DISCUSSION PAPER SERIES

DP17756

WHO WATCHES THE WATCHMEN? LOCAL NEWS AND POLICE BEHAVIOR IN THE UNITED STATES

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ORGANIZATIONAL ECONOMICS AND POLITICAL ECONOMY

CEPR

ISSN 0265-8003

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Discussion Paper DP17756
Published 19 December 2022
Submitted 15 December 2022

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JEL Classification: K42, D73

Keywords: Police, Ownership concentration, Local news, Political responsiveness

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Who Watches the Watchmen?

Local News and Police Behavior in the United States*

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November 4, 2022

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1 Introduction

The media is a fundamental determinant of government responsiveness. By providing information to the public, the media helps citizens select public officials who hold positions that are in line with their policy preferences (Berry and Howell (2007), Snyder Jr and Strömberg (2010)). In addition, by focusing on certain topics at the expense of others, the media impacts what issues are salient to citizens (Eisensee and Strömberg (2007), Djourelova (2020)) and, in turn, which policies public officials decide to implement (Clinton and Enamorado (2014), Arceneaux et al. (2016), Durante and Zhuravskaya (2018)). In this paper, we explore the relationship between media content and public officials' responsiveness by focusing on a specific type of news—news about crime on local TV stations—and a specific bureaucracy—municipal police departments in the United States. We find that the police respond to media content: a decline in news coverage of local crime is reflected into lower violent crime clearance rates, our proxy for police behavior.[1]

The question of responsiveness is particularly relevant for the police. On the one hand, the fact that police officers are protected by civil service systems and strong union contracts implies that explicit re-election incentives are absent. On the other, because police chiefs are appointed (and removed at will) by the head of local government, their incentives tend to be aligned with those of the municipality's administration (Owens (2020)). To the extent that perceptions of public safety matter for local politicians (Levitt (1997)), the police might respond to them as well.

This raises the question of how perceptions of public safety are shaped, and it is where the media comes in. The fact that most people do not have direct experience with the criminal justice system (Owens and Ba (2021)) makes news coverage of crime particularly relevant for public safety perceptions, more so than actual crime rates (see, among others, Esberg and Mummolo (2018), Ajzenman, Dominguez-Rivera and Undurraga (2021), Mastrorocco and Minale (2018)). In addi-

[1] Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. A crime is considered cleared if at least one person has been arrested, charged, and turned over for prosecution or if the offender has been identified, but external circumstances prevent an arrest. Clearance rates are highly sensitive to what resources are allocated to investigations and have often been used by economists to study police behavior (see, among others, Mas (2006), Shi (2009), and Premkumar (2022)).

tion, local news tend to have a strong crime focus: in local TV news—the focus of our study—crime is the most popular topic, appearing in almost 25% of all local stories. This suggests that there is scope for media content to influence police behavior, which is the question we investigate in this paper.

The key challenge to addressing the question of how news coverage of local crime impacts police behavior is that we expect profit-maximizing media outlets to cater to demand for news on topics that are already prominent: i.e., media coverage is endogenous to salience. We overcome this challenge by exploiting a shock in the local news environment induced by acquisitions of local TV stations by a large broadcast group, Sinclair.

Sinclair ownership affects content in two ways. First, it reduces coverage of local events in favor of a national focus. This gives us variation in news coverage of local crime, which is the change in content that we are interested in identifying. But in addition to this, Sinclair—a right-leaning media group—also makes content more conservative. The need to disentangle the effect of these two changes in content is why we cannot rely on a simple differences-in-differences design exploiting the staggered timing of Sinclair acquisitions to answer our research question.

Instead, we combine the staggered timing of Sinclair entry in different media markets with variation across municipalities in exposure to the local news shock in a triple differences design. This research design relies on the fact that the relevant geography for local TV stations is a media market, by definition a region in which all households have access to the same TV stations. This means that, once Sinclair acquires a station, all municipalities that belong to the station's media market experience its conservative messaging. However, there is large variation in the extent to which municipalities are exposed to the decline in the station's coverage of local crime.

The proxy for exposure that we use is the baseline probability that a municipality appears in the news.[2] The intuition for this is that municipalities often in the news at baseline (i.e., covered municipalities) should bear the brunt of the decline in coverage of local crime. Instead, municipalities that

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

were never in the news in the first place (i.e., non-covered municipalities) are also not going to be in the news after Sinclair acquires a station: they do not experience any change in news coverage of local crime. As a result, they give us the counterfactual of how clearance rates would have evolved in covered municipalities in the absence of the decline in news coverage of local crime.[3]

Identification rests on covered and non-covered municipalities being on parallel trends. We provide suggestive evidence for this assumption using an event study specification that allows the relative effect of Sinclair entry in covered and non-covered municipalities to vary in time since treatment. In addition, Sinclair's decision to acquire a station must not be driven by differential trends in the two types of municipalities. We show that this is not the case by looking at cases in which Sinclair enters a media market by acquiring an entire broadcast group, where entry is less likely to be endogenous to a specific media market's conditions. Finally, we provide extensive evidence that non-covered municipalities do not themselves experience a change in news coverage of local crime, which means that they provide the correct counterfactual for covered municipalities.

We begin by characterizing in detail how Sinclair ownership affects news coverage of local crime using a novel dataset containing the transcripts of almost 8.5 million stories in 300,000 newscasts. We identify crime stories using a pattern-based sequence-classification method that labels a story as being about crime if it contains a "crime bigram." That is, if it contains an adjacent two-word combination (i.e., 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 mentions of the municipality's name.

Ownership matters for content. After Sinclair acquires a station, covered municipalities are 1.8 percentage points (20% of the baseline outcome mean) less likely to be mentioned in a crime story relative to non-covered municipalities. In line with the intuition behind the research design, the effect is explained by a large decline in the probability that covered municipalities appear in the news

[3] In other words, we estimate the effect of a decline in the probability that a municipality appears in the news with a crime story on the violent crime clearance rate by focusing on the relative effect of the Sinclair entry on covered municipalities, that experience *both* Sinclair's conservative slant *and* a large decline in the probability that their local crime events appear in the news, and non-covered municipalities, that also experience Sinclair's conservative slant *but* no change in the probability that their crime events appear in the news.

with a crime story, while non-covered municipalities do not experience any change. Importantly, the change in news coverage of local crime appears to be an editorial decision on part of Sinclair: other stations in the same media market do not change their crime coverage after Sinclair entry.

The police respond to the decline in news coverage of local crime. After Sinclair enters a media market, covered municipalities experience 3.4 percentage points (7.5% of the baseline outcome mean) lower violent crime clearance rates relative to non-covered municipalities. The effect is explained by non-covered municipalities experiencing an increase in their violent crime clearance rate, perhaps as a consequence of media market trends or of Sinclair's conservative coverage of crime-related news. In covered municipalities instead, this increase is completely offset by the negative effect of the decline in news coverage of local crime. This highlights the importance of using a triple differences design to separately identify the consequences of the twofold change in content.

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

In contrast, property crime clearance rates do not experience a similar decline, which can be explained by local TV news having a clear violent crime focus. We document this in our data by training a classifier model to identify whether local crime stories are about a violent or a property crime. We show that 91% of the stories are about a violent crime and only 17% are about a property crime (8% are about both), a difference which is even starker if we consider that property crimes are significantly more common. Our unique content data underpin one of the most novel contributions of the paper: the ability to characterize in detail the content shock and, as a result, precisely map content changes into police actions. This placebo check confirms that the effect on violent crime clearance rates we estimate is truly related to media content.

We interpret these results through the lenses of public officials' responsiveness. When stories about a municipality's violent crimes are less common in the news, the topic of crime loses salience in the eyes of local citizens and the police find themselves operating in a political environment where there is less pressure to clear violent crimes. As a result, the police reallocate their resources away from clearing these crimes in favor of other policing-related activities.

Three pieces of evidence are consistent with this explanation. First, we show using both Google Trends data and individual-level survey data from Gallup that the salience of crime is indeed lower after Sinclair enters a media market. Second, we note that the key audience of local news, individuals over 55, are also an important interest group for local politics and law enforcement in particular (Goldstein (2021)). In line with this, the effect is driven precisely by those municipalities where individuals over 55 constitute a larger share of the population. Finally, we document an increase in arrests for drug-related offenses in covered relative to non-covered municipalities after Sinclair enters a media market, which is consistent with the police reallocating their resources to other policing-related activities. Overall, we interpret this evidence as supporting the idea of a feedback mechanism from salience to police behavior through citizens' and politicians' pressure.

Our contribution is threefold. First, we build a novel dataset containing the transcripts of almost 300,000 local TV newscasts, tracking news coverage of 325 stations weekly from 2010 to 2017. Our dataset has a significantly larger time and geographic coverage with respect to previous studies of local TV news (see, for example, Moskowitz (2021)) and allows us to quantify the content changes, document their timing, and precisely map how content influences policy. Second, by focusing on the police, we show that even organizations that are generally considered to be insulated from external forces are responsive to media content. Third, we provide evidence that this responsiveness is likely to be explained by media-induced changes in perceptions. The two papers that are closest to ours are Galletta and Ash (2022) and Ash and Poyker (2021), which study how Fox News influences local government spending and judges' sentencing decisions. They also show that the way in which conservative slant influences preferences might have a policy impact. We add to these papers by studying the role played by crime perceptions in influencing police behavior.

In addition, our findings contribute to the growing literature aimed at understanding the determinants of police behavior (see, among others, Ba (2020), Chalfin and Goncalves (2021), Dharmapala, McAdams and Rappaport (Forthcoming), Grosjean, Masera and Yousaf (Forthcoming),Stashko (2022)) and the role played by institutional level incentives in particular (Makowsky and Stratmann (2009), Thompson (2020), Goldstein, Sances and You (2020)). To the best of our knowledge, this is one of the first studies to provide causal evidence on how crime news influences the police. It is particularly interesting to contrast our finding that a reduction in news coverage of local crime decreases clearance rates with the evidence that increases in monitoring following scandals can have the same effect (Ba and Rivera (2022), Premkumar (2022), 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 have very different effects than increases in crime salience driven by media coverage of crime.

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

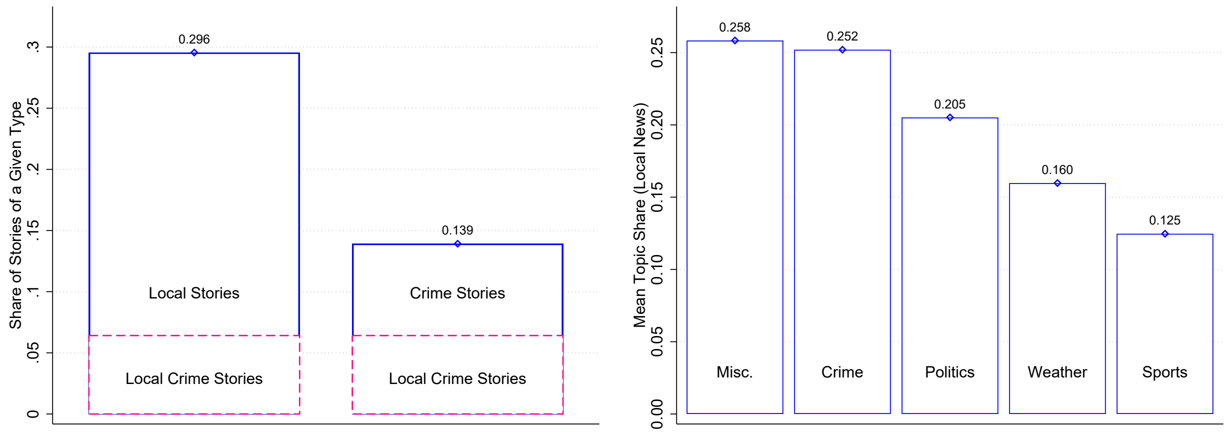
2 Background

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 mentioned often getting their news from television, a higher share than those turning to online sources (43%), the radio (25%), or print newspapers (18%) (Gottfried and Shearer (2017)). Among TV sources, news stories airing on local TV stations have larger audiences than those on cable or national networks (Matsa (2018)).

In fact, the overarching narrative regarding the decline in TV news masks substantial heterogeneity. First, the decrease in viewership has been limited outside top-25 media markets (Wenger and Papper

Figure 1: 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 panels, the sample is restricted to media markets that never experienced Sinclair entry.

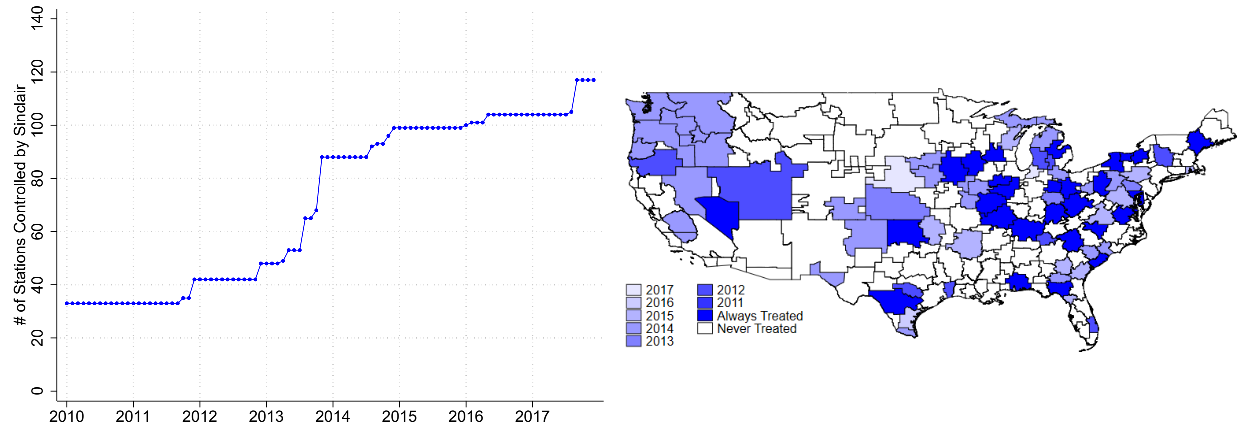
(2018)). Local TV news still plays an important role in small and medium sized markets, both in terms of viewership and because there tend to be fewer outlets such as newspapers producing original news focusing on the area (Wenger and Papper (2018)). Second, the decline has been concentrated in younger demographics, while the core audience of local TV news—those above 50, who constitute 73% of the viewership—has not been affected (Wenger and Papper (2018)).

