Crime News Bias: Extent and Effects on Housing Markets

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January 5, 2026

Abstract

I estimate how routine crime coverage capitalizes into housing prices by separating the realized amenity from the information conveyed about it. I assemble a novel dataset linking nearly all U.S. gun homicides (2014-2018) to $\sim$1.5 million station-days of local news and to housing transactions, which allows me to benchmark crimes the media *could* cover against those it *does*. Identification leverages predictable crowd-out in airtime from local sports schedules. Coverage of a nearby homicide lowers sale prices by 8.1-8.5 percent. Additional evidence points to demand-side responses: buyers react to news reports while short-run housing supply remains unchanged. Having established this information channel, I document systematic coverage disparities across race: incidents with non-white victims or suspects are more likely to be covered than white-white cases, even conditional on extensive incident and neighborhood characteristics. Using the price estimates, I quantify the implied valuation effects of these coverage gaps.

Keywords:housing markets, crime, media bias; **JEL Codes:** R21, R31, J15, H00, L82

*University of Texas at San Antonio (jonathan.moreno-medina@utsa.edu). I would like to thank Pat Bayer, Chris Timmins, Rob Garlick, Fernando Ferreira, Paul Carrillo, Attila Ambrus, Maisy Wong, Daniel Xu, Viviana Rodriguez, Adam Rosen, Hugh Macartney, Rahul Gupta, Jimmy Roberts, Marcos Rangel, Sally Sadoff, Bocar Ba, CarlyWill Sloan, Brittany Street, Emily Leslie, as well as to members and attendees of the Duke Public Lab, Wharton Real Estate Workshop, Rady UCSD, Davidson College, National Urban Economics Association, European Urban Economics Association, Oregon State University, Southern Economic Association, NBER Real Estate Summer Institute, AREUEA National Conference, and Banco de la Republica. Usual caveat applies.

1 Introduction

How do prospective homeowners learn about neighborhood attributes-especially those that are hard to see? Research shows that when new information suddenly makes such attributes salient, housing prices revalue rapidly: air-quality alerts, updated cancer-risk scores, and the public release of a sex-offender's address, all shift nearby sale prices (Chay & Greenstone, 2005, Gayer *et al.*, 2000, Linden & Rockoff, 2008, Pope, 2008b,a). Crime perception is a canonical case in which information matters. Because direct victimization is rare, households typically form beliefs about neighborhood safety from ubiquitous local-news coverage (Mastrorocco & Ornaghi, 2025). This means that news coverage can affect market valuations of residential properties by shifting perceptions of crime risk. Furthermore, systematic distortions in the type of news that get more coverage, say across race demographics, could imply that the effect of the media in housing markets is not homogeneous. Yet, the link between media coverage of crime and housing markets is not well understood. This paper aims to fill in this gap.

To investigate this relationship and quantify its consequences for housing markets, I proceed in two parts. First, I estimate the causal effect of crime news coverage on housing markets and housing prices. Second, I document systematic racial disparities in what crimes receive coverage, and combine these results to show that biased reporting depresses prices in communities with more non-white residents (thereby compounding existing wealth gaps). To implement this study, I merge a near-census of U.S. gun homicides from 2014-2018 with a comprehensive archive of local television news-about 1.5 million station-days of broadcasts. Homicides are ideal for this purpose: they are rarely undercounted in police data and dominate crime airtime. With these data, I can construct an objective benchmark with which to measure biases in coverage, as it allows me to observe the crimes the media *could* cover and the subset it *does* cover. That is, these data allow me to define bias (also known as filtering bias) as the tendency to over-report or under-report homicides with different characteristics. I merge these data with real estate transaction-level data from Zillow (ZTRAX). I measure crime news exposure for homicides in the previous quarter before the estimated closing date for each transacted house, with the estimated closing time being 2 months before the date recorded in the deed (Anenberg & Laufer, 2017). This is done both in the extensive margin (probability of having at least one homicide that occurred in the vicinity of the house appearing in the news cycle), and in the total number of stories.

Leveraging this linkage, I show that crime news has a sizable impact on property values. Identifying the effects of news coverage on prices is challenging because media coverage of crime is not random. It is possible that the media responds to observable and unobservable characteristics of homicides, which in turn, can be correlated with other amenities affecting housing prices. For example, the salaciousness of a crime can be correlated with proximity to a school (which can affect housing prices), but this factor can, in turn, affect the decision by the media to cover said homicide or not. To address these endogeneity concerns I employ several strategies. First, to address the potential endogeneity of the actual crime level around a house, I include tract and month fixed-effects. This strategy relies on the assumption that given the high frequency of crime data, the exact location of crimes within a neighborhood in a given month is as good as random.

To identify the effect of crime news on nearby sale prices, I leverage predictable, exogenous variation in local stations' opportunity cost of airtime created by the schedule of local sporting events. The key idea is simple: when a homicide occurs the *day before* a local team's game, the following week is more likely to feature sustained discussion of that game, raising the opportunity cost of covering other stories; accordingly, we expect lower likelihood of coverage for that crime in the subsequent week. Consistent with this expectation, a homicide occurring one day before a game day decreases a nearby homicide's probability of being covered by about 10 percentage points (roughly 14% of the baseline coverage rate). The identification assumption is that whether a homicide falls before a game day is as good as random especially after conditioning on tract and month fixed effects. Balance checks show that the day-before-game indicator is balanced across victim sex and age, incident flags (e.g., domestic violence, gang, officer-involved), and key house characteristics once tract and month fixed effects are included. Notice that *I do not* use variation from homicides happening *on or after* the game day, which could raise concerns about reverse causality or emotion-driven effects from the game itself. Hence, the exclusion restriction is plausible in this setting. This stance is further supported by an event-study analysis showing parallel pre-trends in prices for areas near covered versus non-covered homicides. This design yields *house-level* variation in crime coverage that shifts within media markets, across neighborhoods, and week by weekgiving quasi-experimental exposure contrasts that single-city or aggregate settings cannot deliver.

The main result of the paper is as follows: the 2SLS estimates indicate that any news coverage of the homicides around a house decreases housing prices by between 8.1% to 8.5%. This effect is precisely estimated across different specifications, including adding other controls for the property characteristic. Using the total number of stories presents a very similar picture. I show that these results are robust to matching homicides to homes in a larger geographical area as well as using a more flexible set of trends including tract-by-month-by-year fixed effects. I also present evidence that these crime coverage effects persist for at least a year, are smaller for crimes that convey less information for unrelated buyers (like domestic violence ones), and that the effects are homogeneous across the victim's race.

I then explore whether these effects are demand- or supply-driven. In principle, the marginal impact of news on demand would likely be larger if buyers were less informed about a homicide than sellers in the absence of news. This is because sellers are likely aware of both covered and non-covered homicides, while buyers, especially those from other areas, may only learn of them through coverage. If sellers perceive their area as less secure, they might list their house for sale regardless of coverage, suggesting non-covered homicides could also affect prices. However, I find no evidence that non-covered homicides impact housing prices, consistent with a model where supply is unresponsive, but demand drives the effect. Furthermore, a difference-in-differences analysis comparing ZIP codes with covered versus non-covered homicides shows no significant difference in number of listings, indicating price effects stem from demand changes. I also find an immediate decline in median listing prices per square foot in ZIP codes with covered homicides. While these latter estimates are noisy, the evidence suggests demand shifts downward immediately after coverage.

In the second part of this paper I explore to what extent crime coverage is biased across race. Here, I present two new stylized facts about local media coverage of crime. Comparing within media market coverage rates between types of homicides by race profile, I find that there are systematic gaps in their coverage: cases with a white victim and a non-white suspect are around 6 percentage points (p.p.) more likely to appear in the news at least once - around 10% of the overall mean. Similarly, white victim/non-white suspect incidents have 27% more stories in the media. I also find evidence for gaps, although smaller, for homicides with non-white victims/white suspect and non-white victims/non-white suspect. These gaps are not explained by any observable characteristics of the homicide, including atypicality of the incident, victim or suspect level characteristics (including age, sex, or knowing the name of victim/suspect), incident level characteristics (number of killed victims or type of homicide), nor neighborhood level characteristics (within-DMA neighborhood wealth, neighborhood population, or race composition). I do find suggestive evidence that these gaps in coverage can be partially driven by the demographic composition of each media market. These estimates allow for a simple back-of-the-envelope calculation of the effect of crime news biases on welfare, through the loss in housing prices from communities that are over-represented in the news cycle. I calculate that for houses in predominantly minority neighborhoods, racial bias in news coverage further decreases prices by 0.49% lower than what they would be without racial bias in crime coverage.

Literature. This paper contributes to several strands of research. First, I relate to work on crime and housing markets (Curci & Masera, 2023, Linden & Rockoff, 2008, Pope, 2008b), which typically studies capitalization of *observed* crime risk or registry signals rather than variation in information per se. Existing evidence on capitalization of crime is mixed and frequently relies on risk proxies: cross-sectional work reports effects for property damage (Gibbons, 2004), while quasi-experimental studies focus on sex-offender arrivals (Linden & Rockoff, 2008, Pope, 2008b) - a *risk signal* rather than a realized neighborhood incident. By contrast, I separate the realized amenity (a proximate homicide) from *local TV information* about that incident, and identify the information channel directly. Second, I speak to the literature on racial discrimination in housing and related markets (Christensen & Timmins, 2022, 2023, Avenancio-León & Howard, 2022, Hanson *et al.*, 2016). Although studies outside economics have noted racial bias in crime news, they are largely case-based; this is, to my knowledge, the first paper to document systematic, nationwide bias in local TV crime coverage and link it to housing outcomes. Finally, I join a growing body of work on local media's influence on socio-economic behavior and political polarization (Martin & McCrain, 2019, Enikolopov *et al.*, 2011), police clearance rates (Mastrorocco & Ornaghi, 2025), voter turnout (Cagé, 2020), international conflict (Durante & Zhuravskaya, 2018), juror decisions (Philippe & Ouss, 2018), attitudes toward migrants (Djourelova, 2023), and views on police killings (Moreno-Medina® *et al.*, 2025). Where prior media work largely targets political outcomes (DellaVigna & Kaplan, 2007, Martin & McCrain, 2019), I extend the lens to markets that depend on non-political information: housing. This is done using *multi-market, high-frequency* crowd-out to separate information from the amenity and trace its capitalization into housing prices.

The remainder of the paper is organized as follows. Section 2 describes the data. Section 3 estimates the effects of crime coverage on housing prices. Section 4 defines and measures media bias in crime reporting, and quantifies the welfare implications of biased coverage for neighborhood wealth. Section 5 concludes.

2 Data

2.1 Crime and News Data

Measuring filtering bias in crime news and evaluating its effects on housing values requires three key data features.[1] First, I need to observe the universe of homicides across media markets with sufficient information so they can be matched with news stories. Namely, I need to see either the exact location where the homicide takes place or the name of the victim. Second, I need to observe the universe of news stories about homicides across media outlets. Finally, data on housing transactions need to be spatially linked to each homicide incident and its media coverage. I rely on three distinct sources to construct the data with the needed features. Overall, I observe the almost universe of gun-related homicides and their media coverage. Table 1 presents descriptive statistics of the data. In total, I observe close to 49,000 homicides from January 2014 to March 2018 in the U.S. A total of 624,000 news stories cover these incidents. The data on housing is presented in Subsection 2.2 below.

