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

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Abstract

Does media content influence local institutions? We study this question by looking at how a negative shock to local crime-related news, induced by the acquisition of local TV stations by the Sinclair Broadcast Group, affects U.S. municipal police departments. In particular, we implement a triple differences-in-differences design that exploits the staggered timing of acquisitions 2010-2017, together with cross-sectional variation in whether municipalities are covered by local news at baseline, a proxy for exposure to the shock. First, using a newly collected dataset of 300,000 transcripts of local newscasts, we document that once acquired by Sinclair, TV stations decrease news coverage of local crime. Second, we find that after Sinclair enters a media market, municipalities that were likely to be in the news at baseline experience 8% lower violent crime clearance rates with respect to municipalities that were very rarely in the news in the first place. The main mechanism we propose is that the change in content induces police officers to decrease the effort allocated to clearing violent crimes, due to a decline in the salience of crime as an issue 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 forces 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 on police is a first order question. By providing information to the public, the media have 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 news have large viewerships and garner high levels of trust (Mitchell et al. (2016)). In addition, local news often have a clear crime focus. This, combined with the highly decentralized nature of law enforcement in the U.S., makes local media uniquely positioned to play a role in influencing the behavior of police officers.

In this paper, we study how changes in TV news coverage of a municipality's crime 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 U.S. has seen an increase in concentration driven by large broadcast groups acquiring high numbers of local TV stations, and 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 (2018)). 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. But, 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 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. Importantly, 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 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 the media market's municipalities is affected by the Sinclair acquisitions. We identify crime stories using a pattern-based sequence-classification method that classifies a story to be about crime if it contains a "crime bigram." That is, if it contains a bigram that is much more likely to appear in crime-related stories of the Metropolitan Desk Section of the New York Times than in non-crime related ones. In addition, we assign stories to municipalities based on 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 2.4 percentage points less likely to be mentioned in a crime story after a station gets acquired by Sinclair with respect to non-covered municipalities.[1] The effect is significant at the 1% level and economically important, corresponding to 27% of the outcome mean in 2010. Interestingly, 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 with time. The change in coverage is the result of an editorial decision on part of 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 turn our attention to how the change in news coverage of local crime impacts clearance rates. We find that after Sinclair enters a media market, covered municipalities experience 3.9 percentage points lower violent crime clearance rates with respect to non-covered municipalities. The effect is precisely estimated, and corresponds to 8% of the baseline mean. Using an event-study specification, we find no difference between covered and non-covered municipalities in the two years before Sinclair acquires control over the station. The effect appears within the first year since treatment, but 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 have 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 16.7% 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 of the most novel contributions of this paper: they provide the ability to characterize in detail the

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

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 some evidence of an increase in property crime rates after Sinclair entry. We also 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.

The main mechanism that we propose is the following. When Sinclair acquires a local TV station, stories about a municipality's violent crimes are less frequent and crime becomes less salient for local citizens. As the pressure that citizens put on the police to solve these crimes decreases, police officers reallocate their effort away from clearing these types of crime in favor of other policing related activities. Two main 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 declines. 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 mechanisms from salience to police behavior through local citizens' action.

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 influence 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)), as also reflected by 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 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, differently than for 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 the increasing trend in concentrated media ownership (Stahl (2016)). Consistent with existing work in this area (Martin and McCrain, 2018), we confirm that large broadcast group acquisitions lead to a crowding out of local news in favor of national stories. Given that our content data span multiple years, we are able to map out how acquisitions affect content changes over time. In addition, we investigate 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 evidence on how media content influences the behavior of police officers. It is interesting to contrast our finding that decreases in local TV news coverage of a municipality's crime lower clearance race 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. Section 2 presents the background, section 3 presents the data, and section 4 discusses the empirical strategy. The main results of the effect of Sinclair on local news is presented in section 5, and the results of the effect of Sinclair on police behavior is provided in section 6. Section 7 presents the robustness checks and section 8 discusses potential mechanisms. Finally, section 9 concludes.

2 Background

2.1 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).

The overarching narrative regarding the decline in TV news masks substantial heterogeneity, with the decrease in viewership being limited outside the top-25 media markets (Wenger and Papper, 2018*b*). Local TV news still plays an important role in small and medium sized markets, not only in terms of viewership but also because there tend to be fewer outlets such as newspapers producing original news focusing on the area (Wenger and Papper, 2018*a*).

In addition, 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 been not been strongly affected (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), they constitute an important news source that has the potential to shape public information and perceptions.

What are local TV newscasts about? Our 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 stations newscasts: almost 25% of local stories are crime-related (13% overall).[2]

To have a more complete picture of the breakdown of topics covered in local TV news, we also trained an unsupervised LDA topic model on the 1.8 million local stories in our content data. Appendix Figure I shows word clouds with the 50 words that have the highest weight for each of the resulting five topics, which can be easily identified to be related to crime, events (also possibly a filler topic), politics, weather, and sports. In Figure I Panel (b), we show the average topic share across all local news stories. Again, the most covered topic in local stories is crime (26%), followed by local events (23%), and politics (21%). Weather and sports also appear in local stories, although

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

to a lesser extent. Given the crime focus of local TV newscasts, studying the relationship between local news and police departments appears to be first order.

2.2 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). As broadcast groups centralize news production in an effort to cut costs, acquisitions have the potential to significantly affect news content (Stahl, 2016). Consistent with this, existing research has shown that acquisitions are often associated with a decrease of local news coverage (Martin and McCrain, 2018).

This paper focuses on the most active player 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. Over the period considered, 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, acquisitions have taken place in media markets across the United States, although Sinclair was particularly active in medium-sized media markets.

With respect to other broadcast groups, the conservative leaning Sinclair appears to be particularly interested in controlling the messaging of its local stations (New York Times (2018)), which gives them even stronger incentives to move away from local towards national coverage. Consistent with this, Martin and McCrain (2018) use a differences-in-differences design to show that when Sinclair acquired Bonten in 2017, Bonten stations started covering more national news to the detriment of local news, and their ideological slant shifted significantly to the right. Sinclair's conservative leaning might have real word effects: as shown by Miho (2018), exposure to Sinclair appears to have increased the Republican vote share in Presidential elections.

3 Data and Measurement

This paper combines multiple data sources.

Station Sample. 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). We focus on big-four affiliates as they tend to take up most of the viewership in a media market and tend to be the ones producing original newscasts.[3] 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.

[3] Local TV stations in the United States are usually affiliated to national networks, which are publishers that distribute branded content. Affiliated local TV stations, although under separate ownership, carry the television lineup

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.[4] We consider stations to be controlled by Sinclair if they are owned and operated by the Sinclair Broadcast Group, if they are owned and operated by Cunningham Broadcasting, or if Sinclair controls the station's programming through a local marketing agreement.[5] With a slight abuse of nomenclature, we use Sinclair acquisitions to refer to Sinclair control determined by any of these instances, unless otherwise specified.[6]

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 A, which gives us 9.5m separate stories.

We use the segmented transcripts data to measure whether a municipality appears in a crime story in a station's newscast. 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. For each station, we search the name of all municipalities with at least 10,000 inhabitants 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.

offered by the network. 85% of local TV stations that produce their own newscasts are affiliated with one of the big four networks (Papper, 2017).

[4]We use annual reports as our primary source because we are interested in Sinclair control in addition to ownership, and the BIA/Kelsey data focus on outright ownership instead. In particular, the BIA/Kelsey data does not report information on local marketing agreements under which Sinclair effectively controls programming of the station.

[5]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 would control the programming of the station.

[6]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)).

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 downloaded from Factiva. Instead, the non crime-related training library is composed of all Metropolitan Desk articles without the same tags over the same period. Each library is composed of all adjacent two word combinations (i.e. bigrams) contained in the articles, 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 on 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 appear in the crime library 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)).[7]

3. We combine the definitions to create an indicator variable equal to 1 if a given municipality was mentioned in a crime story in an evening newscast of a given station in a given week.

The analysis restricts the sample to stations continuously present in the content data from 2010 to 2017. In order 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. To reduce measurement error, we drop municipalities whose name never appears in the transcript data

[7] 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, we determined that these results indicate that the procedure we follow successfully identifies crime stories.

(14 municipalities). The resulting sample includes 323 stations and 2201 municipalities.

Crime. Crime data are from the Uniform Crime Reports (UCRs) published by the Federal Bureau of Investigation 2010-2017.[8] 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).[9] Given that many municipalities only report their full-year crime and arrests counts, we aggregate the monthly agency data at the year level. 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.[10]

The sample is restricted to municipalities with more than 10,000 inhabitants that are located in media markets part of the content data and with a municipal police department that reports crime data to UCR. We further exclude municipalities that contract out law enforcement services to the local sheriff's office. Finally, we restrict the sample in the crime analysis to municipalities that report at least one violent and one property crime in all years 2010-2017. The resulting sample includes 1936 municipalities.

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 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 share Republican 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 Expenditures and Employment. Data on police departments' employment are from the

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

[9] UCR data need extensive cleaning, as they are provided by the FBI without pre-processing. We clean the data following the same procedure used in Mello (2019). In particular, the procedure identifies true missing or implausible observations in the data and replaces them using linear interpolation.

[10] To define crime rates, we use a smoothed version of the population count included in the UCRs. A crime is considered cleared by arrest if at least one person has been arrested, charged, and turned over for prosecution (FBI website) or if the offender has been identified, but external circumstances prevent an arrest. There is no perfect correspondence between the crimes that are reported as being cleared in a certain year and the offenses taking place in that year, but the vast majority of arrests happen close to the date of the incident. Using data at the yearly level minimizes the mismatch.

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.

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 10% of them. Panel B reports the average property and violent crime and clearance rates for the same year, and Panel C reports socio-economic characteristics of these municipalities.

As we describe above, 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 inhabitants for comparison.[11] The municipalities included in the analysis appear to be highly comparable to other municipalities with more than 10,000 inhabitants, as is confirmed by the p-values reported in column (11).

4 Empirical Strategy

Our empirical strategy exploits the acquisition of local TV stations on part of the Sinclair Broadcast Group as a shock to local news content. The shock to local news content induced by Sinclair is twofold. First, when Sinclair acquires a station, newscasts tend to increase their national focus to the detriment of original local content (*effect #1*). This is the treatment effect we are interested in identifying. But in addition to this, Sinclair acquisitions also change the overall content of the stations, in particular by transmitting more conservative content that might also be law enforcement related (*effect #2*). For example, Sinclair is notorious for requiring its stations to air must-run

[11]The full sample includes 2849 municipalities; of these municipalities, 2584 have an independent police department (i.e. a police department that does not depend on a sheriff's office), report crime data to the UCR, and have at least one violent and one property crime every year 2010-2017. We have coverage information for 2201 municipalities, out of which 1936 are included in the crime sample as well. The sample for the content analysis includes 265 municipalities not in the crime analysis. We include them in order to maximize power, but show in the robustness check section that this does not affect our results.

segments that include law and order features such as the "Terrorism Alert Desk," which provides frequent updates on terrorism-related news (Newscast Studio, 2015).

In order 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: 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 across media markets and time 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.

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. The identification assumption is that municipalities covered by local news would have experienced the same change in police behavior after a Sinclair acquisition as non-covered municipalities, were it not for the acquisition itself.