Newscasts of local TV stations include both national and media market-specific stories. Figure 1 Panel (a) shows that approximately 30% of stories are specific to the media market (i.e., they mention at least one same media market municipality with more than 10,000 people). Crime is a prime subject of local TV news: 22% of all local stories are crime-related (13% overall).

To have a more complete picture of what local TV news stories are about, we also train an unsupervised Latent Dirichlet Allocation (LDA) topic model with five topics on the 2 million local stories in our content data.[4] Figure 1 Panel (b) shows average topic shares. Apart from a miscellaneous topic with no clear meaning, the most covered topic is crime (with a share of 25%), followed by politics (20.5%), weather (16%), and sports (12.5%). Given the crime focus of local

[4] Appendix Figure 1A and Appendix Figure 1B show the highest weight tokens for the five topics. Four of the five topics can be easily identified to be related to crime, politics, weather, and sports. The last topic appears to be a miscellaneous topic with no clear meaning.

Figure 2: Sinclair Ownership over Time and Space



(a) Number of Stations Controlled by Sinclair (b) Media Markets Experiencing Sinclair Entry

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

TV newscasts, we believe that studying the relationship between local news and police departments is first order.

2.2 The Sinclair Broadcast Group

Since 2010, the local TV market in the United States has seen a stark increase in ownership concentration, primarily explained by the emergence of large broadcast groups owning a significant share of local TV stations (Matsa (2017)). We focus on one of the most active players in the local TV market: the Sinclair Broadcast Group. As Figure 2 Panel (a) shows, Sinclair went from owning 33 stations in January 2010 to 117 in December 2017. This corresponds to about 14% of all big-four affiliates. Acquisitions have taken place in media markets across the country (Figure 2 Panel (b)), although Sinclair was particularly active in medium-sized media markets.

With respect to other broadcast groups, Sinclair holds a right-leaning political orientation (Miho (2020)) and appears to be particularly interested in controlling the messaging of its stations (Fortin and Bromwich (2018)). Existing research supports the anecdotal evidence. Martin and McCrain (2019) show using a differences-in-differences design that when Sinclair bought the Bonten Media Group in 2017, the ideological slant of Bonten stations moved to the right.

Miho (2020) shows that Sinclair's conservative leaning might have real word effects, with exposure to Sinclair-owned stations increasing the Republican vote share in presidential elections. In addition, Martin and McCrain (2019) also show that Sinclair ownership increases national coverage, mostly at the expense of local stories. These content changes have limited negative effects on viewership, at least in the very short run.

3 Data and Measurement

This paper combines multiple data sources.

Station Data. Our starting sample includes 835 full-powered commercial TV stations that are affiliated to one of the big four networks (ABC, CBS, FOX, and NBC).[5] Information on the market served by each station and yearly network affiliation 2010-2017 is from from BIA/Kelsey, an advisory firm focusing on the media industry.

Sinclair Ownership. We collect the dates in which stations started being owned by Sinclair from the group's annual reports to shareholders, which we complement using the BIA/Kelsey data. With a slight abuse of terminology, we consider a station as being under Sinclair ownership if the station is owned and operated by Sinclair, if it is owned and operated by Cunningham Broadcasting, or if the station has entered into a local marketing agreement with Sinclair.[6]

Newscast Transcripts. To study how Sinclair ownership affects content, we use transcripts of local TV newscasts from a media monitoring company (ShadowTV). For each station, we collected the closed caption transcripts of all evening newscasts (5-9pm) for a randomly selected day per week. The data cover 325 stations in 113 media markets from 2010 to 2017, for a total of 293,045 newscasts. We segment each transcript into separate stories using an automated procedure based

[5] As discussed in Appendix A, big-four affiliates tend to have the largest viewerships and produce their own newscasts. We exclude low-powered stations (which are sometimes affiliated to a big four network, especially in smaller markets) as they generally have limited geographic reach and smaller viewership.

[6] Sinclair has a controlling interest in Cunningham Broadcasting, although it does not have a majority of voting rights. At the end of 2017, the estate of Carolyn C. Smith (the mother of the two controlling shareholders of Sinclair) owned all of the voting stock of Cunningham Broadcasting. The strong ties between Sinclair and Cunningham are also evidenced by the fact that most Cunningham stations are at least partly operated by Sinclair through local marketing agreements or joint sales agreements. Local marketing agreements give Sinclair control over the programming of a station owned by a third party. 90% of the stations we consider owned by Sinclair are owned and operated by Sinclair directly (Appendix Table 1).

on content similarity across sentences described in detail in Appendix B. This gives us 8.5 million separate stories.

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

1. We define a story to be about a municipality if the name of the municipality appears in it.[7]

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 pre-tagged crime-related library as opposed to a non-crime-related one, and is similar to the one used by Hassan et al. (2019) to identify firms' exposure to political risk from quarterly earnings calls.

   The crime-related training library we consider are articles from the Metropolitan Desk of the New York Times with the tags Crime Statistics, Criminal Offenses, or Law Enforcement 2010-2012, that we download from Factiva. The non-crime-related training library is composed by all other Metropolitan Desk articles over the same time period. Each library is composed of all bigrams contained in the articles. We focus on bigrams because they tend to convey more information than single words. We remove punctuation and stop words and lemmatize the remaining words using WordNet's lemmatizer. We use articles from the New York Times as they are a readily available, previously tagged corpus, but focus on the Metropolitan Desk to capture language that is appropriate to local news stories.

   Specifically, 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 show up only because of noise by

[7] If multiple municipalities' names appear in the same story, we define the story to be local to all of them. 76.5% of local crime stories mention a single media market municipality, 18.5% mention two municipalities, and the remaining 4% mention three or more.

excluding bigrams that appear in the crime library less than 50 times.

This procedure identifies 179 crime bigrams. We report the top 25 bigrams by relative frequency and by overall frequency in Appendix Figure 2A and Appendix Figure 2B. The crime bigrams are quite general and make intuitive sense. Importantly, they do not display an ideological view of crime, which lowers the concern of measurement error systematically varying with Sinclair ownership.

Two pieces of evidence validate the procedure. First, Figure 1 shows that the share of local stories about crime that we identify with our methodology (22%) is very similar to the overall weight of the crime topic (25%). Second, Appendix Figure 3 shows that stories about crime have significantly higher crime topic shares than stories not about crime. The procedure we follow successfully identifies crime stories.

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

Our starting sample is composed by stations that are continuously present in the content data 2010-2017 and same media market municipalities that have more than 10,000 people. We exclude smaller municipalities as they receive a negligible share of overall coverage and we want to increase the comparability of the sample. To maximize sample size in the presence of short gaps in the content data, we replace missing observations in spells shorter than two consecutive months using linear interpolation, but we show that our findings are robust to leaving these observations as missing in Appendix D. The resulting sample includes 325 stations and 2253 municipalities in 113 media markets. Appendix B provides more details.

Crime and Clearance Data. Crime and clearance data are from the Uniform Crime Reports (UCRs) published by the Federal Bureau of Investigation (FBI) 2010-2017. UCRs are compiled from returns voluntarily submitted to the FBI by police departments. UCRs report monthly counts of offenses known to the police and counts of offenses cleared for three property crimes (burglary, larceny-theft, and motor vehicle theft) and four violent crimes (murder, rape, robbery, and aggravated assault). We use these data to study crime rates, defined as crimes per 1,000 people under the inverse hyperbolic sine (IHS) transformation, and clearance rates, defined as cleared crimes over total crimes.[8] In addition, UCRs include arrest counts (but no crime counts) for a broader set of offenses. We use these data to study arrests for drug-related crimes.

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

UCR data may contain record errors and need extensive cleaning, as shown by Evans and Owens (2007) and Maltz and Weiss (2006). Following the state of the art in the crime literature (see, among others, Chalfin and McCrary (2018), Mello (2019), Premkumar (2022)), we use a regression-based method to identify and correct record errors, and define crime rates using a smoothed version of the population reported in the UCRs. We describe the data cleaning procedure in detail in Appendix B. Finally, we winsorize crime and clearance rates at the 99% level to minimize the influence of outliers. Nonetheless, we show that our results are robust to the data cleaning procedure in Appendix D.

Our starting sample is composed by municipalities with more than 10,000 people with a municipal police department. To create a balanced sample, we exclude municipalities that do not continuously report crime data to the FBI and do not have at least one violent and one property crime in every year. In addition, the empirical strategy requires restricting the sample to municipalities located in media markets included in the content data. Our final sample includes 1792 municipalities.[9] Appendix B provides more details.

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

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

Municipality Characteristics. Municipality characteristics are from the 2006-2010 American Community Survey (Manson et al. (2019)). Since municipal election results are not available at a sufficiently large scale, we focus on presidential elections and construct the Republican vote share in 2008 aggregating precinct level returns from the Harvard Election Data archive (Ansolabehere, Palmer and Lee (2014)) to the municipal level. When these are not available ($\sim$10% of the sample), we assign to the municipality the Republican vote share of 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 2010-2017 are from the Census Bureau (demographics), the Bureau of Labor Statistics (unemployment), and the Bureau of Economic Advisers (income per capita). Turnout and Republican vote share in presidential elections are from the MIT Election Data and Science Lab (2017). In all cases, we start from county level data and aggregate them to the media market level.

Police Expenditures and Employment. Data on police departments' employment are from the UCRs' Law Enforcement Officers Killed in Action (LEOKA) files. We supplement these data with expenditures and employment from the Annual Survey of State and Local Government Finances and the Census of Governments 2010-2016, which are published by the Census Bureau.

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

Gallup. We use data from the Gallup Poll Social Series 2010-2017, a set of public opinion surveys, to define an indicator variable equal to one if at least one respondent living in the municipality reports crime as being the most important problem facing the country (see Appendix B for more details).

3.1 Descriptive Statistics

Appendix Table 2 columns (1) to (3) report descriptive statistics for the main variables considered in the analysis. Panel A shows that the average municipality was mentioned in 27% of newscasts in

2010 and appeared with a local crime story in 10% of them. Panel B reports the average property and violent crime and clearance rates for the same year, and Panel C reports average socio-economic characteristics of these municipalities.

Our 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 4 shows that the content sample has good geographic coverage. In addition, Appendix Table 2 columns (4) to (6) report descriptive statistics for all municipalities with more than 10,000 people that satisfy the conditions to be included in the police behavior analysis for comparison. The municipalities included in our sample are highly comparable to other municipalities, as is confirmed by the *p*-values reported in column (7).

4 Empirical Strategy

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

Figure 2 shows that Sinclair entry is staggered across space and time, which suggests we could use a differences-in-differences design to study its effect. However, this would not allow us to identify the treatment of interest. This is because the shock to news content induced by Sinclair is twofold. First, when Sinclair acquires control over a station, newscasts increase their national focus to the detriment of local coverage (*effect #1*). This gives us variation in news coverage of local crime, which is the change in content we are interested in identifying. But in addition to this, because Sinclair is a right-leaning media group, acquisitions make content more conservative (*effect #2*), which might also affect the way in which crime and police are discussed.

To disentangle the effect of these two changes in content, we make use of the fact that the relevant geography for a local TV station is a media market. By definition, all households in a media market receive the same TV offerings: all municipalities in media markets that Sinclair enters experience its conservative messaging. However, not all municipalities are equally exposed to the change in the probability of appearing in the news with a crime story. Our empirical strategy is a triple differences design that combines variation from the staggered timing of Sinclair entry with cross-sectional variation across municipalities in whether they are covered by the news at baseline, our proxy for exposure to the local news shock.[10] This design allows us to capture solely the effect of variation in news coverage of local crime and control for any changes in content that all municipalities in the media market are exposed to, including *effect #2*.

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

Appendix Figure 5 provides supporting evidence for this idea, based on the fact that crime reporting is a function of a municipality's violent crime rate. The graphs are unconditional binned scatter plots of the relationship between a municipality's violent crime rate and the share of weeks in a year in which the same municipality is in the news with a local crime story, separately for years before and after Sinclair acquires the station. The sample is restricted to stations ever acquired by Sinclair. Panel (a) shows the relationship for non-covered municipalities: the probability of being in the news with a crime story is at very low levels both before and after the acquisition. For covered municipalities (Panel (b)), higher violent crime rates are always correlated with a higher probability of being in the news with a crime story, but for every level of violent crime, crime reporting is lower after Sinclair acquires the station.

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

More precisely, we define a municipality to be covered if it appears in the news more than the median municipality in our baseline year, 2010.[11] As Appendix Figure 6 shows, covered and non-covered municipalities differ on a number of characteristics. To ensure that the effect is not confounded by other municipality attributes but is truly driven by exposure, our baseline specification includes interactions between Sinclair ownership and baseline socio-economic characteristics of the municipalities. This implies that the effect is going to be driven by those idiosyncrasies that make one municipality more likely to be in the news than another. Given that covered and non-covered municipalities are especially different in population size, we check whether our results survive restricting the analysis to medium sized municipalities between 10,000 and 50,000 people.

4.1 Identification

Identification in our triple differences design primarily relies on covered and non-covered municipalities being on parallel trends. As a start, we provide supporting evidence for this assumption by estimating event study specifications in which the treatment effect varies in time since Sinclair entry. The event studies allow us to test empirically whether outcomes in covered and non-covered municipalities begin evolving differently prior to the event.

However, even if event studies show convincing patterns, we might still be concerned about contemporaneous shocks influencing both Sinclair's decision to enter a media market and the evolution of the outcome. In other words, we might worry about Sinclair entry being endogenous to demographic or economic trends. Because our triple differences specification allows us to explicitly control for any shock at the media market level that equally affects covered and non-covered municipalities, we should only be concerned about differential trends in the two groups.[12]

We test whether this is likely to be driving our results by checking robustness to focusing on stations

[11] We begin by calculating the share of weeks a municipality is mentioned in the news in 2010. If we have data for multiple stations in the same media market, we assign to each municipality the median share of weeks a municipality is mentioned in the news across the different stations. Finally, we define an indicator variable equal to one if the municipality is in the news more than the median municipality in 2010, and zero otherwise.

