Who Watches the Watchmen? Local News and Police Behavior in the United States*

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Abstract

Are the police responsive to media coverage of crime? We address the question by studying how a decline in news coverage of local crime, induced by acquisitions of TV stations by the Sinclair Broadcast Group, affects municipal police departments in the United States. In particular, we implement a triple differences-in-differences design that combines the staggered timing of acquisitions with cross-sectional variation in whether municipalities are covered by the news at baseline, a proxy for exposure to the shock. Using a unique dataset of almost 300,000 newscasts, we show that stations that are acquired by Sinclair decrease their coverage of local crime. This matters for policing: after Sinclair enters a media market, covered municipalities experience 10% lower violent crime clearance rates relative to non-covered municipalities. Finally, we provide suggestive evidence that the effect is consistent with a decrease in the salience of crime in the public opinion.

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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)). In recent years, high-profile cases of police misconduct have cast doubts on the extent to which officers, who are protected by civil service laws and strong union contracts, are responsive to the constituencies they serve. In this paper, we explore a fundamental force that might play a role in this respect: local media.

Studying the effect of local media, and more specifically local news, on police is a first order question. By providing information to the public, the news has the potential to influence the behavior of bureaucrats and politicians (see, among others, Lim et al. (2015); Snyder Jr and Strömberg (2010); Martin and Yurukoglu (2017)). This is especially true at the local level, where the news reaches a large audience and garners high levels of trust (Mitchell et al. (2016)). Importantly, local news often has a clear crime focus. This, combined with the highly decentralized nature of law enforcement in the United States, makes local news uniquely positioned to influence the behavior of police officers.

In this paper, we study how changes in TV news coverage of crime in a municipality impact the behavior of its police officers, as proxied by clearance rates. More precisely, we exploit the fact that in the last ten years the local TV market in the United States has seen an increase in concentration driven by large broadcast groups acquiring high numbers of local TV stations, and that acquisitions are likely to affect content (Stahl (2016)). We focus in particular on the most active player in this sense: the Sinclair Broadcast Group.

Sinclair acquisitions affect content in two ways. First, Sinclair tends to reduce local news coverage in favor of a national focus (Martin and McCrain (2019)). This is the effect we are interested in estimating, as it allows us to identify the effect of a change in the TV news coverage of a municipality's crime. However, Sinclair – a right-leaning media group – is also likely to introduce more conservative content overall. Our empirical strategy exploits the staggered timing of Sinclair entry across media markets. To disentangle the two effects on content highlighted above, we combine this with cross-sectional variation across municipalities in exposure to the decline in news coverage of crime in a triple differences-in-differences design.

Our proxy for exposure is the baseline probability that a municipality appears in the news. The intuition is that the decline in coverage driven by acquisitions should only matter for municipalities that are likely to appear in the news in the first place. Instead, municipalities that are never in the news should not experience a change in local coverage, and therefore function as our control group. The presence of this control group has the additional advantage of allowing us to control for endogeneity in Sinclair entry.

We begin by documenting how Sinclair acquisitions affect local TV content using a novel dataset that includes 9.5 million stories as part of 300,000 newscasts. This dataset allows us to track 323 local TV stations weekly from 2010 to 2017. We use the content data to study how crime coverage of municipalities within a media market is affected by Sinclair acquisitions. We identify crime stories using a pattern-based sequence-classification method that classifies 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. We assign stories to municipalities based on whether the name of the municipality is mentioned in the story.

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 2.2 percentage points less likely to be mentioned in a crime story after a station gets acquired by Sinclair compared to non-covered municipalities.[1] The effect is significant at the 1% level and economically important, corresponding to almost 25% of the outcome mean in 2010. Examining the timing of content changes, we find a reduction in local crime coverage immediately in the year following the acquisition, with the effect increasing over time. The change in coverage is the result of an editorial decision by Sinclair, partly explained by the centralization of news production. Consistent with this, other stations in the same media market do not change their crime coverage after Sinclair entry.

We then look at how the change in news coverage of local crime impacts clearance rates. We find that after Sinclair enters a media market, covered municipalities experience 4.5 percentage points lower violent crime clearance rates with respect to non-covered municipalities. The effect is precisely estimated, and corresponds to 10% of the baseline mean. 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.

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 75% of local crime stories are about a violent crime and only 17% are about a property crime, a difference which is even starker if we consider that property crimes are more common by orders of magnitude. In this sense, the content data are one

[1] We define covered (non-covered) municipalities as municipalities that are mentioned in the news more (less) than the median municipality in 2010 using our content data.

of the most novel contributions of this paper: they provide the ability to characterize in detail the content shock, and therefore allow us to precisely map specific content changes into the real-word outcomes we are interested in studying.

The effect on violent crime clearance rates 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, which 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 in this case.

We propose the following explanation for our results. When stories about a municipality's violent crimes are less frequent, crime becomes less salient in the eyes of local citizens. As a result, the police find themselves operating in a political environment where there is less pressure to tackle the problem of violent crime. This might create incentives for the police to reallocate their resources away from clearing these crimes in favor of other policing activities. Two 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 salience of these issues decreases. Second, 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 exactly 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 local 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 appears to be 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.

A long tradition in the economics of media literature shows that the media influences the behavior of public officials, especially by performing an important monitoring function (Ferraz and Finan (2011); Lim et al. (2015); Snyder Jr and Strömberg (2010). In addition, media content has been shown to impact individuals' perceptions and beliefs (Mastrorocco and Minale (2018)) and voting (DellaVigna and Kaplan (2007); Martin and Yurukoglu (2017); Durante et al. (2019)). We contribute to this literature in two ways. First, our extensive content data spanning multiple years allow us to precisely document location-specific content changes and their timing following acquisitions. As a result we can exactly map out how content influences policy. Second, in the discussion of the mechanisms, we provide evidence on how media-induced changes in perceptions impact 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 entry 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 how local TV news content might influence crime perceptions and therefore police behavior.

In addition, our finding that Sinclair acquisitions affect local coverage shows that, unlike in other media markets (Gentzkow and Shapiro (2010)), ownership of local TV stations matters for content. This highlights how important it is to understand the consequences of concentrated media ownership (Stahl (2016)). Consistent with existing work in this area (Martin and McCrain, 2019), we confirm that large broadcast group acquisitions lead to a crowding out of local news in favor of national stories. We add to this paper by investigating the consequence of this highly policy-relevant trend for the behavior of public officials.

Finally, we contribute to the growing literature aimed at understanding the functioning of police departments (see, among others, Ba, 2018; Dharmapala et al., 2019; Facchini et al., 2020; Harvey and Mattia, 2019; Goldstein et al., 2020; Mas, 2006; Mastrobuoni, Forthcoming; McCrary, 2007; Stashko, 2020). Ours is one of the first studies to provide systematic causal evidence on how media content influences the behavior of police officers. It is interesting to contrast our finding that a reduction in news coverage of a municipality's crime decreases clearance races 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. In Section 7 we present the robustness checks and in Section 8 potential mechanisms. Finally, we conclude in Section 9.

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 (Nielsen (2019)).[2] As a result, media markets are non-overlapping geographies. There are 210 media markets in the United States.

Each media market contains multiple stations. We focus on stations that are affiliated to one of the so-called big-four networks: ABC, CBS, FOX, and NBC. Networks are publishers that distribute branded content.[3] Affiliated stations, although under separate ownership, carry the television lineup offered by the network while also producing original content. We focus on big-four affiliates as they tend to take up most of the viewership in a media market and be the ones producing local newscasts: in 2017, 85% of local TV stations that did so belonged 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 reported 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 other 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*b*). 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*a*).

Second, the decline has been concentrated in younger demographics, while the core audience of local TV news – those above 50, who constitute 73% of viewership – has not been been affected

[2]Counties can be split across media markets, but this happens rarely in practice. As noted by Moskowitz (Forthcoming), 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.

[3]With few exceptions, each network has a single affiliate by media market.

(Wenger and Papper, 2018*a*). 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).[4]

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 1.8 million local stories in our content data.[5] In Figure I Panel (b), we show the average topic share across all local news stories. Again, the most covered topic is crime (with a topic share of 26%), followed by events (23%), and politics (21%). Weather and sports also appear in local stories, although to a lesser extent. 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 shows the number of local TV stations under Sinclair control monthly from 2010 to 2017. Sinclair expanded from 33 stations in January 2010 to 117 stations in December 2017, which corresponds to about 14% of all big-four affiliates. As shown in Figure III, there have been acquisitions in media markets across the United States, although Sinclair was particularly active in medium-sized media markets.

With respect to other broadcast groups, Sinclair holds a right-leaning political orientation (see, among others, Kolhatkar (2018), Miho (2020), and Fahri (2017)) 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,

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

[5] Appendix Figure I shows word clouds with the 50 words that have the highest weight for the five topics, which can be easily identified to be related to crime, events (also possibly a filler topic), politics, weather, and sports.

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 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 agencies 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).[6] Information on the market served 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.[7] 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

[6] 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.

[7] 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.

a local marketing agreement.[8] We use Sinclair acquisitions to refer to Sinclair control over the station's content determined by any of these instances, unless otherwise specified.[9]

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 covers 323 (39%) stations in 112 media markets from 2010 to 2017, for a total of 291,323 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 9.5m 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:

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.[10] 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,

[8] 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 of the station.

[9] 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)).

[10] 75% of local crime stories mention a single media market municipality, 20% mention two municipalities, and the remaining 5% mention three or more.

as 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 filter out uncommonly used bigrams that might showq 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 topic model on the same stories. First, going back to Figure I, we can 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 (26%). Second, Appendix Figure III shows that the 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 in an evening newscast of a given station in a given week.

Our starting sample is composed by municipalities with more than 10,000 people located in media markets for which we have content data. We restrict the sample to municipality-station pairs continuously present in the data from 2010 to 2017. In order to maximize sample size in the presence of short gaps, 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 7. In addition, we drop municipalities whose name never appears in the content data (14 municipalities). The resulting sample includes 323 stations and 2201 municipalities in 112 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.[11] 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, and clearance rates, defined as number of cleared crimes over total number of crimes.[12]

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 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 (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.

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 analysis. Our final sample includes 1752 municipalities (see Appendix B for more details).[13]

Municipality Characteristics. Municipality characteristics are from the 2006-2010 American Community Survey (Manson et al., 2019). We construct the Republican vote share in the 2008

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

[12] A crime is considered cleared by arrest 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.