Appendix Figure V provides a visual intuition of the argument, 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 which the same municipality is in the news with a local crime story in the same year, 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 hovering around very low levels both before and after acquisition. Instead, 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 to be 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 then 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 trult 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 inhabitants.

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 the design allows to control for trends in both observable and unobservable characteristics strengthens the credibility of the results.

5 Effect of Sinclair Control on Reporting of Local Crime Stories

5.1 Specification

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

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

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 1 after a station is acquired by Sinclair, $Covered_m$ is an indicator variable equal to 1 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.[12] Each

[12] In particular, X_{m2010} includes the following variables: population, share male, share male between 15 and 30,

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 municipality-station pairs that belong to the same media market. The sample is restricted to 323 stations and 2201 municipalities continuously reporting content data. Standard errors are clustered at the media market level.

The station by week fixed effects (δ_{st}) control non-parametrically for station specific trends in content, in particular trends that might correlate with Sinclair control. Covered status by week fixed effects (δ_{ct}) allow us to take into account national trends experienced by different types of municipalities, while municipality by station (δ_{sm}) fixed effects control for station specific level differences across municipalities.

We provide evidence supporting the parallel trend assumption by estimating a version of the baseline specification that allows the effect to vary over time, and we present event-study graphs that plot leads and lags of the effect of Sinclair control on covered relative to non-covered municipalities. 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). We find that a Sinclair acquisition decreases the probability that the station reports a local crime story about a covered municipality by 2.4 percentage points with respect 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 27% of the baseline mean.

share white, 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.

Covered and non-covered municipalities differ along a number of dimensions, most notably population. In column (2), we show that the effect is virtually unchanged by controlling for the interaction between Sinclair control and socio-economic characteristics of the municipality at baseline. This confirms that the effect we are finding is indeed related to coverage, and not Sinclair control having differential effects for, say, municipalities with a large population. To further improve the comparability of the sample, in column (3), we additionally exclude municipalities with more than 50,000 inhabitants. The coefficient is smaller in size but similar in magnitude, corresponding to 29% of the baseline outcome mean of the restricted sample.

In our triple differences-in-differences design, identification rests on the assumption that, absent treatment, the probability of covered municipalities being in the news with a local crime story would have evolved similarly to the one of non-covered municipalities. We provide evidence supporting the assumption 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 three years leading up to the station coming under Sinclair control. Stations under Sinclair control are less likely to report a local crime story about covered municipalities than non-covered municipalities beginning in the first year after acquisition, after which the effect slowly becomes larger over time.[13]

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 IV, we show that local news does decrease across the board, but that the effect is larger for stories about crime. Column (1) shows that Sinclair control lowers the probability that a station reports a local crime story about covered municipalities with respect to non-covered municipalities by 3.8 percentage points (16% of the baseline mean). However, column (2) and column (3) show that the effect is especially strong for local crime stories with respect to non-crime local stories more generally. A possible explanation for this is that producing crime news might be more expensive, for example because reporters need to follow the story and often be at the crime location, than other local news such as weather or sports, which can be produced without journalists being on the ground.

Overall Crime Coverage. How is non-local crime coverage affected by Sinclair acquisitions? We address this question in Appendix Table V, where we estimate a difference-in-difference specification at the station level.[14] The main outcome is the share of stories that are about crime (column (1)), which we further decompose in stories about crime that are local (column (2)) or not (column

[13] We can exploit our data to further explore the timing of the effect by estimating an event-study graph by semesters. Appendix Figure VIII shows no effect in the first six months after treatment, after which the effect becomes negative and of similar magnitude as the main effect. As is the case in the main event-study graph, the effect becomes larger in magnitude over time.

[14] In particular, we regress the outcome on an indicator variable for the station being owned by Sinclair, media

(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: the control municipalities are exposed to the same level of non-local crime news both before and after acquisition.[15]

Same-Media Market Stations. Our result might still reflect an underlying change in a municipality's crime prevalence or demand for crime stories. To shed light on this question, 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 that are not subject of the acquisition themselves. In Appendix Figure IX, 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 two years leading up to Sinclair entry, there is no difference in the reporting behavior of Sinclair and non-Sinclair stations. However, once Sinclair enters the media market, we do not see a decrease in local crime coverage for non-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 = 0.038). Taken together, the evidence supports the interpretation that decreasing local crime coverage is an editorial decision on the part of Sinclair stations. This is not just reassuring but also interesting, as it shows that other media groups are not responding to Sinclair changing content, at least as far local crime coverage is concerned.

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 VI, we estimate the main specification separately 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.

Decomposing the Main Effect. Finally, to understand what variation is driving our main effect,

market characteristics measured in the 2010 Census interacted with week fixed effects, station fixed effects and week fixed effects. The characteristics included are log population, share male, share male between 15 and 30, share white, share Hispanic, and log income per capita. Standard errors are clustered at the media market level. A story is defined to be about crime following the methodology explained in Section 3. A story is defined to be local if it directly refers to one of the municipalities with at least 10,000 people in the media market.

[15]Given that Sinclair is a conservative media group, it might be surprising to not see an increase in the share of non-local crime stories. However, the result has to be interpreted with caution as our methodology for identifying crime stories is constructed focusing on local stories, and language from the Metropolitan Desk section of the New York Times might not be suited to identify stories about, say, terrorism.

we estimate a differences-in-differences specification with heterogeneous effects in baseline municipality exposure.[16] Appendix Figure X 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. Instead, for covered municipalities, Sinclair entry implies a large decline in the probability of being mentioned in the news with a crime story. Overall, decomposing the effect shows a pattern that supports the intuition behind the triple differences-in-differences design: non-covered municipalities do not experience a change in their local crime coverage, while covered municipalities bear the brunt of the decline.

6 Effect of Sinclair Control on Police Behavior

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

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 with respect to non-covered municipalities. While from Sinclair's point of view, cutting local coverage may simply be a way to cut costs, this decline may have tangible implications. In particular, we are interested in understanding the effect of the decline of local crime coverage on police behavior.

The outcome we study is clearance rates. Arrests are highly dependent on what actions are taken by the police immediately after a crime takes place (Blanes i Vidal and Kirchmaier (2017), Cook et al. (2019)), and as a result they are often used to study police behavior and effort (see, among others, Mas (2006), Shi (2009), and Premkumar (2020)).

Importantly, not all crime types are equally likely to be the subject of 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

[16] In particular, we estimate the following differences-in-differences specification with heterogeneous effects:

$$y_{mst} = \beta Sinclair_{st} + \sum_{k=2}^{8} \beta^k Sinclair_{st} * CoveredQuantile^k_m + W'_{dt}\eta + \delta_t * X'_{m2010}\gamma + \delta_{sm} + \delta_t + \epsilon_{mst} \quad (3)$$

where y_{mst} is the share of weeks in month t in which municipality m was mentioned in a crime story by station s in month t, $Sinclair_{st}$ is an indicator variable equal to 1 after a station is acquired by Sinclair, $CoveredQuantile^k_m$ is an indicator variable equal to 1 if a municipality is in the k-th coverage quantile, W_{dt} are media market time-varying controls, X_{m2010} are baseline municipality characteristics, δ_t are month fixed effects, and δ_{sm} are municipality-station pair fixed effects. W_{dt} includes population, share male, share male 15 to 30, share white, share Hispanic, unemployment rate, and income per capita. X_{m2010} includes the same variable as the main specification with the addition of state fixed effects. The figure shows the β coefficient for the first quantile, and the linear combination of β and β^k for all other quantiles, together with 95% confidence intervals.

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 A. 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 abour property crimes (i.e. burglary and theft). Local crime news has a clear violent crime focus: 75% of local crime stories are about a violent crime, while only 16.7% of crimes stories relate to a property crime.

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 0.16 stories. Instead, property crimes, at 0.002 stories per offense, have a negligible probability of being covered in the news. This evidence guides our analysis on 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.[17]

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:

$$y_{mdt} = \beta Sinclair_{dt} * Covered_m + Sinclair_{dt} * X'_{m2010}\gamma + \delta_{dt} + \delta_{ct} + \delta_m + \epsilon_{mdt} \tag{4}$$

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 1 after a media market experiences Sinclair entry, $Covered_m$ is an indicator variable equal to 1 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.[18]

[17] We use our classifier model to also estimate the direct effect of Sinclair acquisitions on local coverage of violent and of property crimes. Appendix Table VII 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 crime story about a violent crime and 0.4 percentage points (30% of the baseline mean) less likely to appear in the news with a crime story about a property crime. The effect is almost 4.5 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 to appear 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.

[18] 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.

The regression is estimated on a balanced municipality by year panel 2010-2017. Standard errors are clustered at the media market level.

The media market by year fixed effects (δ_{dt}) control non-parametrically for the overall change in content that is associated with Sinclair entering a media market such as the likely increase in 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.[19]

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 present in the media market the entire time. This decision is justified by the fact that 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 decisions in the robustness check section.

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:

$$\begin{aligned} y_{mdt} = \sum_{y=1}^{T_{min}} \beta_y * Pre_{t-y,d} * Covered_m + \sum_{y=0}^{T_{max}} \gamma_y * Post_{t+y,d} * Covered_m \\ + \delta_{dt} + \delta_{ct} + \delta_m + \epsilon_{mdt} \end{aligned} \tag{5}$$

where all variables are defined as above.

6.3 Main Results

Table II shows the effect of Sinclair entry in 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, estimated from the baseline specification outlined in equation (4). Column (1) shows that after Sinclair enters a media market, the violent crime clearance rate is 4.2 percentage points lower in covered than in non-covered municipalities. The effect is significant at 1% level, and it is sizable in economic magnitude, corresponding to 8% of

[19] 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.

the baseline mean. As before, controlling for the interaction between an indicator for Sinclair's entrance in the media market and socio-economic characteristics of the municipality does not affect the results (column (2)), and neither does restricting the sample to municipalities with fewer than 50,000 inhabitants (column (3)). Importantly, controlling for crime rates and population, two factors that we might worry influence violent crime clearance rates but that we do not include in the main specification because they are potentially endogenous to the treatment, does not change the result (column (4)).

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 two years leading up to Sinclair's entrance in the media market. The effect is then fully realized in the first year after the acquisition, but the gap between covered and non covered municipalities seems to be shrinking over time.

While the overall pattern displayed by the event study graph is supportive of the identification assumption, we might be worried about the positive coefficient at $t-3$ suggesting pre-existing differences between covered and non-covered municipalities. To convince ourselves that this is not the case and to also better understand the time pattern of the effect, we estimate an event-study graph aggregating monthly data at the semester rather than year level.[20] Appendix Figure XI shows the resulting event-study graph. While the estimates are substantially noisier than for the yearly data, the graph confirms that there is no difference between covered and non-covered municipalities in the three years leading up to treatment, which we find reassuring. In addition, there is no effect in the first six months after treatment, after which the coefficient becomes negative (six to twelve months after treatment). Over time, the magnitude of the effect starts shrinking, with the gap between covered and non-covered municipalities almost disappearing three years after Sinclair enters the media market.

