and Kaplan (2007)), it is not surprising to see a short-run effect that tapers: it takes time for viewers to learn about Sinclair's biased coverage and adjust accordingly.

Property Crime Clearance Rates. If the police are responding to news coverage of local crime as we hypothesize, the clearance rate of crimes that are minimally covered by the news, such as property crimes, should not be affected by Sinclair entry. Table III shows that the property crime clearance rate is not differentially affected by Sinclair acquisitions in covered as opposed to

Table III: Effect of Sinclair Entry on the Property Crime Clearance Rate

Dependent Variable	Property Crime Clearance Rate			
Type of Crime	All	Burglary	Theft	MVT
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.000	-0.007	0.002	0.001
	(0.009)	(0.009)	(0.011)	(0.015)
Observations	14336	14336	14329	14279
Clusters	112	112	112	112
Municipalities	1792	1792	1792	1792
Outcome Mean in 2010	0.191	0.131	0.211	0.171
Media Market by Year FE	X	X	X	X
Covered by Year FE	X	X	X	X
Municipality FE	X	X	X	X
Sinclair * Controls	X	X	X	X

Notes: This table shows the effect of Sinclair entry on the property crime clearance rate of covered municipalities relative to non-covered municipalities, overall and for different types of property crimes. We regress the municipality's clearance rate for a given type of property crime on the interaction between between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level. MVT stands for motor vehicle theft.

non-covered municipalities. The coefficients are small and not statistically significant. This shows that the change in clearance rates is specifically related to how Sinclair influences news content, and does not depend on some other factors affecting clearance rates across the board.[36]

Crime Rates. A potential concern is that the change in the violent crime clearance rate might be explained by an increase in violent crimes, and not by a response of police officers to the changing media environment. Appendix Table XI suggests that this is not the case. The table reports the effect of Sinclair entry on the violent crime rate of covered municipalities relative to non-covered municipalities, for all violent crimes (column (1)) and separately by type of crime (column (2) to column (5)). Reassuringly, we do not find a statistically significant difference in the violent crime rate of covered and non-covered municipalities after Sinclair enters a media market. Even if we take the positive coefficient on the violent crime rate at face value, the magnitude of the effect (2.9%) is too small to explain the decline in the violent crime clearance rate. The same is true if we use as outcomes indicator variables equal to one if the municipality reports at least one crime of the

[36]To the extent that, as we discuss below, the volume of property crimes increases in covered versus non-covered municipalities, constant property crime clearance rates are potentially consistent with resources being reallocated from clearing violent to clearing property crimes.

specified type (Panel B).

Appendix Table XII looks instead at property crime rates. Column (1) shows that Sinclair entry is associated with 5.4% higher property crime rates in covered municipalities relative to non-covered ones. The effect is significant at the 5% level. This result could be explained by a decreased incapacitation or deterrence effect due to the lower clearance rates. Alternatively, the positive effect on property crime rates might be due to a reduction in overall police performance in covered relative to non-covered municipalities, which would be consistent with a decrease in monitoring induced by lower crime news coverage. Finally, it is possible that that individuals who commit property crimes are directly affected by the decline in crime content of local news (see Dahl and DellaVigna (2009) and Lindo et al. (2019)). Given that the local news audience tends to be above 55, we believe that this explanation has a limited role in this setting.[37]

Differences-in-Differences Decomposition. We might be interested in exploring the effect that Sinclair entry has separately on covered and non-covered municipalities. Appendix Table XIII reports coefficient estimates from a differences-in-differences specification that only exploits variation from the staggered timing of Sinclair acquisitions, separately for non-covered (columns (1) and (2)) and covered municipalities (columns (3) and (4). In addition, Appendix Figure XI reports the corresponding event study graphs, estimated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2020).

Column (1) and (2) show that the increase in conservative content (*effect #2* from Section 4) induced by Sinclair has a direct effect on clearance rates: the violent crime clearance rate in non-covered municipalities increases after Sinclair enters a media market. This effect can be rationalized by Sinclair's conservative messaging building support for tough-on-crime policies, which might feedback into police behavior.[38] Why do we not find the same effect in covered municipalities

[37] See Appendix B for a detailed discussion of reporting issues in crime data.

[38] The idea that conservative content might impact the criminal justice system has recently been explored by Ash and Poyker (2019), which finds that exposure to Fox News Channel induces judges to impose harsher criminal sentences. Consistent with this explanation, we show in Appendix Table VII that, although the volume of non-local crime- and police-related stories is constant after Sinclair acquisitions (columns (1) and (2)), the way in which crime and police are covered is not. In particular, the table shows that Sinclair stations are less likely to mention police misconduct (column (3)) and more likely to talk about crimes related to immigration (column (4)) and drugs (column (5)).

(columns (3) and (4))? The answer is that these municipalities not only experience the increase in conservative slant (*effect #2*), but also a decline in the probability that local crime is covered in the news (*effect #1*). The direct effect of Sinclair's conservative messaging is offset in covered municipalities by the decrease in their probability of appearing in the news with a local crime story.[39]

These estimates are not only interesting per se, but also exemplify why we decided to employ a triple differences design as our main identification strategy. Non-covered municipalities provide us with the counterfactual of how clearance rates would have evolved in covered municipalities following Sinclair entry, had there been no decrease in their probability of appearing in the news with a local crime story. We need to focus on the differential effect between the two groups of municipalities to disentangle between the two changes in content and answer the main research question of the paper.

Heterogeneity by Type of Crime. Not all violent crimes are the same, and we might wonder whether the effect of Sinclair entry on clearance rates is heterogeneous by crime type. In Appendix Table XV, we show that the decline in the violent crime clearance rate appears to be driven by the clearance rates of robberies and rapes.[40]

Heterogeneity by Municipal Characteristics. We also explore whether our results are heterogeneous by municipal characteristics. In Appendix Figure XII we find that the main effect on the violent crime clearance rate is larger for municipalities with share black and share Hispanic below the median, but is quite consistent across education levels, income levels, and political leanings.

[39]Decomposing the effect between covered and non-covered municipalities can also help us exclude the following interpretation of the results. As we show in the previous paragraph, after Sinclair enters a media market the property crime rate is higher in covered relative to non-covered municipalities. We might be concerned that the effect on the violent crime clearance rate that we estimate is a direct consequence of this increase in the property crime rate, if to deal with the higher volume of property crimes the police have fewer resources to dedicate to clearing violent crimes. However, Appendix Table XIV shows that the change in the property crime rate is non driven by the same sub-samples as the change in the violent crime clearance rate. In particular, we do not see a decrease in the property crime rate in non-covered municipalities or an increase in covered municipalities.

[40]We might be concerned that Appendix Table XV shows an increase in the robbery crime rate, and that the effect we see on the violent crime clearance rate might be entirely driven by this. We do not believe this to be the case. First, because the magnitude of the effect of the robbery clearance rate is larger than the increase in the robbery crime rate. Second, because we do not see a similar increase for the rape crime rate.

This suggests that the police might be less responsive to changes in news coverage of local crime in areas with higher share minority.

Alternative Interpretation: Heterogeneous Effects of Conservative Slant. We interpret the differential effect of Sinclair entry in covered and non-covered municipalities as being explained by covered municipalities experiencing a decline in the probability of appearing in the news with a crime story relative to non-covered municipalities. An alternative interpretation is that the effect might be driven by a differential effect of Sinclair's conservative content on the two types of municipalities, in particular due to some correlate of local news coverage.

However, this interpretation is inconsistent with the following findings. First, Table II shows that controlling for baseline municipality characteristics interacted with Sinclair entry barely affects the point estimate, which suggests that the effect is really driven by coverage. Second, Appendix Figure XIII shows that the effect on the violent crime clearance rate is increasing in pre-treatment coverage. Again, this suggests that coverage is salient for the effect we are estimating. Finally, Appendix Figure XII shows no heterogeneity of the effect based on whether the municipality is Republican or Democratic leaning. This is reassuring since, were the results driven by a different response of covered and non-covered municipalities to Sinclair's conservative content, we should expect the effect to be smaller for municipalities that are already more conservative, i.e. Republican leaning.

6.4 Additional Findings

Police Violence. Does the reduced news coverage of local crime also affect the probability that officers are involved in episodes of police violence? In Appendix Table XVI we address this question using data from Fatal Encounters. We find no evidence supporting the idea of news coverage of crime stories influencing police violence. The large confidence intervals suggest however that, given that officer-involved fatalities are rare events, we might not have sufficient power to detect an effect.

Municipal Police Spending. It is possible for the main result to be explained by covered munici-

palities having lower police spending as opposed to non-covered municipalities after Sinclair entry. Appendix Table XVII shows that this is not the case: after Sinclair entry, covered and non-covered municipalities have similar police expenditures and employment per capita.

6.5 Robustness of the Effect of Sinclair on Clearance Rates

Appendix Table XVIII shows that the effect of Sinclair entry on the violent crime clearance rate is robust to a number of potential concerns. Column (1) reports the baseline estimate for reference.

Robustness to Data Cleaning. We begin by showing that the result is not sensitive to the data cleaning procedure. First, in column (2) we show that not winsorizing the outcome only minimally impacts the estimates. In addition, column (3) shows that the result is virtually unchanged if we do not replace record errors using the regression-based procedure described in Appendix B.

Robustness to Treatment Definition. We also show that using alternative definitions of Sinclair control does not affect the result. The estimates are robust to dropping media markets where Sinclair divested a station (column (4)) and considering only media markets where Sinclair directly owns and operates a station (column (5)). Finally, we consider the possibility that Sinclair acquisitions might correlate with trends in covered relative to non-covered municipalities. In column (6), we shown that this is unlikely to explain our results: the coefficient is unchanged when we only consider markets that Sinclair entered as part of multi-station deals, where acquisitions are less likely to be driven by specific media market conditions.

6.6 Robustness to Heterogeneous Effects in TWFE Models

Recent work in the econometrics literature has highlighted that two-way fixed effects (TWFE) regressions (i.e. regressions that control for group and time fixed effects) recover a weighted average of the average treatment effect in each group and time period (de Chaisemartin and D'Haultfœuille (2020)). This is problematic because weights can be negative, which means that if treatment effects are heterogeneous, the TWFE estimates might be biased. No formal extension of these concepts to higher dimensional fixed effect models, such as the ones we use in this paper, is available at the moment.

Nonetheless, we provide three pieces of evidence consistent with the effect on the violent crime clearance rate being robust to concerns related to heterogeneous treatment effects. First, we note that issues with negative weights are most severe when the majority of units in the sample are treated as some point. The fact that we have a large number of media markets that never experience Sinclair entry suggests that negative weights might have more limited relevance in our setting.