[13] The sample for the content analysis includes 476 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 7 that this does not affect our results.

presidential election aggregating precinct level returns at the municipal level. Precinct level returns are from the Harvard Election Data archive (Ansolabehere et al., 2014). When precinct level returns 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.[14] We use the data to define an indicator variable equal to one if the police department was involved in at least one death 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 2010-2017, 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).

3.1 Descriptive Statistics

Appendix Table II columns (1) to (5) show descriptive statistics for the main variables considered in the analysis. Panel A shows that the average municipality was mentioned in 27% of the newscasts in 2010, and appeared with a local crime story in 11% 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.

[14] 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. The database can be accessed here.

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 (6) to (10) 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 included in the analysis appear to be highly comparable to other municipalities with more than 10,000 people, as is confirmed by the p-values reported in column (11).

4 Empirical Strategy

The objective of the 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 crime that is exogenous to clearance rates. We address this issue by exploiting a supply driven change in local TV news coverage. Specifically, we exploit a change in content driven by acquisitions of local TV stations by a large broadcast group, Sinclair.

Figure II and Figure III show 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 tend to increase their national focus to the detriment of local coverage (*effect #1*). This is the change in content we are interested in identifying. But in addition to this, because Sinclair is a conservative broadcast group, acquisitions are likely to also affect the overall content of the stations (*effect #2*). In particular, Sinclair might transmit more conservative content and affect the way in which crime and police are presented in the news. 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 (Hill, 2015).

To disentangle the two, we introduce a control group that is exposed to the overall content change related to Sinclair acquisitions, but does not experience a change in its probability of being mentioned in the news with a crime story: same media market municipalities that are not likely to appear in the news in the first place. The presence of this control group allows us to control for media market trends and thus net *effect #2* out. More precisely, we estimate a triple differences-in-differences specification that combines variation from the staggered timing of Sinclair acquisitions and within media market variation in whether municipalities are likely to be covered in the news at baseline or not.[15] The identification assumption is that covered and non-covered municipalities

[15]Nonetheless, we also always estimate separate differences-in-differences designs for covered and non-covered

are on parallel trends.

The intuition is the following. If Sinclair acquisitions decrease local news coverage, municipalities often in the news at baseline would bear the brunt of the decline. Instead, municipalities that are never in the news in the first place are not going to 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 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 therefore use non-covered municipalities as a control group that experiences the same media market shocks as covered municipalities, but are not be directly affected by the decline in local coverage.

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 1 if the municipality is in the news more than the median municipality in 2010, and 0 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.

Appendix Figure VII shows that 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.

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.

Finally, it is important to note that the presence of a within-media market control group has the additional advantage of allowing us to control for demographic or economic trends that might be inducing Sinclair to enter some media markets before others. While Appendix Table III shows no change in a media market's socio-economic characteristics following Sinclair entry, the fact that our design allows us to control for observable and unobservable trends strengthens the credibility of the results.[16]

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 1 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.[17]

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 interest is the municipality-station pair. We estimate the regression on a municipality-station pair by week panel that only includes pairs where the station and the municipality belong to the same media market. The sample is restricted to 323 stations continuously reporting content data. Standard errors are clustered at the media market level.

[16] Even if we control for media market level trends in observable and unobservable characteristics, we might still worry of Sinclair acquisitions being driven by differential trends in covered relative to non-covered municipalities. This is unlikely to explain our findings as the result is unchanged if we focus on instances when Sinclair acquired stations by buying a smaller broadcast group. Given that in such instances stations come as a bundle, acquisitions are unlikely to be driven by specific media market conditions.

[17] In particular, X_{m2010} includes the following variables: population, share male, share male between 15 and 30, share white, share black, share over 55, share Hispanic, share with 2 years of college, median income, share of population below the poverty rate, share unemployed, municipality area, and Republican vote share in the 2008 presidential election. Population, median income, and area are in logs.

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 our assigment of stories to municipalities.[18]

We provide evidence supporting the parallel trend 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:

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

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

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 control 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 story about a covered municipality by 2.2 percentage points compared to a municipality that was 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

[18] 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 these differences using the municipality by station fixed effects as long as they are 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.

in size but similar in magnitude, corresponding to 29% 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 was 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 supporting evidence by estimating an event-study specification that allows the effect of Sinclair control to vary by time since treatment. Figure IV 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 becomes larger over time, almost tripling 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 of Sinclair control but 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 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 effect is indeed statistically different (p-value = 0.017).

Taken together, the 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-in-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 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)). 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 control lowers the probability that a station reports a story about covered municipalities with respect to non-covered municipalities by 3.9 percentage points or 16% of the baseline mean (column (1)). However, the effect is much larger in magnitude for crime (25% of the baseline mean) compared to non-crime stories more generally (11%). Overall, 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 the 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. Importantly, 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 acquisition.[19]

Heterogeneity by Political Leaning of the Municipality. Since Sinclair is a conservative media outlet, we might worry that the decline in coverage could be influenced by political considerations. To explore this possibility, in Appendix Table VIII, we estimate the main specification separately

[19] 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.

for municipalities with different political leanings. 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. The coefficient is the same across the two subsamples (p-value=0.956), which suggests a limited scope for strategic coverage decisions based on the political leaning of the municipalities.[20]

6 Effect of Sinclair Control on Police Behavior

6.1 How Should the Decline in Coverage of Local Crime Influence Police Behavior?

In the previous section, we documented that when a local TV station is acquired by Sinclair, covered municipalities are 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. In particular, we are interested in understanding the effect of the decline in coverage of local crime on police behavior.

We study in particular 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 response time to crime calls have a negative effect on clearances. After the immediate aftermath of a crime, the involvement of specialized detective squads also increases the probability of a crime getting cleared (Cook et al. (2019)). 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 the police department are affected by news coverage.

Not all crime types are equally likely to be reported in local news. This is important to the extent that we should expect arrest rates of different crimes to respond differently, depending on how important local news coverage is for them. We explore this heterogeneity in our content data by developing 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 V 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: 75% of local crime stories are about a violent crime, while only 17% of crimes stories are about a property crime.

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

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. As shown in Figure V Panel (b), where 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, each violent crime is covered in approximately 0.145 stories in our content data. Property crimes, at 0.003 stories per offense, have a negligible probability of being covered in our content data.[21] This evidence guides our analysis of police behavior. Given that property crimes appear to be significantly less important than violent crimes for local news, we expect the decline in local crime coverage to be less relevant for them: the main outcome of interest for our analysis is the violent crime clearance rate.[22]

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 municipality characteristics according to the 2010 Census, 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.[23] The regression is estimated on a yearly balanced panel 2010-2017 including 1752 municipalities. Standard errors are clustered at the media market level.

The media market by year fixed effects (δ_{dt}) control non-parametrically for any non municipality-specific change in content that is associated with Sinclair entering a media market, in particular

[21] 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.

[22] We use our classifier model to also estimate the direct effect of Sinclair acquisitions on local coverage of violent and property crimes. Appendix Table IX shows that after Sinclair acquires a station, covered municipalities are 1.8 percentage points (27% of the baseline mean) less likely to appear in the news with a story about a violent crime and 0.4 percentage points (30% of the baseline mean) less likely to appear in the news with a story about a property crime. The effect is almost 4.5 times larger for violent crimes than it is for property crimes, although the decline in coverage is proportionally similar across crime type because of the substantially lower probability of property crimes appearing in the news in the first place. As a result, we expect the decline in coverage to be less consequential for property crimes rather than for violent crimes, which confirms the interpretation proposed in the main text.

[23] 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.

increased conservative slant. In addition, they 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.[24]

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 a 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. Nonetheless, we ensure that the results are robust to this decision in Section 7.

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:

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

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. As before, column (1) reports the estimates from a specification that only controls for the fixed effects, while column (2) additionally includes the interaction between Sinclair control and socio-economic characteristics of the municipality at baseline (equation (4)).

After Sinclair enters a media market, the violent crime clearance rate is 4.5 percentage points lower in covered than in non-covered municipalities. The effect is significant at the 1% level, and it is sizable in economic magnitude, corresponding to 10% of the baseline mean. Restricting the sample to municipalities with fewer than 50,000 people does not affect the result (column (3)), and neither does controlling for crime rates and population, two factors that we might worry influence

[24] 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-in-differences specification, redundant.

violent crime clearance rates but that we do not include in the main specification because they are potentially endogenous to the treatment (column (4)). We interpret this result as suggesting that the decline in the probability of local crime appearing in the news induces police departments to decrease the resources that are allocated to investigating violent crimes.[25]

Event Study. We provide evidence supporting the identifying assumption of parallel trends between covered and non-covered municipalities by estimating an event-study specification that allows the effect of Sinclair entry in a media market to vary by time since treatment. Figure VI reports the β_y and γ_y coefficient estimates from equation (5), together with 95% confidence intervals. The figure shows no difference between covered and non-covered municipalities in the four years leading up to Sinclair's entrance into the media market.[26]

The effect is fully realized in the first year in which Sinclair is present in the media market, but the gap between covered and non-covered municipalities seems to be shrinking after that. This is consistent with viewers learning about the status of crime and clearances over time, either from their personal experience or from other news sources. To the extent that the change in content is driven by a supply-side shock that might be opaque to viewers, it is not surprising to see a short-run effect that tapers: it takes time for viewers to learn about Sinclair's coverage changes and adjust for them accordingly.

Heterogeneity by Type of Crime and Municipal Characteristics. Not all violent crimes are the same, and we might wonder whether the effect of Sinclair entry on clearance rates is heterogeneous for different crimes. In Appendix Table X, we show that the decline in the violent crime clearance rate appears to be particularly driven by the clearance rates of robberies and rapes. Another important source of heterogeneity arises from municipal characteristics. In Appendix Figure XI we find that the main effect on the violent crime clearance rate is quite consistent across different municipalities.

Property Crime Clearance Rates. As discussed in Section 6.1, given that crime news has a clear violent crime focus, property crime clearance rates should be minimally impacted by the change

[25] 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. According to theories of "de-policing" (Owens (2019)), it is possible that decreasing arrest rates might be socially optimal.