Heterogeneity of the Effect by Type of Crime. Not all violent crimes are the same. As a consequence, it is natural to explore the effect of Sinclair entry on the clearance rate of different types of violent crime. This is what we show in Table III. Column (1) reports the main effect for reference, and columns (2) to (5) present the effect, looking separately at the clearance rates of murders,

[20] As mentioned in the data section, our preferred specification is to aggregate the monthly data at the yearly level because many municipalities only report full-year totals, and as a result, specifications that require monthly data are estimated on a smaller sample. In addition, the requirement of the municipality reporting at least one violent crime for every time period is also more stringent for data at a finer temporal level. Taken together, these limitations imply that the event study graph by semesters is estimated on a more selected sample of 1387 instead of 1620 municipalities.

assaults, robberies, and rapes. The results reported in Panel A are based on the full sample, but to avoid the results being driven by the sample being unbalanced, Panel B reports estimates for a balanced sample of municipalities for each type of crime. The table shows that the decline in the violent crime clearance rate is not only driven by a decline in the clearance rate of murders, but also appears in the clearance rates of assaults, robberies and rapes. Because of data limitations, we cannot draw strong conclusions on what is happening to the murder clearance rate; however, it is suggestive that we are seeing an effect on crimes that are especially likely to be affected by the decline in coverage.

Property Crime Clearance Rates. As discussed in section 6.1, given that local crime news have a clear violent focus, we should expect limited effects on property crime clearance rates. Table IV 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 idea that property crimes should be minimally impacted by the change in content, given that they have a very low baseline probability of appearing in the news in the first place.

Crime Rates. A potential concern is that the change in the violent crime clearance rate might be explained by an increase in the violent crime rate, and not by a response of police officers to the changing media environment. Appendix Table VIII suggests that this is not the case. The table reports the effect of Sinclair entry on the violent crime rate of covered municipalities relative to non-covered municipalities, for all violent crimes (column (1)) and separately by type of crime (column (2) to column (5)). Panel A reports the effect on crime rates in levels, while panel B defines as the outcome as an indicator variable equal to 1 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 (2.8% of the baseline mean) is too small to fully explain the decline in the violent crime clearance rate.

Appendix Table IX looks instead at property crime rates. Column (1) shows that Sinclair entry is associated with 1.195 higher property crime rate (3.5% of the baseline mean) in covered municipalities relative to non-covered ones. The effect is marginally significant at the 10% level, and seems to be mostly driven by burglaries (column (2)). This result is potentially consistent with 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 to play in this setting.

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 X shows that after Sinclair entry, covered and non-covered municipalities have similar police expenditures and employment per capita. The only measure that appears to be differentially affected are judicial expenditures per capita (column (2)). Given that judicial expenditures include all arrests related costs, this is consistent with the main result on the violent crime clearance rate.

Police Violence. We might wonder whether the decrease of news coverage of local crime also affects the probability that officers are involved in episodes of police violence. In Appendix Table XI we address this question using data from Fatal Encounters (FE), the most comprehensive dataset of police-involved fatalities.[21] We find no evidence supporting the idea of news coverage of crime stories influencing police violence. The large confidence intervals suggest in particular that, given that officer-involved fatalities are rare events, we might not have sufficient power to detect an effect.

Heterogeneity. What municipalities are driving the result? We address this question by looking at the heterogeneity of the effect on the violent crime clearance rate by different municipality characteristics. In particular, we estimate the baseline specification splitting the sample by whether municipalities are above or below the median for the characteristic being considered. We focus on characteristics that are salient for policing, such as share white or share Hispanic, and on characteristics that might be related to crime, such as income and unemployment. We report the results of this exercise in Appendix Figure XI. Panel (b) shows the estimates for violence clearance rate. Overall, the main effect on the violent crime clearance rate seems to quite consistent across different municipalities. However, the decrease in the violent crime clearance rate appears to be larger in municipalities with a higher minority share. Crucially, this is not explained by heterogeneous declines in crime coverage: Appendix Figure XI panel (a) shows that Sinclair cuts local coverage equally for all municipalities. **Decomposing the Main Effect.** Finally, to understand what variation is behind the main effect, we estimate a differences-in-differences specification with het-

[21] Fatal Encounters is a crowdsourced dataset that aims to document all deaths in the U.S. where police is present or involved. Given that a precise classification of incident types (e.g. incident involving intentional use of deadly force) is still being developed at present, we do not differentiate across different types of incidents here. While the data is notoriously challenging to collect and verify, FE 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).

erogeneous effects in baseline municipality exposure.[22] Appendix Figure XIII shows that after Sinclair enters a media market, non-covered municipalities experience an increase in their violent crime clearance rate. This might be explained by media market level trends correlated with Sinclair acquisitions, or it could be a direct effect of Sinclair entry, which is not necessarily surprising, since, as we previously discussed, Sinclair is a conservative media outlet, which might build support for tough on crime policies. Instead, covered municipalities do not experience a change in the violent crime clearance rate when compared to other covered municipalities. The overall negative coefficient in the triple difference-in-difference specification is therefore explained by a differential effect of Sinclair across covered and non-covered municipalities, in which had there not been a decline in the probability of being in the news, the violent crime clearance rate would have increased. Instead, the decline in crime coverage that is specific to covered municipalities fully undoes the effect.

7 Robustness Checks

In this section, we show that our results are robust to a number of potential concerns. To avoid repetitions, we discuss the robustness of our two main sets of results together in this section. Appendix Table XII and Appendix Table XIII show the robustness of the effect of Sinclair ownership on local crime coverage, while Appendix Table XIV and Appendix Table XV show the robustness of the effect of Sinclair on the violent crime clearance rate.

Robustness to Outcome Variable Definition. We begin by showing that the precise way in which we identify crime stories does not matter for the main result. In particular, Appendix Table XII column (1) shows that selecting crime stories based on the presence of bigrams that are five (as opposed to ten) times more likely to appear in the crime-related library versus the non crime-related library leaves the result virtually unchanged. The same is true for when we use bigrams that are more distinctively about crime, i.e. bigrams that are twenty times more likely to appear in the

[22] We estimate the following differences-in-differences specification with heterogeneous effects:

$$y_{mdt} = \beta Sinclair_{dt} + \sum_{k=2}^{8} \beta^k Sinclair_{dt} * CoveredQuantile_m^k + W'_{dt}\eta + \delta_t * X'_{m2010}\gamma + \delta_m + \delta_t + \epsilon_{mdt} \quad (6)$$

where y_{mdt} is the violent crime clearance rate of municipality m in year t, $Sinclair_{dt}$ is an indicator variable equal to 1 after Sinclair enters the media market, $CoveredQuantile_m^k$ is an indicator variable equal to 1 if a municipality is in the k-th coverage quantile, W_{dt} are media market time-varying controls, X_{m2010} are baseline municipality characteristics, δ_t are year fixed effects, and δ_{mm} are municipality fixed effects. W_{dt} includes population, share male, share male 15 to 30, share white, share Hispanic, unemployment rate, and income per capita. X_{m2010} includes the same variable as the main specification with the addition of state fixed effects. The figure shows the β coefficient for the first quantile, and the linear combination of β and β^k for all other quantiles, together with 95% confidence intervals.

crime-related library versus the non crime-related library (column (2)). In addition, the main effect is unchanged when we segment newscasts into stories using a fixed number of tokens for each story (column (3)). Appendix Table XIV columns (1) and (2) show that the results are unchanged in we use respectively unadjusted or non-winsorised violent crime clearance rates.

Robustness to Sample Restrictions. Appendix Table XII column (4) shows that the main effect on content is unchanged by restricting the sample to municipalities that report crime data to the UCR and have at least one violent and one property crime every year 2010-2017.

Robustness to Definition of Sinclair Control. In the baseline analysis, we consider a station to be controlled by Sinclair in all months after acquisition, independently on whether Sinclair retains control of the station in a given media market or not. In Appendix Table XII column (5) and Appendix Table XIV column (3), we show that dropping the three stations that were divested by Sinclair in the 2010 to 2017 period does not make a difference.

Next, we consider whether focusing on stations that were directly owned and operated by Sinclair impacts our result. Column (6) of Appendix Table XII shows no difference in the result if we focus on stations over which Sinclair had tighter control. The same is true in column (4) of Appendix Table XIV.

In addition, Appendix Table XII column (7) and Appendix Table XIV column (5) show that the main results are unchanged when we only include markets that were entered by Sinclair as part of multi-station deals. This is reassuring to the extent that we might be less concerned of endogenous entry of Sinclair when the station is acquired as part of a deal to acquire multiple stations, as opposed a single one.

Finally, we show in Appendix Table XII column (6) that the main effect on clearance rates is robust to considering a media market to be treated in a given year if Sinclair owns one of the media market's stations in December of that year, i.e. if a partially treated year is considered to be treated.

Robustness to Staggered Timing. Recent developments in the differences-in-differences literature have highlighted that when these designs exploit the staggered timing of treatment, the estimate recovered is a weighted average of underlying two-by-two differences-in-differences estimates (Goodman-Bacon, 2019). This is potentially problematic given that weights can be negative, which means that even if all underlying two-by-two effects are positive, they might be aggregated to a negative coefficient in the estimation. No formal extension to these concepts to triple differences-in-differences exists at the moment. Nonetheless, we believe the issue to be limited in this case because negative weights arise from using earlier treated units as control for later treated units. Instead, in our setting, we have many never treated and always treated stations, which suggests that most of the weight is going to come from these types of comparisons.

Nonetheless, in Appendix Table XIII and in Appendix Table XV, we present results when we repeat the estimates artificially eliminating staggered timing. In particular, in the regressions reported in Appendix Table XIII, we include only stations that are never treated, stations that are always treated, and stations that are acquired at four separate points in time corresponding to the acquisition of smaller broadcast groups. Out of the four moments in time we consider, three reproduce a negative and significant coefficient. The magnitude of the effect is larger in two of them, but larger standard errors produce confidence intervals consistent with the main point estimate. If we focus on stations acquired from Barrington in November 2013 only, we find a negative effect that is similar in magnitude, but is not statistically significant. In Appendix Table XV, we restrict the sample to media markets that were never exposed to Sinclair, media markets that were always exposed to Sinclair, and media markets that were acquired by Sinclair in the year specified in the column header. We only perform a separate estimation for years in which Sinclair entered of more than three media markets. In all years but 2015, we find a negative coefficient (significant in two out of three specifications).

8 Mechanisms

Why does the decline in local crime coverage affect clearance rates? The mechanism that we put forward is that when stories about a municipality's violent crimes are less likely to appear in local news, crime becomes less salient in the eyes of local citizens. As a result, the pressure that these citizens put on the local police to clear these violent crimes decreases, inducing police officers to alter their effort allocation away from clearing violent crimes in favor of other policing related activities.[23] In this section, we provide three pieces of evidence supporting this mechanism but also discuss alternative explanations such as direct media monitoring and community cooperation.

Salience of Crime and Police. 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. Implementing our empirical strategy requires highly localized but nationally representative data on perceptions over time, which is not available to the best of our knowledge. For example, most surveys specifically about crime perceptions (e.g. Gallup Poll Social Series) have a limited sample; instead, general surveys such as the Cooperative Congressional Election Study only ask about crime and law enforcement in a few waves and often change the specific question asked.

[23] It is important to note that we are not able to draw welfare conclusion from our analysis, as it is unclear what the optimal arrest rate looks like. According to theories of "de-policing" (Owens, 2019), it is possible that decreasing arrest rates might be socially optimal.

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".[24] Because the Google trends data are not consistently available below the media market level, we run a difference-in-differences model exploiting the staggered entry of Sinclair across media markets.[25] 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 never censored.