Crime: Data on shooting incidents is taken from the Gun Violence Archive (GVA). This database is collected by a non-profit organization aiming at registering all known shootings in the country. Incidents in the GVA are collected daily from over 7,500 law enforcement, media, government and commercial sources. Each incident is verified by an initial researcher and a secondary validation process. Importantly, the GVA provides detailed information regarding the place where the incident occurred, as well as the date and time. The main benefit of the GVA is that it reports individual characteristics of the crime which can be matched with the news coverage. An equally important benefit of these data is that it allows to evaluate changes in housing prices exploiting high frequency variation in crime. This is in contrast with aggregated crime statistics, as these are potentially correlated with unobservables characteristics of the neighborhood. That is, the high frequency and detailed geographical data in the GVA reduce attenuation bias and partially solve omitted variable bias in the analysis of the effect of coverage of homicides on housing prices.

I restrict the analysis to shootings that lead to a homicide (excluding suicides that do not involve a shooting for anyone else). There are several reasons why focusing on coverage of homicides is a good proxy for overall crime. First, there is evidence that homicides are the best single predictor of an individual's perception of overall crime (Hipp, 2013).

[1] I provide a more detailed framework for the definition of bias in Subsection 4.1

Second, homicides are rarely underreported, both in official crime statistics and in the GVA, strengthening the case for why these data allow me to measure the space of possible covered news. Third, crime news cover homicides disproportionately more with respect to any other type of criminal incident (Mastrorocco & Ornaghi, 2025).[2]

For each homicide, I observe the characteristics of the victims and the suspects, if available. These characteristics include the age, sex, name, and precise location of the incident. When available, I use the name of the victim, and the geographical information of where the incident occurred, to impute the race of the subject.[3] Lastly, the GVA data oftentimes includes descriptions of the incident itself. I group these incident descriptions into dummies, as specified in Subsection C.2.

News stories: Data on media crime coverage comes from News Exposure, a news media monitoring company. News Exposure collects records of all news stories from local TV channels. The information they gather includes the station, the network affiliation, the media market, the date and time of the broadcast, run time, publicity value, ad value, rating estimate, and a snippet of the caption.

For each victim recorded in the GVA data, I match all the news stories associated with the homicide in question using a matching procedure that follows three sets of requirements. First, stories need to relate to a homicide. I ensure this by conditioning that the caption has words that relate to a homicide such as "murder", "killed", etc. (in English or Spanish) - excluding any article containing the word "suicide". Second, the stories need to contain either the name of the victim or the address (block and street) in which the event happened. Third, stories need to be aired within 7 days of the victim's death. In evaluating the role of local news coverage, I further restrict the sample to news stories provided only by networks operating on the media market where the victim died. For further details on the algorithm used to match news stories to homicides, see Appendix C. The merged stories with homicides come from approximately 1,000 TV stations in the U.S..

[2] Concerns for the accuracy of the GVA data arise if there is potential for selection in reporting of certain types of crimes. I assess the accuracy of the GVA data through restricted-access administrative data from the Medical Examiner's Office (NVDRS-RAD) which is the underlying data used for CDC reporting on crime. Overall, I find that the GVA data tracks the trends of CDC well.

[3] The algorithm uses the probability of being part of the race group based on the name and Census tract, with a 50 percent probability threshold as in Collinson *et al.* (2023). I obtain similar results if the imputation is based on the demographics of the Census tract instead (correlation between probabilities is 0.7 for White, 0.65 for Black, 0.83 for Hispanic, 0.69 for Asian, and 0.77 for Other). This posterior probability is estimated using the package WRU in R, based on Imai & Khanna (2016) and Khanna *et al.* (2017). See Subsection C.3 for more details on this imputation procedure, as well as how possible measurement error would affect my estimate of bias by race.

Table 1: Characteristics of Homicides by Coverage, 2014-2018

	Full Sample			Covered			Not Covered		
	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD	Obs.
Panel A. Victim and Suspect:									
Victim Non-White (NWV)	0.64	0.48	48017	0.66	0.47	33670	0.60	0.49	14347
Victim Male	0.83	0.37	47698	0.82	0.38	33497	0.85	0.36	14201
Victim Age	32.39	14.50	42431	32.14	14.36	30452	33.02	14.85	11979
Victim Has Name	0.84	0.37	48445	0.88	0.33	33892	0.75	0.43	14553
Suspect Non-White (NWS)	0.58	0.49	25865	0.59	0.49	18892	0.54	0.50	6973
Suspect Male	0.94	0.23	24126	0.95	0.23	17661	0.94	0.24	6465
Suspect Age	31.28	14.09	17806	30.95	13.81	13247	32.23	14.83	4559
Suspect Has Name	0.39	0.49	48445	0.41	0.49	33892	0.33	0.47	14553
Panel B. Neighborhood :									
Ln Median House Value	11.72	0.66	47607	11.69	0.65	33331	11.77	0.68	14276
Ln Pop. Tract	8.29	0.52	48398	8.28	0.52	33865	8.30	0.52	14533
Share White	0.37	0.32	48398	0.37	0.31	33865	0.39	0.33	14533
Panel C. DMA:									
Ln Median House Value in DMA	12.08	0.43	48445	12.07	0.41	33892	12.12	0.45	14553
Ln Pop. DMA	14.84	1.05	48445	14.79	0.99	33892	14.95	1.18	14553
# Stations	9.45	6.09	48445	9.15	5.77	33892	10.13	6.73	14553
Dem. Share (2016)	0.48	0.11	48275	0.47	0.11	33792	0.49	0.12	14483
Share White	0.61	0.16	48445	0.62	0.16	33892	0.60	0.17	14553
Share Black	0.16	0.11	48445	0.16	0.11	33892	0.15	0.10	14553
Share Hispanic	0.19	0.00	48445	0.19	0.00	33892	0.19	0.00	14553
Panel D. Stories:									
Stories per Victim				17.84	21.44	33892			
Stories per Victim Per Station				2.44	2.68	33892			

This table shows the descriptive statistics for the homicides in the sample. Victim/Suspect Non-White are dummy variables equal to one if the posterior probability based on the name of the subject and racial composition of the tract where the incident occurred is greater than 0.5. *Source:* GVA; News Exposure.

Table 1 presents characteristics of victims for: the whole sample (Columns 1 to 3); victims covered at least once in the news (Columns 4 to 6); victims without any coverage (Columns 7 to 9). About two-thirds of the victims in the GVA are covered by local TV news stations at least once. Interestingly, the difference is relatively small in levels for age, tract-level share white, and share Hispanic. Larger differences arise when comparing tract-level share black population and median house values, as victims from poorer and predominately black neighborhoods appear in the news with a larger probability. Female victims appear in the news more often than they would by their share in the victim population. Finally, victims for which the name is unknown are relatively less covered, but the difference is surprisingly small (75% for victims with unknown names versus 88% for victims with known names). Overall, there are 12 stories per victim in their media market (from now on DMA - or Designated Market Area) on average, and 17.9 stories per victim with at least one story in the news, as well as 2.46 stories per victim per station (see the whole distribution in Figure B.1, the number of stations by DMA in Figure B.3). The distribution of the news stories by the affiliated network is in Figure B.2, showing a clear dominance by the so-called 'big 4' (NBC, ABC, CBS, and FOX).

Other data: In some analysis I also use the population and homicide rates at baseline (in 2013) at the county level coming from the CDC Wonder dataset. [4]

2.2 Housing and Instrument for Crime News Coverage Data

Real estate transactions: Housing transactions data come from the Zillow Transaction and Assessment Dataset (ZTRAX)[5]. ZTRAX is a comprehensive dataset containing information on sales, prices, buyers, mortgages, geographic information, and property characteristics for close to 400 million transactions across the United States. I limit the data to include transactions for regular, *arm's-length* sales of residential, single-family houses that are recorded as deed transfers, deeds with concurrent mortgage, foreclosures, or mortgages. To ensure comparability, I drop any parcel that is recorded more than once on the same day, trim observations with nominal sale prices below $10,000 or above $900,000. Because homicides are a relatively rare incident in many tracts across the US, I include in the final sample only tracts where there is at least one homicide throughout the whole sample[6]. Geolocation is provided for every transacted unit in the ZTRAX records [7]. I rely on this geolocation to match the house sale with the homicides recorded in the GVA within 0.5 miles, and in the previous 3 months before the expected closing of the house [8].

Table 2 presents the mean and standard deviation of the set of houses with and without matched homicides in the final sample. Figure 1 shows the distribution of house prices by homicides and homicides coverage. Not surprisingly, houses without any homicide in their proximity are more expensive, newer, and larger. For those houses with at least one

[4]The standard homicide definition by the CDC is given by ICD-10 codes U01U02, X85Y09, and Y87.1. I estimate the rate in the last 5 years before the sample (2009-2013) as the ratio of the number of homicide victims divided by the average yearly population. If there are less than 10 victims in those 10 years the CDC supresses the actual number, and so I impute it at the minimum or maximum. The numbers presented in the heterogeneity analysis use the minimum number of victims. The results are virtually unchanged if using the maximum.

[5]Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author(s) and do not reflect the position of Zillow Group.

[6]Subsection C.4 details the full set of criteria used to construct the final sample.

[7]Zillow reports an estimated geolocation within the correct block depending on the number of the property. For example, the property at 1150 Fake Street would be presented to be located halfway through block 1100 of Fake Street. This measurement error is arguably random and thus would bias the coefficients of coverage towards zero.

[8]Housing prices are usually agreed upon in advance from the final transfer. Following Anenberg & Laufer (2017), I consider the closing time to be 2 months before the date recorded in the deed.

homicide, the average number of homicides is 1.3 and the number of stories about those homicides in the media is 15.45. On average, those news stories reached 732,621 people for each transacted house. The figure also shows that the difference in prices between those with a covered homicide and those with a non-covered homicide occurs throughout the price distribution (compare the solid blue line with the dashed yellow). In order to evaluate how much of this difference in prices is driven by the causal effect of coverage, I collect information on my proposed instrument for coverage, based on sporting events.

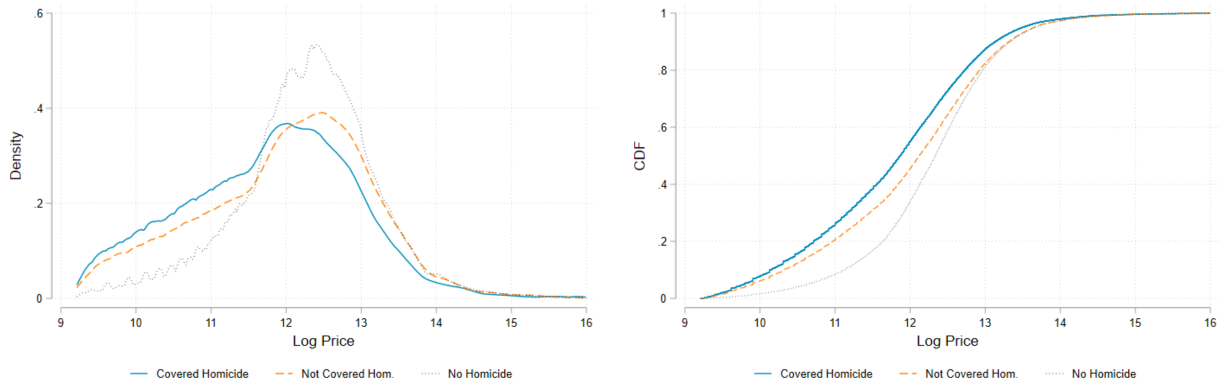
Table 2: Characteristics of Transacted Houses by Homicides, 2014-2018