Second, we apply the machinery introduced by de Chaisemartin and D'Haultfœuille (2020) to the differences-in-differences specifications that underlie our estimates, and show event study graphs using the robust estimator proposed in their paper in Appendix Figure XI.[41] Reassuringly, the robust estimation shows treatment effects that are very similar to the baseline estimates from the differences-in-differences specifications. Given that the estimates that underlie our main effects are robust to allowing for treatment effects to be heterogeneous, we are confident in our triple differences as well.

Finally, we show that our results are robust to artificially eliminating variation from the staggered timing of Sinclair acquisitions. This is important to the extent that the issue of negative weights in staggered designs arises in part from using earlier treated units as control for later treated units (Goodman-Bacon (2019)). We eliminate variation from staggered timing by running regressions including only media markets that are either never treated or that are acquired at specific points in time.[42] Appendix Table XIX shows that out of the four years we consider, three reproduce a negative coefficient. The magnitude of the effect is larger in two of them and not significant in one, but larger standard errors produce confidence intervals consistent with the main point estimate. Instead, we do not find a similar effect if we focus on media markets entered in 2013 only. Note however that this is quite consistent with our intuition since, in 2013, we do not find a clear effect of Sinclair acquisitions on local crime coverage.

[41] Appendix Table XIII shows that the triple differences estimates for both of our main outcomes can be separated in differences-in-differences estimates from specifications that only exploit variation in the staggered timing of Sinclair acquisitions for covered and non-covered municipalities.

[42] We perform a separate estimation for all years in which Sinclair entered more than three media markets.

7 Mechanisms

How does the decline in local crime coverage affect clearance rates? The explanation that we propose is that when stories about a municipality's violent crimes are less frequent, perceptions change. Crime become less salient in the public opinion and the police find themselves operating in a political environment where there is less pressure to clear violent crimes. As a result, the police might have incentives to reallocate their resources away from clearing these crimes in favor of other policing activities. In this section, we provide three pieces of evidence supporting this mechanism but also discuss alternative explanations such as monitoring of police officers on part of the media and community cooperation in solving crimes.

Salience of Crime. To support the idea that the decline in crime content impacts perceptions, we investigate whether general interest about crime changes after Sinclair acquisitions. We do so by using two different data sources: Google trends data on searches for crime-related keywords and survey data from Gallup on whether crime is the most important problem facing the country. Neither dataset is perfect: Google searches are only available at the media market level, while even a large and nationally representative survey such as Gallup gives us few respondents for each municipality. Nevertheless, the results of both analyses point in the same direction: a decrease in the salience of crime in the public opinion.

We begin by looking at the Google trends data. We collect data on monthly searches containing the terms "crime" and "police" (see Appendix B for more details). Because the Google trends data are not consistently available below the media market level, we implement a differences-in-differences design exploiting the staggered entry of Sinclair across media markets. The outcome variable is the monthly volume of searches, and it is expressed in logarithms. The sample is restricted to media markets for which the volume searches for crime and police are always available.

Table IV shows that, when Sinclair enters a media market, the volume of searches containing the keywords crime and police decreases by 4.8% and 4.2%. The effect is not explained by a generalized decline in searches, as shown by placebo regressions looking at monthly searches for

Table IV: Effect of Sinclair Entry on Salience of Crime, Google Trends

Dependent Variable	Monthly Search Volume			
Keyword	Crime	Police	Weather	Youtube
	(1)	(2)	(3)	(4)
Sinclair	-0.048***	-0.042***	-0.001	-0.004
	(0.015)	(0.014)	(0.016)	(0.011)
Observations	14976	14976	14976	14976
Clusters	156	156	156	156
Outcome Mean in 2010	3.627	3.920	3.873	4.285
Media Market FE	X	X	X	X
Month FE	X	X	X	X
Media Market Controls	X	X	X	X

Notes: This table shows the effect of Sinclair entry on the salience of crime and police using Google trend data in differences-in-differences design. We regress the search volume for "crime" (column (1)), "police" (column (2)), "weather" (column (3)) and "youtube" (column (4)) on an indicator variable for Sinclair presence in the media market, baseline media market characteristics interacted with month fixed effects, media market fixed effects, and month fixed effects. The characteristics included are log population, share male, share male between 15 and 30, share white, share Hispanic, share unemployed, and log income per capita. Standard errors are clustered at the media market level. The dataset is at the media market by month level. Treatment is defined at the monthly level. The monthly level of searches is in logs.

popular keywords such as "weather" and "youtube." These results suggest that the decrease in local crime stories triggers a change in public interest for precisely those topics that are now less present in local news. Importantly, this is the opposite direction to what one would expect based on actual crime rates that are, if anything, higher after Sinclair enters a media market.

Second, we turn to the Gallup Poll Social Series, a set of public opinion surveys that we can use to measure individual perceptions about crime. These survey include a question that asks what is the most important problem that the country is facing, with crime being one of the possible answers (see Appendix B for more details). This question allows us to measure crime salience directly and to test whether, when exposed to a lower amount of local crime news, individuals update their perception accordingly. Table V shows that, after Sinclair entry, covered municipalities are less likely to have at least one respondent that reports crime as being the most important problem relative to non-covered municipalities.[43] Controlling for the number of respondents interviewed in each municipality and year (column (2)), or estimating the regression on a quasi-balanced sample of municipalities (column (3)) does not impact the result. Overall, this is consistent with Sinclair entry having a negative effect on the salience of crime in the public opinion. However, our empirical

[43]The large magnitude of the effect relative to the baseline mean in 2010 is explained by the fact that the share of individuals who believe that crime is the most important problem increases sharply over time period we study. For example, the outcome mean is almost 0.05 in 2017 (0.07 for covered municipalities).

Table V: Effect of Sinclair Entry on Salience of Crime, Gallup

Dependent Variable	Most Important Problem is Crime		
	(1)	(2)	(3)
Sinclair * Covered	-0.034**	-0.032*	-0.037*
	(0.017)	(0.016)	(0.022)
Observations	9430	9430	8009
Clusters	112	112	110
Stations	1619	1619	1194
Outcome Mean in 2010	0.014	0.014	0.016
Station FE	X	X	X
Month FE	X	X	X
Media Market Controls	X	X	X
Controls for Number of Respondents		X	
Balanced Sample			X

Notes: This table shows the effect of Sinclair entry on whether individuals report crime as the most important problem the country is facing in covered municipalities relative to non-covered municipalities. We regress an indicator variable equal to one if at least one respondent reported crime as the most important problem on the interaction between between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Column (2) controls for the number of respondents. Column (3) restricts the sample to municipalities in the data for four years or more. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year.

strategy is very demanding even for a large-sample survey such as this one, which means that the results have to be taken with a grain of salt.

Political Feedback. Perceptions become reality within the political arena. If the change in news coverage of local crime makes it less salient in the public opinion, politicians should react to it.[44] We believe this feedback mechanism to be particularly credible in this setting given that the individuals whose opinion is likely to be influenced by local news are exactly the ones whose opinions are likely to matter for local politics: those over 55.[45,46]

[44]We might worry that the effect is instead explained by politicians reacting to a Sinclair-induced change on local news coverage of non-crime topics. This is unlikely to be the case for two reasons. First, Appendix Table V shows that Sinclair limitedly affects local news that are not about crime. Second, it is unclear why a decline in local news across the board might affect the behavior of politicians in a way that impacts violent crime clearance rates.

[45]Police department chiefs are generally appointed (and removed at will) by the head of local government, which implies that their incentives tend to aligned with those of the municipality's administration (Owens (2020)). Consistent with this idea, recent papers have shown that political incentives affect law enforcement (Goldstein et al. (2020), and Magazinnik (2018)). In addition, managerial directives can have important effects on police behavior, supporting the idea that pressure coming from the top might influence the effort allocation of police officers (Ba and Rivera, 2019; Goldstein et al., 2020; Mummolo, 2018).

[46]The following quote, included in a case study on how politics influence police in an American city by Davies (2007), highlights the mechanism we have in mind: "The following case study results show [...] substantial impact of the city council on homicide investigations and, ultimately, on case clearances. [...] The media was seen as the catalyst for formal actions by other components of the authorizing environment to improve the murder clearance rate. The

Appendix Figure XIV shows descriptive evidence supporting this statement. Using the 2010 Cooperative Congressional Election Study (Ansolabehere, 2012), we show that individuals over 55 are 25% more likely to watch local TV news and 50% more likely to attend local political meetings compared to younger individuals. This is important to the extent that it highlights how perceptions of specific crime issues might be reflected in police behavior through the pressure of public opinion in the absence of elections. In addition, Goldstein (2019) shows that people over 55 are an especially important interest group for local politics when it comes to crime and policing.

Consistent with this argument, Appendix Table XX shows that the effect on the violent crime clearance rate appears to be driven by cities with a larger share of population above 55 (p-value = 0.121), even though the change in content is exactly the same across the two groups of municipalities. While the difference in the effect is not statistically significant, we interpret this as potential evidence that a change in public opinion operating through a political feedback mechanism might be behind the main effect on clearance rates.

Direct Media Monitoring. An alternative explanation is that there could be a decrease in direct media monitoring of the police. If police officers anticipate a low probability of being covered in the news for failing to solve crimes, they might shirk the amount of effort they allocate to this activity. To explore whether this is likely to be the case, we use our content data to separately identify stories about crime incidents and about arrests. In particular, we define stories to be about arrests if they contain an arrest-related string.[47] All other stories are about crime.

In Appendix Table XXI, we separately report the effect of a Sinclair acquisition on the relative probability that covered and non-covered municipalities appear in the news with different types of crime stories. The decline in crime reporting appears to be almost entirely driven by stories about crime incidents (column (1)), whereas stories about arrests experience a much smaller decline, which is also not statistically significant (column (2)). These results do not support direct media monitoring through stories about police clearances being the main explanation for the results,

media shaped public opinion about the quality of public safety."

[47] In particular, we use the following arrest-related strings: arrest, capture, detention, custody, apprehend, catch, caught, detain, imprison, incarcerat, jail.

although we cannot exclude the possibility that police officers are updating their overall probability of being the subject of reporting based on the decline in crime coverage.

Community Cooperation. It is also possible for the effect on clearance rates to be driven by decreased community cooperation with the police. Community cooperation is generally considered to be important for successful policing and crime investigations, and it has been shown to decrease after high-profile cases of police misconduct that negatively impact perceptions of police (Desmond et al., 2016). It is unclear why the change in content that we document should have direct negative effects on the public's perception of the police: if anything, people are seeing fewer stories about crimes and a similar number of stories about arrests, so they should perceive the police as being equally effective.[48]

Having said this, we might still worry that, independently of what the public thinks of the police, people might be less likely to spontaneously provide useful information to solve crimes if they do not hear about the crime incidents on TV. Unfortunately, there exists almost no data on the importance of tips for solving crimes, which limits our ability of testing for this mechanism directly. Nonetheless, we believe that the magnitude of the effect on the violent crime clearance rate is too large for tips to be the main driver of what we find.