[26] 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.

in content. Table III shows that the property crime clearance rate is not differentially affected by Sinclair acquisitions in covered as opposed to non-covered municipalities. This is consistent with the change in clearance rates being specifically related to the change in content, and not to some other factors affecting clearance rates across the board.[27]

Crime Rates. One 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. Data in Appendix Table XI suggest 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)). Panel A reports the effect on crime rates, while Panel B defines the outcome as an indicator variable equal to one if the municipality reported at least one crime of the specified type. Reassuringly, we do not find any 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 (2.1%) is too small to explain the decline in the violent crime clearance rate.

Appendix Table XII looks instead at property crime rates. Column (1) shows that Sinclair entry is associated with 5.4% higher property crime rate in covered municipalities relative to non-covered ones. The effect is significant at the 1% level. This result could be explained by an incapacitation effect due to the lower clearance rates, or criminals factoring in lower deterrence for the same reason. In addition, the positive effect on property crime rates might be due to a reduction in overall police performance in treated municipalities, which would be consistent with a reduction in monitoring and scrutiny induced by lower crime news coverage. An alternative explanation is 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 female and above 55, we believe that this explanation has a limited role in this setting.[28]

[27] 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.

[28] 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 it would need to have taken place at a systematic and quite large scale, which we believe is implausible.

Differences-in-Differences Decomposition. Appendix Table XIII reports coefficient estimates from estimating 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)). After Sinclair enters a media market, non-covered municipalities experience an increase in their violent crime clearance rate. This is consistent with Sinclair's conservative content having a direct effect on police behavior, which is not surprising since Sinclair's conservative messaging might build support for tough-on-crime policies.[29]

Instead, covered municipalities do not experience a change in the violent crime clearance rate. As we discussed in Section 4, non-covered municipalities provide us with the counterfactual of how clearance rates would have evolved in covered municipalities following a Sinclair acquisition, had there been no decrease in their probability of appearing in the news with a local crime story. If the local news coverage of covered municipalities' crime had not changed, their violent clearance rate would have increased after Sinclair entry. Instead, the decline in crime coverage that is specific to covered municipalities fully undoes the effect.

6.4 Additional Findings

Police Violence. We might wonder whether the reduced news coverage of local crime also affects the probability that officers are involved in episodes of police violence. In Appendix Table XIV we address this question using data from Fatal Encounters. We find limited 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 effect to be explained by covered municipalities having lower police spending as opposed to non-covered municipalities after Sinclair entry. Appendix Table XV shows that this is not the case: after Sinclair entry, covered and non-covered municipalities have similar police expenditures and employment per capita.

[29]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, 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)).

7 Robustness Checks

In this section, we show that our results are robust to a number of potential concerns.We first show robustness to data cleaning procedures, sample restrictions, and treatment definitions. We conclude by discussing potential concerns related to heterogeneous effects in two-way fixed-effects designs.

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

Appendix Table XVI reports all robustness checks related to the coverage of local crime analysis. Column (1) reports the baseline estimate 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 is not affected by identifying 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 tokens (column (5)) leaves the results unchanged. Similarly, restricting the sample to the same set of municipalities included in the analysis of clearance rates does not impact the results (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 main result is unchanged if we only include markets that Sinclair entered as part of a group acquisition (column (9)), where endogeneous entry is less likely to be a concern.

7.2 Robustness of the Effect of Sinclair on Police Behavior

Appendix Table XVII reports all robustness checks related to violent crime clearance rates. Again, 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 estimate. 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 result is robust to dropping media markets where Sinclair divested a station (column (4)), considering only media markets whether Sinclair directly owns and operates a station (column (5)), or defining partially treated years as treated (column (6)). Finally, we consider the possibility that Sinclair acquisitions might correlate with trends in covered relative to non-covered municipalities. In column (7), we shown that this in 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.

7.3 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 triple differences-in-differences estimates.[30] Appendix Table XVIII reports results using the robust estimator proposed in their paper, while the corresponding event study graphs are shown in Appendix Figure XII. 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-in-differences as well.

[30] Appendix Table IV and Appendix Table XII show that the triple differences-in-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.

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.[31] 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 2015 only.

8 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 as a consequence the police find themselves operating in a political environment where there is less pressure to tackle the problem of violent crime. This might create incentives for the police to reallocate their resources away from clearing these crimes in favor of other policing activities. In this section, we provide two 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 and police activities changes after Sinclair acquisitions. Ideally, we would want to test the effect of Sinclair on crime and police perceptions directly. The main challenge to doing so is finding highly localized but nationally representative data on perceptions over time. We address this issue by using using data on Google searches as a proxy for overall interest in the topic.

In particular, we collect data on monthly Google 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 run a differences-in-differences model exploiting the staggered entry of Sinclair across media markets. The outcome variable is the monthly volume of searches, and it is expressed in logarithm. The sample is restricted to media markets for which the volume searches for crime and police are always available.

Table IV reports the findings of our analysis. The estimates show that when Sinclair enters a media

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

market, the volume of monthly searches containing the keywords crime and police decreases by 4%. The effect is not explained by an generalized decline in searches, as shown by the placebo regressions looking at monthly searches for 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.

Political Feedback. Perceptions become reality when it is election time. If the change in local news coverage makes crime less salient in the public opinion, politicians should react to it. We believe this feedback mechanism to be particularly credible in the setting given that the individuals whose opinion is likely to be influenced by the treatment are exactly the ones whose opinions are likely to matter for local politics: those over 55.[32,33]

Appendix Figure XIII 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, Table V 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 = 0.166), 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 the direct media monitoring of the police. If police officers anticipate a low probability of being covered in

[32] 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), Harris 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).

[33] 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 media shaped public opinion about the quality of public safety".

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 crime bigrams related to arrests or prosecutions (e.g. "police arrested" or "murder charge") or include the string "arrest" (this would also capture words such as "arrested", "arrests"); all other stories are about crime.

In Table VI, 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, although we cannot exclude the possibility that police officers are updating their overall probability of being the subject of reporting based on the overall 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 be negatively affected after high-profile cases of police misconduct (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.[34]

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 is limited data on the importance of tips for solving crimes, but our understanding is that the phenomenon is quantitatively limited. A piece of evidence that supports this interpretation comes from the evaluation of a tip solicitation program, Crimestoppers, that uses data for the year 2000 in the United Kingdom. According to this rare evaluation of the program, only 11% of calls resulted in actionable intelligence; in addition, most calls are for minor offenses such as drug crimes that are not included in our analysis, and overall only "30 calls were received which led to an arrest or change in relation to murder, 25 in relation to attempted murder, and 28 in relation to sexual assault" (Gresham et al., 2003). Overall, while we cannot exclude this alternative story, we believe that it would only be able to explain a small fraction of the effect.

[34] 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.

9 Conclusion

In this paper, we study the effect of a shock in local news content 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-in-differences design.

First, we document that when a station is acquired by Sinclair, covered municipalities experience a decline in the probability of appearing in the news with a crime story with respect to non-covered municipalities. We do so by exploiting a unique dataset of transcripts of local TV newscasts of 323 stations from 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 answer this question by studying clearance rates. 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 1% level and corresponds to a decrease to 10% 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 is less salient in the news, police officers alter their effort allocation away from clearing violent crimes in favor of other police activities, because of an overall decrease in crime salience. To support this interpretation, we provide evidence that, when Sinclair enters a media market, the salience of crime becomes lower. Moreover, we document that our results are stronger in municipalities with a higher share of individuals above 55 years old, which we show are both those more exposed to local TV news and an important interest group for local politics.

We show 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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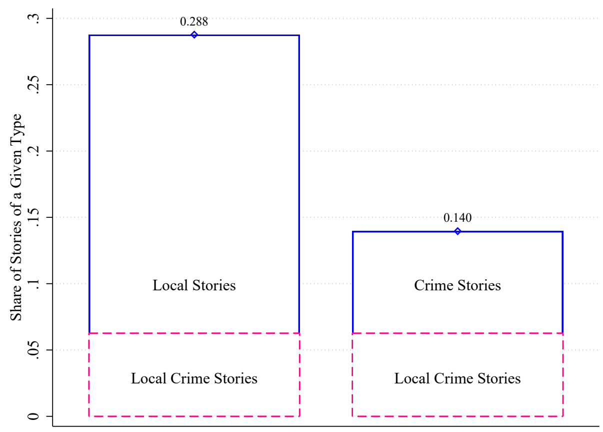
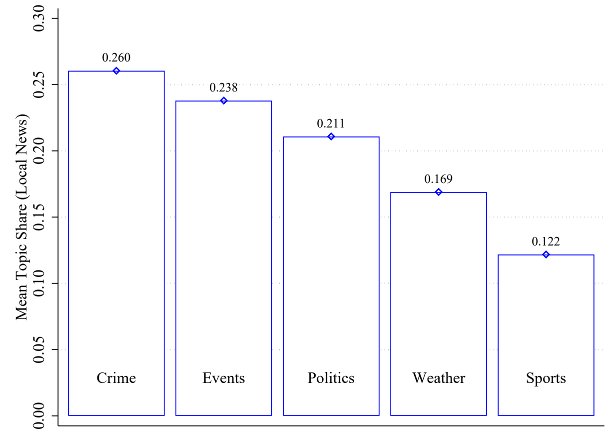
Figures