	1 or More Homicides			No Homicides		
	Mean	SD	Obs.	Mean	SD	Obs.
Panel A. House Chars:						
Price	220,738.45	373,280.99	219,181	261,616.33	394,346.72	2,927,442
# Homicides	1.32	0.71	219,181	0.00	0.00	2,927,442
Lot Size (sqft)	887,104.52	17679312.36	219,181	1,747,108.50	26744003.94	2,927,442
# Floors	1.35	1.92	219,181	1.21	1.66	2,927,442
# Bedrooms	1.98	1.50	219,181	2.15	1.59	2,927,442
Full Baths	1.14	1.01	219,181	1.42	1.18	2,927,442
Dummy Built-Sold Same Year	0.03	0.16	219,181	0.07	0.26	2,927,442
Area (sqft)	1,456.59	8,221.15	219,181	1,757.27	6,728.97	2,927,442
House Age	67.78	43.70	219,181	46.56	47.64	2,927,442
Panel B. News-stories Chars:						
Viewership Homicides	732,621.33	1,554,937.52	219,181	0.00	0.00	2,927,442
# Hom. Stories	15.45	29.34	219,181	0.00	0.00	2,927,442
% Hom. before Gameday	0.63	0.47	219,181	0.00	0.00	2,927,442

This table presents the mean and standard deviation of different variables of the house transaction data in the final sample across two groups: houses with at least one homicide, and houses no homicides. *Source:* GVA; News Exposure; ZTRAX

Sporting events by local teams: I collect information on dates of matches by each team in the NCAA for both football and basketball, as well as the NBA and MLB. NBA, NCAA basketball, and MLB games, in particular, provide useful within-week weekday variation. There is important variation in each media market, as not all media markets have a team in any of those leagues. Thus, the identifying power of the instrument comes from media markets where there *could* have been a game scheduled right after a homicide takes place. In particular, for the final analysis I drop all observations from media markets-month combinations in which there is no sporting event from these leagues, as homicides occurring in those market-months cannot be shifted by the instrument. As shown in Subsection C.5, these are very small media markets, with only 6% of the sample having no month with any events, and 22% being in a market-month with no games in the data. Subsection C.5 in the Appendix further discusses the geographic and temporal variation of the instrument. The high frequency of games, which are scheduled by reasons that seem orthogonal to

Figure 1: Distribution of log prices by coverage and homicides



(a) Density (b) CDF

These figures show the distribution (Panel a) and CDF (Panel b) of log home prices for the house transactions in the main sample for three groups: no homicide in the vicinity; at least one covered homicide; and not-covered homicide. It shows that the distribution of housing prices of houses with at least one covered homicide is shifted to the left compared with that of houses with a non-covered homicide. Vicinity is defined as 0.5 miles from the house, within a 3-month window previous to the estimated closing date. Data sources: GVA and News Exposure.

specific timing of a future homicide in a city, present arguably exogenous variation in the coverage of homicides. As presented in the last two rows of Table 2, the average house with at least one homicide close-by has 60% of homicides occurring the day before a game day.

3 Measuring the effect of news on housing prices

In this section I estimate the effect of crime news coverage on housing prices. Given the importance of housing assets for the median household, understanding the extent to which fluctuations in housing prices are influenced by news reporting is therefore a first-order concern. According to the standard hedonic framework, housing prices reflect the market's valuation of both a house's intrinsic characteristics-such as the number of bathrooms, bedrooms, and square footage-and the amenities associated with its surrounding neighborhood (Rosen, 1974). Among these amenities, perceptions of security are particularly important. Existing studies have estimated the effect of crime on housing prices using data from official crime statistics (Pope & Pope, 2012) or the proximity of registered sex offenders (Linden & Rockoff, 2008, Pope, 2008b). However, the market valuation of neighborhood security depends not only on the actual crime rate but also on buyers' and sellers' perceptions of crime, which may deviate significantly from reality. Although perceptions are hard to measure directly, there is suggestive evidence that media coverage can play a critical role in shaping them (Mastrorocco & Minale, 2016, Ramirez Alvarez, 2017, Esberg & Mummolo, 2018). Furthermore, by testing the effect of the information conveyed by the media on housing transactions one can implicitly test for how the media effects those perceptions. Below I present my estimation framework.

3.1 Estimation Approach for the Effect of News on Housing Prices

The object of interest for this part of the paper is to estimate the effect of media crime coverage on housing prices and disentangle this effect from the non-media effects of homicides. Under a linear model, I would like to estimate the parameters for the following specification:

$$\log P_{ijt} = \alpha_1 Coverage_{ijt} \times 1[Hom_{ijt} \geq 1] + \alpha_2 Hom_{ijt} + X_{ijt}\beta + \delta_j + \mu_t + \epsilon_{ijt} \quad (1)$$

with X_i is a vector of a constant, and possibly other house characteristics, δ_j are tract fixed effects, μ_t are month-year fixed effects, and ϵ_{ijt} is the error term. Hom_i is an indicator equal to one if there is a positive number of homicides in the vicinity of the house, $Coverage_i$ is a measure of coverage of those homicides (see Subsection 2.2 for the details on the definition of the vicinity and the time frame considered).

Under this model, the effect of a non-covered homicide would be given by:

$$E[\log Price_i | Hom_i = 1, Cov_i = 0] - E[\log Price_i | Hom_i = 0, Cov_i = 0] = \alpha_2 \quad (2)$$

while the effect of a covered homicide would be given by:

$$E[\log Price_i | Hom_i = 1, Cov_i = 1] - E[\log Price_i | Hom_i = 0, Cov_i = 0] = \alpha_1 + \alpha_2 \quad (3)$$

This form of price regression is used extensively in the hedonics literature (see Rosen (1974)) to price house and neighborhood-level amenities.[9] The main challenge for identification of the parameter of interest α_1 is that news coverage is not random, and so $Coverage_i$ is likely correlated with ϵ_{ijt}. This concern of omitted variable bias is partially addressed by the use of tract fixed effects (δ_j), so the variation used in the estimation is comparing houses in close geographical areas, which presumably share several neighborhood-level

[9] In Appendix D I present a simple extension to Rosen (1974), showing that the interpretation of the price gradient as representing the Marginal Willingness to Pay (MWTP) for the non-marketable amenity (crime in this case) does not hold under uncertainty of the level of said amenity. Thus, estimating the MWTP to avoid crime without including news coverage will be a biased estimate.

amenities as well. However, the tract fixed effects might not be enough for identification, as news stations might be selecting which crimes to report on even within neighborhoods. For example, if news stations are more likely to cover homicides that are more salacious, and those occur closer to higher income locations within a neighborhood, the OLS estimate of α_1 will be upward biased - likely bringing it closer to zero. In the following section, I explain the instrumental variable identification strategy that allows me to overcome the endogeneity coming from news station crime coverage. [10]

3.1.1 Sporting events instrument

The effect of crime news coverage on prices, α_1 in Equation 1, could be estimated with a valid instrument for $Coverage$. I argue that exploiting the plausibly exogenous timing of the schedule of games played by local teams for each media market allows me to construct such an instrument. Concretely, for each homicide I define an indicator if said homicides occurs the day before a game day by a local team, $HomGD_{ijt}$. As mentioned in Subsection 2.2, I use variation on game days coming from college basketball, college football, the MLB, and the NBA.

To clarify the identification strategy, consider two houses *a* and *b*, in the same neighborhood, both of which have 1 homicide in the vicinity. Suppose the homicide close to house *a* happens the day before a game day, while not so for homicide by house *b*. Thus, the value of the instrument is 1 for house *a*, but zero for house *b*. These houses should be, on average, comparable in every determinant of their price, except for the fact that in one case the homicide around house *a* is less likely to be covered by the news than that around house *b*. As usual, the validity of $HomGD_{ijt}$ as an instrument depends on whether, after conditioning for tract, month, and homicides close by, having a homicide one day before game day is as good as random. We describe these issues below.

Concretely, I propose the following model to be estimated through 2SLS:

[10] A related consideration is if Hom_i is unrelated to other house characteristics after accounting for the time of sale and the fine geographical area. I view this assumption as sensible as most of the variation in crime occurs across neighborhoods and time, not within. That is, the exact location of a crime within a neighborhood and time frame is unlikely related to other house characteristics. Alternative approaches strategies in the literature have looked at variation across neighborhoods using a shift-share type of instrument (Pope & Pope, 2012), or using spatial RD with an event-study for the disclosure of sex offenders (Linden & Rockoff, 2008). I am unaware of alternative studies using this level of very fine disaggregation by time and space.

$$Coverage_{ijt} = \theta Hom_{ijt} + \omega HomGD_{ijt} + X_{ijt}\gamma + \phi_j + \nu_t + \zeta_{ijt} \tag{4}$$

$$\log P_{ijt} = \alpha_1 \hat{Coverage}_{ijt} + \alpha_2 Hom_{ijt} + X_{ijt}\beta + \delta_j + \mu_t + \epsilon_{ijt} \tag{5}$$

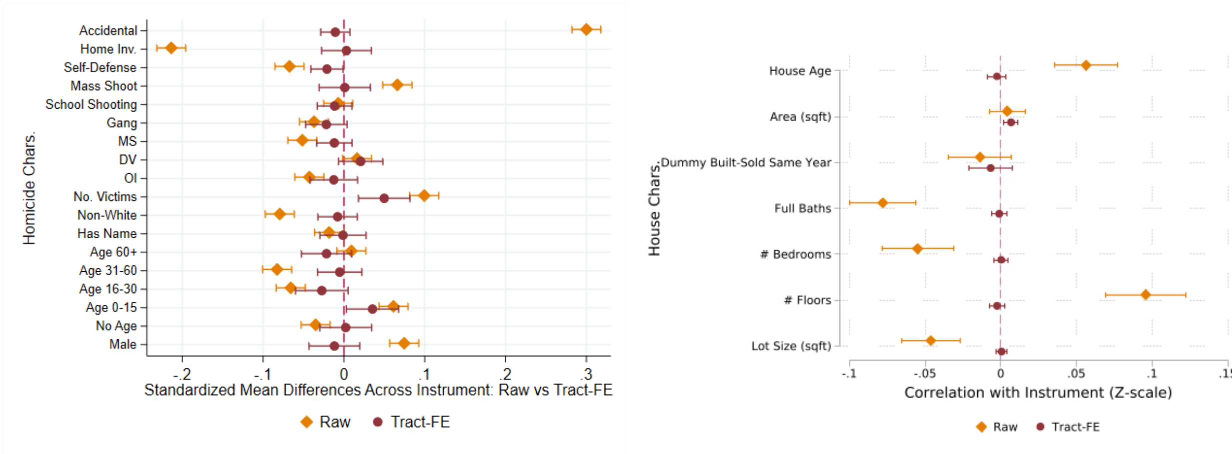
where the first stage (Equation 4) presents coverage as a function of the number of homicides, and the number of homicides before a game day. $Coverage_{ijt}$ represents news coverage of homicides around house i. Hom_{ijt} represent the measurement of homicides around house i. $HomGD_{ijt}$ is the instrument, capturing the number of homicides around house i that occur one day before a local team plays a match. ϕ_j and δ_j are tract fixed effects, and ν_t with μ_t are month-year fixed effects. I estimate this model with and without controls for house characteristics, X_{ijt}. As is usual, the identification argument relies on $HomGD_{ijt}$ being a valid instrument of $Coverage_{ijt}$ - which in turn requires satisfaction of the relevance condition and exclusion restriction.

Relevance: The two main types of content covered by local TV news are crime and sporting events. On days when sports content is more salient, one would expect crime coverage to diminish because airtime is a scarce resource.