Were the decrease in clearance rates caused by a drop in tips, the decline in clearances should be concentrated in those violent crimes that are no longer covered in the news after Sinclair enters a media market (e.g. there should be a one-to-one relationship between crimes that are no longer covered in the news and those not cleared by the police). However, because not all crimes are covered in the news, Sinclair controls one of four stations in the media market, and the other stations are not adjusting their crime coverage, the change in content that we document in Section 5.2 implies too few incidents no longer being covered in the news because of Sinclair for the magnitude of the effect to be credible. Instead, the magnitude of the effect can be more easily reconciled by abandoning the one-to-one correspondence between crimes reported in the news and crimes cleared

[48] Instead, we would interpret a change in the effectiveness of the police coming from the relative decline in clearance rates to be downstream from the effect on police effort, and we do not see it as a threat to our interpretation.

by the police. That is, by thinking that the effect comes from the clearance rates of all violent crimes (i.e. not just the ones covered in the news) changing by 7.5%, as would be the case under the mechanism that we propose earlier in this section.

8 Conclusion

In this paper, we study the effect of a shock in news coverage of crime on municipal police departments in the United States. The source of variation in local news content that we exploit is the acquisition of local TV stations by the Sinclair Broadcast Group. In particular, our empirical strategy combines variation in the staggered timing of acquisitions with cross-sectional variation in exposure to the local news shock in a triple differences design.

Ownership matters for content: once acquired by Sinclair, TV stations decrease news coverage of local crime. We document this by exploiting a unique dataset of transcripts of local TV newscasts of 325 stations 2010-2017. We find a very significant and sizable effect: relative to non-covered municipalities, covered municipalities exhibit a reduction in the probability of appearing in the news with a crime story of about 25% of the outcome mean in 2010.

How does police behavior change in response to the decline in news coverage of local crime? We find that after Sinclair enters a media market, covered municipalities exhibit lower violent crime clearance rates relative to non-covered municipalities. The effect is significant at the 5% level and corresponds to a decrease to 7.5% of the baseline mean. We do not find any effect for property crime clearance rates, which is consistent with local TV news having a violent crime focus.

To explain these results, we argue that when violent crime appears less frequently in the news, the salience of crime in the public opinion decreases. The police find themselves operating in a political environment where there is less pressure to clear violent crimes, and they reallocate resources away from clearing these crimes in favor of other police activities, because of an overall decrease in crime salience.

To conclude, this paper shows that shocks to local media content driven by acquisitions can affect the behavior of the police. Overall, this suggests that the increase in ownership concentration currently

characterizing the local TV market in the United States might have important consequences for local institutions.

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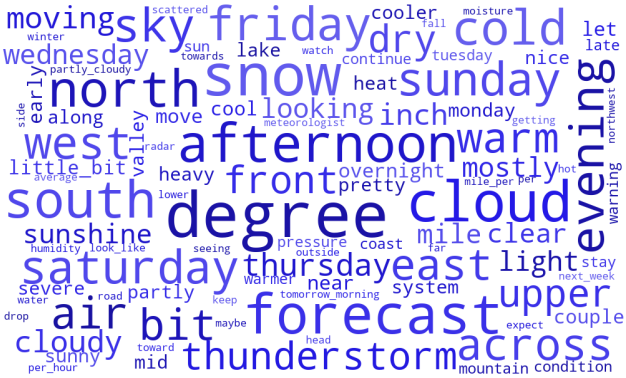
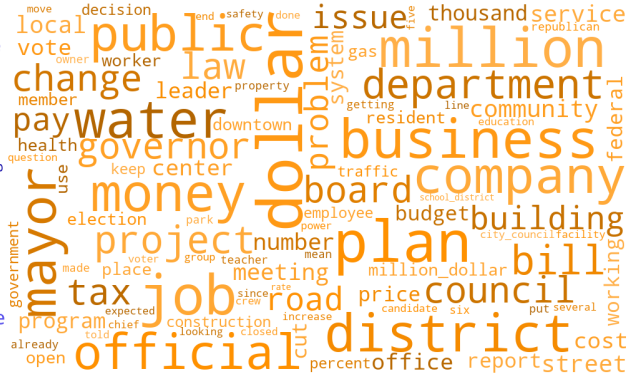
Online Appendix

Appendix Figure I: Local News Topics



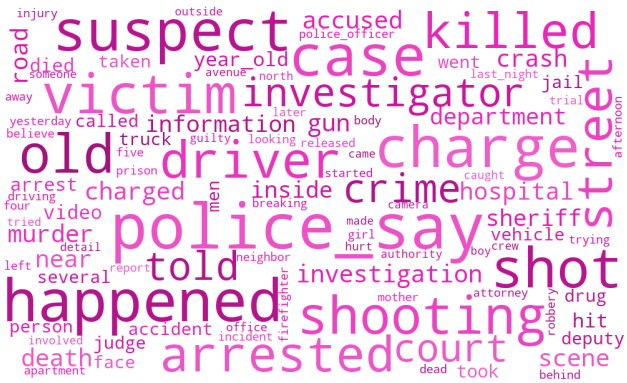
(a) Weather



(b) Politics

(c) Sports

(d) Miscellaneous



(e) Crime

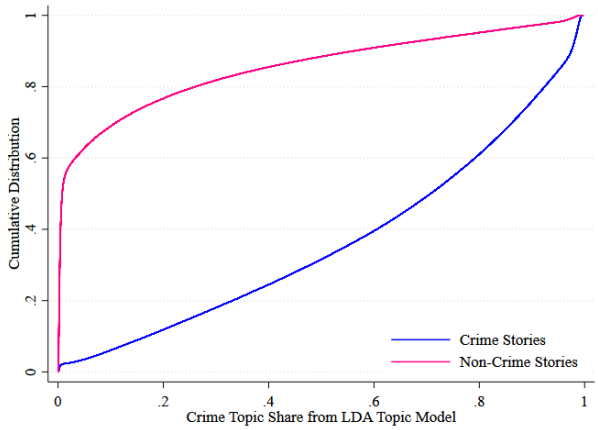
Notes: This figure shows word clouds of the 50 words and bigrams that have the highest probability of being generated by a given topic. The size of the word is proportional to the word's probability.

Appendix Figure II: Crime Bigrams, by Highest Frequency and Highest Relative Frequency

(a) Frequency (b) Relatively Frequency

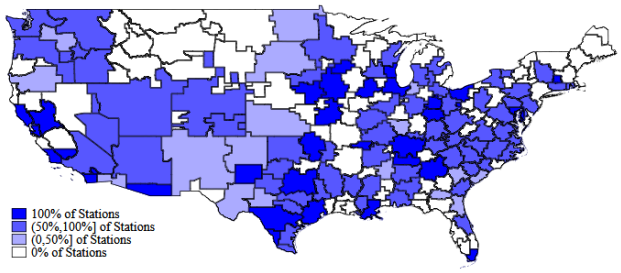
Notes: This figure shows word clouds of the top 50 bigrams that we use to identify crime stories by frequency (Panel (a)) and by relative frequency (Panel (b)). The size of the words is proportional to their absolute and relative frequency.

Appendix Figure III: Classification of Local Stories: Validation



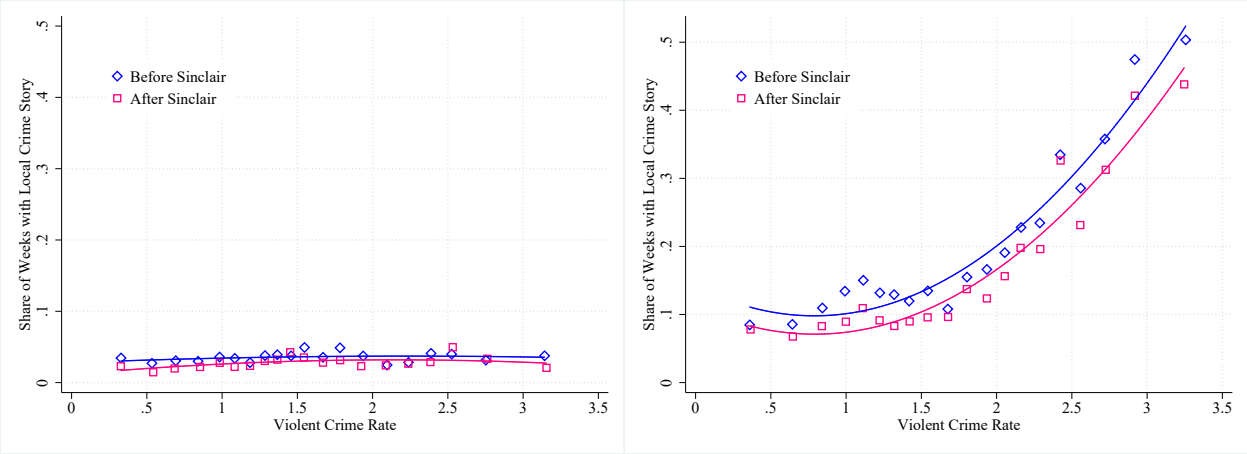
Notes: This figure shows the cumulative distribution of the crime topic share separately by whether local stories are classified to be about crime or not according to the methodology described in Section 3. Crime topic shares are from an unsupervised LDA model trained on local crime stories. Stories are defined to be local if they mention at least one of the municipalities with more than 10,000 people in the media market.

Appendix Figure IV: Map of Media Markets Included in the Content Sample



Notes: This map shows the share of stations for which we have content data continuously from 2010-2017 across media markets in the United States. Darker colors correspond to higher shares of media market stations included in the content data. 61% of media market have at least one station included in our sample, and for 88% of them the sample includes more than half of the stations present in the market.

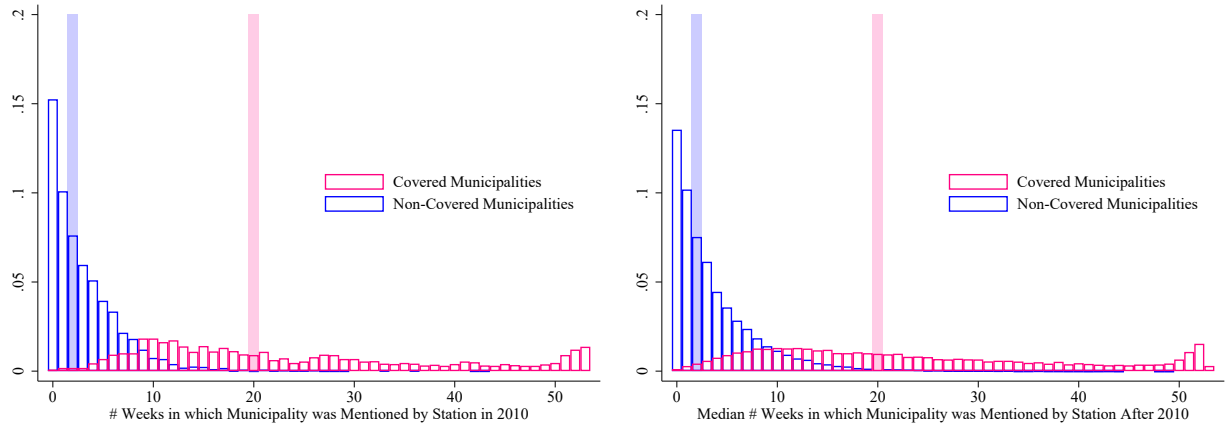
Appendix Figure V: Relationship Between Violent Crime Rates and Share of Weeks with Local Crime Story Before and After Sinclair Control, by Covered Status



(a) Non-Covered Municipalities (b) Covered Municipalities

Notes: This figure shows how the relationship between violent crime rates and local crime reporting changes with Sinclair control, by whether a municipality is covered at baseline or not. Panel (a) shows a binned scatter plot of the relationship between the municipality's violent crime rate and the share of weeks in a year in which the station reports a local crime story about the municipality, separately before and after Sinclair control, for non-covered municipalities. Panel (b) shows the same binned scatter plot for covered municipalities. The sample is restricted to stations that ever experienced Sinclair control. Covered municipalities are mentioned in the news more than the median municipality in 2010. Crime rates are IHS crimes per 1,000 people, and are winsorized at the 99% level.