Figure I: Local TV News Content



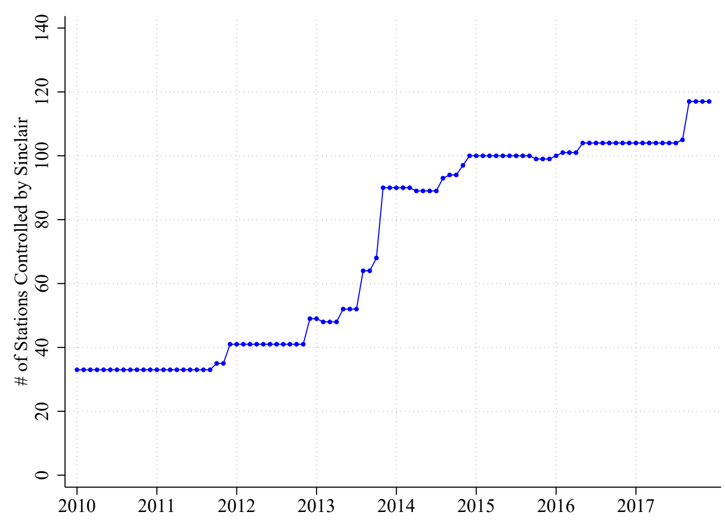
(a) Types of News Stories



(b) Local News Topics

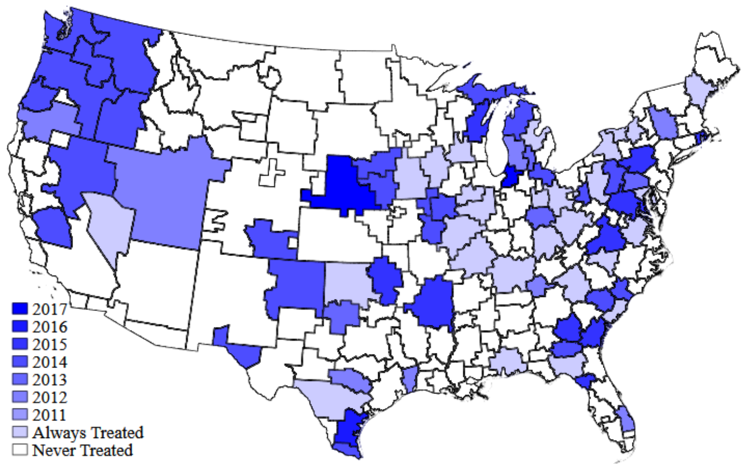
Notes: This figure describes local TV news content. Panel (a) shows the share of stories that are local, that are about crime, and both local and about crime. A story is local if it mentions at least one of the municipalities with more than 10,000 people in the media market. A story is about crime if 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.

Figure II: Number of Stations Controlled by Sinclair 2010-2017



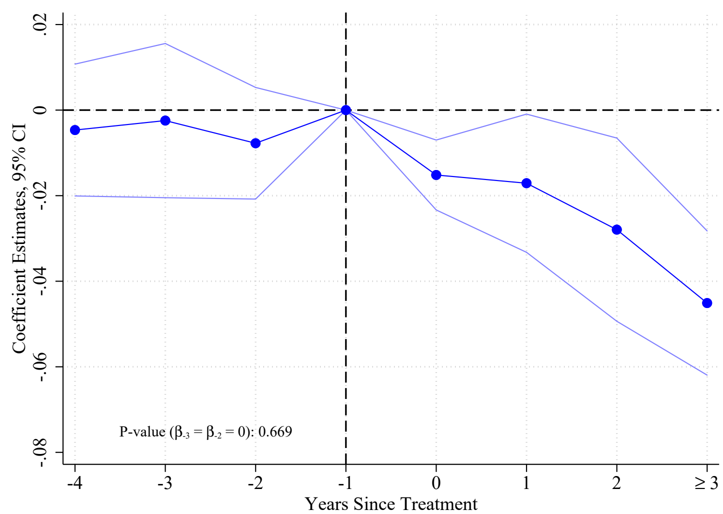
Notes: This figure shows the number of big-four affiliate stations controlled by Sinclair in each month from January 2010 to December 2017. A station is considered controlled by Sinclair if it is owned and operated by the Sinclair Broadcast Group, if it is owned and operated by Cunningham Broadcasting, or if Sinclair controls programming through a local marketing agreement.

Figure III: Map of Media Markets Experiencing Sinclair Entry 2010-2017



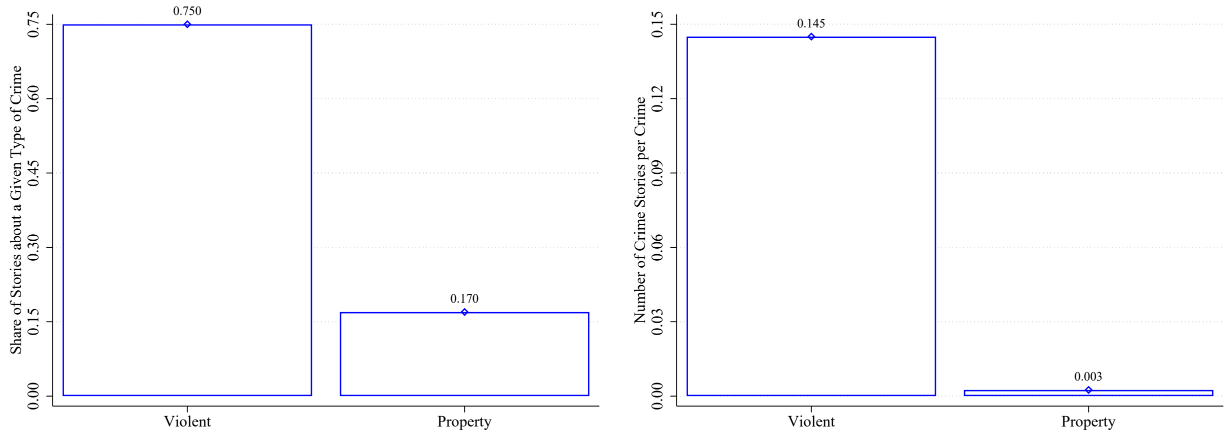
Notes: This map shows year of Sinclair entry across media markets in the United States. Darker colors correspond to later entry. Never treated are media markets that never experience Sinclair entry; always treated are media markets that have at least one station controlled by Sinclair at the beginning of the period of interest (January 2010). There were no additional stations that were acquired in 2010.

Figure IV: 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.

Figure V: Local Crime News of Violent and Property Crimes

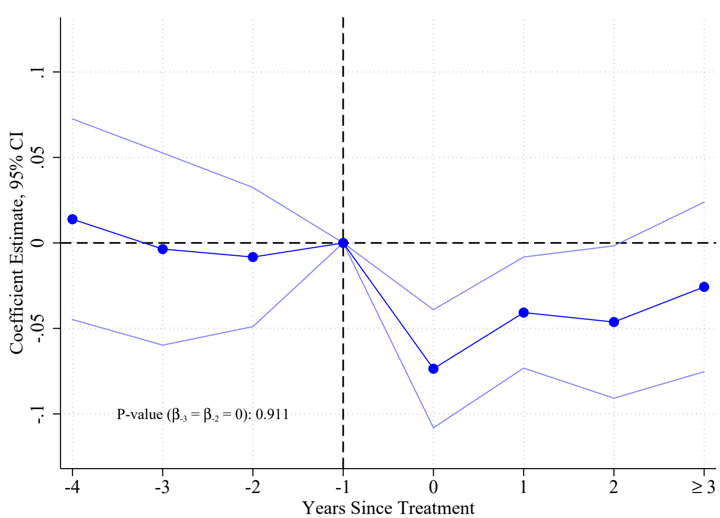


(a) Share of Crime Stories

(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. 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.

Figure VI: 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. 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.

Tables

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***	-0.022***	-0.014***	-0.023***
	(0.007)	(0.006)	(0.005)	(0.006)
Non-Sinclair Stations in Sinclair Media Market * Covered				-0.005
				(0.005)
Observations	3065194	3065194	2334112	3065194
Clusters	112	112	109	112
Municipalities	2201	2201	1673	2201
Stations	323	323	319	323
Outcome Mean in 2010	0.089	0.089	0.048	0.089
P-value Sinclair = Other				.017
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 male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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.

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

Dependent Variable	Violent Crime Clearance Rate			
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.046***	-0.045***	-0.043**	-0.043**
	(0.016)	(0.017)	(0.020)	(0.017)
Observations	14016	14016	10384	14016
Clusters	111	111	107	111
Municipalies	1752	1752	1298	1752
Outcome Mean in 2010	0.463	0.463	0.469	0.463
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 (4)). The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. Crimes rates are crimes per 1,000 people under an inverse hyperbolic sine transformation. Both clearance rates and crime rates are winsorized at the 99% level.

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

Dependent Variable	Property Crime Clearance Rate	By Type of Crime: Burglary	Theft	Motor Vehicle Theft
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.004	-0.013	-0.004	-0.006
	(0.009)	(0.009)	(0.011)	(0.015)
Observations	14016	14013	14009	13953
Clusters	111	111	111	111
Municipalities	1752	1752	1752	1752
Outcome Mean in 2010	0.191	0.131	0.211	0.172
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 (4)). The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. 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.

Table IV: Effect of Sinclair Entry on Salience of Crime and Police

Dependent Variable	Monthly Search Volume			
Keyword	Crime	Police	Weather	Youtube
	(1)	(2)	(3)	(4)
Sinclair	-0.040***	-0.040***	-0.009	-0.011
	(0.014)	(0.014)	(0.016)	(0.009)
Observations	14880	14880	14880	14880
Clusters	155	155	155	155
Outcome Mean in 2010	3.624	3.920	3.872	4.284
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.

Table V: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by Share of the Population above 55

Dependent Variable	Violent Crime Clearance Rate	
Sub-Sample	Share 55+ >= Median	Share 55+ < Median
	(1)	(2)
Sinclair * Covered	-0.079***	-0.012
	(0.030)	(0.029)
Observations	6920	6904
Clusters	97	92
Municipalities	865	863
Outcome Mean in 2010	0.462	0.464
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 (4)). The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. 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.

Table VI: 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	
Type of Story	Crime-Related	Arrest-Related
	(1)	(2)
Sinclair * Covered	-0.022***	-0.003
	(0.006)	(0.002)
Observations	3065194	3065194
Clusters	112	112
Municipalities	2201	2201
Stations	323	323
Outcome Mean in 2010	0.080	0.019
Station by Week FE	X	X
Covered by Week FE	X	X
Station by Municipality FE	X	X
Sinclair * Controls	X	X

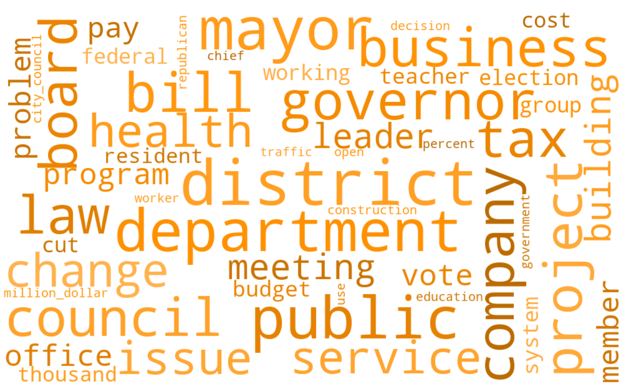
Notes: This table shows the effect of Sinclair control on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities, by whether the story is about a crime incident or is arrest-related. Arrest-related stories are stories that contain 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 male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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.