Panel A of Table 3 presents presents the first-stage results for Equation 4 on two outcomes: extensive-margin coverage (Columns 1-4) and IHS of total coverage (Columns 5-8). All models include tract fixed effects, which are key for identification as they control for unobserved covariates that do not change in time within the neighborhood. Columns 2 and 5 include month-by-year fixed effects to account for possible seasonality. Columns 3 and 6 include county-specific time trends to allow for differential appreciation patterns. Columns 4 and 8 include other house characteristics including squared lot area, total number of bedrooms, number of bathrooms, squared constructed area, and house age.

Overall, the instrument is highly predictive of coverageat both margins a homicide *the day before* a game day lowers the probability any of the nearby homicides are covered by about 9.7% (from a mean of 67%), and reduces the total number of stories by 38%. The instrument passes weak-instrument diagnostics: the Kleibergen-Paap robust F lies between 223.2 and 392.3, well above conventional thresholds (Andrews *et al.*, 2019).

Figure 2: Balance Test for Instrument



(a) Homicide Characteristics

(b) House Characteristics

Left: This figure shows the balance checks between the proposed instrument and victim characteristics at homicide level. I regress each standardized variable on a game day indicator separately. The plot shows the coefficients for the raw difference (yellow diamond) and the difference controlling for tract fixed-effects (red circle). All variables, except 'No.Victims', are indicator variables. The labels represent indicators for: 'Home Inv.' is Home Invasion; 'Gang' is a gang indicator; 'MS' is Murder/Suicide; 'DV' is Domestic Violence; 'OI' is Officer-Involved. For more details about these categories see Subsection C.2. Right: This figure shows the estimated coefficients (in standard-deviation units) in each house characteristic associated with the instrument, with 'Raw' (yellow diamond) showing unadjusted correlation and 'Tract-Month FE' (red circle) showing correlation after removing tract and month variation. Both: Horizontal bars are 95% confidence intervals.

Exclusion: For $HomGD_{ijt}$ to be a valid instrument, it cannot be correlated with any unobservable variable affecting house prices after conditioning by tract, month, and number of homicides. Importantly, the instrument is the number of homicides before a game day - thus my identifying variation does not use homicides happening *on or after* the game day, as that might generate concerns of reverse causation. As presented later in Subsection 3.4 (Figure 5), an event-study analysis shows that the trend in prices in areas with a homicide right before a game day is essentially the same as in places with a homicide not before a game day. [11] Although the exclusion restriction is not directly testable, I interpret the above as supportive of the exclusion restriction. Similarly, for the sporting instrument to be valid it should also be uncorrelated with observable characteristics of the victim and the house. First, I test for the correlation between the instrument and victim characteristics using the data at the individual homicide level. In Figure 2a I present this balance test. I regress a series of homicide characteristics on an indicator for having the homicide occurring right before a game day. These variables include sex, flags for the type of homicide,

[11] This empirical exercise is explained in detail in Subsection 3.4, but suffice to say that Figure 5 shows the average residual price of transacted houses up to 0.5 miles from each of the homicides (those with a game day the next day, and those without).

age of the victim, and the number of victims. After controlling for tract fixed-effect, all but one of the observable characteristics are the same between homicides right before a game day and their counterpart - about the share we would expect to be different at random. The only exception is the number of victims. Given that Equation 4 and Equation 5 always include the total number of victims, this is not much of a concern. Regarding housing characteristics I once more regress a series of house characteristics (in standard-deviation units) on the instrument and the number of homicides. The coefficients for the instrument are presented without further controls (yellow diamond), and after including tract and month fixed effects (red circle). Once more, once tract fixed effects are included in the regression, there is no correlation between the proposed instrument and those observable house characteristics.

3.2 Main Results for Effects of Crime Coverage on Housing Prices

Table 3: Effects of Crime Coverage on Housing Prices

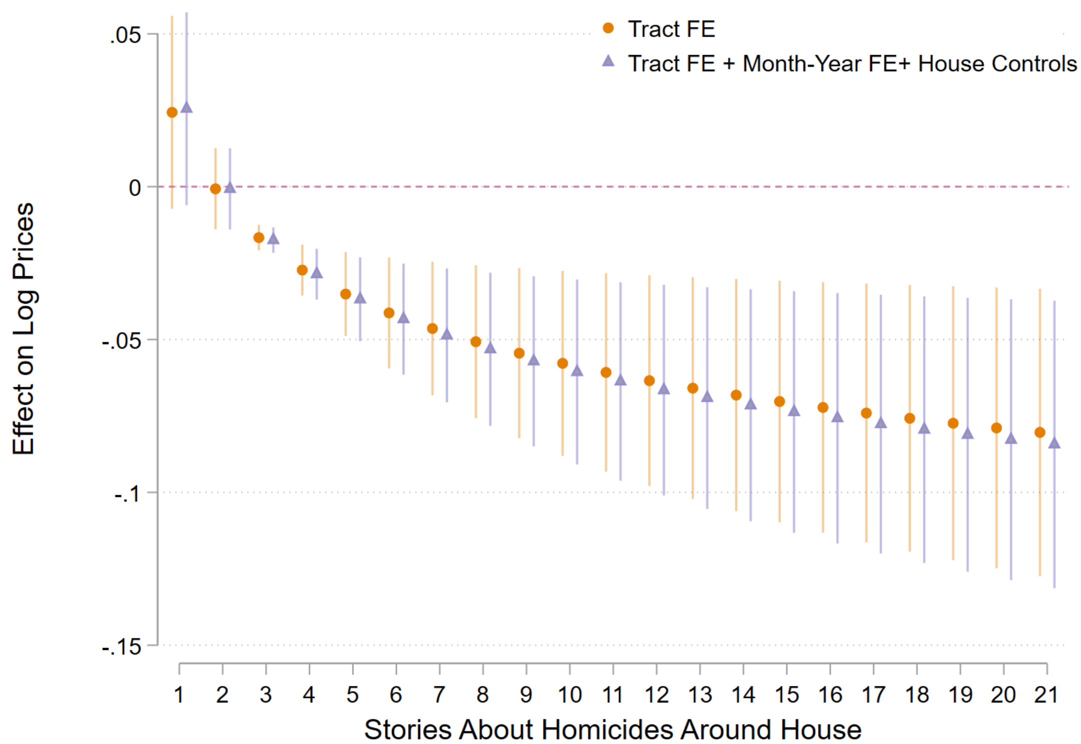
Panel A. First Stage	Outcome:							
	Ext. Mg. Coverage				Total Coverage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hom. Before Gameday	-0.097**	-0.097**	-0.097**	-0.091**	-0.384**	-0.384**	-0.384**	-0.381**
	0.006	0.006	0.006	0.006	0.019	0.019	0.019	0.020
KP Statistic	289.760	291.160	292.072	223.158	389.659	392.306	393.340	347.826
Tract FE	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE		✓	✓	✓		✓	✓	✓
County-Trends			✓	✓			✓	✓
House Characteristics				✓				✓
Observations	3146330	3146330	3146330	2601674	3146330	3146330	3146330	2601674

Panel B. Reduced Form	Outcome: Log. House Price			
	(a)	(b)	(c)	(d)
Hom. Before Gameday	0.011**	0.011**	0.012**	0.008*
	0.004	0.004	0.004	0.004
Tract FE	✓	✓	✓	✓
Month-Year FE		✓	✓	✓
House Characteristics				✓
Observations	3146330	3146330	3146330	2601674

Notes: Panel A reports estimates from the first stage (equation 4), and from the reduced form. Columns 1 to 4 correspond to the 2SLS estimates in Table 4 in Columns 3 to 6, respectively. Similarly, Columns 5 and 8 here match the estimates in Columns 8 to 11 in the same table. Panel B presents the reduced form estimates of the instrument on log house prices. The instrument is the number of homicides around each transacted house which occurred one day before a game day by the local team in the NBA, NCAA college and basketball, and MLB. The sample includes only houses in tracts with at least one homicide throughout the whole sample, and at least one game played by the local team. If there is no homicide next to the house, the instrument is defined as zero. Each homicide gets matched to the house if it takes place within 0.5 miles, and in the quarter leading to 2 months before the recorder date for the transaction. Columns 6 and 11 include controls for house characteristics, including: age, number of bedrooms, number of bathrooms, squared lot area, and squared constructed area. Clustered s.e. by tract. (** $p < 0.01$;* $p < 0.05$).

Reduced form: The reduced form effects are presented in Panel B of Table 3. The results in columns 1-3 suggest that when comparing within tract prices of homes with homicides, those in which the homicide occurs before a game day are sold by 1.1% higher prices. This number does not change when including month-by-year fixed effects or house characteristics. In all cases, the point estimate is statistically different from zero at 5% significance. This shows that if one accepts the exogeneity in the timing of the sporting events with respect to homicides and housing prices, at the very least, there is evidence of a causal effect of the instrument in prices.

Figure 3: Effect of News Coverage on Housing Prices



This figure shows the estimated effects of crime news coverage on log house prices from Equation 5 (Column 6 in Table 4) for houses with one homicide in their vicinity. The bar represents the 95% confidence interval.

2SLS: The OLS and 2SLS estimates of the effect of crime coverage on housing prices are presented in Table 4. All specifications include tract fixed effects, allowing for comparisons across houses with similar neighborhood-related amenities, such as schooling, air quality, access to bars or restaurants, parks, and other factors. Column 1 reports that the naive OLS estimate of the effect of homicides (covered and non-covered) on prices is approximately -1.7%. Column 2 shows that incorporating data on crime coverage leads to a slight change in the naive OLS estimate, with the effect of covered homicides decreasing marginally to -1.6% and the effect of a covered homicide estimated at -2%. However, since coverage is non-random, these estimates may conflate the effects of coverage with other unobservable characteristics of the home.

Columns 3 through 5 present the IV estimates, where crime coverage is instrumented using the number of homicides occurring before a game day. Column 3 includes only tract fixed effects; Column 4 adds month-by-year fixed effects to account for market seasonality; and Column 5 (my preferred specification) adds a county-specific time trend to allow for different appreciation rates across areas. Across all specifications, the estimated effect of crime coverage becomes larger, ranging from -8.1% to -9.2%. Column 6 further controls for house characteristics, which reduces the estimated effect to 6.1% - although the sample is also reduced by about 18%. In all cases the coefficient for the effect of crime coverage is statistically significant at the 1% level. In contrast, the effect of a non-covered homicide is estimated to be approximately 3%, but this effect is not statistically distinguishable from zero. As a benchmark, my estimated effect of homicide coverage is about 2 percentage points larger than that estimated by Linden & Rockoff (2008). The authors find that the arrival of a sex offender within 0.1 miles of a house decreases prices by about 6%. Thus, according to my estimates, having a homicide that is covered in the news within 0.5 miles is about 25% larger than the effect of having a sex offender moving in within 0.1 miles.

The effects for the total number of stories are presented in Columns 7 to 11. The OLS estimate shows that having a homicide close by reduces housing prices by 1.5%, and that there is no effect of coverage by the number of stories on prices. Columns 8 to 11 show the 2SLS estimates. In all specifications, the coefficient on the IHS number of stories is between -2% to -3% and is statistically different from zero at 5% level. As in the results for the extensive margin coverage, the effect of the non-covered homicides is not-statistically different from zero.

Figure 3 shows the implied marginal effect of media coverage across number of stories of a homicide near a house. Both models - one including only tract fixed effects and the other incorporating month-by-year fixed effects and house characteristics - show no detectable effect on prices for up to two stories in the media. However, starting from the third story, a significant effect of coverage emerges, with its magnitude increasing as the number of stories rises, though the marginal effects diminish progressively.