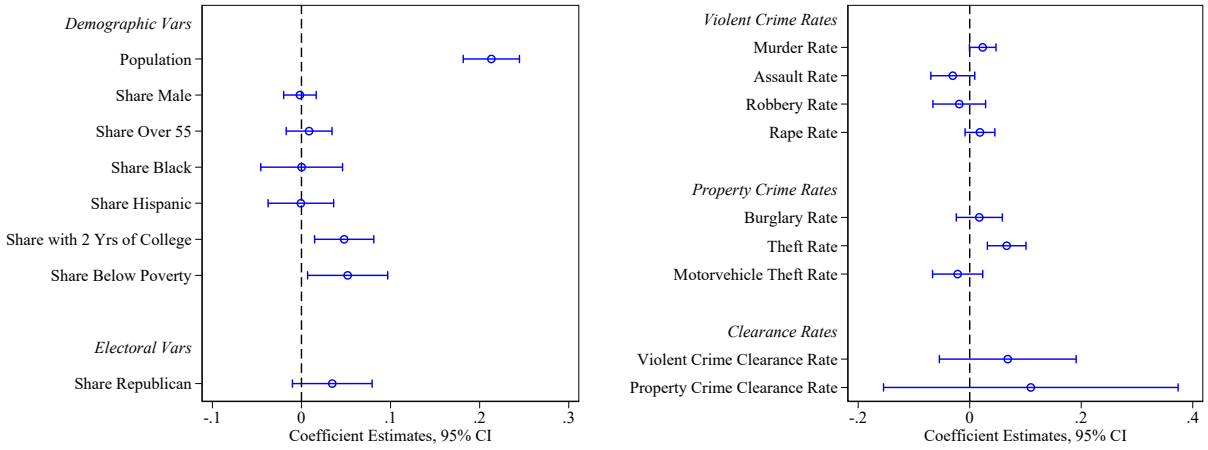
Appendix Figure VI: Number of Weeks in which Municipality is Mentioned by Station in 2010 (Baseline Year) and After 2010, by Covered Status



(a) In 2010 (b) After 2010

Notes: This figure shows that covered status persists over time. Panel (a) presents a histogram of the number of weeks in which the municipality was mentioned by the station in 2010, by whether the municipality is covered at baseline or not. Panel (b) presents a histogram of the median number of weeks in which the municipality was mentioned by the station after 2010, by whether a municipality is covered at baseline or not. The two vertical lines indicate the median number of mentions for each group of municipalities. The overlap between the two distributions can be explained by covered status being determined based on the median share of weeks in which the municipality was mentioned in 2010 across stations. Covered municipalities are mentioned in the news more than the median municipality in 2010.

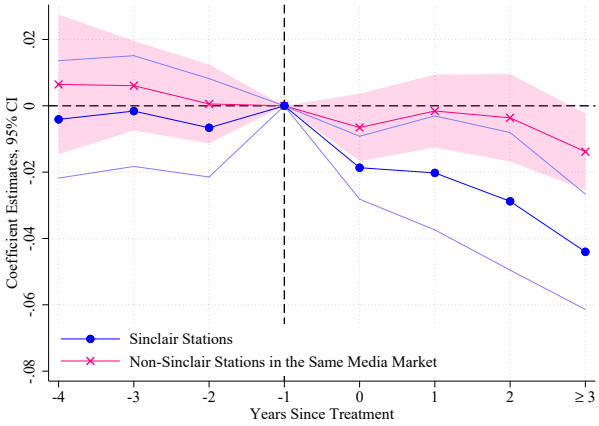
Appendix Figure VII: Differences Between Covered and Non-Covered Municipalities



(b) Socio-economic Characteristics (b) Crime and Clearance Rates

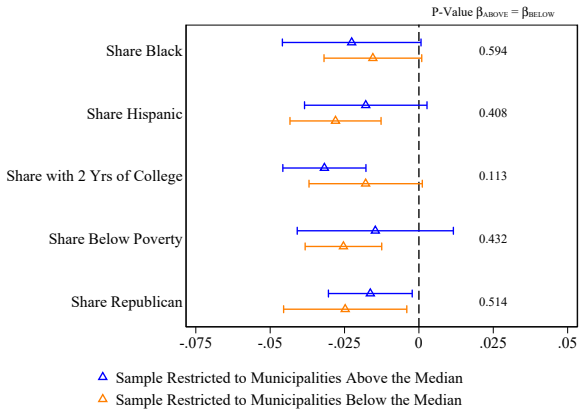
Notes: This figure shows along which dimensions covered and non-covered municipalities differ. We report coefficient estimates together with 95% confidence intervals from a regression of an indicator variable for the municipality being covered at baseline on standardized socio-economic characteristics of the municipality, crime and clearance rates in 2010, and media market fixed effects. All coefficients are estimated in the same regression, but we report them in two separate graphs for ease of exposition. Given that all independent variables are standardized, the coefficients report the effect of a one standard deviation increase. Standard errors are clustered at the media market level. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. Crime rates are IHS crimes per 1,000 people. Both clearance rates and crime rates are winsorized at the 99% level.

Appendix Figure VIII: Effect of Sinclair Control for Sinclair-Controlled Stations and Other Same Media Market Stations on the Probability of Having a Local Crime Story, by Year since Treatment



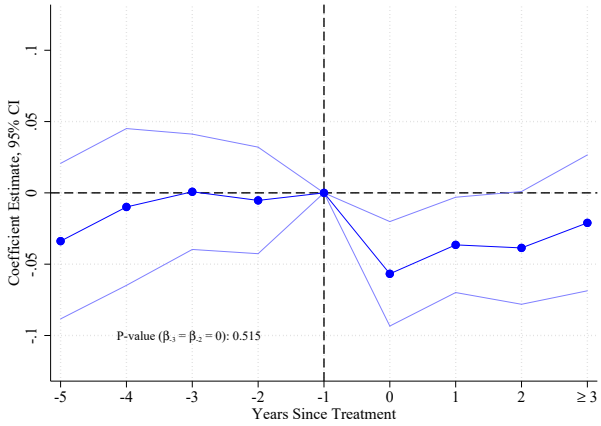
Notes: This figure shows the effect of Sinclair entry, separately for stations directly controlled by Sinclair and for same media market stations not directly controlled by Sinclair, on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities, by year since treatment. We report coefficient estimates and 95% confidence intervals from a regression of an indicator variable for the station reporting a local crime story about the municipality on the interaction between indicator variables for years since Sinclair control and an indicator variable for whether the municipality is covered at baseline for Sinclair stations, the interaction between indicator variables for years since Sinclair entry and an indicator variable for whether the municipality is covered at baseline for non-Sinclair station in a Sinclair media markets, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects (equation (2)). The omitted category is T-1. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level, but the effect is constrained to be the same by year since treatment. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Figure IX: Effect of Sinclair Control on the Probability of Having a Local Crime Story, Heterogeneous Effects by Municipality Characteristics



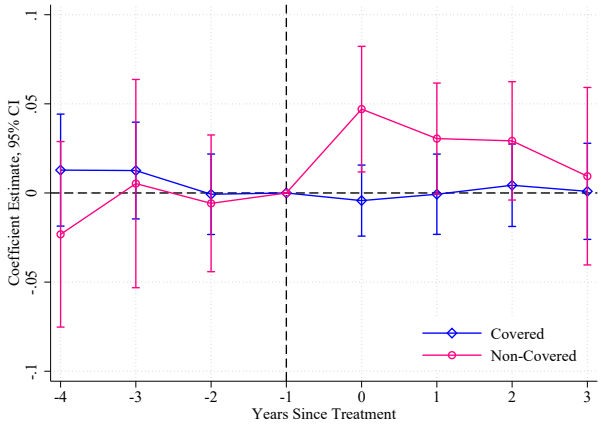
Notes: This figure presents the heterogeneity of the effect of Sinclair entry on local crime reporting. We report coefficient estimates and 95% confidence intervals from two separate regressions for municipalities above and below the median according to the characteristic. The *p*-value reported is from a test of equality of the main coefficients across the two samples. We regress an indicator variable for the station reporting a local crime story about the municipality on the interaction between an indicator variable for the station being under Sinclair control and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for the station being under Sinclair control and baseline municipality characteristics, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects (equation (1)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the month level. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Figure X: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by Year since Treatment, Estimated Including Data for 2009



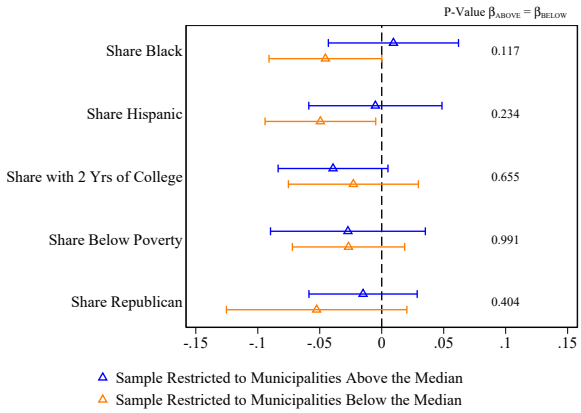
Notes: This figure shows the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities, by year since treatment using data that additionally includes 2009. We report coefficient estimates and 95% confidence intervals from a regression of the municipality's violent crime clearance rate on the interaction between indicator variables for years since Sinclair entry and an indicator variable for whether the municipality is covered at baseline, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (5)). The omitted category is T-1. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix Figure XI: Effect of Sinclair Controls on the Violent Crime Clearance Rate by Year since Treatment, Robustness to Heterogeneous Effects in TWFE Models



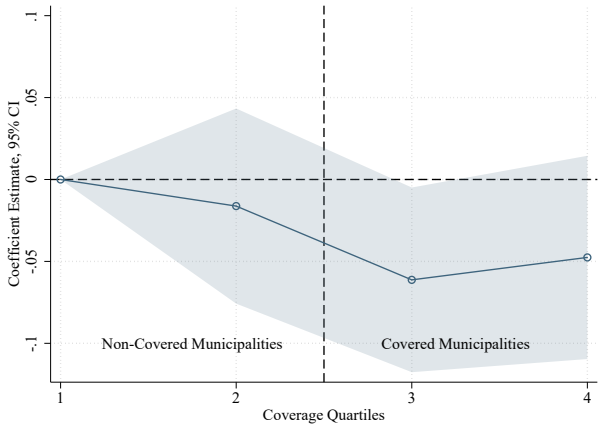
Notes: This figure shows the effect of Sinclair entry on the violent crime clearance rate by year since treatment, estimated separately for covered and non-covered municipalities using an estimator robust to heterogeneous treatment effects in TWFE models. The starting point is a TWFE model that regresses the outcome on year and municipality fixed effects. We estimate placebo coefficients leading up to treatment and dynamic treatment effects using the robust estimator proposed by de Chaisemartin and D'Haultfoeoeuille (2020), which we report together with 95% confidence intervals from 1000 bootstrap repetitions. The analysis is run separately for covered and non-covered municipalities, but we report the coefficients on the same graph for ease of comparison. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the year level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. Clearance rates are winsorized at the 99% level.