Appendix Figures

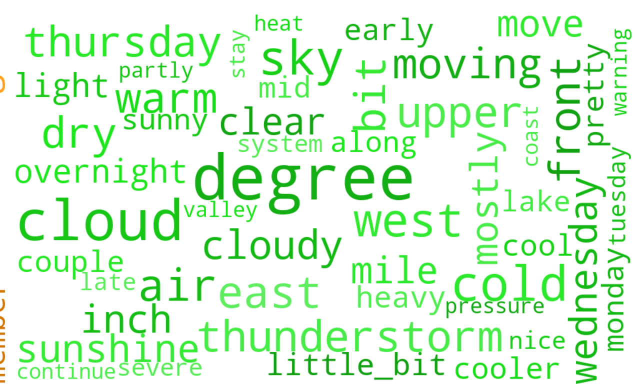
Appendix Figure I: Local News Topics

(a) Crime

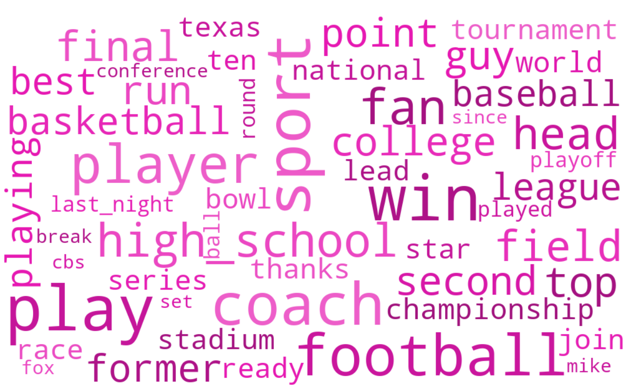
(b) Events



(c) Politics



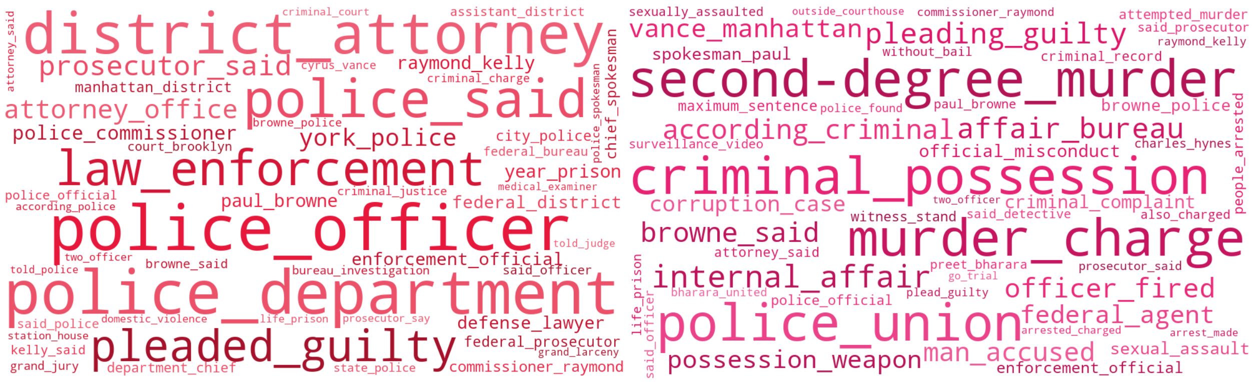
(d) Weather



(e) Sports

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

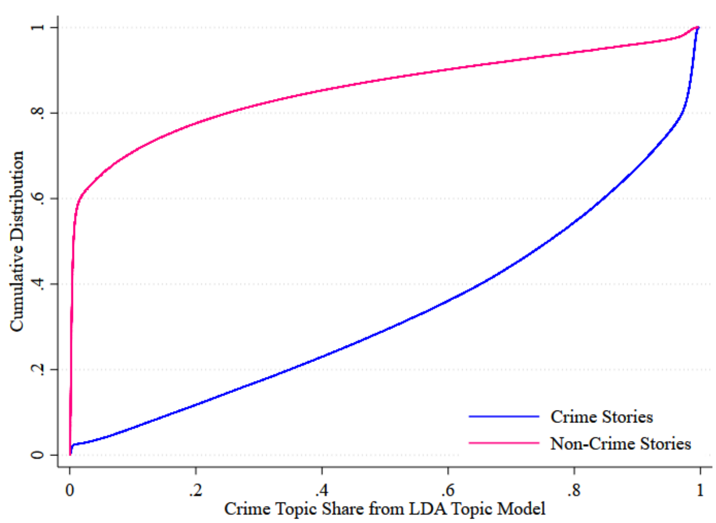


(a) Frequency

(b) Relatively Frequency

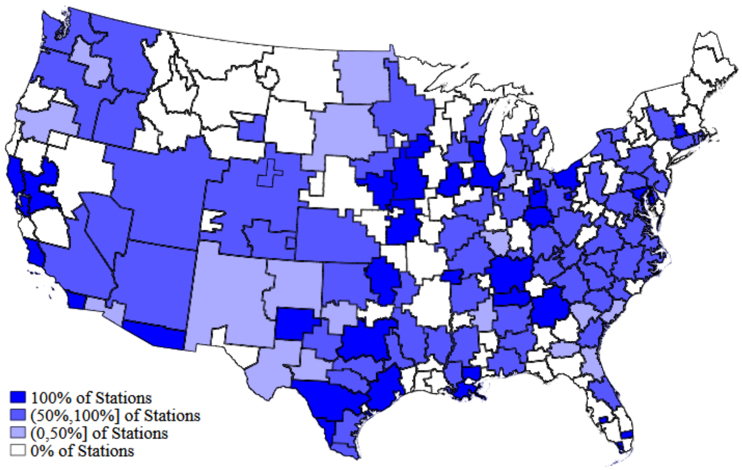
Notes: This figure shows word clouds of the top 50 bigrams that we use to identify crime stories by frequency (Panel (a)) and by relative frequency (Panel (b)). The size of the words is proportional to their absolute and relative frequency.

Appendix Figure III: Classification of Local Stories: Validation



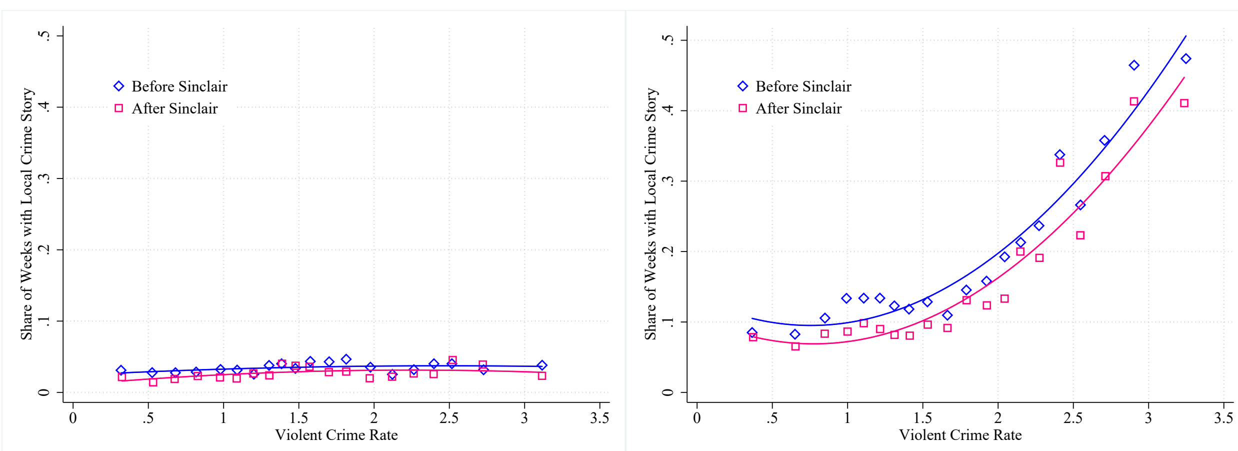
Notes: This figure shows the cumulative distribution of the crime topic share separately by whether local stories are classified to be about crime or not according to the methodology described in Section 3. Crime topic shares are from an unsupervised LDA model trained on local crime stories. Stories 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

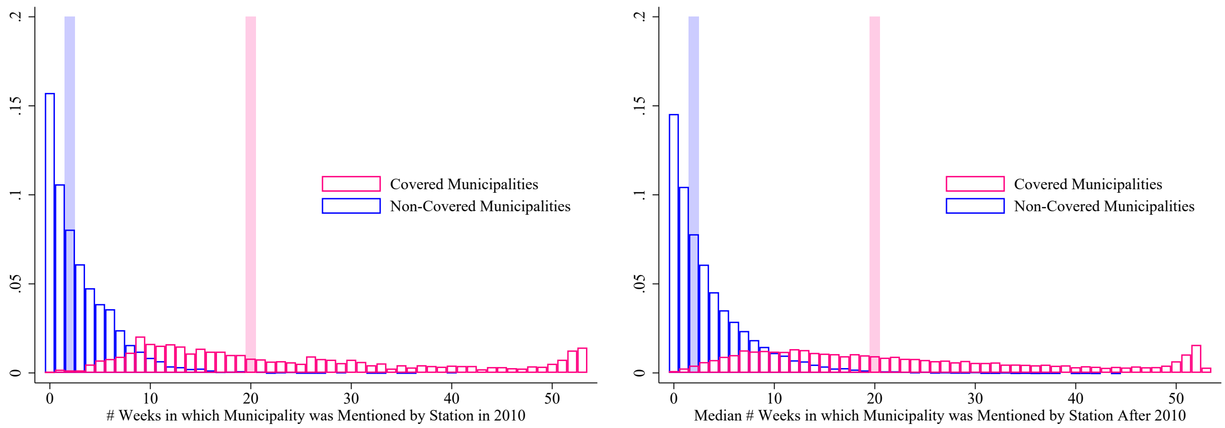


(a) Non-Covered Municipalities

(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. Crime rates are crimes per 1,000 people under an inverse hyperbolic sine transformation, 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