Table 4: Effects of Crime Coverage on Housing Prices

	Outcome: Log. House Price										
	No News	Extensive Mg. Coverage					Total Coverage				
	OLS	OLS	IV				OLS	IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Covered Homicide $(\alpha_1 + \alpha_2)$		-0.020**	-0.081**	-0.089**	-0.092**	-0.061**					
		0.004	0.024	0.023	0.022	0.023					
$1[stories \geq 1]$ (α_1)		-0.004	-0.112**	-0.119**	-0.123**	-0.083*					
		0.005	0.043	0.040	0.039	0.040					
Total Homicide Coverage - IHS(Stories)							-0.001	-0.028**	-0.030**	-0.031**	-0.020*
							0.002	0.011	0.010	0.010	0.010
Homicides (α_2)	-0.017**	-0.016**	0.031	0.029	0.031	0.022	-0.015**	0.024	0.022	0.024	0.015
	0.002	0.003	0.019	0.018	0.017	0.018	0.003	0.016	0.015	0.015	0.014
KP Statistic			289.760	291.160	292.072	223.158		389.659	392.306	393.340	347.826
Tract FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE				✓	✓	✓			✓	✓	✓
County-Trends					✓	✓				✓	✓
House Characteristics						✓					✓
Observations	3146330	3146330	3146330	3146330	3146330	2601674	3146330	3146330	3146330	3146330	2601674

Notes: This table reports estimates from equation 5, which estimates both the OLS and 2SLS coefficients of the effect of homicide coverage on housing prices. Column 1 reports the OLS coefficient if no information on coverage, but only number of homicides, is included in the hedonic model. Columns 2 to 6 report the estimates of coverage in the extensive margin (at least 1 story about the homicides). Columns 7 to 11 report the estimates for the IHS transformation of the total number of stories. Columns 3-6 and 8-11 present the 2SLS estimates where the instrument is the number of homicides which occurred one day before a game day by the local team in the NBA, NCAA college and basketball, and MLB. The sample includes only houses in tracts with at least one homicide throughout the whole sample, and at least one game played by the local team. If there is no homicide next to the house, the instrument is defined as zero. Each homicide gets matched to the house if it takes place within 0.5 miles, and in the quarter leading to 2 months before the recorder date for the transaction. Columns 6 and 11 include controls for house characteristics, including: age, number of bedrooms, number of bathrooms, squared lot area, and squared constructed area. Clustered s.e. by tract. (** $p < 0.01$;* $p < 0.05$).

In the context of this analysis, the 2SLS estimator recovers a Local Average Treatment Effect (LATE) - namely, the causal effect of news coverage on sale prices for those house-homicide observations whose coverage status is "pushed" by the timing of a local game day (the compliers). Compliers here are transactions in the same tract-month with exactly one nearby homicide that would have been covered if it fell on a non-game day, but whose coverage is suppressed when it instead occurs the day before a game. For this marginal group, we estimate an approximately 8 percent drop in transaction prices when coverage is "on."

Because compliers lie at the coverage margin, the 8 percent LATE need not equal the ATE across all houses. In principle, very salient events (almost always covered) might produce larger price declines, and very obscure ones (almost never covered) smaller ones. Thus, one might expect the ATE in absolute terms to exceed the LATE - although its exact magnitude remains unidentified outside the complier subpopulation. Still, by targeting the units whose coverage probability is exogenously shifted, this IV strategy delivers the cleanest causal measure of how local news attention translates into market-based perceptions of neighborhood security.

Robustness: Table A.1 reports the results of a series of specifications examining the robustness of the main results above. For comparison, Column 1 presents once more the results from my preferred specification (Column 5) in Table 4, while the remaining columns show the results for 4 alternative models.

One could worry that the county-trends along with the tract and month-by-year fixed effects might not be flexible enough to account for different gentrification patterns by neighborhood. To address this concern, Column 2 includes tract-by-month-by-year fixed effects. Under this specification, the effects of crime news coverage are even larger, implying that the effect of having a homicide appear in the news cycle decreases housing values by 14%. Column 3 restricts the sample to houses with at least 1 homicide in the quarter before sale. Thus, therein the comparison is only made between houses with a covered homicide to those with a non-covered homicide. The implied effects of crime coverage are similar in this case as well, showing that coverage decreases prices by about 10%. Column 4 includes controls for average characteristics of the victim (age, likelihood-by-race, and male indicator). Here, the estimated effect is once more slightly larger, at about -14%. Lastly, Column 5 shows that these effects decay with space: when matching homicides up to 1 mile, the price effect of a homicide appearing in the news cycle drops to about 5.7% (about a 40% reduction from the effect in Column 1).

3.3 Heterogeneity and Mechanisms

The information provided by the news coverage of a homicide might have differential effects on house prices depending on the type of incident. In this section I test for heterogeneity in the effects' estimation in the previous section across type of incident, demographic characteristics of the victim, and area characteristics.

Domestic Violence versus Home Interest: There are some events that households might assign more weight to when deciding how to price a housing unit. To test this hypothesis, I estimate differential effects for two type of incidents. On the one hand, I define a type of event as likely to be of special interest for a home buyer if it includes a school shooting, or is related to a home invasion. Those events might matter more to the extent that they might deter households with children from buying in the area, or for the latter if the potential buyers interpret these events as informing the future likelihood of being a victim themselves of a home invasion. On the other hand, domestic violence homicides might be less informative about the likelihood of being a subject of violence for individuals unrelated to the victim. Figure 4 presents the results (Columns 3 and 4 of Table A.2). Here I find partial evidence that households respond more strongly for events that might be more informative about being a victim of a crime: homicide exposure where there is at least one domestic violence incident are priced in less strongly than when there are none of those events, with a statically significant difference of about 4% ($p<0.01$). At the same time, although I find that the magnitude of the effect is larger for events that are perhaps more informative to households (those including school shootings, or home invasions) have a point estimate of about 1.5% extra compared to other incidents, the difference in effects is not statistically different from zero. Overall, I read these results as partially supporting the hypothesis that households assign more weight to incidents that are more informative about likelihood of future victimization.

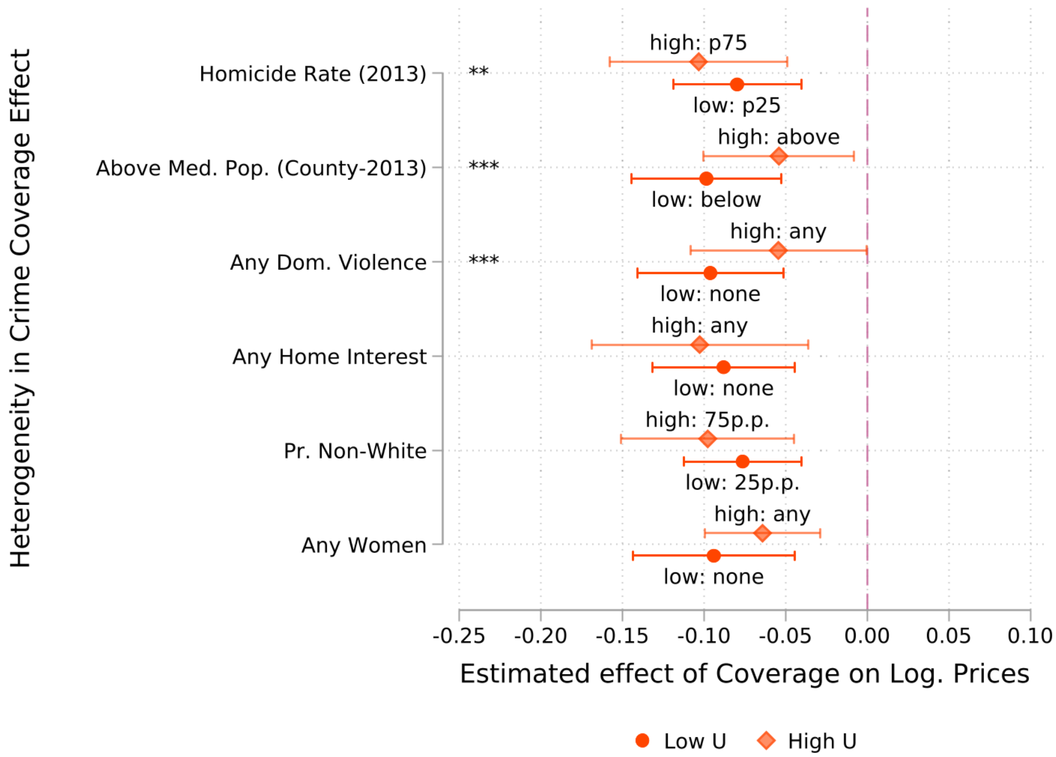
Race and Sex: It is also possible that homebuyers assign different weights to the incidents depending on the demographic characteristics of the victim. I here consider sex and race (measured as the probability of being non-white). For the former I consider the coverage of crime incidents that involve any women victim. For the latter I use the average probability of the victim being non-white. In both cases I cannot reject that the effects of crime coverage are the same for incidents when all victims are men versus when there are women victims, nor for differences in the expected race of the victim. These results are presented in Figure 4, and Columns 1 and 2 of Table A.2. As I will show in Section 4, I do find that news coverage is heterogeneous across race. I come back to this finding in

Subsection 4.6.

Baseline Crime Rate: Are the effects of crime coverage larger in places with higher crime at baseline? In principle home buyers can respond more strongly in places with higher crime at baseline if they think that crimes in locations with low crime to begin with are only transitory, while those in places with more crime are more likely to be permanent. To evaluate this hypothesis I use the homicide rate in 2013, provided by the CDC at the county-level, and interact it with the coverage. I find that indeed the effect is larger in counties that have a larger homicide rate per 100,000 to begin with (see Figure 4, and Columns 6 of Table A.2). This difference is not that large in magnitude (the difference between counties in the 25th percentile and the 75 percentile is about 2%), but this difference is statistically significant at the 5% level.

Population: Lastly, I test if these coverage effects are larger in more populated counties. On the one hand, the effect might be smaller in more populated areas as rare bad events could be less memorable than in less populated areas. On the other hand, the effect might be larger in more populated areas if crime news are reaching a larger fraction of the demand side. I interact the coverage measure with an indicator for houses within a county with a population above the median in the sample (see Figure 4, and Columns 5 of Table A.2). I find evidence that the effect of crime coverage is smaller in larger counties with a statically significant difference of about 4% ($p<0.01$).

Figure 4: Heterogeneity in Treatment Effects of Coverage



This figure shows different treatment effects for a heterogeneity characteristic evaluated at a low level (red circle) and at a high level (orange diamond). p25 and p75 represent the values for percentile 25th and 75th, respectively. 75p.p. (25p.p.) is the effect for average likelihood to be 75% (25%). These effects are based on interacting the heterogeneity characteristic with the coverage, instrumented by the interaction of said characteristic with the instrument. This model includes tract fixed effects. The underlying model results are presented in Table A.2. The bar represents the 95% confidence interval. The stars represent the p-values of the null hypothesis that the effects between the low-level and the high-level are the same. (***: p<0.01; **: p<0.05; *: p<0.1)

3.3.1 Supply or Demand Shifts

In Appendix E I explore if these effects are likely driven by supply or demand responses. One possibility is that supply is shifting outward when there is a homicide in the area while demand is not changing. The first thing to note is that if the individuals living close by to the homicide are more likely to learn about it than non-neighbors irrespectively of coverage, then we should expect to see a drop in prices both for covered and non-covered homicides. This is not the case.

Alternatively, it is possible that neighbors also learn about these events through news coverage, and they immediately respond by increasing the likelihood of selling. To test for this possibility, I evaluate at a more aggregate level what is the effect of crime coverage on the number of houses listed for sale, as well as the price of the listed units. In the mentioned appendix, I show that there is essentially no change in the supply of houses up to 3 quarters after the homicide. I do find, though, that prices respond almost immediately in a relatively similar magnitude as the effects estimated on transacted prices. Thus, I argue that these results are suggestive of the effect of news mainly operating by shifting demand downwards immediately after the coverage of the homicide.