Table V reports the findings of our analysis. The estimates show that when Sinclair enters a media market, the volume of monthly searches containing the keywords crime and police decreases by 4%. The effect is not explained by an overall and 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 the local news. Importantly, this goes in the opposite direction as one would expect based on actual crime rates that are, if anything, higher after Sinclair enters a media market.

Changes in House Prices Suggestive of Decreased Concern of Crime. To further investigate whether a decrease in local crime coverage affects public opinion, we turn our attention to house prices and rents. In a hedonic model of house prices, crime rates and crime perceptions are amenities that are reflected in house prices by market mechanisms: we can use these prices as a proxy for crime perceptions. Data on monthly municipality-level house prices and rents are from Zillow.[26] We estimate an event-study specification similar to the one in equation (5). Given that house prices and rents are likely to reflect local level conditions and crime levels, we additionally interact baseline municipality characteristics with year dummies, and also control for the monthly violent crime rate and the property crime rate. Appendix Figure XIV reports the coefficient estimates from the event study specification. The figures show some suggestive but imprecisely estimated evidence of house prices and rents being higher in covered relative to non-covered municipalities after Sinclair

[24] 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). Importantly, the Google trends API limits the number of geographic location per query to five. We ensure comparability across media markets and time by including 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). We modify the script provided by Goldsmith-Pinkham and Sojourner (2020) to query the Google trends API.

[25] The Google trends API censors observations that are a below a unknown threshold. Monthly Google trends data by municipality are censored with a very high frequency, which makes it impossible to construct a panel of municipalities over time.

[26] In particular, we use the Zillow Home Value Index (ZHVI) and the Zillow Rent Index (ZRI), which provide municipality level monthly estimates of market rate home values prices and rental prices, expressed in logarithms.

enters a media market.

Electoral Feedback. Perceptions become reality when it is election time. If the change in local news coverage makes crime less salient, this will be reflected in political participation and voting choices. Unfortunately, there exists no data on political platforms at the local level that would allow us to test this hypothesis directly, but 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.[27]

Appendix Figure XI 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 with respect 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 public opinion pressure in the absence of elections. In addition, Goldstein (2019) shows that this age group is also especially concerned with crime and policing. Consistent with this argument, Table VI shows that the effect on the violent crime clearance rate appears to be driven by cities with a larger share of population above 55 years old (p-value = 0.20), 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 suggestive evidence that a change in public opinion might be behind the main effect on clearance rates.

Direct Media Monitoring. An alternative mechanism is that there could be a decrease in the direct media monitoring of the police. If police officers anticipate a low probability of being reported in the news for failing to solve crimes, they might shirk on the amount of effort allocated 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 VII, 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

[27] While the extent to which police officers are influenced by politics and the public is debated, recent research has highlighted that managerial directives can have important effects on police behavior, supporting the idea that public opinion might influence the effort allocation of the officers (Ba and Rivera, 2019; Goldstein et al., 2020; Mummolo, 2018).

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 the police officers are updating their overall probability of being the subject of reporting based on the overall decline in crime coverage.

Community Cooperation. Finally, 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 similar stories about arrests, so they should perceive the police to be equally effective.[28]

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 to solve 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.

9 Conclusion

In this paper, we study the effect of a shock in local news content on a prototypical local bureaucracy: municipal police departments in the U.S.. 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 timing of acquisitions with cross-sectional variation in exposure to the local news shock in a triple differences-in-difference 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

[28] 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.

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 27% of the outcome mean in 2010.

How does police behavior change in response to the change in local news content? 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 with respect to non-covered municipalities. The effect is significant at the 1% level and corresponds to a decrease to 8% of the baseline mean. Instead, 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 and police 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.

This paper shows that shocks to local media content driven by acquisitions can affect the behavior of the police. Overall, this suggests that the increase in ownership concentration currently characterizing the local TV market in the United States might have important consequences for local institutions.

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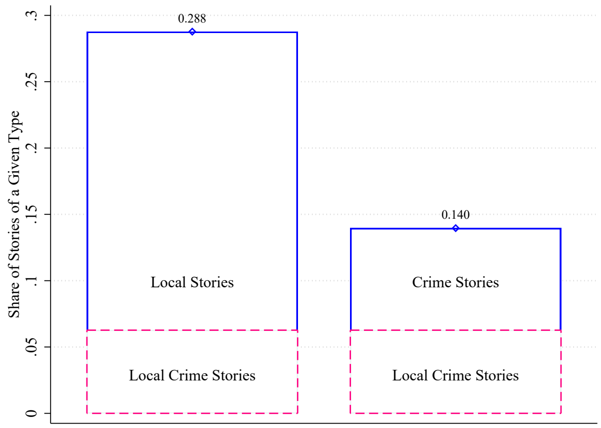
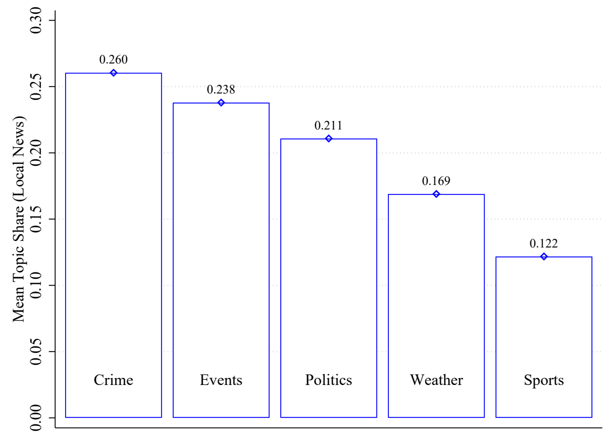
Figures

Figure I: Local TV News Content



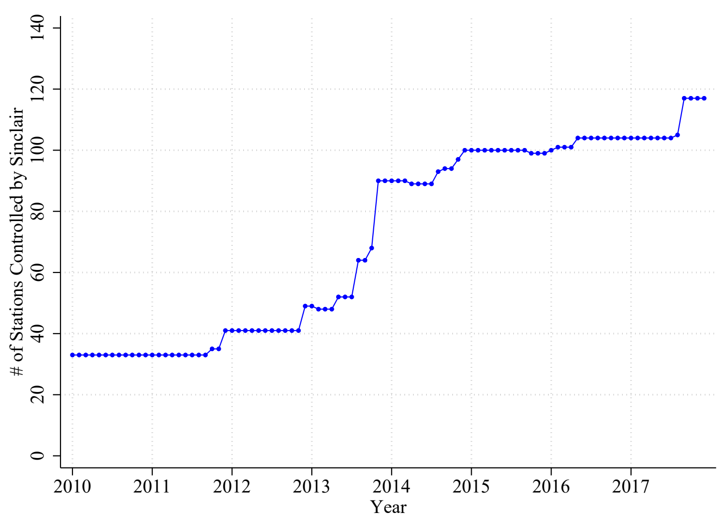
(a) Types of News Stories



(b) Local News Topics

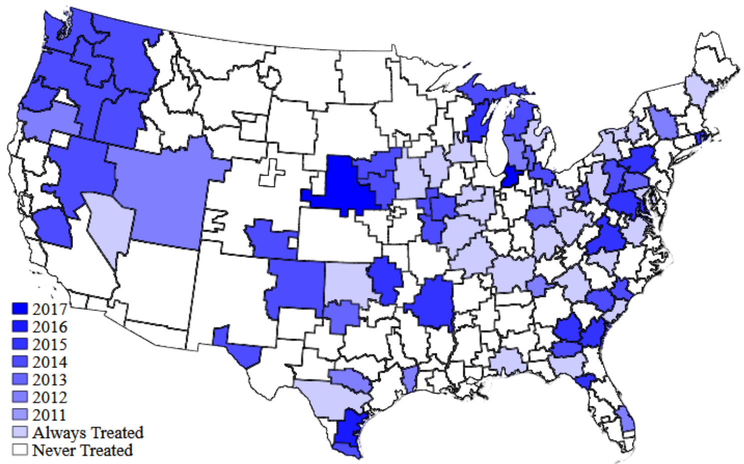
Notes: This figure describes local TV news content. Panel (a) shows the share of stories that are local, that are about crime, and both local and about crime. A story is local if it mentions at least one of the municipalities with more than 10,000 people in the media market. A story is about crime if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



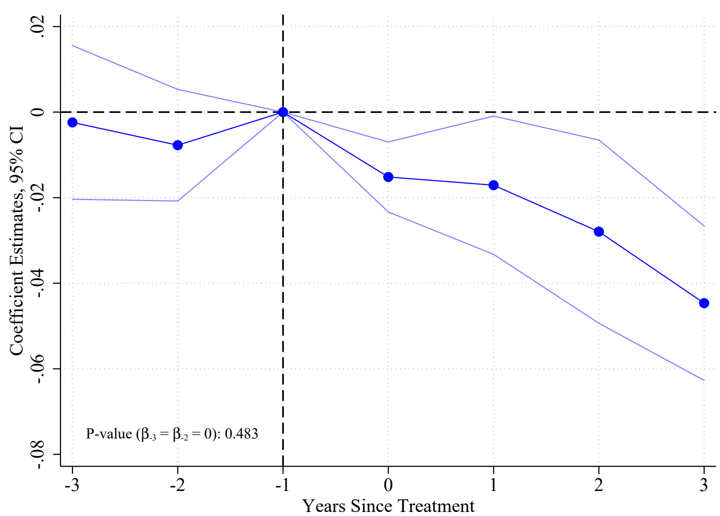
Notes: This figure shows the number of big-four affiliate stations controlled by Sinclair in each month from January 2010 to December 2017. A station is considered controlled by Sinclair if it is owned and operated by the Sinclair Broadcast Group, if it is owned and operated by Cunningham Broadcasting,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

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



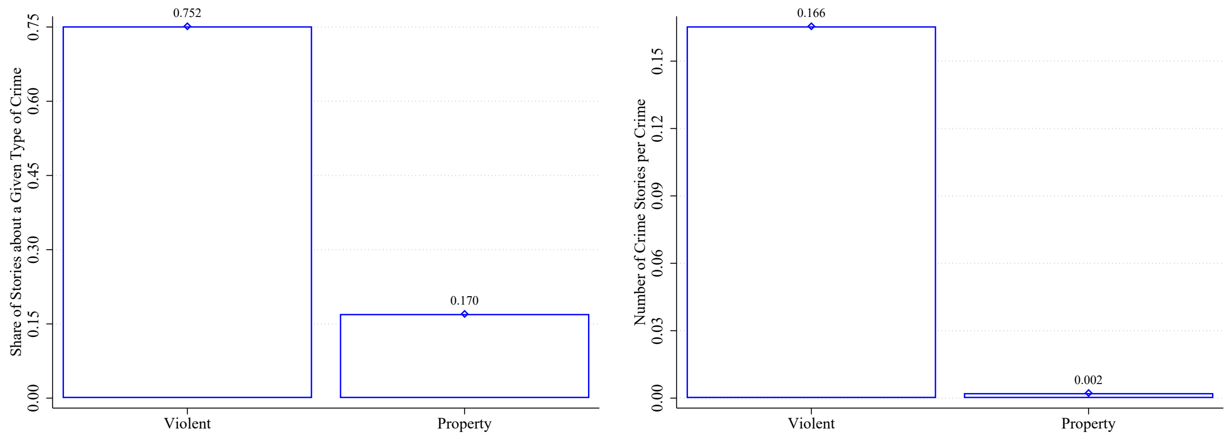
Notes: This map shows year of Sinclair entry across media markets in the United States. Darker colors correspond to later entry. Never treated are media markets that never experience Sinclair entry; always treated are media markets that have at least one station controlled by Sinclair at the beginning 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