[12] While Appendix Table 3 shows no change in media markets' socio-economic characteristics following Sinclair entry, the fact that our design allows us to control for observable and unobservable trends strengthens the credibility of the results.

that get under Sinclair control through the acquisition of a smaller broadcast group, which are less likely to be endogenous to a specific media market's conditions. Importantly, the qualitative evidence is very much in line with the no endogenous timing hypothesis, with Sinclair looking to expand and taking advantage of opportunities to acquire stations as they present themselves.[13]

Finally, for our triple differences design to recover the causal effect of a decline in news coverage of local crime induced by Sinclair, we also need to assume that non-covered municipalities do not themselves experience a change in news coverage of local crime. We highlight evidence suggesting that this is unlikely to be the case throughout the paper, but for now it is important to note that, as shown in Appendix Figure 7 using data from media markets that never experience Sinclair entry, coverage is persistent across years. This suggests that the likelihood of being in the news can be seen as a fixed characteristic of a municipality.

5 Effect of Sinclair Ownership on Coverage of Local Crime

5.1 Specification

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

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

where y_{mst} is an indicator variable equal to one if municipality m was mentioned in a crime story by station s in week t, $Sinclair_{st}$ is an indicator variable equal to one after a station is acquired by Sinclair, $Covered_m$ is an indicator variable equal to one if a municipality is covered 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.[14]

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

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

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

The station by week fixed effects (δ_{st}) control non-parametrically for station specific shocks in content that are common to all municipalities, while covered status by week fixed effects (δ_{ct}) allow the two different types of municipalities to be on different trends. Finally, municipality by station fixed effects (δ_{sm}) control for station-specific level differences across municipalities, including level differences explained by non-time-varying measurement error due to how stories are assigned to municipalities.[15]

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

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

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

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

Table 1: Effect of Sinclair Ownership on the Probability of Having a Local Crime Story

Dependent Variable	Had Local Crime Story			
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.023***	-0.018***	-0.014**	-0.019***
	(0.007)	(0.007)	(0.006)	(0.007)
Non-Sinclair Stations in Sinclair Media Market * Covered				-0.007
				(0.006)
Observations	3143360	3143360	2398902	3143360
Clusters	113	113	111	113
Municipalities	2253	2253	1715	2253
Stations	325	325	323	325
Outcome Mean in 2010	0.092	0.092	0.050	0.092
P-value Sinclair = Other				.104
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	

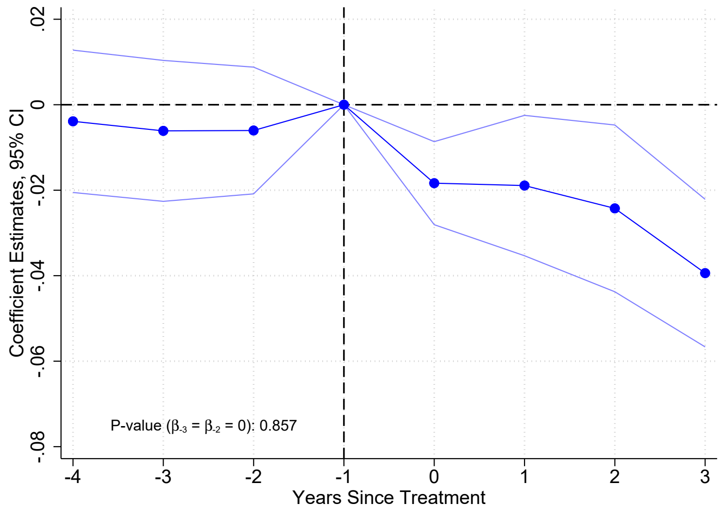
Notes: This table shows the effect of Sinclair ownership on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities. We regress an indicator variable for the station reporting a local crime story about the municipality on the interaction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

5.2 Results

Table 1 shows the effect of Sinclair ownership on a station's coverage of crime in covered versus non-covered municipalities. The table reports the coefficient on the interaction between an indicator variable for the station being owned by Sinclair and an indicator variable for the municipality being covered. Column (1) reports estimates from a specification that only controls for the fixed effects, while column (2) additionally includes the interaction between Sinclair and socio-economic characteristics of the municipality at baseline (equation (1)).

We find that after Sinclair acquires a station, covered municipalities are 1.8 percentage points less likely to appear in the news with a crime story relative to non-covered municipalities. The effect is significant at the 1% level. The magnitude of the effect is large, corresponding to almost 20% of the

Figure 3: Effect of Sinclair Ownership on the Probability of Having a Local Crime Story, by Year since Treatment



Notes: This figure shows the effect of Sinclair ownership 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 acquired the station 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 sample excludes always treated municipality-station pairs. The omitted category is T-1. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level, but the effect is constrained to be the same by year since treatment. Covered municipalities are mentioned in the news more than the median municipality in 2010.

baseline mean. The coefficient is smaller in size but similar in magnitude, corresponding to 28% of the baseline mean, if we exclude municipalities with more than 50,000 people to increase the comparability of the sample (column (3)). For a detailed discussion of the robustness of this result to how we clean the data and how we define Sinclair ownership, we refer the reader to Appendix D.

Event Study. We provide evidence supporting the assumption that covered and non-covered municipalities are on parallel trends leading up to the Sinclair acquisition in Figure 3, which reports the β_y and γ_y coefficient estimates from equation (2), together with 95% confidence intervals. The figure shows no difference between covered and non-covered municipalities in the four years leading up to Sinclair ownership. Immediately after Sinclair acquires the station, covered municipalities become less likely than non-covered municipalities to appear in the news with a crime story. The effect in the first year is large in magnitude and almost comparable to the point estimate from the triple differences specification. After this, the effect becomes larger over time, almost doubling by year three.

Same Media Market Stations. Our result might reflect an underlying change in a municipality's crime prevalence or demand for crime stories. To examine whether this is the case, we replicate our baseline model but also look at the coverage of local crime of stations that are in the same media market as stations that are acquired by Sinclair, but are not themselves bought by the group. In Appendix Figure 8, we report the same β_y and γ_y coefficient estimates from equation (2), together with similarly defined leads and lags for same media market stations that are not under Sinclair control. In the four years leading up to the Sinclair acquisition, there is no difference in how Sinclair and non-Sinclair stations report about crime in covered relative to non-covered municipalities. Once Sinclair enters the media market, we only see a decrease in local crime coverage by Sinclair stations. Table 1 column (4) confirms the result (p-value of a test of equality of the effect of Sinclair entry on Sinclair and non-Sinclair stations = 0.104).

This evidence supports the interpretation that decreasing local crime coverage is an editorial decision on the part of Sinclair. In addition, it shows limited spillovers of Sinclair's change in content to other outlets in the media market. This signals that there might be demand for local news stories, which is in line with stations acquired by Sinclair potentially experiencing a decline in viewership (Martin and McCrain (2019)). Nonetheless, decreasing local news might still be an optimal strategy for Sinclair if economies of scale from jointly operating a large number of stations outweigh the potential decline in advertising revenues due to smaller viewership.

Differences-in-Differences Decomposition. We justify the triple differences design using the intuition that municipalities with a low baseline probability of being in the news should not experience a change in their local crime coverage, while covered municipalities should bear the brunt of the decline. Appendix Table 4 supports this view using separate differences-in-differences designs for covered and non-covered municipalities. Sinclair ownership does not affect the crime coverage of non-covered municipalities (columns (1) and (2)). Instead, after Sinclair acquires a station, covered municipalities experience a large decline in the probability of being mentioned in the news with a crime story (columns (3) and (4)).

Overall Crime Coverage. How is coverage or non-local crime affected by Sinclair ownership? We address this question by estimating differences-in-differences specifications at the station level. In Appendix Table 5 we show that after Sinclair acquires a station, there is no change in the share of stories about non-local crime (column (1)) or police (column (2)). However, while the volume of non-local crime stories is unaffected, Sinclair acquisitions introduce conservative slant in the way in which police and crime are talked about. After Sinclair acquires a station, the station is less likely to mention police misconduct (column (3)), more likely to mention crime and drugs (column (4)), and more likely to mention crime and immigrants (column (5)).

Other Types of Local News. In light of the results in Table 1, it is natural to ask to what extent the decline in local coverage is specific to crime news. In Appendix Table 6, we show that Sinclair ownership lowers the probability that a station reports a story about covered municipalities relative to non-covered municipalities by 3.2 percentage points or 13% of the baseline mean (column (1)). However, the effect is much larger in magnitude for crime compared to non-crime stories (23% versus 10%). We interpret this result as supporting the idea that the effects on police behavior that we identify are related to the change in local coverage of crime, and not the result of decreased coverage of other non-crime events.

Heterogeneity by Political Leaning of the Municipality. Since Sinclair is a conservative media group, we might worry that the decline in coverage could be influenced by political considerations. For example, Sinclair entry might affect differently the typology and the quantity of coverage of Democrat- and Republican-leaning municipalities. Ideally, we would test this possibility using election results for municipal-level races. Unfortunately, these data are not widely available, especially for smaller municipalities (de Benedictis-Kessner and Warshaw (2016)). We get around this problem by using electoral results in presidential elections as a proxy for a municipality's partisanship. In particular, we split the sample by whether the municipality's Republican vote share was above the median (column (1)) or below the median (column (2)) in the 2008 presidential election. Appendix Table 7 shows that the effect is very similar for Democratic- and Republican-leaning municipalities (p-value of a test of equality of the effect of Sinclair in the two groups of municipalities = 0.898). This suggests limited scope for strategic coverage decisions based on political considerations, although we acknowledge that this analysis is indicative in nature because of data limitations.

6 Effect of Sinclair Entry on Clearance Rates

6.1 What Should We Expect?

In Section 5 we document that when a local TV station is acquired by Sinclair, covered municipalities become less likely to appear in the news with a local crime story relative to non-covered municipalities. This decline may have tangible implications: in this section, we investigate whether the decline in news coverage of local crime impacts clearance rates.

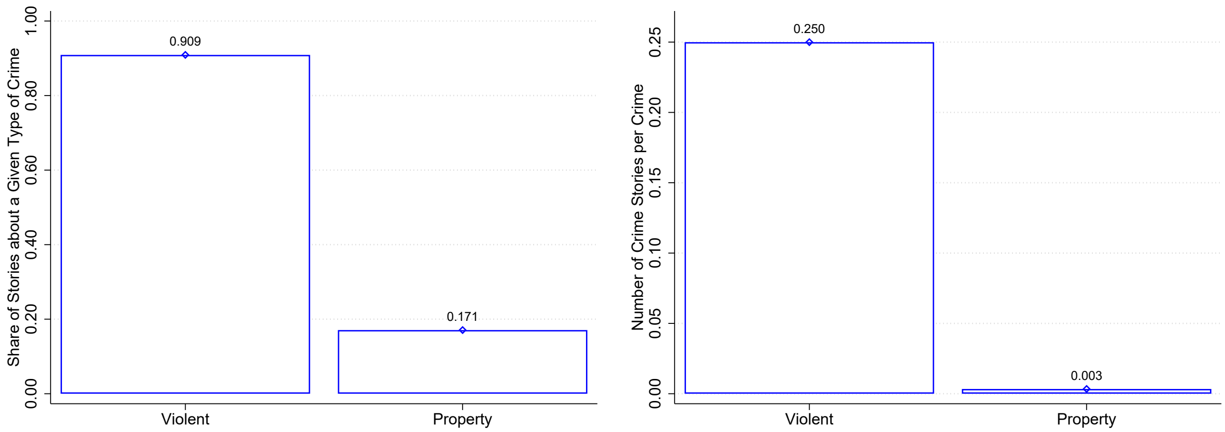
Crime clearances are highly sensitive to what resources are allocated to investigations.[16] As a result, clearance rates are often used to study police behavior (see, among others, Mas (2006), Shi (2009), and Premkumar (2022)). They are especially interesting in our setting as they allow us to consider whether the types of crimes that get prioritized by police departments are affected by news coverage.

However, not all crime types are equally likely to be reported in the news: we should expect clearance rates of different crimes to respond differently, depending on how important news coverage is for them. We focus in particular on the difference in news coverage of property versus violent crimes, which we explore in our content data by training a classifier model to identify the type of crime a local crime story is about (see Appendix C for more details). We use the resulting classification in two ways.

First, in Figure 4 Panel (a), we show that local news have a clear violent crime focus: 91% of local crime stories are about violent crimes, while only 17% are about property crimes (8% are about both). The difference in reporting across crime types is even sharper if we consider the fact that violent crimes are relatively rare, while property crimes are significantly more common. In Figure 4 Panel (b), we normalize the number of crime stories of a given type that were reported about a municipality in 2010 by the number of offenses of the same type for the same municipality.

[16] For example, Blanes i Vidal and Kirchmaier (2017) show that increases in the response time to crime calls have a negative effect on the probability that a crime is cleared. In addition, Cook et al. (2019) show that the involvement of a specialized detective squad also increases the probability that a crime is cleared in the medium run.

Figure 4: Local Crime News of Violent and Property Crimes



(a) Share of Crime Stories

(b) Crime Stories per Offense

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

There are approximately 0.25 stories for each violent crime, while property crimes, at 0.003 stories per offense, receive negligible news coverage.

Second, we test whether Sinclair ownership has a different effect on local news coverage of violent and property crimes. In Appendix Table 8, we show that after Sinclair acquires a station, covered municipalities are 1.7 percentage points (19% of the baseline mean) less likely to appear in the news with a story about a violent crime relative to non-covered municipalities. Instead, they are not significantly less likely to appear in the news with a story about a property crime.

Taken together, these two pieces of evidence suggest that we should expect an effect on the clearance rate of violent rather than property crimes. Our analysis focuses on the violent crime clearance rate, but we use the property crime clearance rate as a helpful placebo check.

6.2 Specification

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

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

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

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

We consider a media market to be treated in a given year if Sinclair owns one of the media market's stations in the January of that year: the year of treatment is the first year in which Sinclair is continuously present in the media market. This is reasonable because 88% of stations are acquired by Sinclair in the second half of the year (53% in the last trimester), which means that in most cases partially treated years only see a Sinclair presence for a couple of months. Importantly for the interpretation of our results, Sinclair entry generally corresponds to Sinclair owning one out of four stations in the media market.

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

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

where all variables are defined as above.

6.3 Results

Table 2 shows the effect of Sinclair entry on the violent crime clearance rate of covered relative to non-covered municipalities. The table reports the coefficient on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline. Column (1) reports estimates from a specification that only controls for the fixed effects, while column (2) additionally includes the interaction between Sinclair and baseline socio-economic characteristics of the municipality (equation (3)).