Appendix Figure XII: Effect of Sinclair Controls on the Violent Crime Clearance Rate, Heterogeneous Effects by Municipality Characteristics



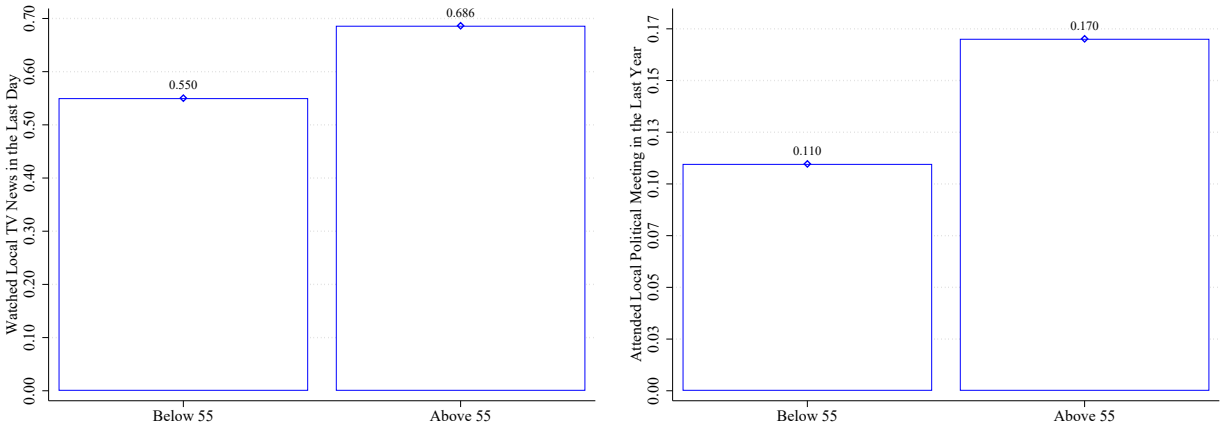
Notes: This figure presents the heterogeneity of the effect of Sinclair entry on the violent crime clearance rate. We report coefficient estimates and 95% confidence intervals from two separate regression models for municipalities above and below the median according to the characteristic. The *p*-value reported is from a test of equality of the main coefficients across the two samples. We regress the municipality's violent crime clearance rate on the interaction between between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (4)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix Figure XIII: Effect of Sinclair Controls on the Violent Crime Clearance Rate, by Coverage Quartile



Notes: This figure shows the effect of Sinclair entry on the violent crime clearance rate by a municipality's coverage quartile. We regress the municipality's violent crime clearance rate on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for the municipality's baseline coverage quartile, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (similar to equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Baseline coverage quartiles are defined based on the number of times the municipality is mentioned in the news in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix Figure XIV: Local News Viewership and Political Participation, by Age



(a) Watched Local TV News

(b) Attended a Local Political Meeting

Notes: This figure reports the share of people who reported watching local TV news in the last day (Panel (a)) or attended a local political meeting in the last year (Panel (b)), separately for individuals below and above 55. Data are from the 2010 Cooperative Congressional Election Study.

Appendix Table I: Sample Summary

	Overall	Included in the Content Analysis
	(1)	(2)
# of Stations	835	325
# of Stations Ever Controlled by Sinclair	121	38
# of Stations Ever Owned and Operated by Sinclair	110	37
# of Stations Ever Owned and Operated by Cunningham	10	1
# of Stations Ever Controlled by Sinclair through a Local Marketing Agreement	10	4

Notes: This table presents summary counts for full-powered commercial TV stations affiliated with a big four network 2010-2017, separately for all stations (column (1)) and for the sample of stations included in the content analysis (column (2)).

Appendix Table II: Descriptive Statistics

	Municipalities in the Analysis			All Municipalities			P-value
	N	Mean	SD	N	Mean	SD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Content						
Had a Local Story	2253	0.267	0.269				
Had a Local Crime Story	2253	0.103	0.171				
	Panel B: Crime and Clearance Rates						
Property Crime Rate	1792	4.072	0.527	2365	4.063	0.540	0.774
Violent Crime Rate	1792	1.673	0.814	2365	1.713	0.807	0.228
Property Crime Clearance Rate	1792	0.191	0.119	2365	0.192	0.117	0.848
Violent Crime Clearance Rate	1792	0.461	0.255	2365	0.465	0.251	0.674
	Panel C: Municipality Characteristics						
Population	1792	59219	159090	2365	58653	217781	0.825
Share Male	1792	0.487	0.025	2365	0.487	0.026	0.773
Share Over 55	1792	0.232	0.064	2365	0.236	0.065	0.060
Share Black	1792	0.117	0.159	2365	0.115	0.157	0.578
Share Hispanic	1792	0.158	0.187	2365	0.155	0.188	0.675
Share with 2 Years of College	1792	0.365	0.149	2365	0.360	0.147	0.276
Share Below Poverty Line	1792	0.136	0.078	2365	0.139	0.078	0.328
Share Republican	1792	0.475	0.159	2365	0.468	0.156	0.231

Notes: This table reports descriptive statistics for the main variables considered in the analysis and for municipality characteristics. Columns (1) to (3) restrict the sample to municipalities included in the main analysis; columns (4) to (6) include all municipalities with more than 10,000 people. Column (7) reports the *p*-value of the difference between the two samples from a regression of the specified characteristics on a dummy for the municipality being included in the analysis, with standard errors clustered at the media market level. The content analysis includes 2201; 1752 are also in the police behavior analysis. The reference sample additionally includes 606 municipalities that satisfy the conditions to be included in the police behavior analysis, but are located in media markets for which we have no content data (see Appendix B for a detailed explanation). Content and crime and clearance rates are measured in 2010. Crime rates are IHS crimes per 1,000 people and clearance rates as total number of crimes cleared by arrest or exceptional means over total number of crimes. Both clearance rates and crime rates are winsorized at the 99% level.

Appendix Table III: Sinclair Entry and Media Market Socio-Economic and Political Characteristics

Dependent Variable	Pop.	Share Male	Share Male 15 to 30	Share White	Share Hispanic	Unempl.	Income per Capita	Turnout	Share Repub.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: All DMAs								
Sinclair	0.001	0.018	-0.001	0.008	0.102	-0.262	0.009*	-0.012	-0.002
	(0.004)	(0.021)	(0.028)	(0.062)	(0.081)	(0.170)	(0.005)	(0.015)	(0.007)
Observations	1648	1648	1648	1648	1648	1648	1648	615	615
Clusters	206	206	206	206	206	206	206	205	205
Outcome Mean in 2010	13.561	49.412	10.783	83.240	11.808	9.454	3.539	0.508	0.515
	Panel B: DMAs in Content Data								
Sinclair	0.000	0.030	-0.007	0.088	0.082	-0.042	0.006	0.001	0.003
	(0.005)	(0.021)	(0.031)	(0.084)	(0.105)	(0.207)	(0.006)	(0.003)	(0.007)
Observations	904	904	904	904	904	904	904	336	336
Clusters	113	113	113	113	113	113	113	112	112
Outcome Mean in 2010	14.157	49.290	10.833	80.730	14.215	9.564	3.580	0.432	0.510

Notes: This table shows the relationship between Sinclair entry and socio-economic and political trends. We regress the outcome on an indicator variable for Sinclair entry, media market fixed effects, and year fixed effects. The sample includes all media markets in Panel A, and is restricted to media markets in the content data in Panel B. Standard errors are clustered at the media market level. The dataset is a media market by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Population and income per capita are defined in logs.

Appendix Table IV: Effect of Sinclair Control on the Probability of Having a Local Story, DID

Dependent Variable	Had Local Crime Story						
Sample	Non-Covered		Covered		Covered and Non-Covered		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sinclair	-0.004	-0.003	-0.035***	-0.031**	-0.002	-0.002	
	(0.003)	(0.003)	(0.013)	(0.013)	(0.003)	(0.003)	
Sinclair * Covered					-0.027**	-0.029***	-0.024***
					(0.011)	(0.011)	(0.007)
Observations	1643158	1643158	1500202	1500202	3143360	3143360	3143360
Clusters	90	90	113	113	113	113	113
Municipalities	1108	1108	1145	1145	2253	2253	2253
Stations	278	278	325	325	325	325	325
Outcome Mean in 2010	0.017	0.017	0.174	0.174	0.092	0.092	0.092

Notes: This table shows the effect of Sinclair control on the probability that a station reports a local story using a differences-in-differences specification estimated separately for non-covered (columns (1) and (2)) and covered (columns (3) and (4)) municipalities. We regress the outcome on an indicator variable for the station being under Sinclair control, station by municipality fixed effects and week fixed effects. Columns (2) and (4) additionally control for baseline municipality characteristics interacted with week fixed effects. Column (5) to (7) show instead how we arrive to the triple differences specification using the full sample. In particular, column (5) estimates a differences-in-differences with heterogeneous treatment effects for covered and non-covered municipalities. We regress the outcome on an indicator variable for the station being under Sinclair control, the interaction between an an indicator variable for the station being under Sinclair control and an indicator variable for whether the municipality is covered at baseline, baseline municipality characteristics interacted with week fixed effects, station by municipality fixed effects and week fixed effects. Column (6) additionally controls for covered status by week fixed effects. Finally, column (7) includes station by week fixed effects and is similar to our baseline triple differences specification. The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the month level. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Table V: Effect of Sinclair Control on the Probability of Having a Local Story, by Whether the Story is about Crime