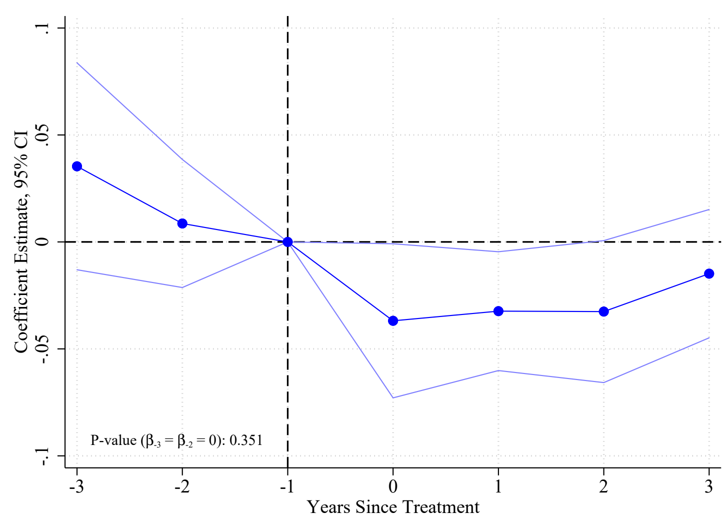


(a) Share of Crime Stories

(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 and theft). Panel (b) shows the average number of crime stories per reported offense across municipalities. 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.

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***	-0.021***	-0.014***	-0.022***
	(0.007)	(0.007)	(0.005)	(0.007)
Non-Sinclair Stations in Sinclair				-0.005
Media Market * Covered				(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				.038
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 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	-0.042***	-0.038***	-0.037**	-0.036***
	(0.014)	(0.013)	(0.016)	(0.014)
Observations	15488	15488	11680	15488
Clusters	112	112	108	112
Municipalies	1936	1936	1460	1936
Outcome Mean in 2010	0.467	0.467	0.472	0.467
Media Market by Week FE	X	X	X	X
Covered by Week 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 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.

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

Dependent Variable	Violent Crime Clearance Rate	By Type of Crime: Murder	Assault	Robbery	Rape
	(1)	(2)	(3)	(4)	(5)
	Panel A: Full Sample				
Sinclair * Covered	-0.038***	0.072	-0.014	-0.063**	-0.052**
	(0.013)	(0.088)	(0.018)	(0.025)	(0.025)
Observations	15488	7723	14016	15045	14572
Clusters	112	111	111	112	112
Municipalities	1936	1479	1760	1931	1920
Outcome Mean in 2010	0.467	0.694	0.600	0.346	0.392
	Panel B: Balanced Sample				
Sinclair * Covered	-0.026*	-	-0.012	-0.037	-0.022
	(0.013)	-	(0.017)	(0.033)	(0.031)
Observations	11216	-	11216	11216	11216
Clusters	111	-	111	111	111
Municipalities	1402	-	1402	1402	1402
Outcome Mean in 2010	0.503	-	0.591	0.374	0.424
Media Market by Week FE	X	X	X	X	X
Covered by Week 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 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 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. 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.

Table IV: 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	-0.006	-0.005	0.007
	(0.006)	(0.008)	(0.008)	(0.013)
Observations	15488	15472	15486	15422
Clusters	112	112	112	112
Municipalities	1936	1936	1936	1935
Outcome Mean in 2010	0.198	0.135	0.218	0.179
Media Market by Week FE	X	X	X	X
Covered by Week 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 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.

Table V: 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 VI: 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	-0.057**	-0.015
	(0.023)	(0.018)
Observations	7672	7664
Clusters	99	97
Municipalities	959	958
Outcome Mean in 2010	0.465	0.469
Media Market by Week FE	X	X
Covered by Week 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 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.

Table VII: 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	-0.021***	-0.002
	(0.007)	(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 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.

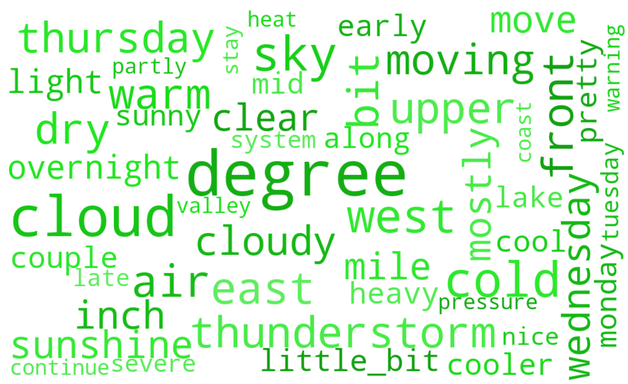
Appendix Figures

Appendix Figure I: Local News Topics

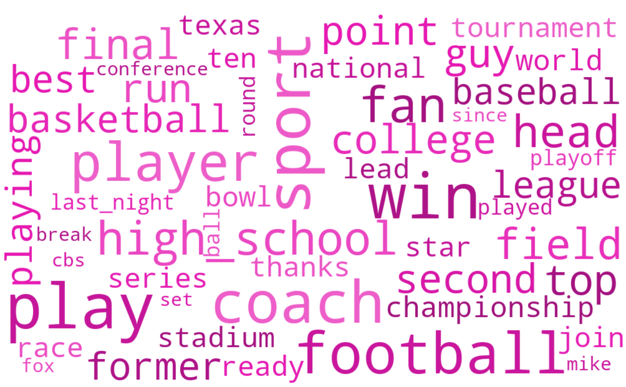
(a) Crime

(b) Events

(c) Politics



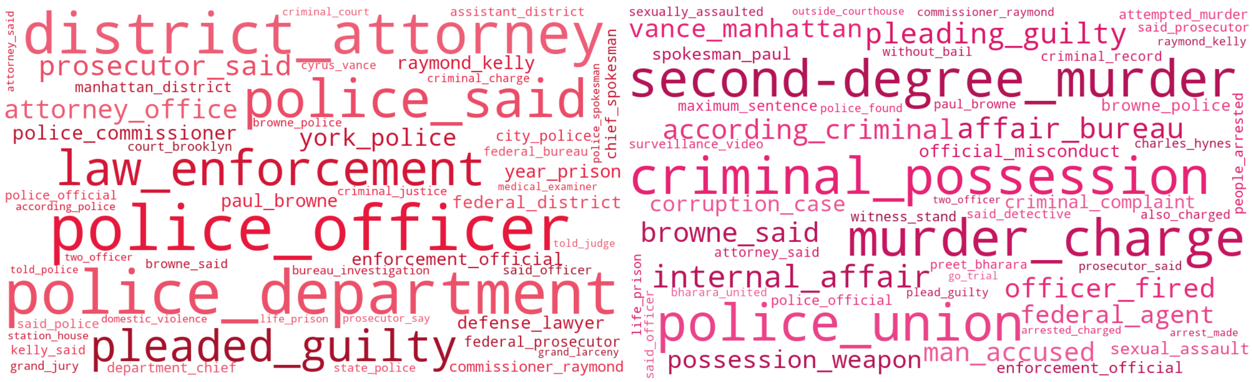
(d) Weather



(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.

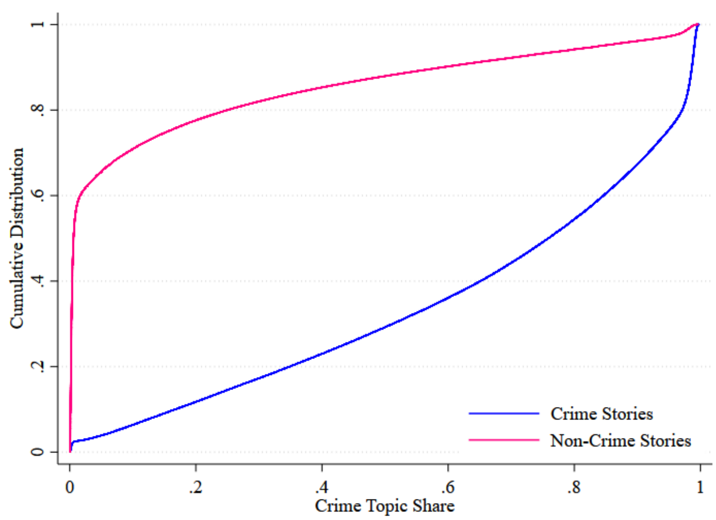
Appendix Figure II: Crime Bigrams, by Highest Frequency and Highest Relative Frequency



(a) Frequency (b) Relatively Frequency

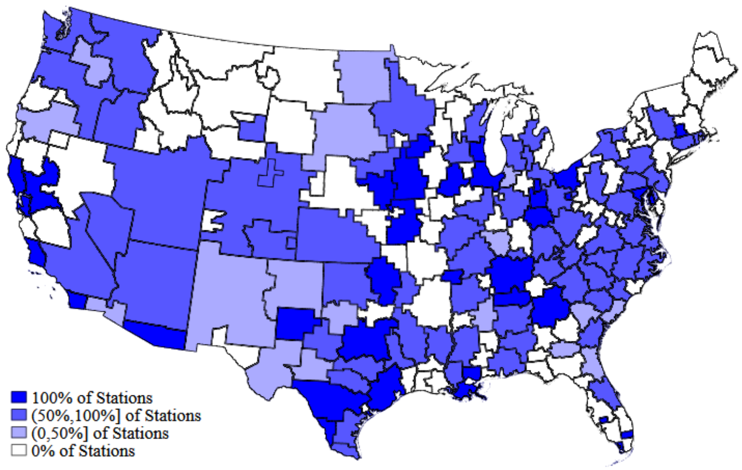
Notes: This figure shows word clouds of the top 50 bigrams that we use to identify crime stories by frequency (panel (a)) and by relative frequency (panel (b)). The size of the words is proportional to their absolute and relative frequency.