After Sinclair enters a media market, the violent crime clearance rate is 3.4 percentage points lower in covered than in non-covered municipalities. The effect is significant at the 5% level, and sizable in magnitude, corresponding to 7.5% of the baseline mean. To put this number in prospective, the median municipality in our sample experiences 69 violent crimes in a year and 32 violent crime clearances: a 7.5% decline in the violent crime clearance rate corresponds to approximately 2.4 fewer clearances per year. When violent crime is less covered by local news, a lower share of violent crimes gets cleared: there is scope for external forces to exert an influence on police behavior, despite the protections that strong union contracts and civil service laws extend to police officers.[17]

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

We further discuss the robustness of our main results to how we clean the data, how we define

[17] Unfortunately, we are unable to follow clearances through the criminal justice system, and know whether they lead to a conviction or an acquittal. As a result, we cannot make inference relative to the quality of the clearances themselves, which limits our ability to draw efficiency or welfare conclusions from our analysis.

Table 2: Effect of Sinclair Entry on the Violent Crime Clearance Rate

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

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

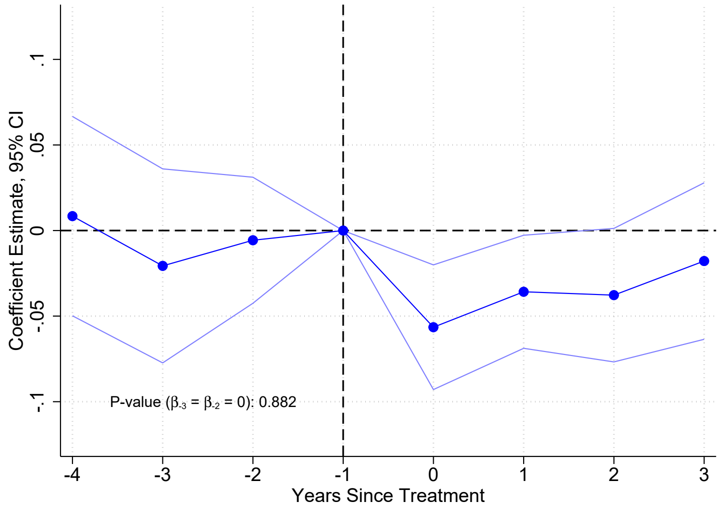
the treatment, how we identify covered municipalities, and concerns to heterogeneous effects in two-way fixed effects estimators in Appendix D.

Event Study. We provide evidence supporting the parallel trends assumption by estimating an event study specification that allows the relative effect of Sinclair entry on covered and non-covered municipalities to vary by time since treatment. Figure 5 reports the β_y and γ_y coefficient estimates from equation (4), together with 95% confidence intervals.

The figure shows no difference between covered and non-covered municipalities in the four years leading up to Sinclair entry in the media market.[18] Consistent with the time pattern of the effect on news coverage of local crime, which showed a large effect immediately in the first year after

[18]The paper focuses on the 2010-2017 period because it is the period for which we have collected the content data. Given that only a handful of municipalities are treated after 2015, the maximum number of pre-periods we can estimate is four. However, UCR data is easily available before 2010. As a result, we also estimate the event study specification on 2009-2017 data, which allows us to both include one additional pre-period and to estimate the other pre-period dummies using a larger sample of municipalities. Appendix Figure 9, which shows the resulting event study graph, confirms the evidence in support of the identification assumption: covered and non-covered municipalities appear to be on comparable trajectories in the five years preceding Sinclair entry.

Figure 5: 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 (4)). The sample excludes always-treated media markets. The omitted category is T-1. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

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

Property Crime Clearance Rates. If the police are responding to news coverage of local crime as we hypothesize, the clearance rate of crimes that are minimally in the news, such as property crimes, should not be affected by Sinclair entry. In line with this, Table 3 shows that after Sinclair enters a media market, covered and non-covered municipalities do not experience differential changes in their property crime clearance rate. The coefficients are small in magnitude and not statistically significant. The change in clearance rates is specifically related to how Sinclair influences news

Table 3: Effect of Sinclair Entry on the Property Crime Clearance Rate

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

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

content, and does not depend on other factors affecting clearance rates across the board.

Crime Rates. A potential concern is that the change in the violent crime clearance rate might be explained by an increase in violent crimes. Appendix Table 9 suggests that this is not the case. The table reports the effect of Sinclair entry on the violent crime rate of covered municipalities relative to non-covered municipalities, for all violent crimes (column (1)) and separately by type of crime (column (2) to column (5)). Reassuringly, we do not find a statistically significant difference in the violent crime rate of covered and non-covered municipalities after Sinclair enters a media market. Even if we take the positive coefficient on the violent crime rate at face value, the magnitude of the effect (2.9%) is too small to explain the decline in the violent crime clearance rate. The same is true if we use as outcomes indicator variables equal to one if the municipality reports at least one crime of the specified type (Panel B).[19]

Appendix Table 10 looks instead at property crime rates. Column (1) shows that Sinclair entry is

[19]This result provides additional support to the interpretation of the relative decline in news coverage of local crime in covered and non-covered municipalities after Sinclair acquires a station being driven by an editorial decision of part of Sinclair. Because crime coverage is increasing in crime rates and in violent crime rates in particular, the effect of crime rates we estimate should, if anything, bias our results on content in the opposite direction.

associated with 5.4% higher property crime rates in covered relative to non-covered municipalities. The effect is significant at the 5% level. This result could be explained by a decreased incapacitation or deterrence effect due to the lower clearance rates. Alternatively, the positive effect on property crime rates might be due to a reduction in overall police performance in covered relative to non-covered municipalities, which would be consistent with a decrease in monitoring induced by lower crime news coverage. Finally, it is possible that individuals who commit property crimes are directly affected by the decline in crime content of local news (see Dahl and DellaVigna (2009) and Lindo, Swensen and Waddell (2022)). Given that the local news audience tends to be above 55, we believe that this explanation has a limited role in this setting.

Differences-in-Differences Decomposition. How does Sinclair entry affect covered and non-covered municipalities? Appendix Table 11 reports coefficient estimates from differences-in-differences specifications that only exploit variation from the staggered timing of Sinclair entry, separately for non-covered (columns (1) and (2)) and covered municipalities (columns (3) and (4)).

Column (1) and (2) show that the increase in conservative content induced by Sinclair has a direct effect on clearance rates: the violent crime clearance rate in non-covered municipalities increases after Sinclair enters a media market. This effect can be rationalized by Sinclair's conservative messaging building support for tough-on-crime policies, which might feedback into police behavior.[20] Instead, Sinclair entry does not impact the violent crime clearance rate in covered municipalities, that experience both the increase in conservative slant and a decline in the probability that local crime is covered in the news. The direct effect of Sinclair's conservative messaging is offset in covered municipalities by the decrease in their probability of appearing in the news with a local crime story.[21]

[20]The idea that conservative content might impact the criminal justice system has recently been explored by Ash and Poyker (2021), who finds that exposure to Fox News Channel induces judges to impose harsher criminal sentences. Consistent with this explanation, we show in Appendix Table 5 that, although the volume of non-local crime- and police-related stories is constant after Sinclair entry, the way in which crime and police are talked about is not. In particular, Sinclair stations are less likely to mention police misconduct (column (3)), more likely to mention crime and drugs (column (4)), and more likely to mention crime and immigrants (column (5)).

[21]Decomposing the effect between covered and non-covered municipalities can also help us exclude the following interpretation of the results. As we show in the previous paragraph, after Sinclair enters a media market the property crime rate is higher in covered relative to non-covered municipalities. We might be concerned that the effect on the

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

Discussion. There are three potential interpretations for the decline in the violent crime clearance rate we observe. First, police departments in covered municipalities might experience a decline in the resources that are available to them, relative to police departments in non-covered municipalities. Appendix Table 13 shows that this is not the case: after Sinclair entry, covered and non-covered municipalities have similar police expenditures and employment per capita, although our effects are imprecisely estimated, potentially due to data limitations.

Second, the police might reallocate resources from clearing violent crimes to other policing-related activities. Two pieces of evidence support this interpretation. First, to the extent that property crime rates are higher in covered versus non-covered municipalities after Sinclair entry, constant property crime clearance rates are consistent with resources being reallocated from clearing violent to clearing property crimes. Second, we show in Appendix Table 14 that arrests for drug-related crimes are also differentially higher in covered municipalities relative to non-covered municipalities after Sinclair entry. This is a highly suggestive result, although it needs to be interpreted with caution as we cannot disentangle whether it is driven by a change in enforcement or by a change in the occurrence of these crimes.[22]

Third, the police might exert less effort across the board. While we cannot reject this interpretation, we believe that the suggestive evidence presented above supports the reallocation view.

violent crime clearance rate that we estimate is a direct consequence of this increase in the property crime rate, if to deal with the higher volume of property crimes the police have fewer resources to dedicate to clearing violent crimes. However, Appendix Table 12 shows that the change in the property crime rate is not driven by the same sub-sample as the change in the violent crime clearance rate. In particular, we do not see a decrease in the property crime rate in non-covered municipalities or an increase in covered municipalities.

[22]Drug-related arrests include arrests for possession and sale of cannabis, heroin and cocaine, synthetic narcotics, and other drugs. No crime counts are reported for these crimes, which is why we cannot define clearance rates.

7 Mechanisms

The explanation that we propose for our findings is that, when stories about a municipality's violent crimes are less common in the news, crime become less salient in the public opinion and the police find themselves operating in a political environment where there is less pressure to clear violent crimes. In this section, we provide three pieces of evidence supporting this explanation, but also discuss alternative mechanisms such as monitoring and community cooperation.

Salience of Crime. We test whether Sinclair entry impacts the salience of crime using two data sources: Google Trends data on searches for crime-related keywords and survey data from Gallup on whether crime is the most important problem facing the country. Neither dataset is perfect: Google searches are only available at the media market level, while even a large and nationally representative survey such as the Gallup Poll Social Series gives us few respondents for each municipality. Nevertheless, the two analyses together provide suggestive evidence of a decrease in the salience of crime in the public opinion.

We begin by looking at the Google Trends data. Because these data are not consistently available below the media market level, we implement a differences-in-differences design exploiting the staggered entry of Sinclair across media markets. The sample is restricted to media markets for which the volume of searches is available throughout the period. Table 4 shows that, when Sinclair enters a media market, the volume of searches for "crime" and "police" decreases by 4.7% and 4.2% (columns (1) and (2)). The effect is not explained by a generalized decline in search volume, as shown by placebo regressions looking at searches for "weather" and "youtube" (columns (1) and (2)). The decrease in local crime stories triggers a change in public interest for precisely those topics that are now less present on local news.

We then turn to the Gallup Poll Social Series, a set of public opinion surveys that include a question about the most important problem facing the country, with crime being one of the possible answers. Table 5 shows that, after Sinclair enters a media market, covered municipalities are less likely to have at least one respondent that reports crime as being the most important problem relative

Table 4: Effect of Sinclair Entry on the Salience of Crime, Google Trends

Dependent Variable	Monthly Search Volume			
Keyword	Crime	Police	Weather	Youtube
	(1)	(2)	(3)	(4)
Sinclair	-0.047***	-0.042***	-0.000	-0.004
	(0.015)	(0.014)	(0.016)	(0.011)
Observations	14976	14976	14976	14976
Clusters	156	156	156	156
Outcome Mean in 2010	3.627	3.920	3.873	4.285
Media Market FE	X	X	X	X
Month FE	X	X	X	X
Media Market Controls	X	X	X	X

Notes: This table shows the effect of Sinclair entry on the salience of crime and police using Google Trends 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 a media market by month panel. Treatment is defined at the monthly level. The monthly level of searches is in logs.

to non-covered municipalities.[23] Controlling for the number of respondents interviewed in each municipality and year (column (2)) or estimating the regression on a quasi-balanced sample of municipalities (column (3)) does not impact the result. This is again consistent with Sinclair entry having a negative effect on crime salience.

Political Feedback. If the change in news coverage of local crime makes crime less salient in the public opinion, we expect politicians and the police chiefs they appoint to react to it.[24,25] This political feedback mechanism is particularly credible in this setting, given that the individuals whose opinion is likely to be influenced by local news are exactly the ones who are more active in local politics: those over 55. Appendix Figure 10 shows descriptive evidence supporting this statement.

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

[24] Police department chiefs are generally appointed (and removed at will) by the head of local government, which implies that their incentives tend to be aligned with those of the municipality's administration (Owens (2020)). Consistent with this, research has shown that political incentives affect law enforcement (Makowsky and Stratmann (2009), Makowsky, Stratmann and Tabarrok (2019), Goldstein, Sances and You (2020)). In addition, managerial directives can have important effects on police behavior (Ba and Rivera (2022), Mummolo (2018)), supporting the idea that pressure coming from the top might influence the effort allocation of police officers.

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

Table 5: Effect of Sinclair Entry on the Salience of Crime, Gallup

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

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

Using the 2010 Cooperative Congressional Election Study (Ansolabehere (2012)), we show that individuals over 55 are 25% more likely to watch local TV news and 50% more likely to attend local political meetings compared to younger individuals. In addition, Goldstein (2021) shows that people over 55 are an especially important interest group for local politics when it comes to crime and policing.

Consistent with this argument, Appendix Table 15 shows that the effect on the violent crime clearance rate is driven by municipalities with a larger share of the population above 55 (p-value of a test of equality of the effect of Sinclair in the two groups of municipalities = 0.121), even though the change in content is exactly the same across the two groups of municipalities (p-value = 0.783). While the difference in the effect is not statistically significant at conventional levels, this evidence supports the idea of a change in public opinion operating through a political feedback mechanism as a possible explanation for the findings of the paper.

Media Monitoring. An alternative explanation is that there could be a decrease in media monitoring of the police. To explore whether this is the case, we use our content data to separately identify stories about crime incidents and about arrests.[26] In Appendix Table 16, we report the effect of Sinclair ownership 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 is almost entirely driven by stories about crime incidents (column (1)), whereas stories about arrests experience a much smaller decline, which is also not statistically significant (column (2)). These results do not support direct media monitoring through stories about police clearances as the main explanation for the results, although we cannot exclude the possibility that police officers are updating their overall probability of being the subject of reporting based on the decline in crime coverage.