3.4 Dynamic Effects

What are the dynamic effects of crime coverage on housing prices? To address this question I use an IV-event study approach where I compare the average transacted price of areas around covered and non-covered homicides, while leveraging the sporting instrument as before. For each homicide h, I define a radius of 0.5 miles, and propose the following model:

$$\log \tilde{P}_{hq} = \sum_{p=-3}^{3} \zeta_q (Covered_h \times 1[q = p]) + \omega_h + \tau_q + \epsilon_{hq} \tag{6}$$

where $\log \tilde{P}_{hq}$ is the mean residual price of housing units sold during quarter q (residualized after regressing the price of each house on tract, month, and year FEs), $Covered_h$ is an indicator equal to 1 if homicide h (which occurs at q=0) was covered in the news, ω_h is a location/homicide FE, and τ_q is a relative quarter to homicide FE. The coefficients of interest are ζ_q. [12]

The set of instruments is based, as before, on the homicide occurring one day before a game day - this time, interacting it with quarter dummies. Thus, the set of instruments is composed by $\{HomGD_h \times 1[q = p]\}_{p=-3}^{3}$, where $HomGD_h$ is an indicator equal to 1 if homicide h occurred one day before a game day. The identification argument depends

[12] Formally, I first run

$$\log P_{imy\tau} = \lambda_m + \mu_y + \phi_\tau + \epsilon_{imy\tau},$$

for every house i, and λ_m, μ_y, and ϕ_τ are fixed effects for month, year, and tract, respectively. Let $\widetilde{\log P_{imy\tau}}$ be the residual from this regression. Then define

$$\log \tilde{P}_{hq} = \frac{1}{N_{hq}} \sum_{i \in h, \tilde{\tau} \in q} \widetilde{\log P_{imy\tau}},$$

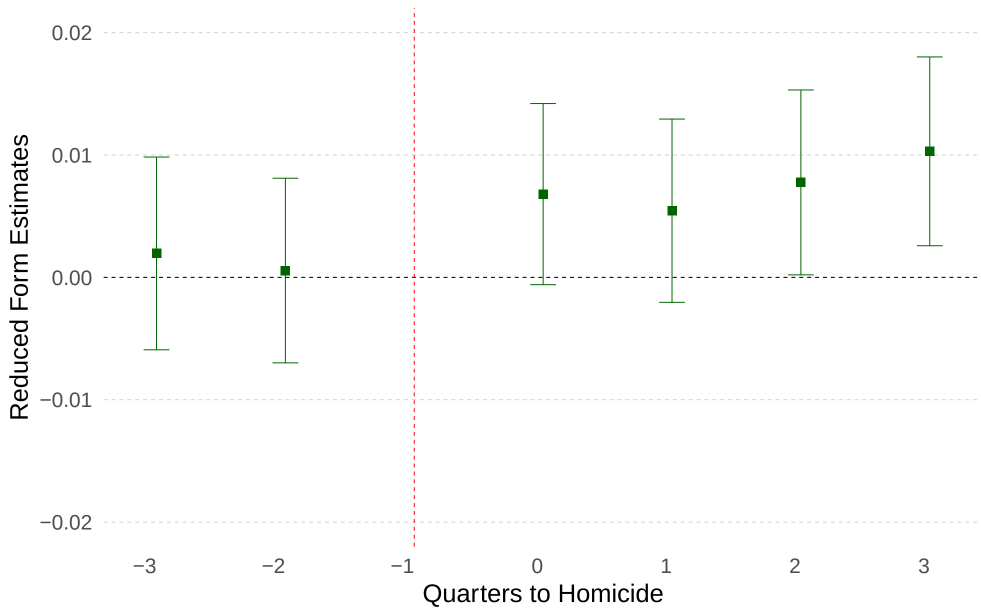
where q is the quarter, N_{hq} is the number of transactions in the radius and quarter, and as before the expected closing date $\tilde{t}$ is given by 2 months before the transacted date (Anenberg & Laufer, 2017).

on a parallel trend assumption for both the outcome and the treatment (Hudson *et al.*, 2017). That is, ζ_q identifies the average effect of coverage on housing prices under two key assumptions: (i) housing prices would have evolved similarly between locations with a homicide before a game day and those with a homicide not before a game day, and (ii) the change in coverage due to the timing of the homicide relative to a game day would have been the same for locations with a homicide not before a game day. Given the rarity of homicides, the second assumption effectively aligns with the exogeneity of the instrument. However, the first assumption warrants closer scrutiny. As is well known, the parallel trends assumption cannot be tested directly, but it is possible here to test for pre-trends in log prices in the reduced form. Panel A of Figure 5 supports assumption (i), showing that, up to the time of the homicide, log prices evolved similarly between locations with a homicide before a game day and those without one.

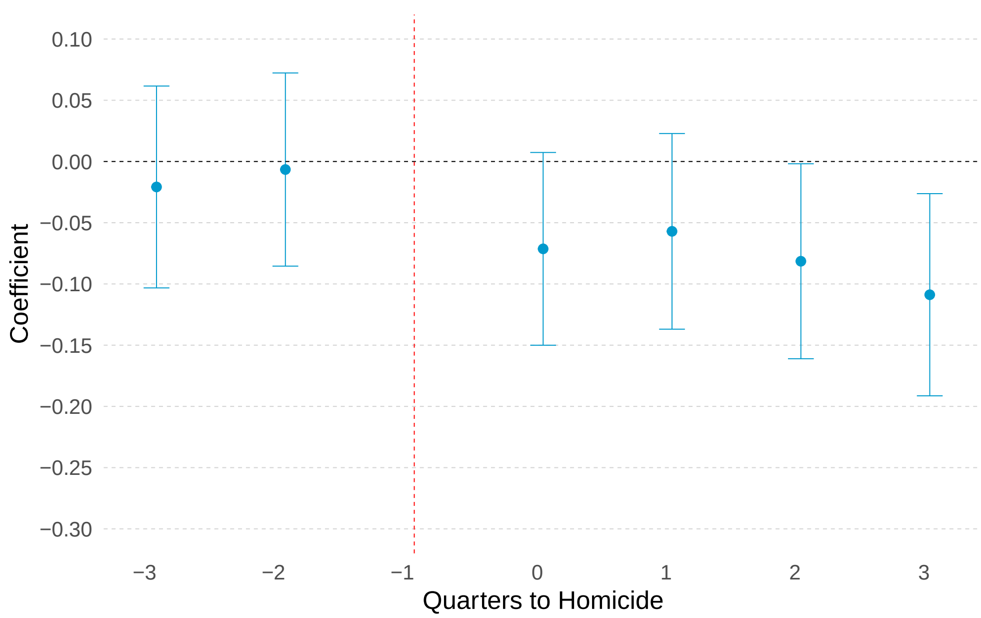
Panel B presents the implied effect of crime coverage on housing prices (Equation 6). The results indicate that the implied effect of news coverage on housing prices is effectively zero up to the time of the homicide (as expected). Following the homicide, there is a negative effect of approximately 7% in the first quarter, which increases to about -10% within one year. These findings suggest that, while housing prices in locations with covered homicides and those without coverage followed similar trends prior to the homicide, they diverged significantly for at least one year afterward.

Figure 5: Dynamic effects

(a) Reduced Form - Effect of Homicide on game day on Housing Prices



(b) IV - Effect of News Coverage on Housing Prices



Panel a of this figure shows the reduced form, while Panel b presents the IV estimated effects of crime news coverage on average residual log prices of houses within 0.5 miles from a homicide at period 0 (Equation 6). There is no estimated effect on house prices before the homicide, but after it occurs, covered homicides reduce housing prices compared to non-covered homicides up to 1 year after. The bar represents the 95% confidence interval, and standard errors are clustered by geographic radius of homicide.

4 Measuring Coverage Bias in Crime News

The previous section documented that crime coverage itself has an effect on housing prices, and that these effects do not seem to differ by race of victim. This section shifts focus to examine if there are detectable biases in the crime coverage by race, providing the foundation for the subsequent analysis that integrates these disparities to quantify the overall impact of racial bias in media coverage on housing market valuations.

4.1 Definition of Bias

News organizations can be thought of as entities that observe a realized state of the world, and then communicate information about that state to their potential audience. News bias, then, can be of two types: *filtering* and *distortion* (following the distinction by Gentzkow *et al.* (2015)). Under distortion bias, news outlets lie outright about the state of the world. *Filtering* bias, on the other hand, does not represent a fabrication of a story but rather the selection of which stories are presented as news. Because this paper focuses on homicide stories, distortion bias is less plausible, as it would involve the fabrication of non-existing homicides.[13] Instead, the more relevant dimension is *filtering* bias.

An important empirical challenge in evaluating filtering bias is that the researcher needs to observe all possible stories the media outlet could have covered, but decided not to. I overcome this challenge by constructing a comprehensive and novel data set that links the universe of homicides and its TV-media coverage in the US. These data allow me to compare all the homicides the TV station *could* have reported on and decided not to.

One can further consider the extensive and total margins of filtering biases. The former relates to any coverage at all about a specific homicide. The latter is defined by the intensity or frequency of coverage in the news. With this framework in mind, I present now how I estimate the bias in crime coverage by race.

4.2 Estimation Approach for Coverage Bias

In this section, I explore possible gaps by race in crime news coverage. I estimate the following model by OLS, where the outcome is news coverage for each victim, both in the

[13] It should be noted, as will be further explained below, that the focus is on news stories about *homicides*, not *murders*. That is to say, any incident with a reported killed victim is considered, independently of the judicial process for the incident. This is important because the data hereby used does not suffer from potential biases in the judicial process.

extensive margin and in the total number of stories:

$$\begin{aligned} coverage_{idt\tau} = & \beta_{NWV-WS}\mathbb{1}(V_i^{non-white} = 1, S_i^{non-white} = 0) + \\ & \beta_{WV-NWS}\mathbb{1}(V_i^{non-white} = 0, S_i^{non-white} = 1) + \\ & \beta_{NWV-NWS}\mathbb{1}(V_i^{non-white} = 1, S_i^{non-white} = 1) + X_{idt\tau} + \delta_d + \epsilon_{idt\tau} \end{aligned} \quad (7)$$

where i indexes the incident, in DMA d, at time t, in tract τ by station s and v and s represent victim, suspect characteristics, respectively. The outcome variable $coverage_{idt\tau}$ represents either the extensive margin (a dummy that takes the value of one when the number of stories about the incident is greater than 0) or the total margin (the inverse hyperbolic sine (IHS) of the number of stories of the incident). The IHS transformation allows me to interpret the β coefficients as approximate elasticities or semi-elasticities, depending on the outcome variable. δ_d represent media market fixed effects, so the comparison is made for victims *within* the same media market. $X_{idt\tau}$ includes covariates at different levels: i) victim level (categorical dummies for the age of the victim, indicator for having name of victim, and sex of victim); ii) suspect level (categorical dummies for the age of the suspect, indicator for having name of suspect, and sex of suspect); iii) incident level (number of killed victims) and; iv) neighborhood level (within DMA wealth of neighborhood proxied by quintile of the median household value, population in the tract, and population white in the tract).[14] Throughout the different specifications in this section, standard errors are clustered at the media market level unless otherwise specified.