Appendix Figure III: Classification of Local Stories: Validation



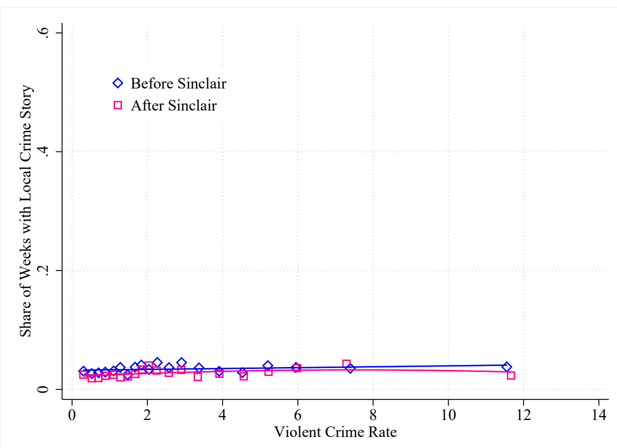
Notes: This figure shows the cumulative distribution of the crime topic share separately by whether local stories are classified to be about crime or not according to the methodology described in Section 3. Crime topic shares are from an unsupervised LDA model trained on local crime stories. Stories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

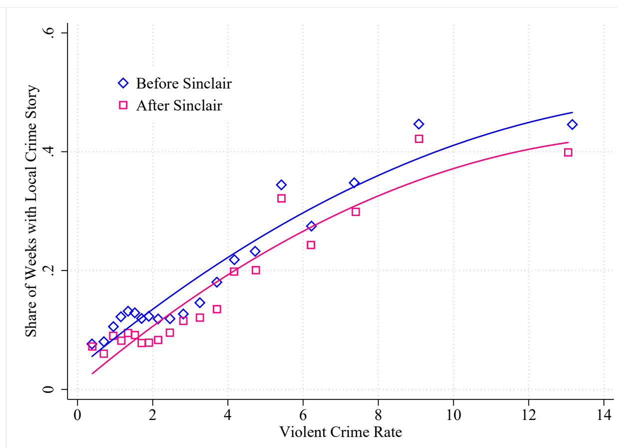


Notes: This map shows the share of stations for which we have content data continuously from 2010-2017 across media markets in the United States. Darker colors correspond to higher shares of media market stations included in the content data. 61% of media market have at least one station included in 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



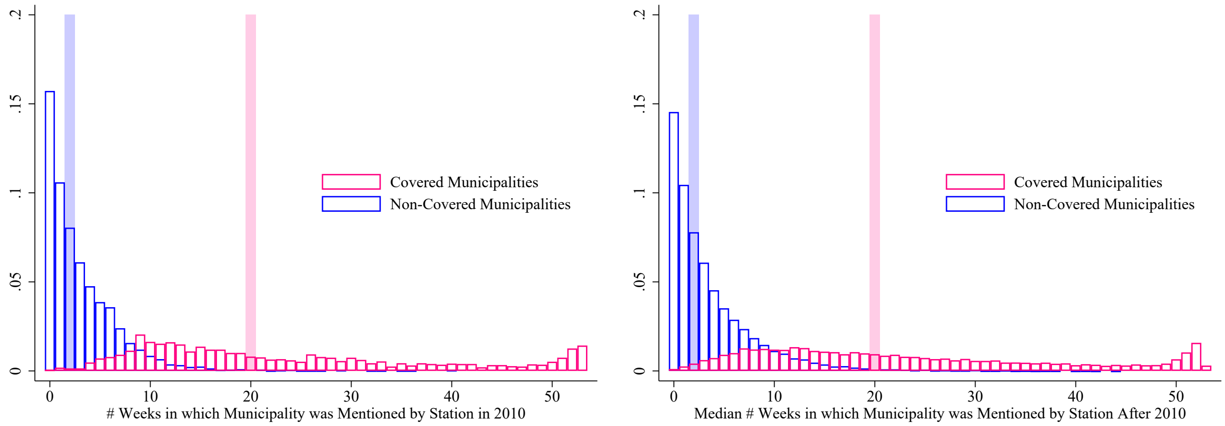
(a) Non-Covered Municipalities



(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.

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

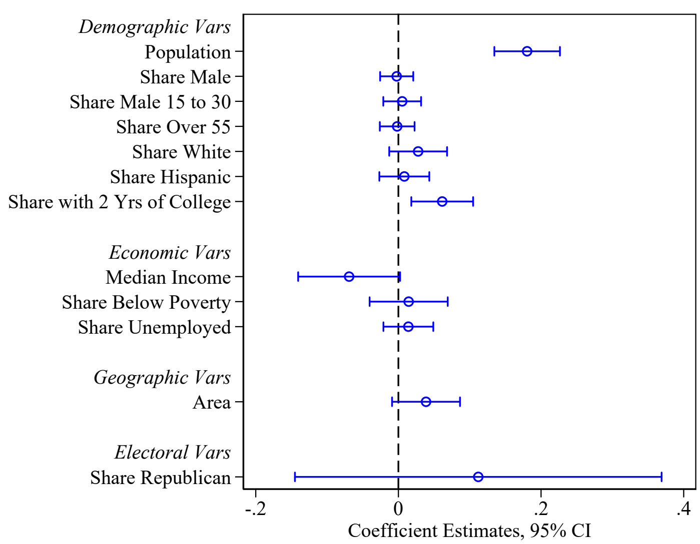


(a) In 2010

(b) After 2010

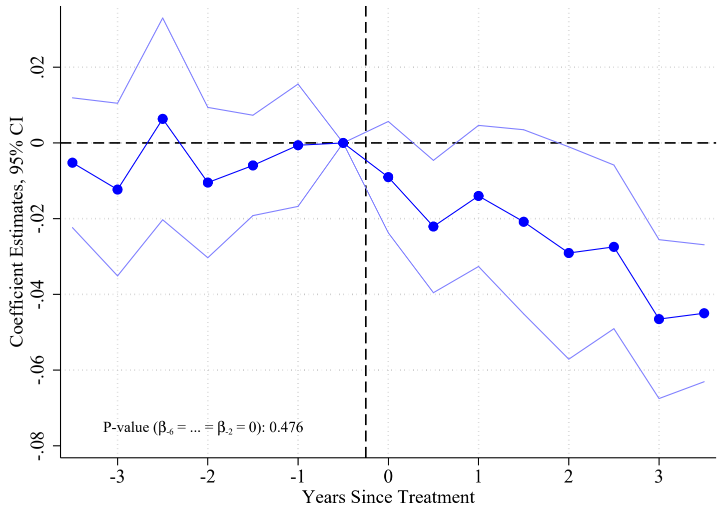
Notes: This figure shows that covered status persists over time. Panel (a) presents a histogram of the number of weeks in which the municipality was mentioned by the station in 2010, by whether the municipality is covered at baseline or not. Panel (b) presents a histogram of the median number of weeks 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



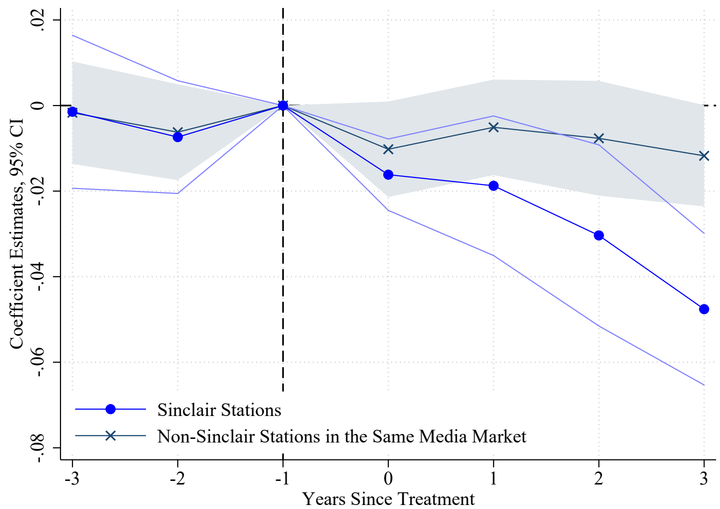
Notes: This figure shows along which dimensions covered and non-covered municipalities differ. We report coefficient estimates together with 95% confidence intervals from a regression of an indicator variable for the municipality being covered at baseline on socio-economic characteristics of the municipality and media market fixed effects. The sample is restricted to media markets that never experience Sinclair entry. Standard errors are clustered at the media market level.

Appendix Figure VIII: Effect of Sinclair Control on the Probability of Having a Local Crime Story, by Semester since Treatment



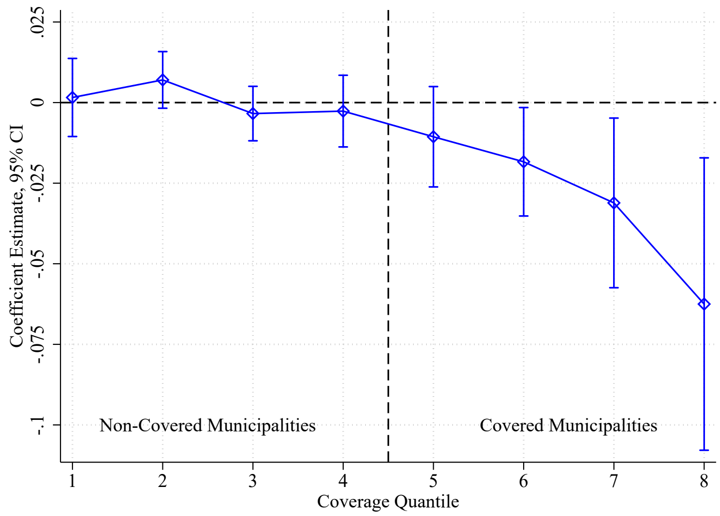
Notes: This figure shows the effect of Sinclair control on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities, by semester since treatment. We report coefficient estimates and 95% confidence intervals from a regression of an 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 semester since treatment.

Appendix Figure IX: 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



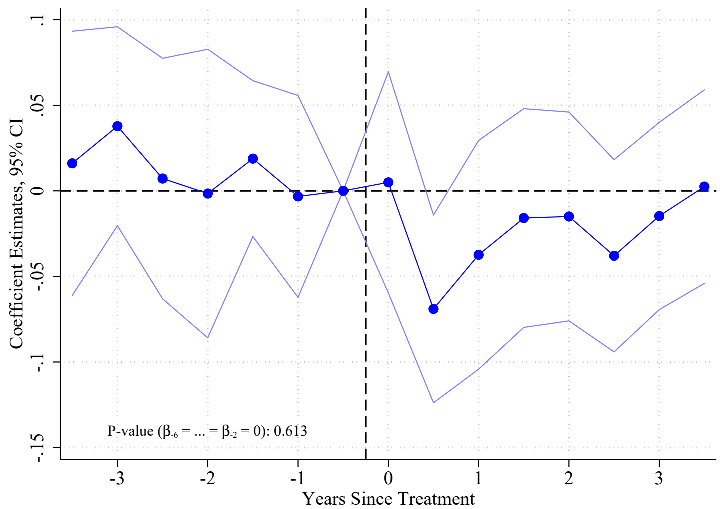
Notes: This figure shows the effect of Sinclair entry, separately for stations directly controlled by Sinclair and for same media market stations not directly controlled by Sinclair, on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities,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

Appendix Figure X: Decomposition of the Main Effect on the Probability of Having a Local Crime Story, Differences-in-Differences Specification



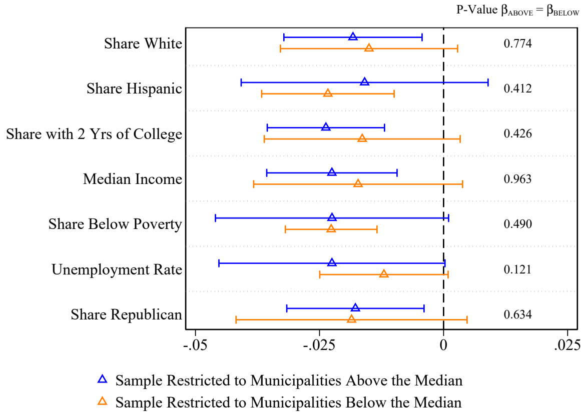
Notes: This figure reports estimates from a differences-in-differences specification with heterogeneous effects in baseline municipality exposure. We regress the share of weeks in a month in which the station reported a local crime story about a municipality on an indicator variable for the station being under Sinclair control, the interaction between an indicator variable for the station being under Sinclair control and dummies for the municipality's coverage quantile, the interaction between month fixed effects and municipality characteristics, time-varying media market characteristics, state by month fixed effects, month fixed effects, and station by municipality fixed effects (equation (3)). The municipality 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. The media market characteristics include log population, share male, share male 15 to 30, share white, share Hispanic, unemployment rate, and log income per capita. The figure shows the β coefficient for the first quantile, and the linear combination of β and β^k for all other quantiles, together with 95% confidence intervals. 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 Figure XI: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by Semester since Treatment

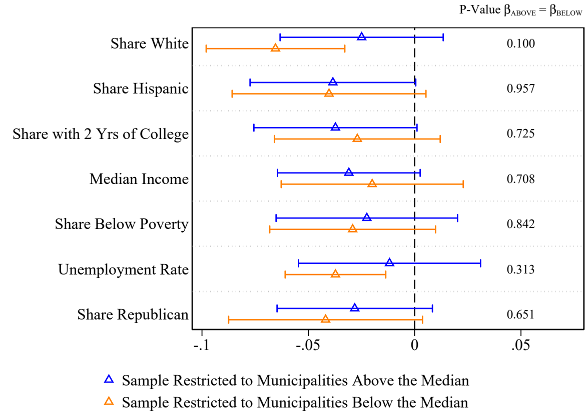


Notes: This figure shows the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities, by semester since treatment. We report coefficient estimates and 95% confidence intervals from a regression of the municipality's violent crime clearance 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. The dataset is a municipality by year panel. Treatment is defined at the semester level. A media market is considered treated in a given year if Sinclair was present in the market at the beginning of that semester. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes.