Community Cooperation. It is also possible for the effect on clearance rates to be driven by decreased community cooperation with the police. Community cooperation is generally considered important for successful policing and crime investigations, and it has been shown to decrease after high-profile cases of misconduct that negatively impact perceptions of the police (Desmond, Papachristos and Kirk (2016)). It is unclear why the change in content that we document should have negative effects on police perceptions: people are seeing fewer stories about crimes and a similar number of stories about arrests, so they should perceive the police as being equally, if not more, effective.

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 exist almost no data on the importance of tips for solving crimes, which limits our ability of testing this mechanism directly. Nonetheless, the magnitude of the effect on the violent crime clearance rate is too large for tips to be the main driver of the effect we estimate. Were the decrease in clearance rates caused by a drop in tips, it should be concentrated in those violent crimes that are no longer covered in the news after Sinclair enters a media market. However, because not all crimes are covered in the news, Sinclair controls one of four stations in the media market, and the other stations are not adjusting their crime

[26] We define stories to be about arrests if they contain one of the following arrest-related keywords: arrest, capture, detention, custody, apprehend, catch, caught, detain, imprison, incarcerat, jail. All other stories are about crime.

coverage, the change in content that we document implies too few incidents no longer appearing in the news for the magnitude of the effect on clearance rates to be credible. Instead, the magnitude of the effect can be more easily reconciled by abandoning the one-to-one correspondence between crimes reported in the news and crimes cleared by the police. That is, by thinking that the effect comes from the clearance rates of all violent crimes (i.e., not just the ones covered in the news) changing by 7.5%, as would be the case under the mechanism that we propose earlier in this section.

8 Conclusion

In this paper, we ask whether municipal police departments in the United States respond to news coverage of local crime. To get exogenous variation in content, we exploit acquisitions of local TV stations by the Sinclair Broadcast Group. We find that ownership matters for content: once acquired by Sinclair, TV stations decrease news coverage of local crime. The police respond to this change in media content: municipalities that experience a decline in news coverage of local crime have lower violent crime clearance rates relative to municipalities that do not.

The fact that ownership matters for content and that this has an effect on the police has far reaching implications for media plurality and, importantly, for its regulation. The deepening of the crisis of the traditional business model of local media has resulted in a trend of increasing concentration, that in fact characterizes not only local TV (Stahl (2016)) but also other media types such as newspapers (Hendrickson (2019)). Our results show that the resulting news nationalization might impact not only voters as has been widely documented (Hayes and Lawless (2015), Darr, Hitt and Dunaway (2018), Moskowitz (2021)), but also public officials such as police officers, thus having tangible externalities for local governments across the board.

This urges a rethinking of media regulations. First, it is important to consider the notion of market that regulators adopt. Many of the restrictions that the FCC imposes on ownership concentration are media market specific, whereas we show that common ownership of outlets across markets is also highly relevant. Second, our results show that the trend of increasing concentration has consequence that go beyond the media industry. As suggested by Prat (2018), Rolnik et al. (2019), media mergers should probably not only be evaluated with a focus on consumer welfare, but also taking into account these downstream consequences.

Answering these questions requires a collective effort within the scholarly community. Even within the setting of this study, a few aspects remain unexplored. Is the effect we document Sinclair-specific, or a more general consequence of the business model of large broadcast groups? Is the accountability of local public officials beyond police officers also affected? We hope to explore this question in future research.

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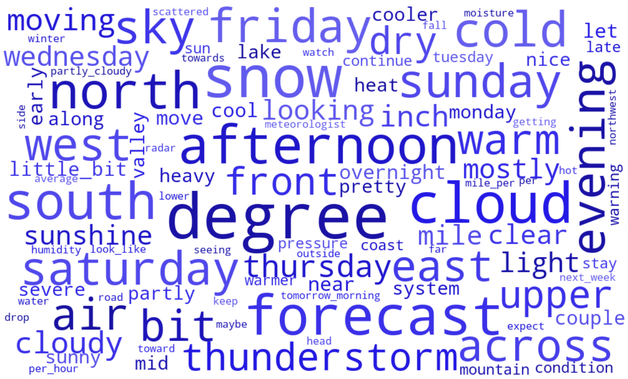
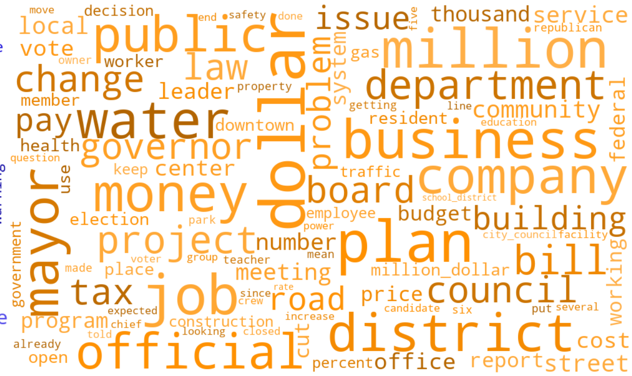
Online Appendix

Appendix Figure 1A: Local News Topics, World Clouds



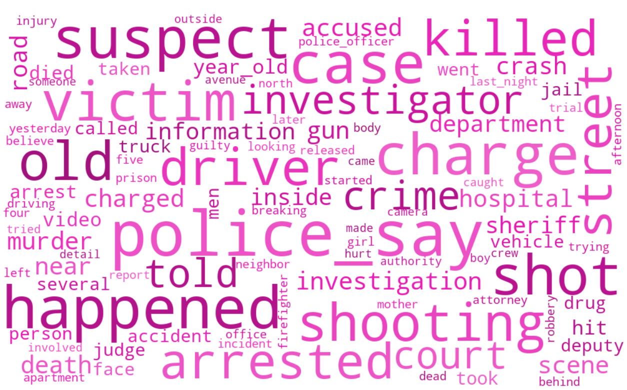
(a) Weather



(b) Politics

(c) Sports

(d) Miscellaneous



(e) Crime

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

Appendix Figure 1B: Local News Topics, Weights

Weather		Politics		Sports		Misc.		Crime	
Unigram or Bigram	Weight	Unigram or Bigram	Weight	Unigram or Bigram	Weight	Unigram or Bigram	Weight	Unigram or Bigram	Weight
degree	0.010	dollar	0.006	season	0.008	kid	0.005	police_say	0.006
snow	0.009	plan	0.005	san	0.008	community	0.003	happened	0.006
cloud	0.008	job	0.005	play	0.007	local	0.003	suspect	0.005
forecast	0.008	million	0.004	win	0.007	something	0.003	case	0.005
afternoon	0.007	business	0.004	sport	0.006	find	0.003	charge	0.005
south	0.007	district	0.004	coach	0.005	event	0.003	shot	0.005
north	0.007	money	0.004	football	0.005	every	0.003	victim	0.005
cold	0.006	water	0.004	fan	0.005	great	0.003	old	0.004
evening	0.006	mayor	0.004	player	0.005	food	0.003	shooting	0.004
sky	0.006	company	0.004	high_school	0.003	com	0.003	driver	0.004
saturday	0.006	public	0.004	head	0.003	getting	0.003	arrested	0.004
sunday	0.006	official	0.003	field	0.003	place	0.003	street	0.004
friday	0.006	department	0.003	great	0.003	center	0.002	killed	0.004
west	0.005	bill	0.003	final	0.003	give	0.002	investigator	0.004
across	0.005	project	0.003	top	0.003	sure	0.002	crime	0.004
air	0.005	governor	0.003	second	0.003	love	0.002	told	0.004
bit	0.005	law	0.003	run	0.003	world	0.002	court	0.004
east	0.005	tax	0.003	guy	0.003	keep	0.002	investigation	0.003
warm	0.005	council	0.003	four	0.003	hope	0.002	death	0.003
thunderstorm	0.005	change	0.003	point	0.003	thank	0.002	charged	0.003
upper	0.005	board	0.003	college	0.003	never	0.002	gun	0.003
front	0.004	building	0.003	six	0.003	let	0.002	near	0.003
dry	0.004	road	0.003	diego	0.003	free	0.002	murder	0.003
thursday	0.004	pay	0.003	best	0.003	dog	0.002	accused	0.003
cloudy	0.004	issue	0.003	san_diego	0.003	friend	0.002	scene	0.003

Notes: This figure shows the 25 words and bigrams that have the highest probability of being generated by a given topic.

Appendix Figure 2A: Crime Bigrams, Word Clouds

(a) Frequency

(b) Relatively Frequency

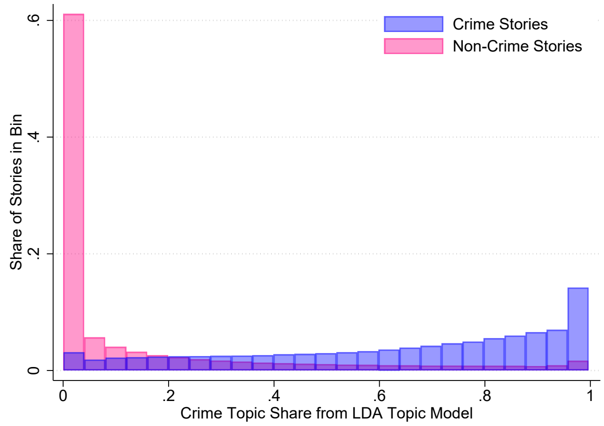
Notes: This figure shows word clouds of the 50 bigrams with the highest frequency (Panel (a)) and of the 50 bigrams with the highest relative frequency (Panel (b)). The frequency is the number of times the bigram appears in the crime library. The relative frequency is the number of times the bigram appears in the crime library over the number of times the bigram appears in the non-crime library. The size of the words is proportional to the value.

Appendix Figure 2B: Crime Bigrams, Weights

Bigram	Frequency	Bigram	Relative Frequency
police_department	890	police_union	999.000
district_attorney	786	murder_charge	999.000
police_said	663	criminal_possession	999.000
law_enforcement	550	internal_affair	221.790
pleaded_guilty	520	affair_bureau	184.380
prosecutor_said	471	pleading_guilty	184.380
attorney_office	467	browne_said	171.909
york_police	385	according_criminal	171.019
police_commissioner	378	officer_fired	165.674
year_prison	339	man_accused	160.330
raymond_kelly	335	vance_manhattan	154.986
paul_browne	328	possession_weapon	152.314
enforcement_official	305	federal_agent	149.641
defense_lawyer	304	corruption_case	146.969
federal_district	298	criminal_complaint	141.625
commissioner_raymond	297	official_misconduct	138.953
chief_spokesman	272	spokesman_paul	133.608
manhattan_district	269	sexual_assault	132.272
federal_prosecutor	264	browne_police	124.701
city_police	263	enforcement_official	116.430
department_chief	230	maximum_sentence	100.206
browne_said	193	witness_stand	93.526
assistant_district	188	attempted_murder	93.526
said_police	184	people_arrested	93.526

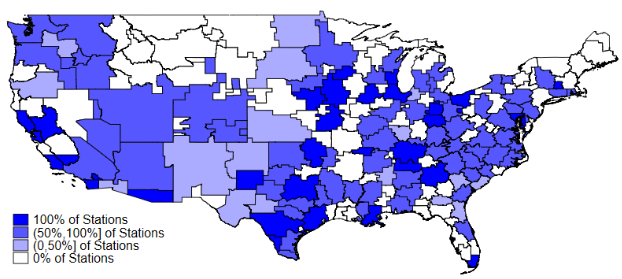
Notes: This figure reports the 25 bigrams with the highest frequency and the 25 bigrams with the highest relative frequency. The frequency is the number of times the bigram appears in the crime library. The relative frequency is the number of times the bigram appears in the crime library over the number of times the bigram appears in the non-crime library. We set the relative frequency equal to 999 in cases in which the bigram only appears in the crime library.

Appendix Figure 3: Validation of Local Stories Classification



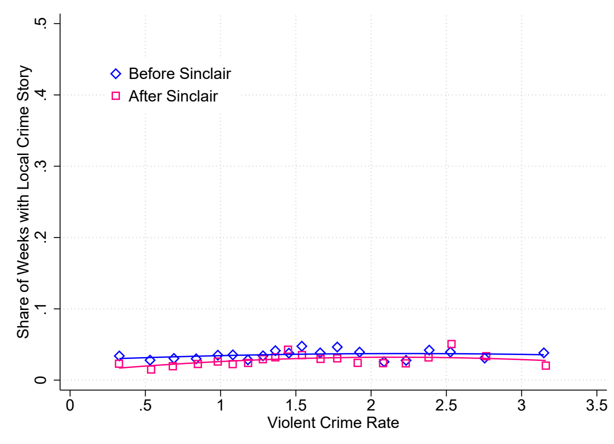
Notes: This figure shows a histogram of the crime topic share separately by whether local stories are classified to be about crime or not according to the methodology described in Section 3. Crime topic shares are from an unsupervised LDA model trained on local stories. Stories are defined to be local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

Appendix Figure 4: Map of Media Markets Included in the Content Sample

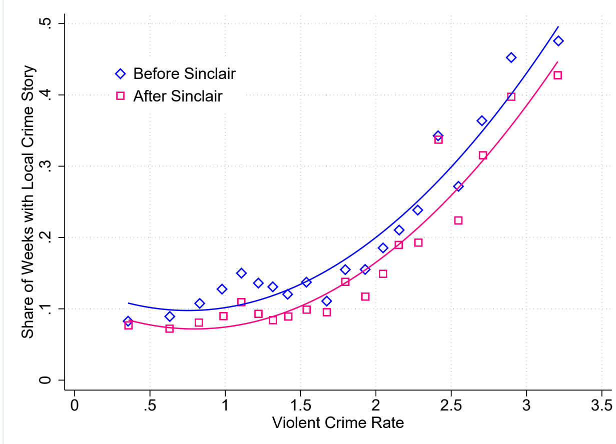


Notes: This map shows the share of stations for which we have content data continuously from 2010-2017 across media markets in the United States. Darker colors correspond to higher shares of media market stations included in the content data. 61% of media market have at least one station included in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

Appendix Figure 5: Relationship Between Violent Crime Rates and Share of Weeks with Local Crime Story Before and After Sinclair Ownership, by Covered Status



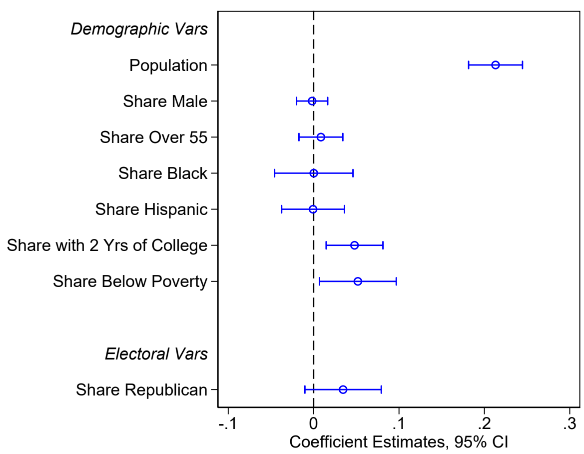
(a) Non-Covered Municipalities



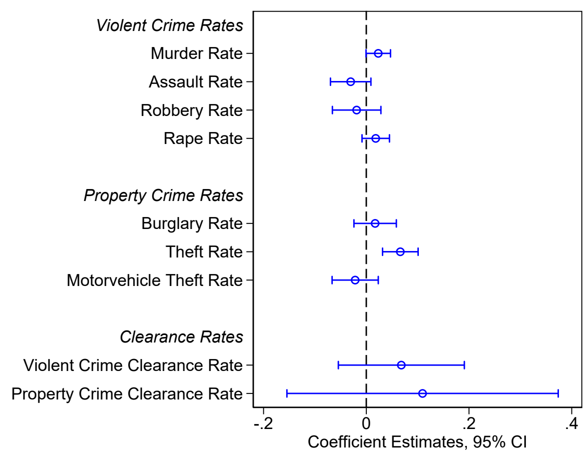
(b) Covered Municipalities

Notes: This figure shows how the relationship between violent crime rates and local crime reporting changes with Sinclair ownership, 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 acquires the station, for non-covered municipalities. Panel (b) shows the same binned scatter plot for covered municipalities. The sample is restricted to stations that are ever owned by Sinclair. Covered municipalities are mentioned in the news more than the median municipality in 2010. Crime rates are IHS crimes per 1,000 people, winsorized at the 99% level.