Dependent Variable	Had a Local Story		
Decomposition	Any	Crime	Non-Crime
	(1)	(2)	(3)
Sinclair * Covered	-0.032**	-0.021***	-0.023*
	(0.014)	(0.007)	(0.014)
Observations	3143360	3143360	3143360
Clusters	113	113	113
Municipalities	2253	2253	2253
Stations	325	325	325
Outcome Mean in 2010	0.248	0.092	0.221
Station by Week FE	X	X	X
Covered by Week FE	X	X	X
Station by Municipality FE	X	X	X
Sinclair * Controls	X	X	X

Notes: This table shows the effect of Sinclair control on the probability that a station reports a local story about covered municipalities relative to non-covered municipalities, overall (column (1)) and by whether the story is about crime (columns (2) and (3)). We regress the outcome on the interaction between an indicator variable for the station being under Sinclair control and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for the station being under Sinclair control and baseline municipality characteristics, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects (equation (1)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Table VI: Effect of Sinclair Control on Overall Crime Coverage, by Whether the Story is Local

Dependent Variable	Share of Stories about Crime		
Decomposition	All	Local	Non-Local
	(1)	(2)	(3)
Sinclair	-0.010*	-0.012***	0.002
	(0.005)	(0.004)	(0.003)
Observations	31120	31120	31120
Clusters	113	113	113
Stations	325	325	325
Outcome Mean in 2010	0.133	0.063	0.070
Station FE	X	X	X
Month FE	X	X	X
Media Market Controls	X	X	X

Notes: This table shows the effect of Sinclair control on the share of crime stories that are about crime, by whether the story is local or not, using a differences-in-differences specification. We define a story to be local if it mentions at least one of the municipalities with more than 10,000 people in the media market. We regress the outcome on an indicator variable for the station being under Sinclair control, baseline media market characteristics interacted with month fixed effects, station fixed effects, and month fixed effects. The characteristics included are log population, share male, share male between 15 and 30, share white, share Hispanic, share unemployed, and log income per capita. Standard errors are clustered at the media market level. The dataset is a station by month panel. Treatment is defined at the monthly level.

Appendix Table VII: Effect of Sinclair Control on Conservative Coverage of Non-Local Crime Stories

Dependent Variable	Share of Stories About…		Has Non-Local Story About…		
Type	Non-Local Crime	Non-Local Police	Police Misconduct	Crime and Drugs	Crime and Immigrants
	(1)	(2)	(3)	(4)	(5)
Sinclair	0.002	0.001	-0.031**	0.057**	0.060***
	(0.003)	(0.002)	(0.012)	(0.024)	(0.018)
Observations	31120	31120	31120	31120	31120
Clusters	113	113	113	113	113
Stations	325	325	325	325	325
Outcome Mean in 2010	0.133	0.063	0.070	0.800	0.188
Station FE	X	X	X	X	X
Month FE	X	X	X	X	X
Media Market Controls	X	X	X	X	X

Notes: This table shows the effect of Sinclair control on coverage of non-local crime stories. We define a story to be local if it mentions at least one of the municipalities with more than 10,000 people in the media market. All other stories are non-local. We define a story to be about crime following the methodology described in Section 3 (column (1)). We define a story to be about police if it contains the word "police" (column (2)), and about police misconduct if it contains both "police" and "misconduct" (column (3)). We define a story of be about crime and drugs if the story is about crime and in contains any of the following strings: "drug", "drugs", "marijuana", "cocaine", "meth", "ecstasy" (column (4)). Finally, we define a story of be about crime and immigrants if the story is about crime and in contains any of the words "immigration", "immigrant", "migrant", "undocumented" (column (5)). We regress the outcome on an indicator variable for the station being under Sinclair control, baseline media market characteristics interacted with month fixed effects, station fixed effects, and month fixed effects. The characteristics included are log population, share male, share male between 15 and 30, share white, share Hispanic, share unemployed, and log income per capita. Standard errors are clustered at the media market level. The dataset is a station by month panel. Treatment is defined at the month level.

Appendix Table VIII: Effect of Sinclair Control on the Probability of Having a Local Crime Story, by Political Leaning of the Municipality

Dependent Variable	Had Local Crime Story	
Share Republican	>= Median	< Median
	(1)	(2)
Sinclair * Covered	-0.018***	-0.024**
	(0.007)	(0.010)
Observations	1567082	1559558
Clusters	99	86
Municipalities	1123	1116
Stations	285	249
Outcome Mean in 2010	0.079	0.104
Station by Week FE	X	X
Covered by Week FE	X	X
Station by Municipality FE	X	X
Sinclair * Controls	X	X

Notes: This table shows the effect of Sinclair control on the share of crime stories that are about crime, splitting the sample by whether the municipality's Republican vote share was above (column (1)) or below (column (2)) the median in the 2008 presidential election. We regress an indicator variable for the station reporting a local crime story about the municipality on the interaction between an indicator variable for the station being under Sinclair control and an indicator variable for whether the municipality is covered at baseline, interactions between an indicator variable for the station being under Sinclair control and baseline municipality characteristics, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects (equation (1)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Table IX: Effect of Sinclair Control on the Probability of Having a Local Crime Story, Robustness Checks

Dependent Variable	Had Local Crime Story								
	Baseline	Data Cleaning and Sample					Treatment Definition		
Robustness to…		Less Restrictive Crime Story Definition	More Restrictive Crime Story Definition	No Imputation	Fixed Division of Newscasts into Stories	Same Sample as UCR Analysis	Drops Divested Stations	Stations Owned and Operated by Sinclair Only	Group Acquis. Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sinclair * Covered	-0.021***	-0.023***	-0.020***	-0.020***	-0.025***	-0.020***	-0.021***	-0.022***	-0.018**
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)
Observations	3143360	3143360	3143360	3054074	3143360	2502984	3137090	3143360	3129984
Clusters	113	113	113	113	113	112	113	113	112
Municipalities	2253	2253	2253	2253	2253	1792	2253	2253	2245
Stations	325	325	325	325	325	324	323	325	321
Outcome Mean in 2010	0.092	0.099	0.072	0.091	0.107	0.102	0.092	0.092	0.092
Station by Week FE	X	X	X	X	X	X	X	X	X
Covered by Week FE	X	X	X	X	X	X	X	X	X
Station by Municipality FE	X	X	X	X	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X	X	X	X	X

Notes: This table shows the robustness of the effect of Sinclair control on the probability that a station reports a local story about covered municipalities relative to non-covered municipalities. We regress an indicator variable for the station reporting a local crime story about the municipality on the interaction between an indicator variable for the station being under Sinclair control and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for the station being under Sinclair control and baseline municipality characteristics, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects (equation (1)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Column (1) reports the baseline estimate. Column (2) identifies crime stories using bigrams that are five (instead of ten) times more likely to appear in the crime library then in the non-crime library. Column (3) identifies crime stories using bigrams that are twenty (instead of ten) times more likely to appear in the crime library then in the non-crime library. Column (4) leaves spells shorter than eight weeks for which we have no content data as missing. Column (5) segments the newscasts into stories using a fixed number of words per story (see Appendix A for further details). Column (6) restricts the sample to municipalities also included in the crime analysis. Column (7) drops stations that were eventually divested from the sample. Column (8) restricts treatment to stations owned and operated by Sinclair. Column (9) drops stations that were not acquired by Sinclair as part of multi-station deal. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Table X: Effect of Sinclair Control on the Probability of Having a Local Crime Story, by Crime

Dependent Variable	Had Local Crime Story	
Type of Crime	Violent	Property
	(1)	(2)
Sinclair * Covered	-0.020***	-0.005
	(0.006)	(0.004)
Observations	3143360	3143360
Clusters	113	113
Municipalities	2253	2253
Stations	325	325
Outcome Mean in 2010	0.089	0.025
Station by Week FE	X	X
Covered by Week FE	X	X
Station by Municipality FE	X	X
Sinclair * Controls	X	X

Notes: This table shows the effect of Sinclair control on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities, by type of crime. We regress an indicator variable for the station reporting a local crime story about the municipality on the interaction between an indicator variable for the station being under Sinclair control and an indicator variable for whether the municipality is covered at baseline, interactions between an indicator variable for the station being under Sinclair control and baseline municipality characteristics, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects (equation (1)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is at the monthly level. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Table XI: Effect of Sinclair Entry on the Violent Crime Rate, by Type of Crime

Type of Crime	All	Murder	Assault	Robbery	Rape
	(1)	(2)	(3)	(4)	(5)
	Panel A: Dependent Variable as Crime Rates				
Sinclair * Covered	0.029	0.003	0.014	0.047***	-0.025
	(0.035)	(0.004)	(0.035)	(0.017)	(0.024)
Outcome Mean in 2010	1.673	0.034	1.233	0.720	0.300
	Panel B: Dependent Variable as Dummy = 1 if ≥ 1 Crime				
Sinclair * Covered	-	0.028	-0.001	-0.010	0.045**
	-	(0.036)	(0.004)	(0.014)	(0.017)
Outcome Mean in 2010	-	0.462	0.910	0.964	0.932
Observations	14336	14336	14336	14336	14336
Clusters	112	112	112	112	112
Municipalities	1792	1792	1792	1792	1792
Media Market by Year FE	X	X	X	X	X
Covered by Year FE	X	X	X	X	X
Municipality FE	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X

Notes: This table shows the effect of Sinclair entry on the crime rates of covered municipalities relative to non-covered municipalities, for different types of violent crimes. We regress the municipality's crime rate for a given type of violent crime on the interaction between between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Crime rates are IHS crimes per 1,000 people, and are winsorized at the 99% level.

Appendix Table XII: Effect of Sinclair Entry on the Property Crime Rate, by Type of Crime

Dependent Variable	Property Crime Rate			
Type of Crime	All	Burglary	Theft	MVT
	(1)	(2)	(3)	(4)
Sinclair * Covered	0.054**	0.067**	0.046	0.026
	(0.022)	(0.027)	(0.028)	(0.030)
Observations	14336	14336	14336	14336
Clusters	112	112	112	112
Municipalities	1792	1792	1792	1792
Outcome Mean in 2010	4.072	2.433	3.752	1.239
Media Market by Year FE	X	X	X	X
Covered by Year FE	X	X	X	X
Municipality FE	X	X	X	X
Sinclair * Controls	X	X	X	X

Notes: This table shows the effect of Sinclair entry on the crime rate of covered municipalities relative to non-covered municipalities, for different types of property crimes. We regress the municipality's crime rate for a given type of property crime on the interaction between between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Crime rates are IHS crimes per 1,000 people, and are winsorized at the 99% level. MVT stands for motor vehicle theft.