Figure XII: Heterogeneous Effects by Municipality Characteristics



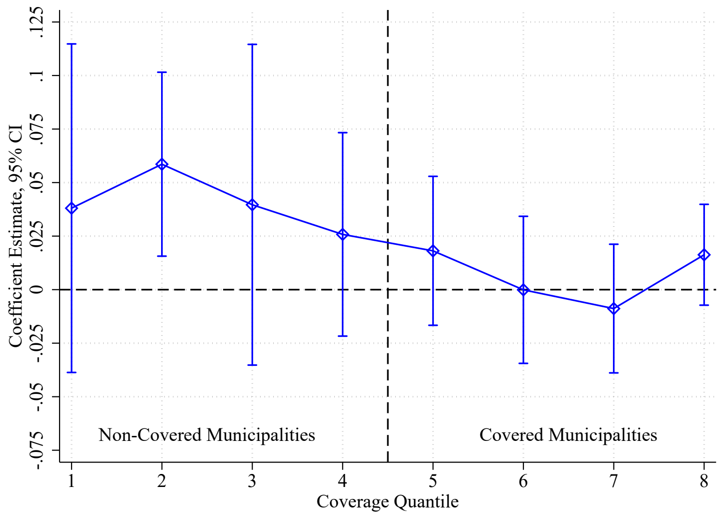
(a) Local Crime Reporting



(b) Violence Crime Clearance Rate

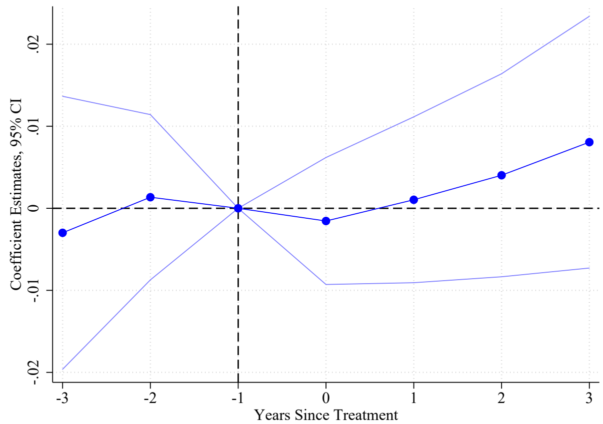
Notes: This figure presents the heterogeneity of the effect of Sinclair entry on local crime reporting (panel (a)) and on the violent crime clearance rate (panel (b)). For each municipality characteristic, we estimate a separate regression model for municipalities above and below the median according to the characteristic. The p-value reported is from a test of equality of the main coefficients across the two samples.
In panel (a), 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 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)). 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.
In panel (b), we report coefficient estimates and 95% confidence intervals from a regression of 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.

Appendix Figure XIII: Decomposition of the Main Effect on Violent Clearance Rates, Difference-in-Differences Specification

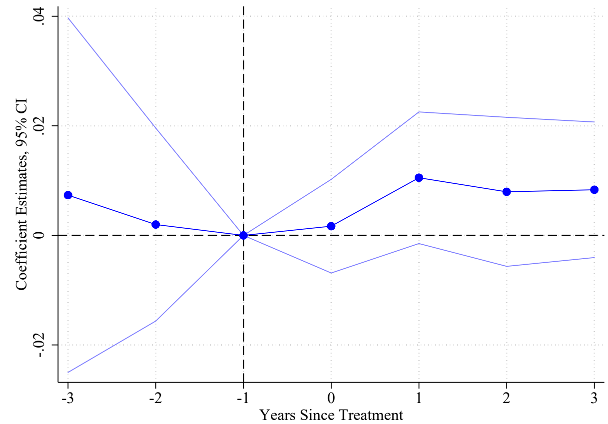


Notes: This figure reports estimates from a differences-in-differences specification with heterogeneous effects in baseline municipality exposure. We regress the municipality's violent crime clearance rate on an indicator variable for Sinclair presence in the media market, the interaction between an indicator variable for Sinclair presence in the media market and dummies for the municipality's coverage quantile, the interaction between year fixed effects and municipality characteristics, time-varying media market characteristics, state by year fixed effects, year fixed effects and municipality fixed effects (equation (6)). The municipality 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. The media market characteristics include log population, share male, share male 15 to 30, share white, share Hispanic, unemployment rate and log income per capita. The figure shows the β coefficient for the first quantile, and the linear combination of β and β^k for all other quantiles, together with 95% confidence intervals. 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.

Figure XIV: Effect of Sinclair Control on House Prices and Rents



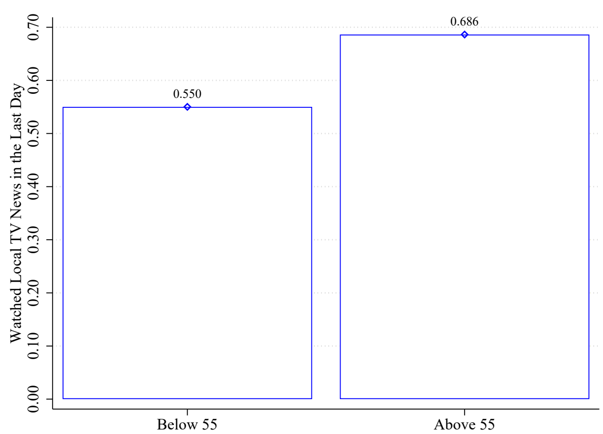
(a) House Prices



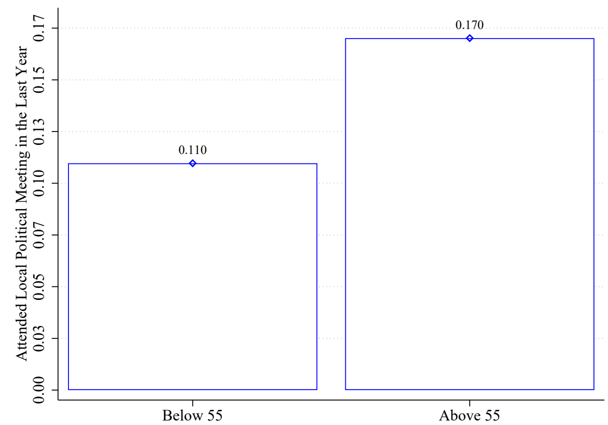
(b) Rents

Notes: This figure shows the effect of Sinclair entry on house prices (panel (a)) and rents (panel (b)) 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 outcome on the interaction between indicator variables for years since Sinclair entry and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair entry and year fixed effects, the municipality's violent and property crime rates, media market by month fixed effects, covered status by month fixed effects, and municipality fixed effects (similar to equation (5)). 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. 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 monthly level, but the effect is constrained to be the same by year since treatment. House prices and rents are in logs. Crime rates are crimes per 1,000 people.

Figure XV: Local News Vierwership 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	1936	33.385	16.843	3.011	94.073	2584	33.344	16.907	3.011	94.073	0.963
Violent Crime Rate	1936	3.536	3.250	0.057	19.218	2584	3.676	3.308	0.057	20.030	0.171
Property Crime Clearance Rate	1936	0.198	0.127	0.000	0.841	2584	0.198	0.126	0.000	0.841	0.934
Violent Crime Clearance Rate	1936	0.467	0.254	0.000	1.250	2584	0.470	0.253	0.000	1.250	0.832
	Panel C: Municipality Characteristics										
Population	2201	54441	145318	10008	3772486	2849	54537	199663	10008	8078471	1
Share Male	2201	0.487	0.025	0.346	0.863	2849	0.487	0.026	0.282	0.863	0.589
Share Male 15-30	2201	0.227	0.073	0.012	0.758	2849	0.229	0.074	0.012	0.803	0.368
Share Over 55	2201	0.232	0.067	0.035	0.945	2849	0.235	0.067	0.035	0.945	0.044
Share White	2201	0.753	0.186	0.012	0.989	2849	0.758	0.185	0.012	0.990	0.376
Share Hispanic	2201	0.155	0.186	0.000	0.987	2849	0.155	0.190	0.000	0.987	0.989
Share with 2 Years of College	2201	0.367	0.153	0.052	0.889	2849	0.362	0.151	0.031	0.889	0.255
Median Income	2201	55.640	22.517	17.301	209.231	2849	54.486	22.388	17.301	237.135	0.318
Share Below Poverty Line	2201	0.133	0.078	0.012	0.487	2849	0.137	0.079	0.012	0.487	0.170
Share Unemployed	2201	0.079	0.032	0.015	0.332	2849	0.081	0.033	0.014	0.332	0.222
Log Area	2201	17.431	0.959	14.595	21.486	2849	17.379	0.987	13.136	21.486	0.270
Share Republican	2201	0.473	0.159	0.005	0.877	2849	0.468	0.157	0.005	0.880	0.308

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 inhabitants. 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 full sample includes 2862 municipalities; 2584 of these municipalities have an independent police department (i.e. a police department that does not depend on a sheriff's office), report crime data to the UCR, and have at least one violent and one property crime every year 2010-2017. The content data covers 2201 municipalities; the crime data includes 1936 of them. Content and crime and clearance rates are measured in 2010. Crime rates are defined as crimes per 1,000 people and clearance rates as total number of crimes cleared by arrest or exceptional means over total number of crimes. Municipality characteristics are from the 2006-2010 American Community Survey.

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.

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

Dependent Variable	Had Local Story	Decomposition: Crime	Decomposition: Non-Crime
	(1)	(2)	(3)
Sinclair * Covered	-0.038***	-0.021***	-0.017
	(0.012)	(0.007)	(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 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 V: Effect of Sinclair Control on Overall Crime Coverage, by Whether Story is Local

Dependent Variable	Share of Stories about Crime	Decomposition: Local	Decomposition: Non-Local
	(1)	(2)	(3)
Sinclair	-0.009*	-0.011***	0.002
	(0.005)	(0.004)	(0.003)
Observations	135014	135014	135014
Clusters	112	112	112
Municipalities	323	323	323
Outcome Mean in 2010	0.131	0.061	0.070
Station FE	X	X	X
Week 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 week fixed effects, station fixed effects, and week 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 week panel. Treatment is defined at the monthly level.