Appendix Figure 6: Differences Between Covered and Non-Covered Municipalities



(b) Socio-economic Characteristics



(b) Crime and Clearance Rates

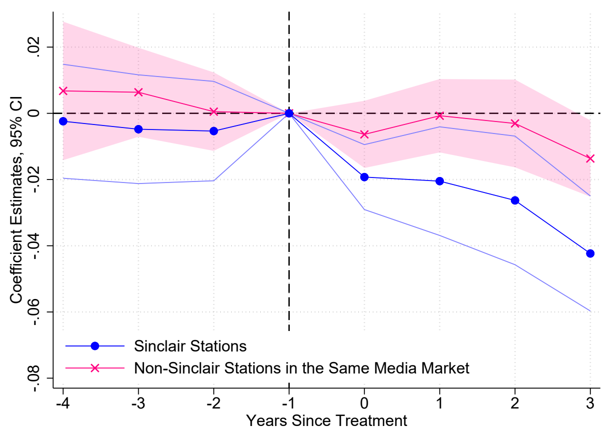
Notes: This figure shows along which dimensions covered and non-covered municipalities differ. We report coefficient estimates together with 95% confidence intervals from a regression of an indicator variable for the municipality being covered at baseline on standardized socio-economic characteristics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

Appendix Figure 7: Correlation of Coverage Over Time

	2010	2011	2012	2013	2014	2015	2016	2017
2010	1.000	0.970	0.960	0.961	0.956	0.946	0.953	0.948
2011	0.970	1.000	0.972	0.966	0.961	0.952	0.957	0.951
2012	0.960	0.972	1.000	0.968	0.960	0.953	0.956	0.953
2013	0.961	0.966	0.968	1.000	0.968	0.958	0.957	0.954
2014	0.956	0.961	0.960	0.968	1.000	0.966	0.963	0.958
2015	0.946	0.952	0.953	0.958	0.966	1.000	0.972	0.964
2016	0.953	0.957	0.956	0.957	0.963	0.972	1.000	0.971
2017	0.948	0.951	0.953	0.954	0.958	0.964	0.971	1.000

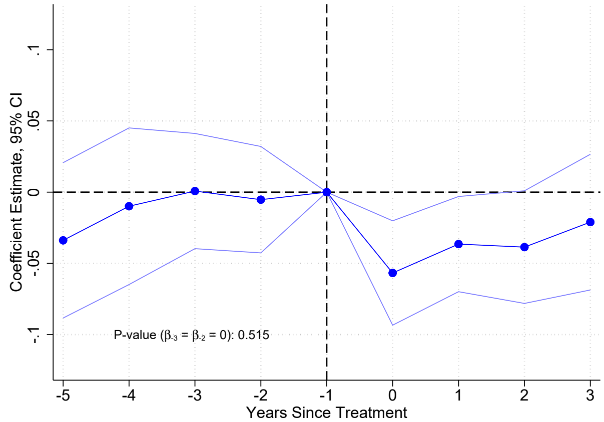
Notes: This figure shows that covered status persists over time. In particular, it shows the correlation of the share of weeks that a given municipalities appears in the news in different years. The sample is restricted to media markets that never experience Sinclair entry.

Appendix Figure 8: Effect of Sinclair Ownership for Sinclair Stations and Stations in the Same Media Market on the Probability of Having a Local Crime Story, by Year since Treatment



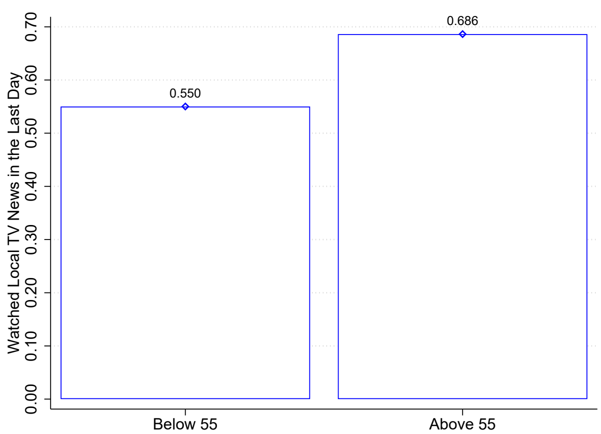
Notes: This figure shows the effect of Sinclair entry on the probability that a station reports local crime stories about covered municipalities relative to non-covered municipalities, by year since treatment, separately for stations owned by Sinclair and for non-Sinclair stations in Sinclair media markets. 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 entry and an indicator variable for whether the municipality is covered at baseline, defined separately for Sinclair and non-Sinclair stations, station by week fixed effects, covered status by week fixed effects, and station by municipality fixed effects. The sample excludes always treated media markets. The omitted category is T-1. Standard errors are clustered at the media market level. The dataset is a municipality-station pair by week panel. There are multiple stations in each media market covering the same municipalities, and the municipality-station pair is the cross-sectional unit of interest. Treatment is defined at the monthly level, but the effect is constrained to be the same by year since treatment. Covered municipalities are mentioned in the news more than the median municipality in 2010.

Appendix Figure 9: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by Year since Treatment, Estimated Including Data for 2009

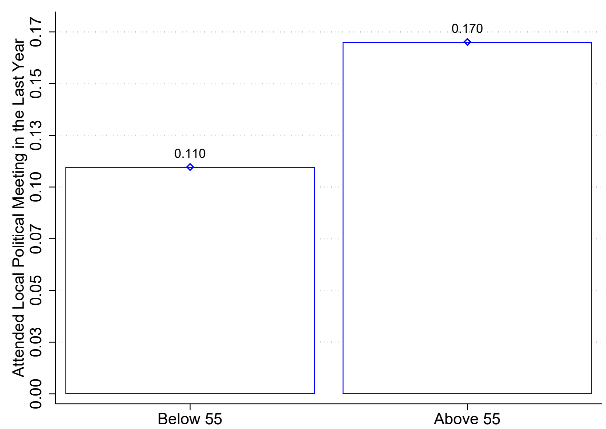


Notes: This figure shows the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities, by year since treatment, using data that include 2009. We report coefficient estimates and 95% confidence intervals from a regression of the municipality's violent crime clearance rate on the interaction between indicator variables for years since Sinclair entry and an indicator variable for whether the municipality is covered at baseline, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (4)). The sample excludes always treated media markets. The omitted category is T-1. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix Figure 10: Local News Viewership and Political Participation, by Age



(a) Watched Local TV News



(b) Attended a Local Political Meeting

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

Appendix Table 1: Sample Summary

	Overall	Included in the Content Analysis
	(1)	(2)
# of Stations	835	325
# of Stations Ever Controlled by Sinclair	117	35
# of Stations Ever Owned and Operated by Sinclair	106	34
# of Stations Ever Owned and Operated by Cunningham	10	1
# of Stations Ever Controlled by Sinclair through a Local Marketing Agreement	11	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 2: Descriptive Statistics

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

Notes: This table reports descriptive statistics for the main variables considered in the analysis and for municipality characteristics. Columns (1) to (3) restrict the sample to municipalities included in the main analysis; columns (4) to (6) include all municipalities with more than 10,000 inhabitants. Column (7) reports the *p*-value of the difference between the two samples from a regression of the specified characteristic on a dummy for the municipality being included in the analysis, with standard errors clustered at the media market level. The content analysis includes 2253 municipalities. 1792 of these municipalities are also in the police behavior analysis. The reference sample additionally includes 573 municipalities that satisfy the conditions to be included in the police behavior analysis, but are located in media markets for which we have no content data (see Appendix B for a detailed explanation). Content and crime and clearance rates are measured in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes. Crime rates are IHS crimes per 1,000 people. Both clearance rates and crime rates are winsorized at the 99% level.

Appendix Table 3: Sinclair Entry and Media Market 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	0.017	-0.001	0.009	0.104	-0.265	0.009*	0.003	-0.002
	(0.004)	(0.021)	(0.028)	(0.063)	(0.080)	(0.170)	(0.005)	(0.002)	(0.007)
Observations	1648	1648	1648	1648	1648	1648	1648	618	618
Clusters	206	206	206	206	206	206	206	206	206
Outcome Mean in 2010	13.561	49.412	10.783	83.240	11.808	9.454	3.539	0.432	0.515
	Panel B: DMAs in Content Data								
Sinclair	0.000	0.029	-0.008	0.089	0.086	-0.045	0.006	-0.000	0.003
	(0.005)	(0.021)	(0.031)	(0.085)	(0.105)	(0.208)	(0.006)	(0.003)	(0.007)
Observations	904	904	904	904	904	904	904	339	339
Clusters	113	113	113	113	113	113	113	113	113
Outcome Mean in 2010	14.157	49.290	10.833	80.730	14.215	9.564	3.580	0.422	0.511

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

Appendix Table 4: Effect of Sinclair Ownership on the Probability of Having a Local Story, Differences-in-Differences Decomposition

Dependent Variable	Had Local Crime Story						
Sample	Non-Covered		Covered		Covered and Non-Covered		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sinclair	-0.004	-0.003	-0.034**	-0.031**	-0.003	-0.002	
	(0.003)	(0.003)	(0.013)	(0.013)	(0.003)	(0.003)	
Sinclair * Covered					-0.027**	-0.029**	-0.023***
					(0.011)	(0.011)	(0.007)
Observations	1643158	1643158	1500202	1500202	3143360	3143360	3143360
Clusters	90	90	113	113	113	113	113
Municipalities	1108	1108	1145	1145	2253	2253	2253
Stations	278	278	325	325	325	325	325
Outcome Mean in 2010	0.017	0.017	0.174	0.174	0.092	0.092	0.092
Station by Municipality FE	X	X	X	X	X	X	X
Week FE	X	X	X	X	X	X	X
Controls by Week FE		X		X	X	X	X
Covered by Week FE						X	X
Station by Week FE							X

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

Appendix Table 5: Effect of Sinclair Ownership on Conservative Coverage of Non-Local Crime Stories

Dependent Variable	Share of Stories About…		Has Non-Local Story About…		
Type	Non-Local Crime	Non-Local Police	Police Misconduct	Crime and Drugs	Crime and Immigrants
	(1)	(2)	(3)	(4)	(5)
Sinclair	0.002	0.001	-0.026**	0.074***	0.066***
	(0.003)	(0.002)	(0.013)	(0.025)	(0.020)
Observations	31120	31120	31120	31120	31120
Clusters	113	113	113	113	113
Stations	325	325	325	325	325
Outcome Mean in 2010	0.133	0.063	0.070	0.800	0.188
Station FE	X	X	X	X	X
Month FE	X	X	X	X	X
Media Market Controls	X	X	X	X	X

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

Appendix Table 6: Effect of Sinclair Ownership on the Probability of Having a Local Story, by Whether the Story is about Crime

Dependent Variable	Had a Local Story		
Decomposition	Any	Crime	Non-Crime
	(1)	(2)	(3)
Sinclair * Covered	-0.032**	-0.018***	-0.023
	(0.014)	(0.007)	(0.014)
Observations	3143360	3143360	3143360
Clusters	113	113	113
Municipalities	2253	2253	2253
Stations	325	325	325
Outcome Mean in 2010	0.248	0.092	0.221
Station by Week FE	X	X	X
Covered by Week FE	X	X	X
Station by Municipality FE	X	X	X
Sinclair * Controls	X	X	X

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

Appendix Table 7: Effect of Sinclair Ownership on the Probability of Having a Local Crime Story, by Political Leaning of the Municipality

Dependent Variable	Had Local Crime Story	
Share Republican	>= Median	< Median
	(1)	(2)
Sinclair * Covered	-0.017**	-0.019*
	(0.007)	(0.010)
Observations	1567082	1559558
Clusters	99	86
Municipalities	1123	1116
Stations	285	249
Outcome Mean in 2010	0.079	0.104
Station by Week FE	X	X
Covered by Week FE	X	X
Station by Municipality FE	X	X
Sinclair * Controls	X	X

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

Appendix Table 8: Effect of Sinclair Ownership 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***	-0.005
	(0.006)	(0.004)
Observations	3143360	3143360
Clusters	113	113
Municipalities	2253	2253
Stations	325	325
Outcome Mean in 2010	0.089	0.025
Station by Week FE	X	X
Covered by Week FE	X	X
Station by Municipality FE	X	X
Sinclair * Controls	X	X