The coefficients of interest for race, compare the coverage for the different race profiles of the participants in the homicide with respect to coverage of homicides with both a white victim and white suspect: β_{NWV-WS}[NWV-WS], for non-white victim and white-suspect; β_{WV-NWS}[WV-NWS], for white victim and non-white suspect; and $\beta_{NWV-NWS}$[NWV-NWS], for non-white victim and non-white suspect.

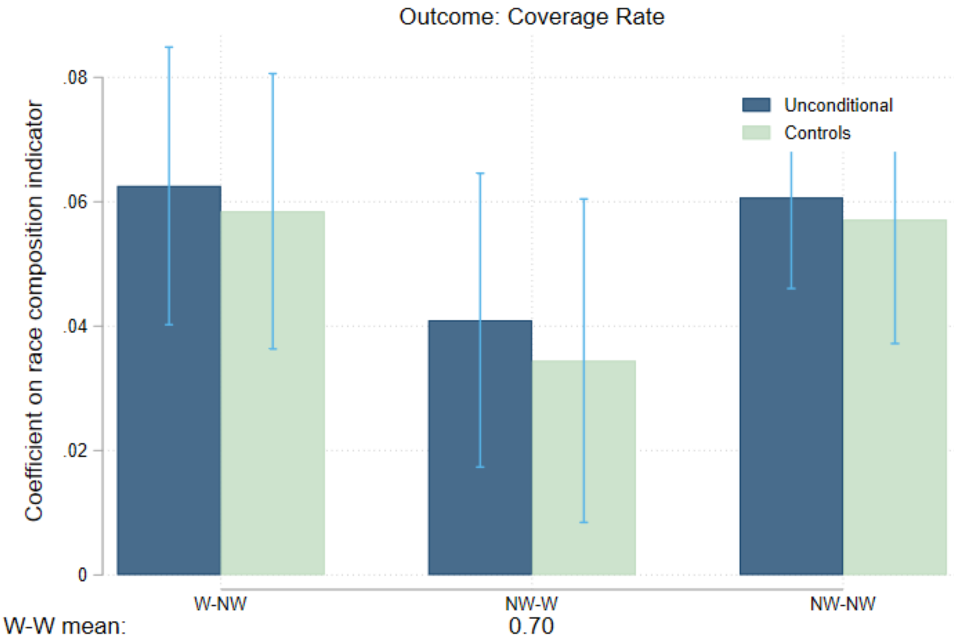
4.3 Main Results for Coverage Bias

Figure 6 shows the within media market estimated gap in coverage, with and without controls, for two different measurements of coverage of these homicides: coverage in the extensive margin (Sub-figure a); and IHS of total number of stories (Sub-figure b). These values are presented in more detail in columns 1 and 6 of Appendix Table A.3. Column 1 shows the gaps for the full sample of homicides, where a dummy is added to homicides

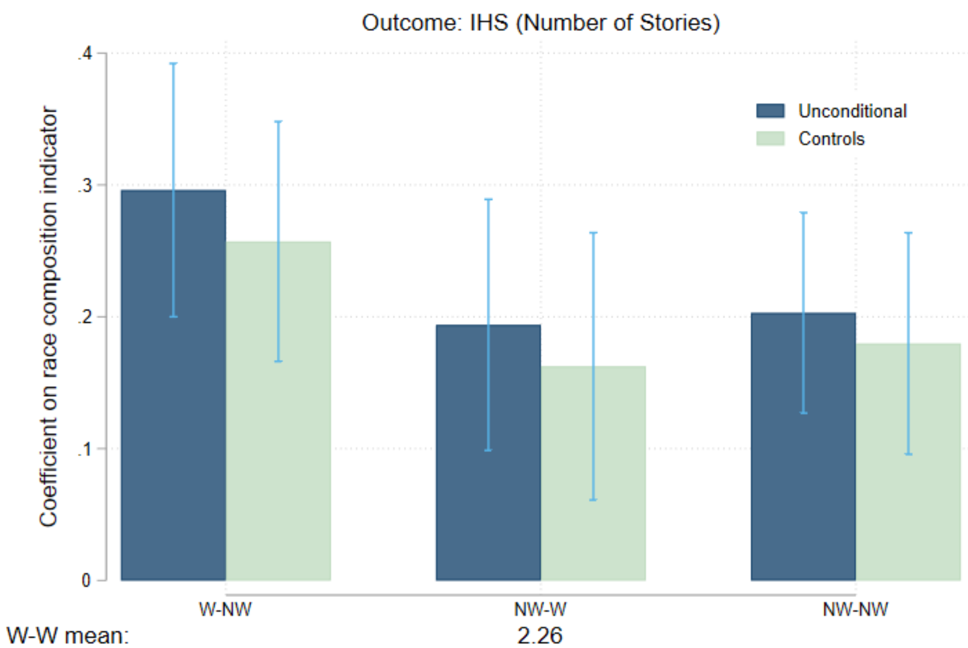
[14] The age categories are 0-15, 16-30; 31-60, and more than 60.

Figure 6: News coverage of homicides by race profile

(a) Coverage rate



(b) Number of stories



This figure presents within media market coefficient values on race composition indicators, measuring the gap in coverage with respect to white-victim/white-suspect homicides. The list of controls include: i) victim level characteristics (categorical dummies for the age of the victim, indicator for having name of victim, and sex of victim); ii) suspect level characteristics (categorical dummies for the age of the suspect, indicator for having name of suspect, and sex of suspect); iii) incident level characteristics (number of killed victims) and; iv) neighborhood level characteristics (within DMA wealth of neighborhood proxied by quintile of the median household value, population in the tract, and population white in the tract).

where the suspect is not known. Columns 2 to 6 consider only the sample where the suspect is known and add controls sequentially. The set of controls includes characteristics of the victim, suspect, incident, and neighborhood. In general, the detected gaps are quite consistent across all specifications. Overall, they show a substantial gap in the coverage of homicides by race: cases with a white victim and a non-white suspect are around 6 percentage points (p.p.) more likely to appear in the news at least once - around 10% of the overall mean. Similarly, white victim/non-white suspect incidents have 27% more stories in the media. Similar results are also found for the coverage rate and number of stories for incidents with non-white victims and non-white suspects. The gap in the case of non-white victims and white suspects is somewhat smaller on both margins (3.6% in coverage rate; 17% more stories), but still sizable. The gaps for non-white victim/non-white suspect incidents are between the previous two. Hence, I find evidence of systematic excess coverage of homicides involving non-white participants, in particular when the suspect is non-white.

4.4 Heterogeneity and Robustness in Coverage Bias

4.4.1 Robustness of bias measurement

I now assess the robustness of the measured coverage bias by race presented above. Table A.4 in the Appendix presents the results from these robustness checks.

Quantile regression: It is possible that the differences in coverage by race are driven by a few outlier observations driving the difference in mean coverage by race, but this might not be indicative of what is happening elsewhere in the distribution, and in the median in particular. To test for this possbility, I run a similar specification to Equation 7, but using a quantile regression at the median. Quantile regressions have the additional benefit of being equivariant, so the IHS transformation should be less important in assessing the bias. The results are presented in **Panel A** of Table A.4. The patterns are similar to those in Table A.3, with the conditional median of homicides in which there is at least one non-white participant being larger than that of white victim with white suspect. The difference ranges between +16% for non-white victim and white suspect, to +33% for white victim and non-white suspect.

Count models: Although the IHS transformation of the number of stories allows for a simple and useful interpretation of the coefficients β, namely as elasticities or semi-elasticities, it is possible that the model is miss-specified given the discrete nature of the outcome variable. In **Panel B** or Table A.4, I present results from a negative binomial model, which allows to model the outcome explicitly as discrete and positive. Once more, I find a similar pattern to the OLS results: incidents with non-white participants get more coverage than those of white-victims and white-suspects. The point estimates show that the difference ranges between 6% (NWV-WS) and 11% (WV-NWS). In all cases, the difference is statistically different from zero at 1% level.

Type of homicide and atypicality: The OLS results show that the difference in coverage of homicides with a non-white participant is quite large compared to those with white victim and suspect, in particular when the suspect is non-white but the victim is white. It is natural to consider how much of this difference can be accounted for by characteristics in the type of homicides. Fortunately, the GVA includes descriptions of the incidents and this can be introduced as dummies in the regressions presented above. Incident characteristics include whether they involved the police, were near schools, were a result of mass shootings, were gang-related, among others (see Subsection C.2). Additionally, it is possible that the estimated gaps represent the atypicality of certain type of homicides by race in that tract. To test of the latter hypothesis, I also include in the set of controls the homicide rates by race and age, in order to account for how rare each of these incidents are. Both the flags and homicide rates by group are included as controls, with the results being shown in **Panel C** for the extensive margin and in **Panel D** for the IHS of the number of stories. Even incorporating these controls does little to the estimated gap in coverage by race. This means that the atypicality of these homicides by type or race does not explain the measured gaps by race.

4.5 A Possible Driver of Bias in Crime Coverage

What are the main determinants of racial bias in news crime coverage? Although a full exploration of this question is beyond the scope of this paper, I here propose a plausible mechanism: the demographic composition of the media market. Television stations, financed by advertising, have an incentive to tailor content toward the audience segments that deliver the highest ad revenue. Adjusting this setting to the Anderson & Waldfogel (2015) preference-externalities framework, the equilibrium bias chosen by a profit-maximizing station is therefore a function of the market's racial make-up. The model (summarized in Appendix F) yields two predictions. First, there will be under coverage of news preferred by non-whites, as media outlets cater more to the white population. Second, as the share of the non-white population increases, the under-coverage of news preferred by non-whites decreases. To assess these predictions, I split media markets into terciles of Black-population share (T1-T3) and interact each homicide-race indicator in Equation 7 with the tercile dummies. The results, displayed in Figure 7, align closely with the theory: for homicides involving a non-white suspect (regardless of victim race), the coverage gap is largest in low-minority markets (T1) and falls to roughly one-third of that magnitude in high-minority markets (T3). This pattern appears on both the extensive margin (probability of any coverage) and the intensive margin (total number of stories). However, the pattern is less clear-cut for incidents involving a non-white victim and a white suspect, where standard errors are noticeably larger.

4.6 Quantifying the Economic Impact of Racial Bias in News Coverage

What is the effect of racial crime news bias for housing prices? One simple way of answering this question is to use a back-of-the-envelope calculation based on the estimated causal effect of news coverage on prices and the estimated differential rate of coverage based on racial characteristics of the incident. The parameter of interest, denoted as η, represents the difference in expected housing prices for a house near an incident involving non-white participants ($hom_i^{nw} = 1$), depending on whether that incident receives the actual coverage rate for such incidents ($news^{nw}$) or the counterfactual coverage rate it would have received if treated like a white-victim incident ($news^{w}$).