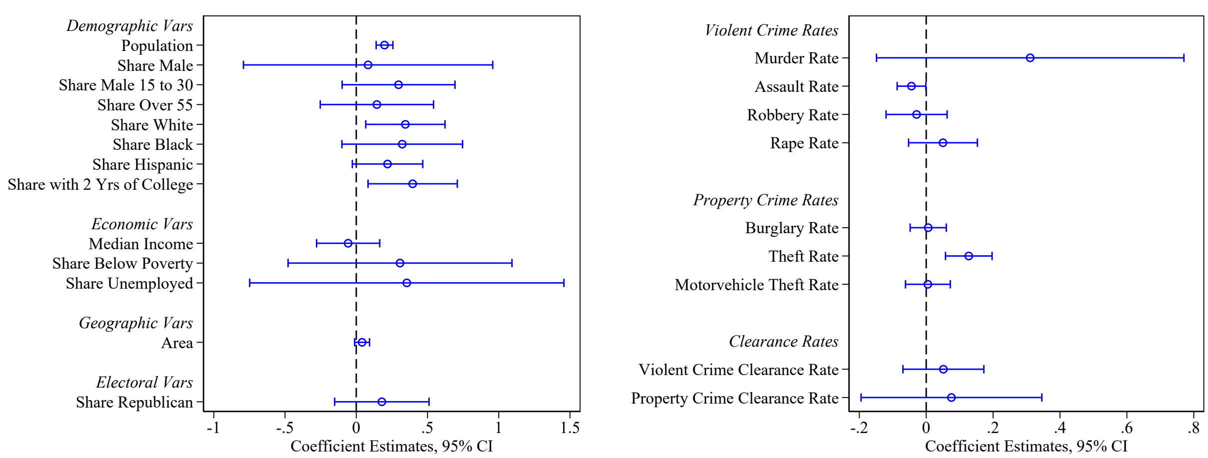


(a) In 2010

(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.

Appendix Figure VII: Differences in Socio-Economic Characteristics Between Covered and Non-Covered Municipalities

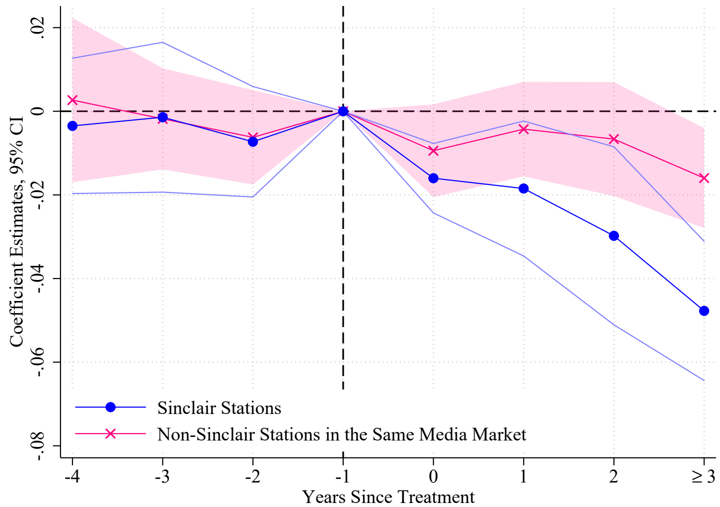


(b) Socio-economic Characteristics

(b) Crime and Clearance Rates

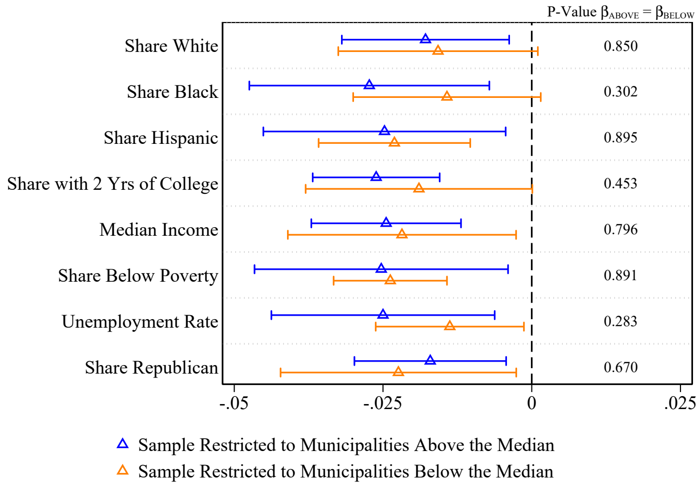
Notes: This figure shows along which dimensions covered and non-covered municipalities differ. We report coefficient estimates together with 95% confidence intervals from a regression of an indicator variable for the municipality being covered at baseline on socio-economic characteristics of the municipality, 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. Standard errors are clustered at the media market level. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. Crimes rates are crimes per 1,000 people under an inverse hyperbolic sine transformation. 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



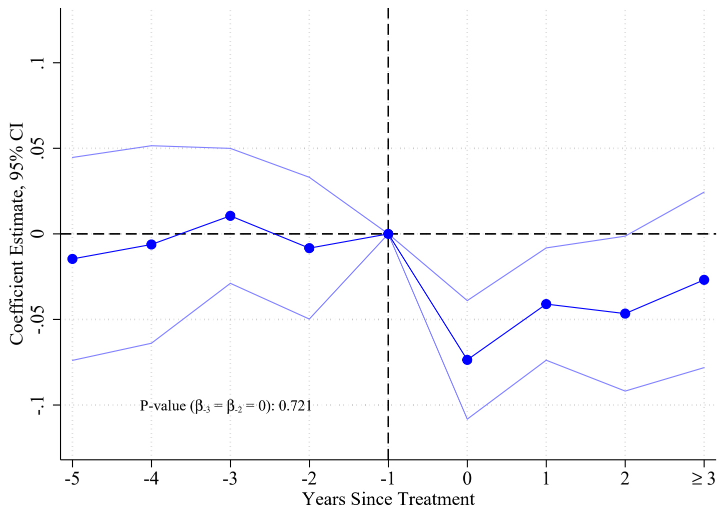
Notes: This figure shows the effect of Sinclair entry, separately for stations directly controlled by Sinclair and for same media market stations not directly controlled by Sinclair, on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities, 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.

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



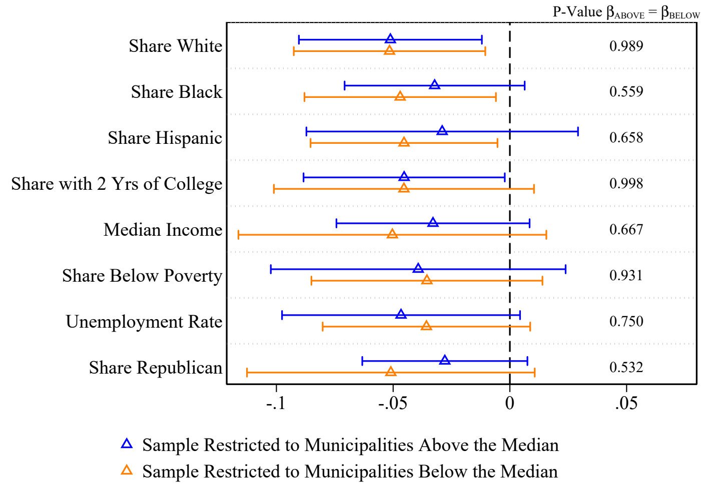
Notes: This figure presents the heterogeneity of the effect of Sinclair entry on local crime reporting. We report coefficient estimates and 95% confidence intervals from two separate regression models for municipalities above and below the median according to the characteristic. The p-value reported is from a test of equality of the main coefficients across the two samples. We regress 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 male between 15 and 30, share over 55, share white, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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.

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



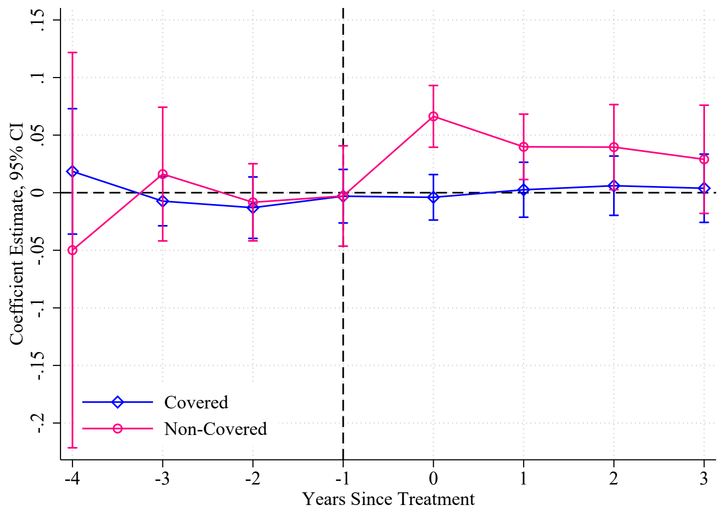
Notes: This figure shows the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities, by year since treatment using data that additionally includes 2009. We report coefficient estimates and 95% confidence intervals from a regression 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. 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.

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



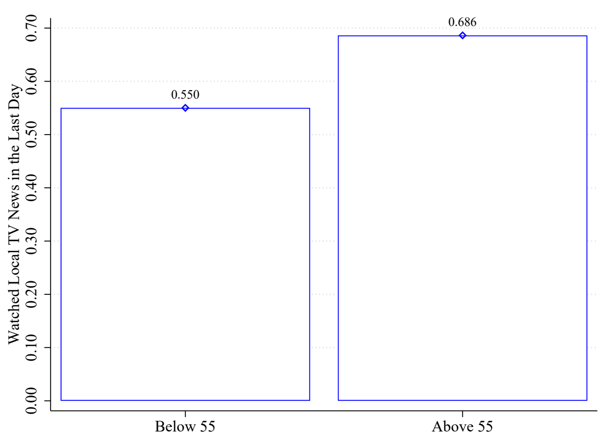
Notes: This figure presents the heterogeneity of the effect of Sinclair entry on the violent crime clearance rate. We report coefficient estimates and 95% confidence intervals from two separate regression models for municipalities above and below the median according to the characteristic. The p-value 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 male between 15 and 30, share over 55, share white, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. 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.