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

Appendix Table 9: Effect of Sinclair Entry on Violent Crime Rates

Type of Crime	All	Murder	Assault	Robbery	Rape
	(1)	(2)	(3)	(4)	(5)
	Panel A: Dependent Variable as Crime Rates				
Sinclair * Covered	0.029	0.003	0.013	0.047***	-0.025
	(0.035)	(0.004)	(0.035)	(0.017)	(0.024)
Observations	14336	14336	14336	14336	14336
Clusters	112	112	112	112	112
Municipalities	1792	1792	1792	1792	1792
Outcome Mean in 2010	1.673	0.034	1.233	0.720	0.300
	Panel B: Dependent Variable as Dummy = 1 if ≥ 1 Crime				
Sinclair * Covered	-	0.029	-0.001	-0.010	0.045**
	-	(0.036)	(0.004)	(0.014)	(0.017)
Observations	-	14336	14336	14336	14336
Clusters	-	112	112	112	112
Municipalities	-	1792	1792	1792	1792
Outcome Mean in 2010	-	0.462	0.910	0.964	0.932
Media Market by Year FE	X	X	X	X	X
Covered by Year FE	X	X	X	X	X
Municipality FE	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X

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

Appendix Table 10: Effect of Sinclair Entry on Property Crime Rates

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

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

Appendix Table 11: Effect of Sinclair Entry on the Violent Crime Clearance Rate, Differences-in-Differences Decomposition

Dependent Variable	Violent Crime Clearance Rate						
Sample	Non-Covered		Covered		Covered and Non-Covered		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sinclair	0.029*	0.032**	-0.002	-0.006	0.026*	0.029**	
	(0.015)	(0.013)	(0.009)	(0.009)	(0.013)	(0.014)	
Sinclair * Covered					-0.027**	-0.033**	-0.032**
					(0.013)	(0.014)	(0.015)
Observations	6480	6480	7856	7856	14336	14336	14336
Clusters	86	86	112	112	112	112	112
Municipalities	810	810	982	982	1792	1792	1792
Outcome Mean in 2010	0.434	0.434	0.483	0.483	0.461	0.461	0.461
Municipality FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Controls by Year FE		X		X	X	X	X
Covered by Year FE						X	X
Media Market by Year FE							X

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

Appendix Table 12: Effect of Sinclair Entry on the Property Crime Rate, Differences-in-Differences Decomposition

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

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

Appendix Table 13: Effect of Sinclair Entry on Police Spending and Employment

Dependent Variable	Police Expend. Per Capita	Judicial Expend. Per Capita	Police Employees per 1,000 People	Police Employees per 1,000 People	Police Officers per 1,000 People
	(1)	(2)	(3)	(4)	(5)
Sinclair * Covered	-0.001	-0.002	0.131	-0.043	-0.031
	(0.004)	(0.002)	(0.168)	(0.028)	(0.020)
Observations	8551	8551	9574	14335	14335
Clusters	109	109	111	112	112
Municipalies	1389	1389	1518	1792	1792
Outcome Mean in 2010	0.242	0.019	2.974	2.381	1.855
Media Market by Year FE	X	X	X	X	X
Covered by Year FE	X	X	X	X	X
Municipality FE	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X

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

Appendix Table 14: Effect of Sinclair Entry on Drug-Related Arrests

Dependent Variable	Number of Drug-Related Arrests				
Type of Crime	All	Cannabis	Heroine/ Cocaine	Synthetic Narcotics	Other
	(1)	(2)	(3)	(4)	(5)
Sinclair * Covered	0.152***	0.133**	0.238*	0.155	0.036
	(0.038)	(0.058)	(0.133)	(0.147)	(0.112)
Observations	9312	9312	9312	9312	9312
Clusters	98	98	98	98	98
Municipalities	1164	1164	1164	1164	1164
Outcome Mean in 2010	5.673	4.879	3.381	1.838	3.475
Media Market by Year FE	X	X	X	X	X
Covered by Year FE	X	X	X	X	X
Municipality FE	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X

Notes: This table shows the effect of Sinclair entry on drug-related arrests in covered municipalities relative to non-covered municipalities. We regress the number of drug-related arrests in the municipality on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). Column (2) to (5) estimate the regression for arrests related to specific drug types. The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share below the poverty line, and Republican vote share in the 2008 presidential election. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is defined at the yearly level. A media market is considered treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Arrests are under the IHS transformation, winsorized at the 99% level.

Appendix Table 15: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by 55+

Share 55+	>= Median	< Median
	(1)	(2)
	Panel A: Had a Local Crime Story	
Sinclair * Covered	-0.017**	-0.019**
	(0.007)	(0.008)
Observations	1551198	1579204
Clusters	102	100
Municipalities	1119	1118
Stations	302	297
Outcome Mean in 2010	0.074	0.107
Station by Week FE	X	X
Covered by Week FE	X	X
Station by Municipality FE	X	X
Sinclair * Controls	X	X
	Panel B: Violent Crime Clearance Rate	
Sinclair * Covered	-0.069**	-0.004
	(0.028)	(0.028)
Observations	7088	7056
Clusters	98	93
Municipalities	886	882
Outcome Mean in 2010	0.461	0.460
Media Market by Year FE	X	X
Covered by Year FE	X	X
Municipality FE	X	X
Sinclair * Controls	X	X

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

Appendix Table 16: Effect of Sinclair Ownership on the Probability of Having a Local Crime Story, by Whether the Story is about a Crime Incident or an Arrest

Dependent Variable	Had Local Crime Story	
Story Related to	Crime	Arrest
	(1)	(2)
Sinclair * Covered	-0.018***	-0.002
	(0.007)	(0.002)
Observations	3143360	3143360
Clusters	113	113
Municipalities	2253	2253
Stations	325	325
Outcome Mean in 2010	0.084	0.019
Station by Week FE	X	X
Covered by Week FE	X	X
Station by Municipality FE	X	X
Sinclair * Controls	X	X

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

Appendix A: Institutional Setting

Media Markets

A media market, also known as designated market area (DMA), is a region where the population receives the same television and radio station offerings. Media markets are defined by Nielsen based on households' viewing patterns: a county is assigned to a media market if that media market's stations achieve the highest viewership share. As a result, media markets are non-overlapping geographies. Counties can be split across media markets, but this happens rarely in practice. As noted by Moskowitz (2021), only 16 counties out of 3130 are split across media markets. Similarly, while media markets are redefined by Nielsen every year, only 30 counties changed their media market affiliation between 2008 and 2016.

Multiple local TV stations belong to the same market. We focus on stations that are affiliated to one of the big-four networks (ABC, CBS, FOX, and NBC) as they they tend to take up most of the viewership and be the ones producing local newscasts. In fact, 85% of local TV stations that do so belong to this category (Papper (2017)). Networks are publishers that distribute branded content. Affiliated stations, although under separate ownership, carry the television lineup offered by the network while also producing original content. With few exceptions, each network has a single affiliate by media market.

Law Enforcement in the United States

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

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

Appendix B: Data Cleaning

Newscast Transcripts

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

Interpolation. To maximize sample size in the presence of short gaps in the data, we replace missing observations in spells shorter than two consecutive months using linear interpolation. In particular, we linearly interpolate the number of crime stories in which a municipality is mentioned in a given week. We define our main outcome, which is an indicator variable equal to one if the municipality was mentioned in a station's crime story in a given week, based on the interpolated variable. 3% of total observations are missing in the raw data and get replaced using this procedure.

UCR Data

Identifying and Cleaning Record Errors. UCR data have been shown to contain record errors and need extensive cleaning (Maltz and Weiss (2006), Evans and Owens (2007)). Following the state of the art in the crime literature, we use a regression-based method to identify record errors and correct them. The method is similar to procedures used, among others, by Evans and Owens (2007), Chalfin and McCrary (2018), Weisburst (2019) and Ba and Rivera (2022), but most closely follows Mello (2019).

For each city, we fit the time series of crimes and clearances 2009-2017 using a local linear regression with bandwidth two. We compute the absolute value of the percent difference between actual and predicted values (adding 0.01 to the denominators to avoid dealing with zeros) and identify an observation to be a record error if the percent difference exceeds a given threshold. The threshold is computed as the 99th percentile of the distribution of percent differences for cities within a population group.[27] We substitute observations that are identified as record errors using the predicted value from the time-series regression. We follow this procedure to clean the crime and clearance series of each type of crime (property, violent, murder, assault, robbery, rape, burglary, theft, and motor vehicle theft). Overall, around 1% of observations are substituted using this procedure.

Population Smoothing. To define crime rates we use a smoothed version of the population count included in the UCRs, again following the crime literature. In particular, we fit the population time series of city using a local linear regression with a bandwidth of 2 and replace the reported population with the predicted values. This is necessary because population figures are reported yearly, but tend to jump discontinuously in census years (Chalfin and McCrary (2018)).

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

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

Crime Reporting Issues. It is important to note that our findings on crime rates refer to crimes that the public reports to the police, so changes in crime reporting behavior might be potentially

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

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

Google Trends Data

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

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

Gallup Data

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

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

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

Appendix C: Classifying Local Crime News

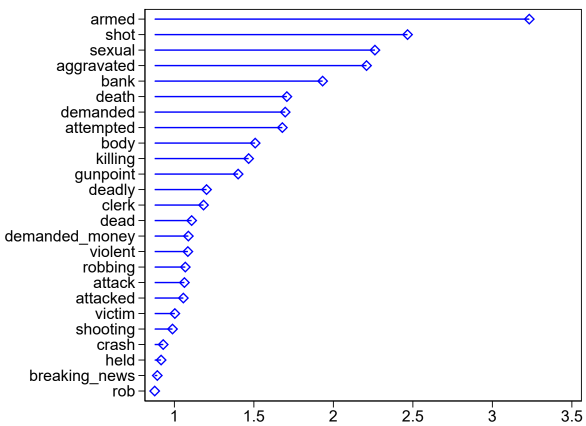
We build a classifier model that assigns a specific type of crime to each of the 464,356 local news stories about this topic in our sample. To train the model, we need a sub-sample of the stories to be labeled with the correct crime type. We create this sub-sample by performing a naive keyword search,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

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

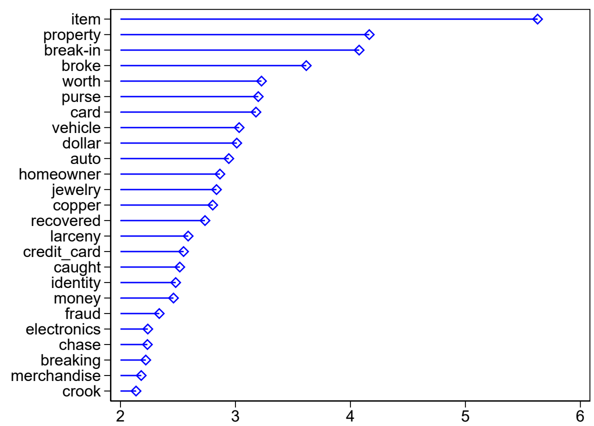
We selected these terms to minimize the presence of false positives. In fact, we checked using the full vocabulary that these keywords return words and bigrams that appear to be closely related to the crime considered. The training sample is then defined to be the sample of crime stories that contain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

We use this sub-sample to train a classifier model. In particular, we train a support vector machine model using stochastic gradient descent. The features that are used to predict the label are the most frequent 25,000 words and bigrams in the full corpus. We exclude the keywords used to define the original labels from the features, as they contain significant information for the training sample, but we already know that we will not be able to leverage this information for out-of-sample predictions. The features are TF-IDF weighted. We train the model on 80% of the sample, and use the remaining 20% as a test sample to evaluate model performance. We find that the three models perform well, with F1-scores of 0.84 (violent) and 0.80 (property). Appendix C Figure 1 shows the most predictive feature for each category. Reassuringly, the features selected by the different models appear to intuitively link to the respective crimes. We use the models to predict the category of the remaining 237,853 stories. Using this method, we are able to assign a crime type to almost all local crime stories. Overall, 38,177 stories (8%) are classified as having both a violent and a property crime.

Appendix C Figure 1: Most Predictive Features for News Type Classifier



(a) Violent



(b) Property

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

Appendix D: Robustness Checks

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

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

Robustness to Data Cleaning and Sample. We begin by showing that the choices we make when cleaning the content data and defining the outcome do not matter for the effect on the probability that a municipality appears in the news with a crime story. First, columns (2) and (3) show that the result is not affected if we identify crime stories using bigrams that are less (more) distinctively about crime, i.e., bigrams that are five (twenty) times more likely to appear in the crime-related versus the non-crime-related library. In addition, not replacing missing observations using linear interpolation as described in Appendix B (column (4)) or segmenting newscasts using a fixed number of words (column (5)) leaves the result unchanged. Similarly, restricting the sample to the same set of municipalities included in the analysis of clearance rates does not impact the result (column (6)).

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

Robustness of the Effect of Sinclair Entry on Clearance Rates

Appendix D Table 2 shows that the effect of Sinclair entry on the violent crime clearance rate is robust to decisions taken during data cleaning and alternative ways of defining Sinclair entry. Appendix D Table 3 shows robustness to alternative ways of defining the covered status of a municipality. In both tables, column (1) reports the baseline estimates for reference.

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

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

Robustness to Covered Status Definition. Finally, we show that our main result is also robust to alternative ways of identifying covered and non-covered municipalities. In our baseline specification, we define a municipality to be covered if it is mentioned in the news more than the median municipality in 2010. This decision is motivated by the fact that having control and treatment of similar size helps with power, but it is potentially concerning for two reasons.

First, this could be seen as an ad hoc decisions. In Appendix D Table 3 we show that the main result does not change if we split municipalities at the median after having residualized coverage on media market fixed effects (column (2)), if we predict covered status based on observable characteristics (column (3)), or if we measure coverage in different time periods (columns (4) to (6)).

Second, splitting at the median implies that municipalities close to the median might end up with a different covered status while receiving similar news coverage at baseline. To speak to this concern, we begin by showing in Appendix D Figure 1 that the effect on the violent crime clearance rate is increasing in pre-treatment coverage. In addition, we estimate a "donut" version of our baseline specification dropping municipalities between the 40th and 60th percentile of baseline coverage.