Appendix Table VI: 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***	-0.019
	(0.006)	(0.012)
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 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 VII: 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	-0.017***	-0.004**
	(0.006)	(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 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 VIII: Effect of Sinclair Entry on the Violent Crime Rate, by Type of Crime

Dependent Variable	Violent Crime Rate	Murder	Assault	Robbery	Rape
		By Type of Crime			
	(1)	(2)	(3)	(4)	(5)
	Panel A: Crime Rates				
Sinclair * Covered	0.105	0.002	0.077	0.050*	-0.025
	(0.115)	(0.004)	(0.096)	(0.028)	(0.028)
Observations	15488	15488	15488	15488	15488
Clusters	112	112	112	112	112
Municipalities	1936	1936	1936	1936	1936
Outcome Mean in 2010	3.536	0.037	2.203	0.961	0.324
	Panel B: Dummy = 1 if at least one Crime				
Sinclair * Covered	-	0.022	0.008	-0.003	0.032*
	-	(0.033)	(0.007)	(0.010)	(0.016)
Observations	-	15488	15488	15488	15488
Clusters	-	112	112	112	112
Municipalities		1936	1936	1936	1936
Outcome Mean in 2010	-	0.478	0.904	0.967	0.941
Media Market by Week FE	X	X	X	X	X
Covered by Week FE	X	X	X	X	X
Municipality FE	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X

Notes: This table shows the effect of Sinclair entry on the crime rates of covered municipalities relative to non-covered municipalities, for different types of violent crimes. We regress the municipality's crime rate for a given type of violent crime on the interaction between between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (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. 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.

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

		By Type of Crime		
Dependent Variable	Property Crime Rate	Burglary	Theft	Motor Vehicle Theft
	(1)	(2)	(3)	(4)
Sinclair * Covered	1.195*	0.599***	0.706	0.111
	(0.685)	(0.219)	(0.569)	(0.073)
Observations	15488	15488	15488	15488
Clusters	112	112	112	112
Municipalities	1936	1936	1936	1936
Outcome Mean in 2010	33.385	7.156	24.208	2.010
Media Market by Week FE	X	X	X	X
Covered by Week 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 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.

Appendix Table X: 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	-0.005	-0.003*	0.002	-0.034	-0.017
	(0.005)	(0.001)	(0.154)	(0.028)	(0.021)
Observations	9325	9325	10512	15487	15487
Clusters	110	110	112	112	112
Municipalies	1512	1512	1669	1936	1936
Outcome Mean in 2010	0.239	0.019	2.960	2.368	1.851
Media Market by Week FE	X	X	X	X	X
Covered by Week 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, media market by year fixed effects, covered status by year fixed effects, covered status by media market fixed effects, and municipality fixed effects (equation (4)). Outcome variables are normalized by 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.

Appendix Table XI: Effect of Sinclair Entry on Police Violence

Dependent Variable	Any Fatality	Any White Fatality	Any Minority Fatality
	(1)	(2)	(3)
Sinclair * Covered	-0.040	-0.031	-0.004
	(0.032)	(0.026)	(0.020)
Observations	15488	15488	15488
Clusters	112	112	112
Municipalies	1936	1936	1936
Outcome Mean in 2010	0.141	0.057	0.072
Media Market by Week FE	X	X	X
Covered by Week FE	X	X	X
Municipality FE	X	X	X
Sinclair * Controls	X	X	X

Notes: This table shows the effect of Sinclair entry on the probability of experiencing an officer-involved fatality in covered municipalities relative to non-covered municipalities. We regress an indicator variable equal to 1 if the municipality experienced an officer-involved fatality 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, covered status by media market fixed effects, and municipality fixed effects (equation (4)). 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. Data on officer-involved fatality is from Fatal Encounters.

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

Dependent Variable	Had Local Crime Story						
Robustness to…	Less Restrictive Crime Story Definition	More Restrictive Crime Story Definition	Fixed Division of Newscasts into Stories	Same Sample as UCR Analysis	Drops Divested Stations	Treatment Based Only on Stations Owned and Operated by Sinclair	Group Acquis. Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sinclair * Covered	-0.023***	-0.019***	-0.025***	-0.021***	-0.021***	-0.021***	-0.018**
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)
Observations	3065194	3065194	3065194	2693174	3058924	3065194	3051818
Clusters	112	112	112	112	112	112	111
Municipalities	2201	2201	2201	1936	2201	2201	2193
Stations	323	323	323	323	321	323	319
Outcome Mean in 2010	0.096	0.070	0.106	0.096	0.089	0.089	0.088
Station by Week FE	X	X	X	X	X	X	X
Covered by Week FE	X	X	X	X	X	X	X
Station by 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 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 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) 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 (2) 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 (3) segments the newscasts into stories using a fixed number of tokens per story (see Appendix A for further details). Column (4) restricts the sample to municipalities also included in the crime analysis. Column (5) drops stations that were eventually divested from the sample. Column (6) restricts treatment to stations owned and operated by Sinclair. Column (7) 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 XIII: Effect of Sinclair Control on the Probability of Having a Local Crime Story, No Staggered Timing

Dependent Variable	Had Local Crime Story			
Restricted to Stations Treated in…	December 2012 (Newport)	August 2013 (Fisher)	November 2013 (Barrington)	August 2014 (Allbritton)
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.045***	-0.022***	-0.047	-0.014**
	(0.010)	(0.006)	(0.052)	(0.007)
Observations	2927254	2948154	2924328	2926000
Clusters	111	111	111	111
Municipalities	2193	2193	2193	2193
Stations	301	300	302	300
Outcome Mean in 2010	0.087	0.086	0.087	0.086
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	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 to eliminating variation in treatment coming from the staggered timing of Sinclair control. In particular, we restrict the sample to stations that were never under Sinclair control, stations always under Sinclair control, and stations that were acquired by Sinclair in the month specified in the column header. We only estimate separately months in which Sinclair acquired control of more than three stations. 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 monthly level.

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

Dependent Variable	Violent Crime Clearance Rate					
Robustness to…	Unadjusted	Non-Windorized Measure	Drops DMAs with Divested Stations	Treatment Based Only on Stations Owned and Operated by Sinclair	Group Acquis. Only	Year Treatment based on December Status
	(1)	(2)	(3)	(4)	(5)	(6)
Sinclair * Covered	-0.035**	-0.045***	-0.038***	-0.029**	-0.040***	-0.039***
	(0.015)	(0.016)	(0.013)	(0.012)	(0.014)	(0.012)
Observations	15200	15488	15208	15488	14952	15488
Clusters	112	112	107	112	104	112
Municipalities	1936	1936	1901	1936	1869	1936
Outcome Mean in 2010	0.458	0.469	0.470	0.467	0.465	0.467
Media Market by Week FE	X	X	X	X	X	X
Covered by Week FE	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X	X

Notes: This table shows the robustness of the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities. We regress the municipality's violent crime clearance rate on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (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. Column (1) uses an unadjusted version of the outcome. Column (2) does not winsorize clearance rates. Column (3) drops media markets with stations that were eventually divested. Column (4) defines the year to be treated if Sinclair was present in the market in the December of that year. Column (5) restricts treatment to media markets with stations owned and operated by Sinclair. Column (6) drops markets that were entered by Sinclair not as part of multi-station deals. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is 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 unless otherwise specified. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes.

Appendix Table XV: 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	-0.068*	-0.056**	-0.033*	0.000
	(0.035)	(0.027)	(0.018)	(0.009)
Observations	12824	12456	13640	12816
Clusters	84	83	94	86
Municipalities	1603	1557	1705	1602
Outcome Mean in 2010	0.460	0.456	0.461	0.459
Media Market by Week FE	X	X	X	X
Covered by Week 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, media markets that were always 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 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.

Appendix A – Text Analysis

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.

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:

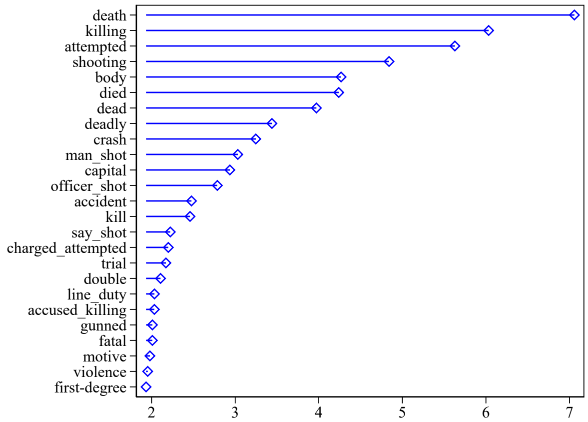
1. Murder: MURDER, HOMICID, KILLE;
2. Assault: ASSAULT;
3. Robbery: ROBBE;
4. Rape: RAPE, SEXUAL ASSAULT;
5. Burglary: BURGLAR;
6. Theft: THIEF, STEAL, STOLE, THEFT.

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.

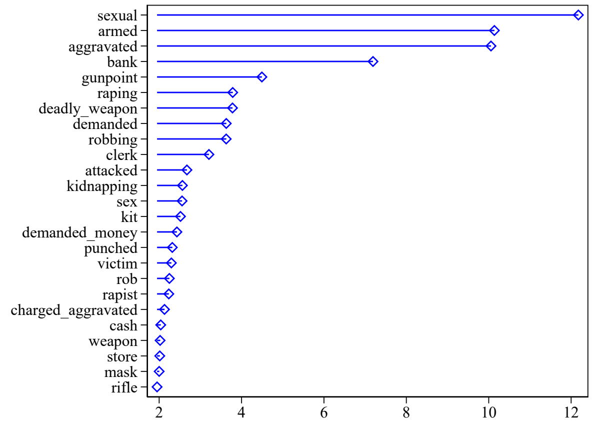
We use this sub-sample to train a classifier model. In particular, we train a support vector machine model using stochastic gradient descent. The features that are used to predict the label are the top most frequent 25,000 tokens and bigrams in the full corpus. We exclude the keyword used to define the 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 B 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.

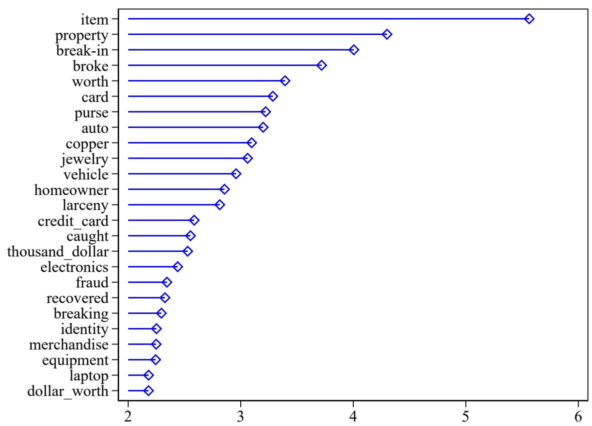
Appendix B Figure I: Most Predictive Features for News Type Classifier



(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.