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

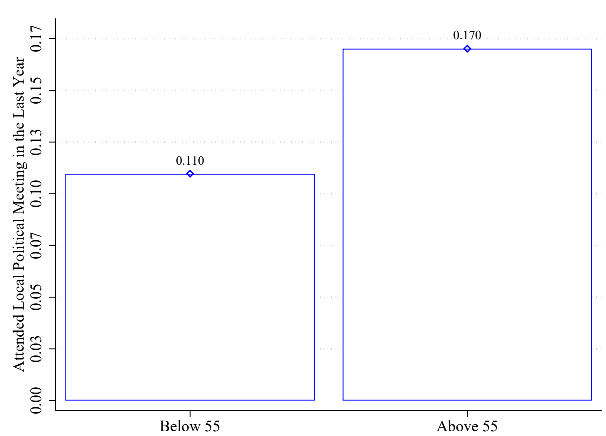


Notes: This figure shows the effect of Sinclair entry on the violent crime clearance rate by year since treatment, estimated separately for covered and non-covered municipalities using an estimator robust to heterogeneous treatment effects in TWFE models. The starting point is a TWFE model that regresses 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. 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.

Figure XIII: 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 Tables

Appendix Table I: Sample Summary

	Overall	Included in the Content Analysis
	(1)	(2)
# of Stations	835	323
# 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 Included in the Analysis					All Municipalities					P-value
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Panel A: Content										
Had a Local Story	2201	0.269	0.265	0.000	0.999						
Had a Local Crime Story	2201	0.107	0.173	0.000	0.935						
	Panel B: Crime and Clearance Rates										
Property Crime Rate	1752	4.071	0.525	2.440	5.101	2358	4.064	0.540	2.440	5.101	0.849
Violent Crime Rate	1752	1.668	0.810	0.168	3.486	2358	1.715	0.807	0.168	3.486	0.173
Property Crime Clearance Rate	1752	0.191	0.119	0.000	0.600	2358	0.191	0.117	0.000	0.600	0.875
Violent Crime Clearance Rate	1752	0.463	0.255	0.000	1.000	2358	0.465	0.251	0.000	1.000	0.872
	Panel C: Municipality Characteristics										
Population	1752	58779	156552	10008	3772486	2358	58394	216189	10008	8078471	0.882
Share Male	1752	0.487	0.025	0.422	0.863	2358	0.487	0.026	0.282	0.863	0.581
Share Male 15-30	1752	0.230	0.074	0.071	0.758	2358	0.231	0.074	0.071	0.803	0.642
Share Over 55	1752	0.232	0.063	0.069	0.683	2358	0.236	0.064	0.068	0.695	0.043
Share White	1752	0.755	0.177	0.012	0.989	2358	0.760	0.177	0.012	0.990	0.374
Share Hispanic	1752	0.117	0.158	0.000	0.978	2358	0.115	0.157	0.000	0.978	0.681
Share with 2 Years of College	1752	0.154	0.182	0.001	0.987	2358	0.155	0.188	0.001	0.987	0.939
Median Income	1752	0.365	0.148	0.052	0.879	2358	0.360	0.147	0.031	0.883	0.299
Share Below Poverty Line	1752	54.321	21.389	17.526	182.237	2358	53.397	21.312	17.526	237.135	0.450
Share Unemployed	1752	0.136	0.078	0.012	0.435	2358	0.140	0.078	0.012	0.442	0.316
Log Area	1752	0.079	0.031	0.015	0.317	2358	0.080	0.031	0.014	0.317	0.196
Share Republican	1752	17.476	0.959	14.595	21.486	2358	17.409	0.994	13.136	21.486	0.221

Notes: This table reports descriptive statistics for the main variables considered in the analysis and for municipality characteristics. Columns (1) to (5) restrict the sample to municipalities included in the main analysis; columns (6) to (10) include all municipalities with more than 10,000 people. Column (11) 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 defined as crimes per 1,000 people under an inverse hyperbolic sine transformation 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	0.021	-0.002	0.003	0.113	-0.255	0.007	-0.012	-0.002
	(0.004)	(0.021)	(0.029)	(0.062)	(0.080)	(0.170)	(0.005)	(0.015)	(0.007)
Observations	1640	1640	1640	1640	1640	1640	1640	615	615
Clusters	205	205	205	205	205	205	205	205	205
Outcome Mean in 2010	13.519	49.407	10.725	83.283	11.638	9.433	3.572	0.508	0.515
	Panel B: DMAs in Content Data								
Sinclair	-0.000	0.033	-0.011	0.113	0.101	-0.075	0.005	0.001	0.003
	(0.005)	(0.021)	(0.032)	(0.084)	(0.105)	(0.208)	(0.007)	(0.003)	(0.007)
Observations	896	896	896	896	896	896	896	336	336
Clusters	112	112	112	112	112	112	112	112	112
Outcome Mean in 2010	14.127	49.283	10.806	80.661	13.729	9.526	3.595	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, Differences-in-Differences Decomposition

Dependent Variable	Had Local Crime Story						
Sample	Non-Covered		Covered		Covered and Non-Covered		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sinclair	-0.003	-0.002	-0.035***	-0.030**	-0.002	-0.001	
	(0.003)	(0.002)	(0.013)	(0.012)	(0.003)	(0.003)	
Sinclair * Covered					-0.027**	-0.030***	-0.024***
					(0.010)	(0.011)	(0.007)
Observations	1633962	1633962	1431232	1431232	3065194	3065194	3065194
Clusters	89	89	111	111	112	112	112
Municipalities	1108	1108	1093	1093	2201	2201	2201
Stations	277	277	320	320	323	323	323
Outcome Mean in 2010	0.016	0.016	0.172	0.172	0.089	0.089	0.089
Station by Municipality FE	X	X	X	X	X	X	X
Week FE	X	X	X	X	X	X	X
Controls by Week FE		X		X	X	X	X
Covered by Week FE						X	X
Station by Week FE							X

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-in-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-in-differences specification. The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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.

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 Local Story	Decomposition	
		Crime	Non-Crime
	(1)	(2)	(3)
Sinclair * Covered	-0.039***	-0.022***	-0.017
	(0.012)	(0.006)	(0.013)
Observations	3065194	3065194	3065194
Clusters	112	112	112
Municipalities	2201	2201	2201
Stations	323	323	323
Outcome Mean in 2010	0.242	0.089	0.153
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 male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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.

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

		Decomposition	
Dependent Variable	Share of Stories about Crime	Local	Non-Local
	(1)	(2)	(3)
Sinclair	-0.009*	-0.012***	0.002
	(0.005)	(0.004)	(0.003)
Observations	30928	30928	30928
Clusters	112	112	112
Stations	323	323	323
Outcome Mean in 2010	0.132	0.061	0.071
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 Coverage of Non-Local Crime Stories

Dependent Variable	Share of Stories About Non-Local Crime	Share of Stories About Non-Local Police	Has Non-Local Story About Police Misconduct	Has Non-Local Story About Crime and Drugs	Has Non-Local Story About Crime and Immigrants
	(1)	(2)	(3)	(4)	(5)
Sinclair	0.002	0.001	-0.030**	0.052**	0.052***
	(0.003)	(0.002)	(0.012)	(0.024)	(0.019)
Observations	30928	30928	30928	30928	30928
Clusters	112	112	112	112	112
Stations	323	323	323	323	323
Outcome Mean in 2010	0.132	0.061	0.071	0.801	0.186
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	
Sub-Sample	Share Republican >= Median	Share Republican < Median
	(1)	(2)
Sinclair * Covered	-0.018***	-0.021**
	(0.006)	(0.011)
Observations	1526536	1519012
Clusters	98	82
Municipalities	1097	1087
Stations	283	240
Outcome Mean in 2010	0.076	0.100
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 male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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.

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

Dependent Variable	Had Local Crime Story	
Type of Crime	Violent	Property
	(1)	(2)
Sinclair * Covered	-0.018***	-0.004**
	(0.005)	(0.002)
Observations	3065194	3065194
Clusters	112	112
Municipalities	2201	2201
Stations	323	323
Outcome Mean in 2010	0.067	0.013
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 male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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.

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

		By Type of Crime			
Dependent Variable	Violent Crime Clearance Rate	Murder	Assault	Robbery	Rape
	(1)	(2)	(3)	(4)	(5)
	Panel A: Full Sample				
Sinclair * Covered	-0.045***	0.116	-0.014	-0.053*	-0.066**
	(0.017)	(0.091)	(0.019)	(0.030)	(0.026)
Observations	14016	6789	12744	13597	13126
Clusters	111	110	110	111	111
Municipalities	1752	1350	1600	1749	1739
Outcome Mean in 2010	0.463	0.649	0.591	0.337	0.375
	Panel B: Balanced Sample				
Sinclair * Covered	-0.044**	-	-0.009	-0.079**	-0.061*
	(0.020)	-	(0.024)	(0.034)	(0.035)
Observations	9360	-	9360	9360	9360
Clusters	109	-	109	109	109
Municipalities	1170	-	1170	1170	1170
Outcome Mean in 2010	0.492	-	0.576	0.358	0.407
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 (4)). The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. 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 XI: Effect of Sinclair Entry on the Violent Crime Rate, by Type of Crime

Dependent Variable	Violent Crime Rate	By Type of Crime: Murder	Assault	Robbery	Rape
	(1)	(2)	(3)	(4)	(5)
		Panel A: Crime Rates			
Sinclair * Covered	0.021	0.003	0.006	0.027	-0.011
	(0.032)	(0.005)	(0.034)	(0.017)	(0.021)
Observations	14016	14016	14016	14016	14016
Clusters	111	111	111	111	111
Municipalities	1752	1752	1752	1752	1752
Outcome Mean in 2010	1.668	0.033	1.227	0.716	0.301
		Panel B: Dummy = 1 if at least one Crime			
Sinclair * Covered	-	0.027	-0.000	0.002	0.051***
	-	(0.040)	(0.005)	(0.011)	(0.019)
Observations	-	14016	14016	14016	14016
Clusters	-	111	111	111	111
Municipalities		1752	1752	1752	1752
Outcome Mean in 2010	-	0.463	0.908	0.965	0.933
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 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 (4)). The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. In Panel A, reports outcomes are defined as crime rates; in Panel B, outcomes are defined as indicator variables for experiencing at least one crime. Crime rates are defined as crimes per 1,000 people under an inverse hyperbolic sine transformation, 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	By Type of Crime: Burglary	Theft	Motor Vehicle Theft
	(1)	(2)	(3)	(4)
Sinclair * Covered	0.054***	0.046*	0.051**	0.041
	(0.019)	(0.026)	(0.025)	(0.028)
Observations	14016	14016	14016	14016
Clusters	111	111	111	111
Municipalities	1752	1752	1752	1752
Outcome Mean in 2010	4.071	2.431	3.750	1.238
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 (4)). The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. Crime rates are defined crimes per 1,000 people under an inverse hyperbolic sine transformation, and are winsorized at the 99% level.