Appendix D Table 3 column (7) shows that the point estimate is barely affected by imposing this sample restriction. Finally, we show in column (8) that our main result is robust to a matching specification.[28]

Robustness to Heterogeneous Effects in TWFE Models

Recent work in the econometrics literature has highlighted that two-way fixed effects (TWFE) regressions recover a weighted average of the average treatment effect in each group and time period (de Chaisemartin and D'Haultfœuille (2020)). This is problematic because weights can be negative, which means that if treatment effects are heterogeneous, the TWFE estimates might be biased. No formal extension of these concepts to higher dimensional fixed effect models, such as the ones we use in this paper, is available as far as we are aware. Nonetheless, we provide four pieces of evidence consistent with the effect on the violent crime clearance rate being robust to concerns related to heterogeneous treatment effects in TWFE regressions.

First, we note that issues with negative weights are most severe when the majority of units in the sample are treated at some point. The fact that we have a large number of media markets that never experience Sinclair entry suggests that negative weights might have limited relevance in our setting. To quantify this statement, we implement the diagnostic test proposed by Jakiela (2021) by focusing on two specifications that only exploit the staggered timing of Sinclair entry, separately for covered and non-covered municipalities. We find that 31% of all treated observations receive a negative weight when we focus on non-covered municipalities (28% when we focus on covered municipalities). Consistent with what theory suggests, these observations are all in always treated units after 2014, as shown by the heat maps in Appendix D Figure 2. Because our event-study graphs exclude always treated observations but display patterns that are very much in line with our

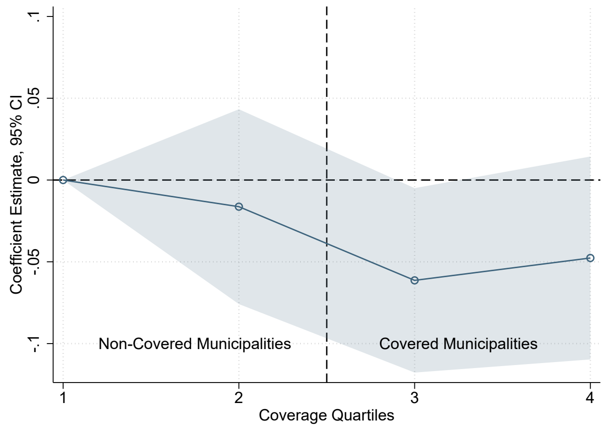
[28] We define a sample of covered municipalities and non-covered municipalities which are similar on a set of pre-specified characteristics, among municipalities in the top and bottom 40th percentile of the baseline coverage distribution. We match with common support and without replacement. The resulting sample includes 1366 municipalities, split between 658 covered and 658 non covered municipalities. To perform our matching algorithm, we employ the following set of covariates: log population, demographic characteristics (namely, share male, share over 55, share black, share Hispanic, and share with 2 years of college), economic characteristics (share below the poverty line) and, finally, political leaning (Republican vote share in the 2008 election). These are measured at baseline (i.e., in 2010) to avoid any post-treatment bias.

two-way fixed effects estimates, we are not concerned that the negative weights of always treated observations post-2014 drive our results.

Second, we ask directly whether there is evidence of treatment effect heterogeneity, again following Jakiela (2021). Appendix D Table 4 shows that we cannot reject that the slope of the relationship between the residualized outcome variable and the residualized treatment variable is linear, which suggests that the homogeneity assumption might not be off-base in our setting. In line with this result, Appendix D Figure 3 shows that event study graphs estimated using the robust estimator developed by de Chaisemartin and D'Haultfœuille (2020) display treatment effects consistent with our baseline estimates. Given that the differences-in-differences estimates that underlie our main effects are robust to allowing for treatment effects to be heterogeneous, we are confident in our triple differences estimates as well.

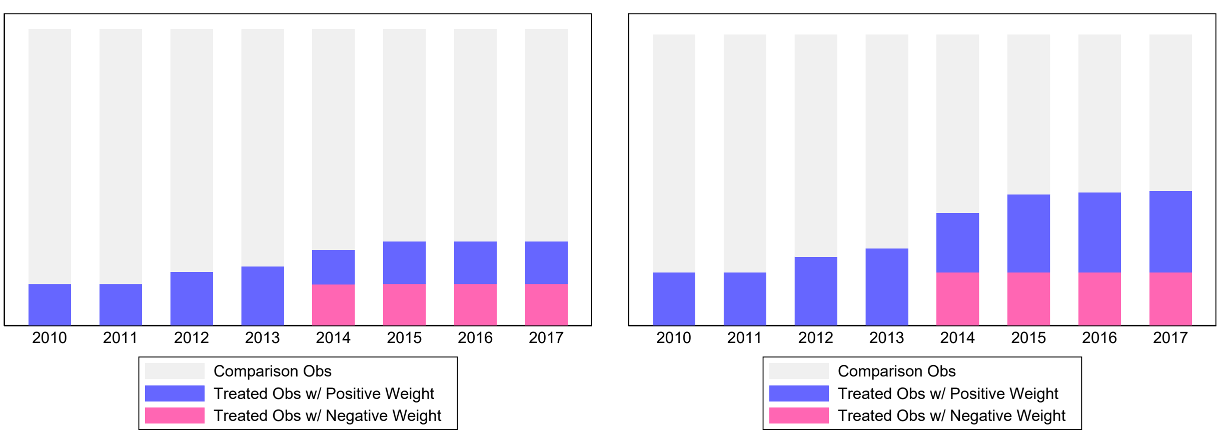
Finally, we show that our results are robust to artificially eliminating variation from the staggered timing of Sinclair entry. This is important to the extent that the issue of negative weights in staggered designs arises in part from using earlier treated units as control for later treated units (Goodman-Bacon (2021)), in line with what Appendix D Figure 2 also shows in our case. We eliminate variation from staggered timing by running regressions including only media markets that are either never treated or that are acquired at specific points in time, for all years in which Sinclair entered more than three media markets. Appendix D Table 5 shows that out of the four years we consider, three reproduce a negative coefficient. The magnitude of the effect is larger in two of them and not significant in one, but larger standard errors produce confidence intervals consistent with the main point estimate. Instead, we do not find a similar effect if we focus on media markets entered in 2013 only.

Appendix D Figure 1: Effect of Sinclair Entry on the Violent Crime Clearance Rate, by Coverage Quartile



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

Appendix D Figure 2: Effect of Sinclair Entry on the Violent Crime Clearance Rate, Test for Negative Weights in TWFE Models

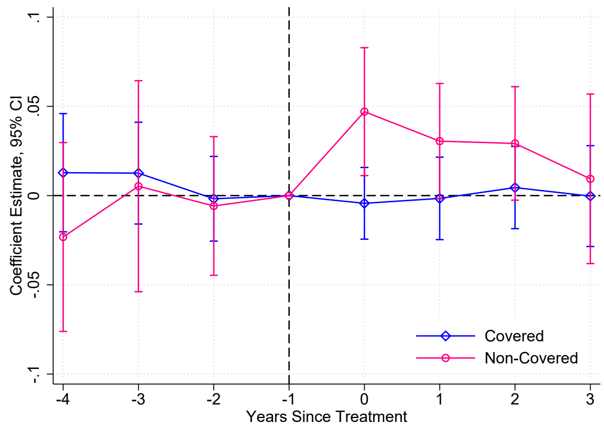


(a) Non-Covered Municipalities

(b) Covered Municipalities

Notes: The figure shows the weights used to calculate the two-way fixed effects estimates of the impact of Sinclair entry on the violent crime clearance rate, for two differences-in-differences designs that only exploit variation from the staggered timing of Sinclair entry separately for covered and non-covered municipalities. The weights are calculated following Jakiela (2021).

Appendix D Figure 3: Effect of Sinclair Entry on the Violent Crime Clearance Rate by Year since Treatment, Robustness to Heterogeneous Effects in TWFE Models



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

Appendix D Table 1: Effect of Sinclair Ownership on the Probability of Having a Local Crime Story, Robustness to Data Cleaning and Treatment Definition

Dependent Variable	Had Local Crime Story							
	Baseline	Data Cleaning and Sample					Treatment Definition	
Robustness to…		Less Restrictive Crime Story Definition	More Restrictive Crime Story Definition	No Imputation	Fixed Division of Newscasts into Stories	Same Sample as UCR Analysis	Stations Owned and Operated by Sinclair Only	Group Acquis. Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sinclair * Covered	-0.018***	-0.021***	-0.018***	-0.018***	-0.023***	-0.017**	-0.018***	-0.014**
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
Observations	3143360	3143360	3143360	3054074	3143360	2502984	2502984	2492952
Clusters	113	113	113	113	113	112	112	111
Municipalities	2253	2253	2253	2253	2253	1792	1792	1787
Stations	325	325	325	325	325	324	324	321
Outcome Mean in 2010	0.092	0.099	0.072	0.091	0.107	0.102	0.102	0.101
Station by Week FE	X	X	X	X	X	X	X	X
Covered by Week FE	X	X	X	X	X	X	X	X
Station by Municipality FE	X	X	X	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X	X	X	X

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

Appendix D Table 2: Effect of Sinclair Entry on the Violent Crime Clearance Rate, Robustness to Data Cleaning and Treatment Definition

Dependent Variable	Violent Crime Clearance Rate					
	Baseline	Data Cleaning		Treatment Definition		
Robustness to…		No Winsorizing	No Imputation	Drops DMAs with Divested Stations	Stations Owned and Operated by Sinclair	Group Acquis. Only
	(1)	(2)	(3)	(4)	(5)	(6)
Sinclair * Covered	-0.034**	-0.038**	-0.035**	-0.034**	-0.024*	-0.033*
	(0.016)	(0.017)	(0.017)	(0.016)	(0.014)	(0.018)
Observations	14336	14336	14336	14304	14336	13840
Clusters	112	112	112	111	112	104
Municipalities	1792	1792	1792	1788	1792	1730
Outcome Mean in 2010	0.461	0.462	0.461	0.460	0.461	0.459
Media Market by Year FE	X	X	X	X	X	X
Covered by Year FE	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X	X

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

Appendix D Table 3: Effect of Sinclair Entry on the Violent Crime Clearance Rate, Robustness to Covered Status Definition

Dependent Variable	Violent Crime Clearance Rate							
	Baseline	Covered Status Definitions					Donut	
Robustness to…		Residualized	Predicted	Jan-Jun 2010	Jul-Dec 2010	Jan-Jun 2011	Donut	Matching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sinclair * Covered	-0.034**	-0.029**	-0.029*	-0.028**	-0.025	-0.042**	-0.038**	-0.045*
	(0.016)	(0.013)	(0.017)	(0.014)	(0.016)	(0.017)	(0.018)	(0.026)
Observations	14336	14336	14336	14336	14336	14336	11600	10928
Clusters	112	112	112	112	112	112	112	112
Municipalities	1792	1792	1792	1792	1792	1792	1450	1366
Outcome Mean in 2010	0.461	0.461	0.461	0.461	0.461	0.461	0.451	0.458
Media Market by Year FE	X	X	X	X	X	X	X	X
Covered by Year FE	X	X	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X	X	X
Sinclair * Controls	X	X	X	X	X	X	X	X

Notes: This table shows the robustness of the effect of Sinclair entry on the violent crime clearance rate of covered municipalities relative to non-covered municipalities. We regress the municipality's violent crime clearance rate on the interaction between an indicator variable for Sinclair presence in the media market and an indicator variable for whether the municipality is covered at baseline, the interaction between an indicator variable for Sinclair presence in the media market and baseline municipality characteristics, media market by year fixed effects, covered status by year fixed effects, and municipality fixed effects (equation (3)). The characteristics included are log population, share male, share over 55, share black, share Hispanic, share with 2 years of college, share below the poverty line, and Republican vote share in the 2008 presidential election. Column (1) reports the baseline estimates, in which covered municipalities are municipalities mentioned in the news more than the median municipality in 2010. Column (2) defines a municipality as being covered if the residual from a regression of its baseline coverage in 2010 on media market fixed effects is above the median. Column (3) uses covered status predicted from a LASSO regression of the baseline municipality characteristics, the baseline municipality characteristics squared, and the baseline municipality characteristics cubed. In columns (4), (5), and (6), covered municipalities are municipalities mentioned in the news more than the median municipality in the first half of 2010, in the second half of 2010, and in the first half of 2011 respectively. Column (7) drops municipalities with baseline news coverage between the 40th and 60th percentile in 2010, while column (8) implements propensity score matching on the same sample. Standard errors are clustered at the media market level. The dataset is a municipality by year panel. Treatment is at the yearly level. A media market is treated in a given year if Sinclair was present in the market in the January of that year. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix D Table 4: Effect of Sinclair Entry on the Violent Crime Clearance Rate, Test for Heterogeneous Treatment Effects in TWFE Model

Dependent Variable	Residualized Violent Crime Clearance Rate	
Sample	Non-Covered	Covered
	(1)	(2)
Residualized Treatment	0.025**	-0.006
	(0.012)	(0.007)
Treatment	-0.001	-0.001
	(0.004)	(0.003)
Treatment * Residualized Treatment	0.011	0.010
	(0.020)	(0.012)
Observations	6480	7856

Notes: This table test whether treatment effect are likely to be heterogeneous across treated units following Jakiela (2021). We regress the residualized outcome on the treatment, the residualized treatment, and the interaction between the two, separately for non-covered (column (1)) and covered municipalities (column (2)). The residualized outcome is the residual from a regression of the municipality's violent crime clearance rate on municipality and year fixed effects. The treatment is an indicator variable for Sinclair presence in the media market. The residualized treatment is the residual from a regression of the treatment on municipality and year fixed effects. The dataset is a municipality by year panel. Treatment is at the yearly level. A media market is treated in a given year if Sinclair was present in the market in the January of that year. Covered municipalities are mentioned in the news more than the median municipality in 2010. Clearance rates are defined as total number of crimes cleared by arrest or exceptional means over total number of crimes, winsorized at the 99% level.

Appendix D Table 5: Effect of Sinclair Entry on the Violent Crime Clearance Rate, No Staggered Timing

Dependent Variable	Violent Crime Clearance Rate			
Media Markets Treated in…	2012	2013	2014	2015
	(1)	(2)	(3)	(4)
Sinclair * Covered	-0.101**	0.008	-0.022	-0.028*
	(0.047)	(0.043)	(0.020)	(0.014)
Observations	9536	9192	10168	9544
Clusters	62	59	71	63
Municipalities	1192	1149	1271	1193
Outcome Mean in 2010	0.439	0.434	0.442	0.437
Media Market by Year FE	X	X	X	X
Covered by Year FE	X	X	X	X
Municipality FE	X	X	X	X
Sinclair * Controls	X	X	X	X

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