Appendix Table XIII: Effect of Sinclair Control on the Violent Crime Clearance Rate, DID

Dependent Variable	Violent Crime Clearance Rate						
Sample	Non-Covered		Covered		Covered and Non-Covered		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sinclair	0.029*	0.032**	-0.002	-0.006	0.026*	0.029**	
	(0.015)	(0.013)	(0.009)	(0.009)	(0.013)	(0.014)	
Sinclair * Covered					-0.028**	-0.033**	-0.032**
					(0.013)	(0.014)	(0.015)
Observations	6480	6480	7856	7856	14336	14336	14336
Clusters	86	86	112	112	112	112	112
Municipalities	810	810	982	982	1792	1792	1792
Outcome Mean in 2010	0.434	0.434	0.483	0.483	0.461	0.461	0.461
Municipality FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Controls by Year FE		X		X	X	X	X
Covered by Year FE						X	X
Media Market by Year FE							X

Notes: This table shows the effect of Sinclair entry on the violent crime clearance rate using a differences-in-differences specification estimated separately for non-covered (columns (1) and (2)) and covered (columns (3) and (4)) municipalities. We regress the outcome on an indicator variable for the station being under Sinclair control, municipality fixed effects and year fixed effects. Columns (2) and (4) additionally control for baseline municipality characteristics interacted with year fixed effects. Column (5) to (7) show instead how we arrive to the triple differences specification using the full sample. In particular, column (5) estimates a differences-in-differences with heterogeneous treatment effects for covered and non-covered municipalities. We regress the outcome on an indicator variable Sinclair presence in the media market, the interaction between an an indicator variable Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, baseline municipality characteristics interacted with year fixed effects, municipality fixed effects and year fixed effects. Column (6) additionally controls for covered status by year fixed effects. Finally, column (7) includes media market by year fixed effects and is similar to our baseline triple differences specification. The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the year level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. Clearance rates are winsorized at the 99% level.

Appendix Table XIV: Effect of Sinclair Control on the Property Crime Rate, DID

Dependent Variable	Property Crime Rate			
Sample	Non-Covered		Covered	
	(1)	(2)	(3)	(4)
Sinclair	0.005	0.017	-0.011	-0.005
	(0.037)	(0.036)	(0.027)	(0.024)
Observations	6480	6480	7856	7856
Clusters	86	86	112	112
Municipalities	810	810	982	982
Outcome Mean in 2010	3.919	3.919	4.198	4.198
Municipality FE	X	X	X	X
Year FE	X	X	X	X
Controls * Year FE		X		X

Notes: This table shows the effect of Sinclair entry on the property crime rate using a differences-in-differences specification estimated separately for non-covered (columns (1) and (2)) and covered (columns (3) and (4)) municipalities. We regress the outcome on an indicator variable for the station being under Sinclair control, municipality fixed effects and year fixed effects. Columns (2) and (4) additionally control for baseline municipality characteristics interacted with year fixed effects. The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the year level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Crime rates are IHS crimes per 1,000 people, and are winsorized at the 99% level.

Appendix Table XV: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by Type of Crime

Type of Crime	Violent Crime Clearance Rate				
	All	Murder	Assault	Robbery	Rape
	(1)	(2)	(3)	(4)	(5)
	Panel A: Full Sample				
Sinclair * Covered	-0.034**	0.110	-0.003	-0.052*	-0.055*
	(0.016)	(0.069)	(0.017)	(0.027)	(0.031)
Observations	14336	6919	13069	13886	13404
Clusters	112	111	111	112	112
Municipalities	1792	1377	1640	1788	1778
Outcome Mean in 2010	0.461	0.654	0.584	0.337	0.376
	Panel B: Balanced Sample				
Sinclair * Covered	-0.033	-	0.008	-0.081**	-0.059
	(0.020)	-	(0.022)	(0.031)	(0.042)
Observations	9528	-	9528	9528	9528
Clusters	110	-	110	110	110
Municipalities	1191	-	1191	1191	1191
Outcome Mean in 2010	0.489	-	0.569	0.357	0.406
Media Market by Year FE	X	-	X	X	X
Covered by Year FE	X	-	X	X	X
Municipality FE	X	-	X	X	X
Sinclair * Controls	X	-	X	X	X

Notes: This table shows the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities, for different types of violent crimes. We regress the municipality's clearance rate for a given type of violent crime on the interaction between between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the year level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Panel A includes the full sample; Panel B restricts the sample to municipalities that experience at least one assault, one robbery, and one rape in every year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix Table XVI: Effect of Sinclair Entry on Police Violence

Dependent Variable	Had Incident Involving Intentional Use of Force		
Victim Race	Any	White	Minority
	(1)	(2)	(3)
Sinclair * Covered	-0.025	-0.015	0.002
	(0.023)	(0.024)	(0.015)
Observations	14336	14336	14336
Clusters	112	112	112
Municipalies	1792	1792	1792
Outcome Mean in 2010	0.099	0.048	0.036
Media Market by Year FE	X	X	X
Covered by Year FE	X	X	X
Municipality FE	X	X	X
Sinclair * Controls	X	X	X

Notes: This table shows the effect of Sinclair entry on the probability of experiencing an officer-involved fatality in covered municipalities relative to non-covered municipalities. Columns (1) to (3) look at all fatalities, while columns (4) to (6) focus on fatalities that are classified as involving intentional use of force (this excludes suicides and fatalities involving a vehicle pursuit). We regress an indicator variable equal to one if the municipality experienced an officer-involved fatality of a given type on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the year level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Table XVII: Effect of Sinclair Entry on the Police Spending and Employment

Dependent Variable	Police Expend. Per Capita	Judicial Expend. Per Capita	Police Employees per 1,000 People	Police Employees per 1,000 People	Police Officers per 1,000 People
	(1)	(2)	(3)	(4)	(5)
Sinclair * Covered	-0.001	-0.002	0.131	-0.044	-0.031
	(0.004)	(0.002)	(0.168)	(0.028)	(0.020)
Observations	8551	8551	9574	14335	14335
Clusters	109	109	111	112	112
Municipalies	1389	1389	1518	1792	1792
Outcome Mean in 2010	0.242	0.019	2.974	2.381	1.855
Media Market by Year FE	X	X	X	X	X
Covered by Year FE	X	X	X	X	X
Municipality FE	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X

Notes: This table shows the effect of Sinclair entry on the spending and employment of police departments of covered municipalities relative to non-covered municipalities. We regress the municipality's spending or employment measure on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. All outcome variables are winsorised at the 99% level.

Appendix Table XVIII: Effect of Sinclair Entry on the Violent Crime Clearance Rate, Robustness

Dependent Variable	Violent Crime Clearance Rate					
	Baseline	Data Cleaning		Treatment Definition		
Robustness to…		No Winsorizing	No Imputation	Drops DMAs with Divested Stations	Stations Owned and Operated by Sinclair	Group Acquis. Only
	(1)	(2)	(3)	(4)	(5)	(6)
Sinclair * Covered	-0.034**	-0.038**	-0.035**	-0.033**	-0.024*	-0.033*
	(0.016)	(0.017)	(0.017)	(0.016)	(0.014)	(0.018)
Observations	14336	14336	14336	14080	14336	13840
Clusters	112	112	112	107	112	104
Municipalities	1792	1792	1792	1760	1792	1730
Outcome Mean in 2010	0.461	0.462	0.461	0.464	0.461	0.459
Media Market by Year FE	X	X	X	X	X	X
Covered by Year FE	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X	X

Notes: This table shows the robustness of the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities. We regress the municipality's violent crime clearance rate on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Column (1) reports the baseline estimate. Column (2) does not winsorize clearance rates, while column (3) does not correct for likely erroneous observations using the methodology described in Appendix B. Column (4) drops media markets with stations that were eventually divested. Column (5) restricts treatment to media markets with stations owned and operated by Sinclair. Column (6) drops markets that were entered by Sinclair not as part of multi-station deals. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is at the year level. A media market is treated in a given year if Sinclair was present in the market in the January of that year unless otherwise specified. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix Table XIX: Effect of Sinclair on the Violent Crime Clearance Rate, No Staggered Timing

Dependent Variable	Violent Crime Clearance Rate			
Media Markets Treated in…	2012	2013	2014	2015
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.102**	0.010	-0.022	-0.028*
	(0.046)	(0.042)	(0.020)	(0.014)
Observations	9512	9216	10168	9544
Clusters	61	60	71	63
Municipalities	1189	1152	1271	1193
Outcome Mean in 2010	0.439	0.433	0.442	0.437
Media Market by Year FE	X	X	X	X
Covered by Year FE	X	X	X	X
Municipality FE	X	X	X	X
Sinclair * Controls	X	X	X	X

Notes: This table shows the robustness of the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities to eliminating variation in treatment coming from the staggered timing of Sinclair entry. We restrict the sample to media markets never exposed to Sinclair and acquired by Sinclair in the year specified in the column header, for years in which Sinclair entered more than three media markets. We regress the municipality's violent crime clearance rate on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is at the yearly level. A media market is treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix Table XX: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by 55+

Dependent Variable	Violent Crime Clearance Rate	
Share 55+	>= Median	< Median
	(1)	(2)
Sinclair * Covered	-0.069**	-0.003
	(0.028)	(0.028)
Observations	7088	7056
Clusters	98	93
Municipalities	886	882
Outcome Mean in 2010	0.461	0.460
Media Market by Year FE	X	X
Covered by Year FE	X	X
Municipality FE	X	X
Sinclair * Controls	X	X

Notes: This table shows the effect of Sinclair control on the share of crime stories that are about crime, by whether the share of the population over 55 was above the median (column (1)) or below the median (column (2)) in 2010. We regress the municipality's violent crime clearance rate on the interaction between between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix Table XXI: Effect of Sinclair Control on the Probability of Having a Local Crime Story, by Whether the Story is about a Crime Incident or an Arrest

Dependent Variable	Had Local Crime Story	
Story Related to	Crime	Arrest
	(1)	(2)
Sinclair * Covered	-0.021***	-0.002
	(0.007)	(0.002)
Observations	3143360	3143360
Clusters	113	113
Municipalities	2253	2253
Stations	325	325
Outcome Mean in 2010	0.084	0.019
Station by Week FE	X	X
Covered by Week FE	X	X
Station by Municipality FE	X	X
Sinclair * Controls	X	X

Notes: This table shows the effect of Sinclair control on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities, by whether the story is about a crime incident or is arrest-related. Arrest-related stories are stories that contain crime bigrams related to arrests or prosecutions (e.g. "police arrested" or "murder charge") or include the string "arrest". Crime-related stories are all other crime stories. We regress an indicator variable for the station reporting a local crime-related (column (1)) or arrest-related (column (2)) story about the municipality on the interaction between an indicator variable for the station being under Sinclair control and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for the station being under Sinclair control and baseline municipality characteristics, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects (equation (1)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share of population below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix A – Law Enforcement in the United States

Law enforcement in the United States is highly decentralized. Municipal police departments are the primary law enforcement agencies in incorporated municipalities. Non-incorporated areas fall instead under the responsibility of county police, state police, or sheriff's offices, depending on the state's local government statutes. Tribal departments have jurisdictions on Native-American reservations, while special jurisdiction agencies such as park or transit police provide limited policing services within the specific area. Sheriff's offices are also responsible for the functioning of courts. Sheriffs are the only law enforcement heads that can be elected as well as appointed, again depending on the state. Finally, the FBI has jurisdiction over federal crimes (i.e. crimes that violate U.S. federal legal codes or where the individual carries the criminal activity over multiple states). However, most crimes are prosecuted under state criminal statutes. Owens (2020) explains in detail the functioning of law enforcement agencies in the United States.