Appendix Table XIII: Effect of Sinclair Control on the Violent Crime Clearance Rate, Differences-in-Differences Decomposition

Dependent Variable	Violent Crime Clearance Rate						
Sample	Non-Covered		Covered		Covered and Non-Covered		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sinclair	0.043***	0.051***	-0.003	-0.008	0.040***	0.045***	
	(0.014)	(0.012)	(0.009)	(0.009)	(0.013)	(0.012)	
Sinclair * Covered					-0.039***	-0.050***	-0.046***
					(0.014)	(0.014)	(0.015)
Observations	6528	6528	7488	7488	14016	14016	14016
Clusters	86	86	110	110	111	111	111
Municipalities	816	816	936	936	1752	1752	1752
Outcome Mean in 2010	0.440	0.440	0.483	0.483	0.463	0.463	0.463
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-in-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-in-differences specification. The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. 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 Entry on Police Violence

Dependent Variable	All Fatalities			Fatalities Involving Intentional Use of Force		
Victim Race	Any	White	Minority	Any	White	Minority
	(1)	(2)	(3)	(4)	(5)	(6)
Sinclair * Covered	-0.046	-0.038	0.011	-0.025	-0.021	0.003
	(0.030)	(0.026)	(0.017)	(0.023)	(0.026)	(0.016)
Observations	14016	14016	14016	14016	14016	14016
Clusters	111	111	111	111	111	111
Municipalies	1752	1752	1752	1752	1752	1752
Outcome Mean in 2010	0.146	0.072	0.053	0.114	0.055	0.044
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 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 1 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 (4)). The characteristics included are log population,share male, share male between 15 and 30, share over 55, share white, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. Data on officer-involved fatality is from Fatal Encounters.

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

Data Source	Census of Government			UCR	
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	0.085	-0.028	-0.012
	(0.005)	(0.002)	(0.173)	(0.029)	(0.022)
Observations	8449	8449	9472	14015	14015
Clusters	109	109	111	111	111
Municipalies	1371	1371	1501	1752	1752
Outcome Mean in 2010	0.240	0.019	2.967	2.370	1.846
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 (4)). The characteristics included are log population,share male, share male between 15 and 30, share over 55, share white, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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. All outcome variables are winsorised at the 99% level.

Appendix Table XVI: 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	-0.022***	-0.024***	-0.020***	-0.021***	-0.026***	-0.022***	-0.022***	-0.022***	-0.019***
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)
Observations	3065194	3065194	3065194	2978841	3065194	2440702	3058924	3065194	3051818
Clusters	112	112	112	112	112	111	112	112	111
Municipalities	2201	2201	2201	2201	2201	1752	2201	2201	2193
Stations	323	323	323	323	323	322	321	323	319
Outcome Mean in 2010	0.089	0.096	0.070	0.088	0.106	0.098	0.089	0.089	0.088
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 male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, 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 tokens 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.

Appendix Table XVII: 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	Partially Treated Years as Treated	Group Acquis. Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sinclair * Covered	-0.045***	-0.047***	-0.047***	-0.045***	-0.036**	-0.031*	-0.047**
	(0.017)	(0.018)	(0.018)	(0.017)	(0.015)	(0.016)	(0.018)
Observations	14016	14016	14016	13760	14016	14016	13528
Clusters	111	111	111	106	111	111	103
Municipalities	1752	1752	1752	1720	1752	1752	1691
Outcome Mean in 2010	0.463	0.464	0.464	0.466	0.463	0.463	0.461
Media Market by Year FE	X	X	X	X	X	X	X
Covered by Year FE	X	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X	X
Sinclair * Controls	X	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 (4)). The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, and Republican vote share in the 2008 presidential election. Column (1) reports the baseline estimate for reference. 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) defines the year to be treated if Sinclair was present in the market in the December of that year. Column (7) 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 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 unless otherwise specified. 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 XVIII: Robustness to Heterogeneous Effects in TWFE Models

Dependent Variable	Violent Crime Clearance Rate	
Sample	Non-Covered	Covered
	(1)	(2)
Sinclair	0.066***	-0.004
	(0.014)	(0.010)

Notes: This table shows the effect of Sinclair on the violent crime clearance rate, estimated separately for covered and non-covered municipalities using an estimator robust to heterogeneous effects in TWFE models. The starting point is a TWFE model that regresses the outcome on year and municipality fixed effects. We estimate the treatment effect using robust estimator proposed by de Chaisemartin and D'Haultfoeoeuille (2020), which we report together with standard errors estimated from 1000 bootstrap repetitions. The analysis is run separately for covered and non-covered municipalities. Column (1) reports the robust estimator for non-covered municipalities, and columns (2) for covered municipalities. 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. 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 XIX: Effect of Sinclair Entry on the Violent Crime Clearance Rate, No Staggered Timing

Dependent Variable	Violent Crime Clearance Rate			
Restricted to Media Markets Treated in…	2012	2013	2014	2015
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.106**	-0.032***	-0.024	0.003
	(0.046)	(0.012)	(0.022)	(0.013)
Observations	9320	8944	9976	9320
Clusters	60	59	70	62
Municipalities	1165	1118	1247	1165
Outcome Mean in 2010	0.446	0.438	0.447	0.442
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. In particular, we restrict the sample to media markets that were never exposed to Sinclair and media markets that were acquired by Sinclair in the year specified in the column header. We only estimate separately 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 (4)). The characteristics included are log population, share male, share male between 15 and 30, share over 55, share white, share black, share Hispanic, share with 2 years of college, log median income, share of population below the poverty rate, share unemployed, log municipality area, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. 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. 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 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 tokens, 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 1 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.[35] 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 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 (2623 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 (236 municipalities) and do not have at least one violent and one property crime in every year (29 municipalities). This leaves us with 2358 municipalities. The empirical strategy requires restricting the sample to municipalities located in media markets included in the content data (which further drops 601 municipalities) and the regressions drops 5 singleton municipalities (Correia (2015)). The final sample includes 1752 municipalities.

[35] 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.

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 Müller and Schwarz (2019) and 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.

Appendix C – Classifying Local Crime News

We build a classifier model that assigns a specific type of crime to each of the 415,604 local news stories 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:

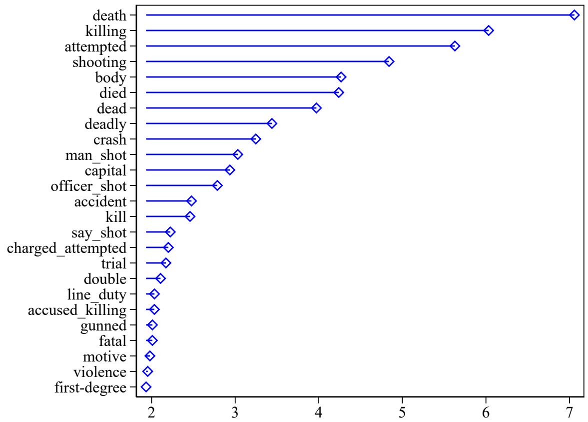
1. Murder: MURDER, HOMICID, KILLE;
2. Assault: ASSAULT;
3. Robbery: ROBBE;
4. Rape: RAPE, SEXUAL ASSAULT;
5. Burglary: BURGLAR;
6. Theft: THIEF, STEAL, STOLE, THEFT.

We selected these terms to minimize the presence of false positives. In fact, we checked using the full vocabulary that these keywords return tokens 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 (205,299 stories). Because it is difficult to distinguish between assault and rapes and burglary and theft, we classify stories into three categories: stories about murder, stories about other violent crimes (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.

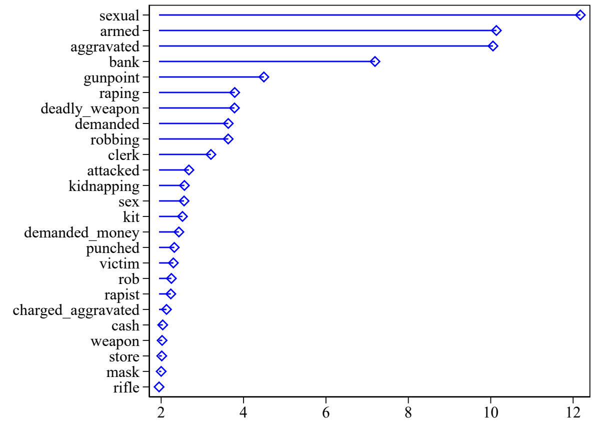
We use this sub-sample to train a classifier model. In particular, we train a support vector machine model using stochastic gradient descent. The features that are used to predict the label are the top most frequent 25,000 tokens and bigrams in the full corpus. We exclude the keyword used to define the 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 0.83 (murder), 0.77 (other violent crimes), 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 210,305 stories. Using this method, we are able to assign a crime type to 85% of all local crime stories.

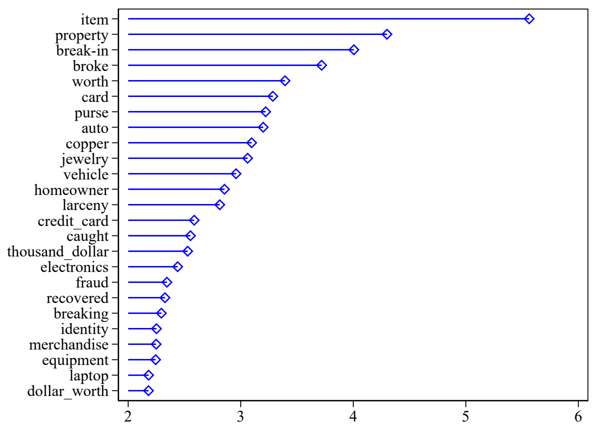
Appendix C Figure I: Most Predictive Features for News Type Classifier



(a) Murder



(b) Other Violent



(c) Property

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