Appendix B – Data Cleaning

Newscast Transcripts

Separating Newscasts into News Stories. We segment each newscast into separate stories using an automated procedure based on content similarity across sentences. We begin by selecting the number of stories each newscast is composed of using texttiling (Hearst, 1997), an algorithm that divides texts into passages by identifying shifts in content based on word co-occurrence. We then divide sentences into passages using the Content Vector Segmentation methodology proposed by Alemi and Ginsparg (2015), which identifies content shifts by leveraging the representation of sentences into a vector space using word embeddings. In addition, we show that our results are robust to a simple segmentation procedure that separates the newscast into stories of 130 words, based on the fact that the average person speaks at around 130 words per minute.

Interpolation. To maximize sample size in the presence of short gaps in the data, we replace missing observations in spells shorter than two consecutive months using linear interpolation. In

particular, we linearly interpolate the number of crime stories in which a municipality is mentioned in a given week. We define our main outcome, which is an indicator variable equal to one if the municipality was mentioned in a station's crime story in a given week, based on the interpolated variable. 3% of total observations are missing in the raw data and get replaced using this procedure.

UCR Data

Identifying and cleaning record errors. UCR data have been shown to contain record errors and need extensive cleaning (Evans and Owens (2007) and Maltz and Weiss (2006)). Following the state of the art in the crime literature, we use a regression-based method to identify record errors and correct them. The method is similar to procedures used, among others, by Chalfin and McCrary (2018), Evans and Owens (2007), Ba and Rivera (2019) and Weisburst (2019), but most closely follows the one proposed by Mello (2019).

For each city, we fit the time series of crimes and clearances 2009-2017 using a local linear regression with bandwidth two. We compute the absolute value of the percent difference between actual and predicted values (adding 0.01 to the denominators to avoid dealing with zeros) and identify an observation to be a record error if the percent difference exceeds a given threshold. The threshold is computed as the 99th percentile of the distribution of percent differences for cities within a population group.[49] We substitute observations that are identified as record errors using the predicted value from the time-series regression. We follow this procedure to clean the crime and clearance series of each type of crime (property, violent, murder, assault, robbery, rape, burglary, theft, and motor vehicle theft). Overall, around 1% of observations are substituted using this procedure.

Population smoothing. To define crime rates we use a smoothed version of the population count included in the UCRs, again following the crime literature. In particular, we fit the population time series of city using a local linear regression with a bandwidth of 2 and replace the reported

[49] Mello (2019) supports this choice by noting that the percent differences tend to be more dispersed for smaller than for larger cities, perhaps because the number of crimes and arrests is increasing with city size. We follow the same size categories: 10,000-15,000, 15,000-25,000, 25,000-50,000, 50,000-100,000, 100,000-250,000, and >250,000.

population with the predicted values. This is necessary because population figures are reported yearly, but tend to jump discontinuously in census years (Chalfin and McCrary (2018)).

Sample Definition. Our starting sample is composed by municipalities with more than 10,000 people with a municipal police department (2629 municipalities). This excludes 116 municipalities, mainly located in California, that contract their contract out law enforcement services to the local sheriff's office.

To create a balanced sample, we exclude municipalities that do not continuously report crime data to the FBI 2010-2017 (235 municipalities) and do not have at least one violent and one property crime in every year (29 municipalities). This leaves us with 2365 municipalities. The empirical strategy requires restricting the sample to municipalities located in media markets included in the content data, which further drops 568 municipalities. The final sample includes 1792 municipalities.

Crime Reporting Issues. It is important to note that our findings on crime rates refer to crimes that the public reports to the police, so changes in crime reporting behavior might be potentially conflated with changes in crimes. Given that our results on crime rates are quite stable across crime types, we believe that our results are unlikely to be purely explained by a differential reporting behavior on part of the public. In particular, violent crimes such as murders and assaults are less likely to be under-reported, so we are not concerned that the null effect on violent crime rates is masking a different dynamic. Similarly, to the extent that under-reporting is less likely for crimes crimes that involve insured goods such as burglaries and vehicle thefts (as insurance companies often would not honor theft claims without a police report), we do not believe that changes in reporting behavior can explain our findings. Under-reporting is less concerning for our results on clearance rates, as the police can only investigate crimes that are known to them. While it is true that there is potential for manipulation in clearance statistics, for manipulation to fully explain the result it would need to be systematic and at quite a large scale, which we believe is implausible.

Google Trends Data

The Google Trends API normalizes the search interest between 0 and 100 for the time and location of each query. In particular, "each data point is divided by the total searches of the geography and time range it represents to compare relative popularity. [...] The resulting numbers are then scaled on a range of 0 to 100 based on a topic's proportion to all searches on all topics" (Stephens-Davidowitz, 2014). We modify the script provided by Goldsmith-Pinkham and Sojourner (2020) to query the Google trends API.

Importantly, the Google trends API limits the number of geographic locations per query to five. We ensure comparability across media markets and time by including that of the New York media market in all our queries, and normalizing search volume to the one of New York media market following Goldsmith-Pinkham and Sojourner (2020). The Google trends API censors observations that are a below an unknown threshold. Google trends data by municipality are censored with a very high frequency, which makes it impossible to construct a panel of municipalities over time.

Gallup Data

The Gallup Poll Social Series surveys are public opinion surveys that Gallup has been conducting monthly since 2001. The surveys focus on a specific topic each month (e.g. the October survey focuses on crime perceptions), but a question on what is the most important problem facing the country is always asked. Gallup interviews approximately 1,000 individuals per month, which gives us a total of almost 99,000 individual observations 2010-2017.

The Gallup data do not include municipality identifiers, but we use the reported zip codes to link observations to specific municipalities. Zip codes are missing for 1.7% of the observations, which we drop. We begin by intersecting zip codes and municipality shapefiles using ArcGIS. To avoid assigning zip codes to municipalities that they very minimally intersect with, we drop all intersections that are less than 1% of the zip code area. Zip codes are not subdivisions of municipalities and can cross municipal boundaries. If a zip code intersects one municipality only, we assign it to that municipality. If a zip code intersects multiple municipalities, we assign it to the

municipality that has the largest overlap with the zipcode.

Following this procedure, we are able to assign 51,000 respondents to specific municipalities. Of them, almost 34,000 are in municipalities included in the police behavior analysis. We aggregate the individual-level survey data at the municipality by year data, and define the outcome as an indicator variable equal to one if at least one respondent in the municipality reported crime as being the most important problem facing the nation.

Appendix C – Classifying Local Crime News

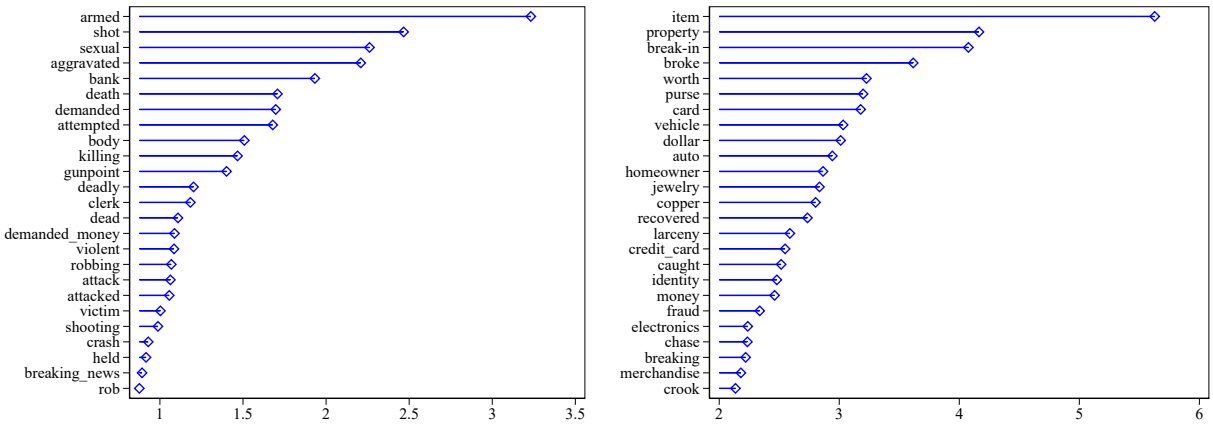
We build a classifier model that assigns a specific type of crime to each of the 464,356 local news stories about this topic in our sample. To train the model, we need a sub-sample of the stories to be labeled with the correct crime type. We create this sub-sample by performing a naive keyword search, using the following keywords:

1. Murder: MURDER, HOMICID, KILLE;
2. Assault: ASSAULT;
3. Robbery: ROBBE;
4. Rape: RAPE, SEXUAL ASSAULT;
5. Burglary: BURGLAR;
6. Theft: THIEF, STEAL, STOLE, THEFT.

We selected these terms to minimize the presence of false positives. In fact, we checked using the full vocabulary that these keywords return words and bigrams that appear to be closely related to the crime considered. The training sample is then defined to be the sample of crime stories that contain at least one of the keywords (226,503 stories). Because it is difficult to distinguish between assault and rapes and burglary and theft, we classify stories into two categories: stories about violent crimes (murder, assault, robbery, and rape) and stories about property crimes (burglary and theft). Because a story can potentially cover different types of crimes, we train separate binary models for each category.

We use this sub-sample to train a classifier model. In particular, we train a support vector machine model using stochastic gradient descent. The features that are used to predict the label are the top most frequent 25,000 words and bigrams in the full corpus. We exclude the keyword used to define the original labels from the features, as they contain significant information for the training sample, but we already know that we will not be able to leverage this information for out-of-sample predictions. The features are TF-IDF weighted. We train the model on 80% of the sample, and use the remaining 20% as a test sample to evaluate model performance. We find that the three models perform well, with F1-scores of 0.84 (violent) and 0.80 (property). Appendix C Figure I shows the most predictive feature for each category. Reassuringly, the features selected by the different models appear to intuitively link to the respective crimes. We use the models to predict the category of the remaining 237,853 stories. Using this method, we are able to assign a crime type to almost all local crime stories. Overall, 38,177 stories (8%) are classified as having both a violent and a property crime.

Appendix C Figure I: Most Predictive Features for News Type Classifier



(a) Violent

(b) Property

Notes: This figure shows the most predictive features for the classification models used to identify the content of local crime news.