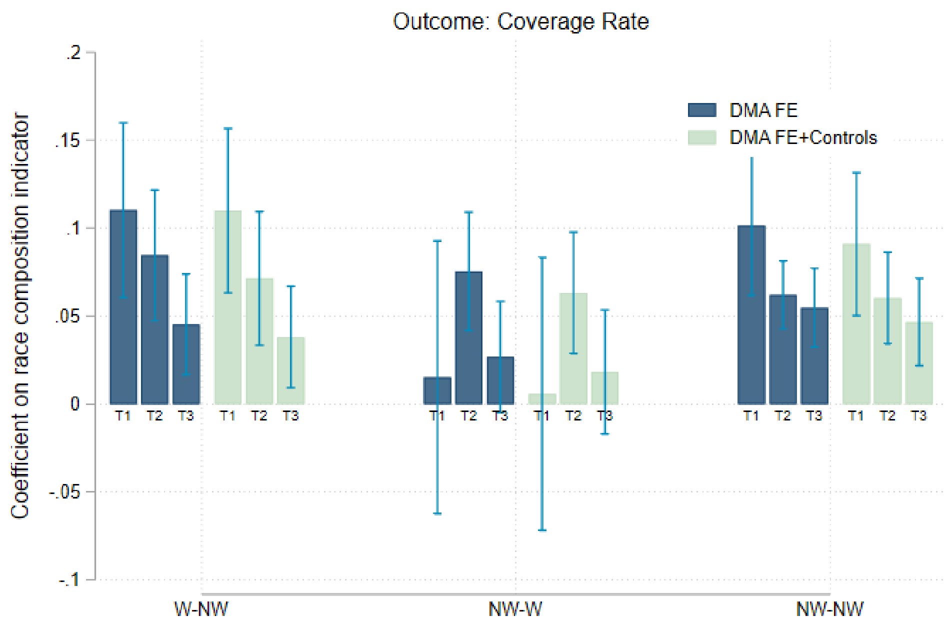
Mathematically, this difference is initially conceptualized as:

$$\eta = E[\log P_{ijt} | news^{nw}(hom_i^{nw}), hom_i^{nw} = 1, X_{ijt}] - E[\log P_{ijt} | news^{w}(hom_i^{nw}), hom_i^{nw} = 1, X_{ijt}] \quad (8)$$

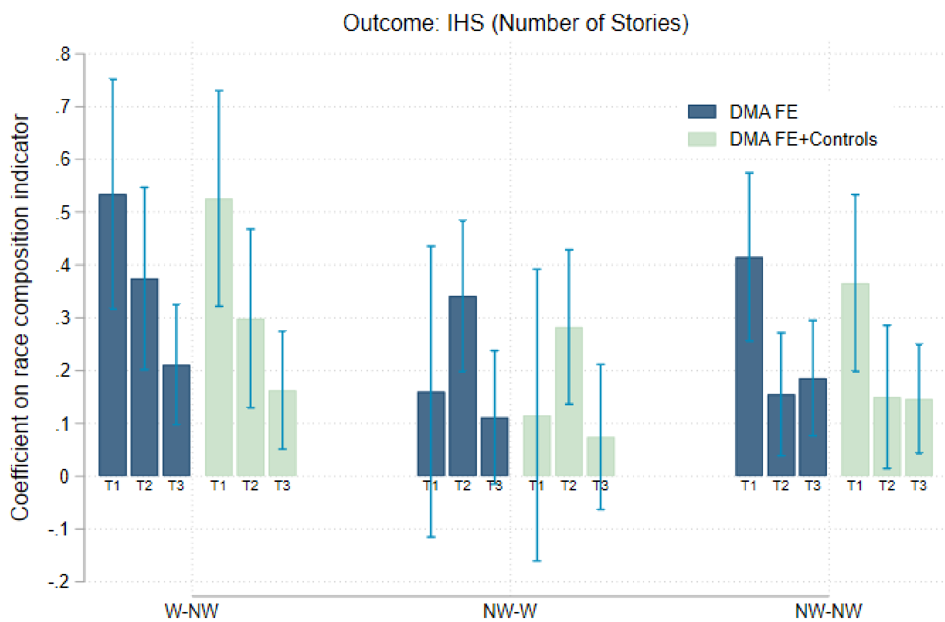
where P_{ijt} is the price of house i in tract j at time t, $\text{hom}^* = 1$ indicates the presence of a nearby homicide, and X_{ijt} represents other controls. To simplify the analysis I make an important assumption: the effect of coverage on housing prices is homogeneous across the types of homicides being compared for bias. This is supported by the fact that there does not seem to be any differential effect of crime coverage by race of the victim (see Figure 4). Given this assumption, the formula becomes: $\eta = (\alpha_1 + \alpha_2) \times \beta_{nw-v}$. Given the estimated effects in Subsection 3.2 ($\alpha_1 + \alpha_2 = 8.1\%$) and Table A.3 ($\beta_{nw-v}$ is 6 p.p), $\eta = 0.49\%$. That is, for a neighborhood where all the victims are non-white, the difference in the reduction of housing prices between a world with no media bias and one with the currently estimated bias is 0.49%. At the median house value of neighborhoods with a minority share of 50% or higher ($184,440), this represents a reduction of $903.75 per house. Of course, this estimated effect is germane for the homicides that would be compliers in my IV strategy -

Figure 7: News coverage of homicides by race profile by Black share DMA tercile

(a) Coverage rate



(b) Number of stories



This figure the estimated coverage gap of homicides by race composition similar to 7 but estimated by tercile (T1 to T3) of media markets by share population black. The set of controls for the second specification are the same as in Figure 6.

that is, homicides that are on the margin of coverage depending on other sporting events occurring in that media market.

5 Conclusions

This paper links nearly the universe of gun homicides, local-TV news, and housing transactions to unpack how information about crime, rather than crime per se, affects housing markets. Two facts emerge. First, exploiting exogenous crowd-out created by sporting events I find that a marginal increase in coverage depresses nearby sale prices by about eight percent - an effect likely driven by decreased demand. Second, local stations systematically over-represent incidents involving non-white participants, specially non-white suspects. Combining the estimated bias to over-cover non-white homicides, back-of-the-envelope calculations suggest that housing prices in minority neighborhoods can be suppressed by close to half a percentage point.

Two implications follow. First, hedonic studies that treat crime rates as fully observed amenities risk conflating disamenity and information channels; so researchers should model the information environment explicitly whenever possible. Second, expanding the availability of standardized, map-based crime statistics-along the lines of municipal open-data portals - would give consumers a less editorially filtered signal.

There are several possible avenues for future research. On the one hand, although it is clear that potential buyers respond to the information conveyed by the media, the extent to which this leads to overall crime misperception is an open question. On the other hand, the relationship between media and housing markets might be even closer that what I present here. As stated by Rose '[w]hile substantial work has explored the motivations and applications of stereotypic thinking (Hilton & Von Hippel, 1996), the formation and maintenance of groups over which stereotypes are formed has received less attention, especially in economics[.]' (Rose, 2023, pp. 19). Thus, exploring the effect of racial bias in the coverage of crime on negative stereotype formation can be a promising avenue for further research (Bursztyn & Yang, 2022). To the extent to which agents act on these stereotypes, they can in turn affect other important economic outcomes, including racial segregation and involvement in the criminal justice system (Philippe & Ouss, 2018).

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Appendix

A Additional Tables

Table A.1: Robustness - Effects Extensive Mg. Coverage on Housing Prices

	Outcome: Log. House Price				
	Main	Tr-Month-Yr FE	Hom.Sample	Hom.Controls	1 mi
	(1)	(2)	(3)	(4)	(5)
Covered Homicide $(\alpha_1 + \alpha_2)$	-0.092**	-0.140**		-0.144*	-0.057**
	0.022	0.028		0.067	0.020
$1[stories \geq 1]$ (α_1)	-0.123**	-0.169**	-0.098*	-0.166*	-0.064*
	0.039	0.053	0.049	0.078	0.027
Homicides (α_2)	0.031	0.029		0.022	0.006
	0.017	0.025		0.011	0.008
KP Statistic	292.072	192.893	458.032	121.971	49.577
Tract FE	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓
Observations	3,146,330	3,044,915	217,574	3,146,330	3,209,458

Notes: This table reports robustness checks for the main results in Table 4 (Column 5 - presented here in the first column as well). Tr-Month-Yr FE includes tract-by-month-by-year fixed effects. Hom.Sample restricts the sample to houses with at least 1 homicide in the quarter before sale. Hom.Controls includes controls for average characteristics of the victim (age, likelihood-by-race, and male indicator). 1 mi matches crimes up to 1 mile. Clustered s.e. by tract. (** $p < 0.01$;* $p < 0.05$).

Table A.2: Heterogeneity - Effects of Coverage on Log House Price

	Interaction Variable					
	Any Women	Pr. Non-White	Any Home Interest	Any Dom. Violence	Above Med. Pop. (County-2013)	Homicide Rate (2013)
	(1)	(2)	(3)	(4)	(5)	(6)
Covered Homicide	-0.124***	-0.100***	-0.118***	-0.123***	-0.112***	-0.096***
	(0.043)	(0.033)	(0.040)	(0.040)	(0.040)	(0.034)
Cov × Interaction Var (U)	0.030	-0.043	-0.015	0.042***	0.044***	-0.013**
	(0.022)	(0.029)	(0.019)	(0.016)	(0.013)	(0.006)
KP Statistic	141.660	114.754	142.376	145.784	150.660	135.156
Tract FE	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓
Tract FE	✓	✓	✓	✓	✓	✓
County-Trends						
House Characteristics						
Observations	3146330	3146330	3146330	3146330	3146330	3146330

Notes: This table presents an equivalent model to those in Table 4 and estimated by Equation 5, but adding a term interacting coverage with each interaction variable. 'Covered Homicide' represents the news coverage measurement and 'Cov × Interaction Var (U)' measures the change in the treatment effect for the 'High U' group compared to the 'Low U' group. This value is instrumented with gameday times the corresponding interaction variable. 'Any Women', 'Pr. Non-White', 'Any Home Interest', and 'Any Dom. Violence' are measured at the incident level. 'Above Med. Pop.' and 'Homicide Rate' are measured at the county level in 2013. Each homicide gets matched to the house if it takes place within 0.5 miles, and in the quarter leading to 2 months before the recorder date for the transaction. Clustered s.e. by tract. (** $p < 0.01$;* $p < 0.05$). See Data Appendix for description of each variable and sources.

Table A.3: Gaps in News Coverage

	(1)	(2)	(3)	(4)	(5)	(6)
Race Composition Indicator	Panel A: Coverage Rate Outcome					
WV-NWS	0.063***	0.066***	0.058***	0.056***	0.056***	0.057***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
NWV-WS	0.041***	0.043***	0.035***	0.033**	0.032**	0.033**
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)
NWV-NWS	0.061***	0.060***	0.063***	0.061***	0.061***	0.055***
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)
	Panel B: IHS (Number of Stories) Outcome					
WV-NWS	0.296***	0.301***	0.272***	0.243***	0.243***	0.247***
	(0.049)	(0.048)	(0.047)	(0.046)	(0.046)	(0.047)
NWV-WS	0.194***	0.200***	0.162***	0.145***	0.148***	0.154***
	(0.049)	(0.048)	(0.049)	(0.050)	(0.049)	(0.052)
NWV-NWS	0.203***	0.192***	0.204***	0.186***	0.186***	0.165***
	(0.039)	(0.040)	(0.037)	(0.037)	(0.038)	(0.041)
Sample	Full	With Suspect	With Suspect	With Suspect	With Suspect	With Suspect
DMA FE	✓	✓	✓	✓	✓	✓
Victim Controls			✓	✓	✓	✓
Suspect Controls				✓	✓	✓
Incident Controls					✓	✓
Neighborhood Controls						✓
Observations	48,017	25,865	25,753	25,753	25,753	25,152

Notes: This table reports coefficients from equation 7, which estimates the OLS coefficient on a race composition indicator to measure gaps in coverage compared to homicides with a white victim and a white suspect. Victim and suspect controls are dummies for 5-year age groups, a dummy for no known-age, a dummy sex, a dummy for no-known sex and a dummy if name of victim is known for victim and suspect characteristics, respectively. Incident controls include the number of killed individuals. Neighborhood characteristics include the wealth quintile within the DMA of the tract, population in the tract, and population white in the tract. The sample only includes incidents in which the suspect is known. Column 1 also includes dummies for white victim with unknown suspect, and non-white victim with unknown suspect. Clustered s.e. by DMA.

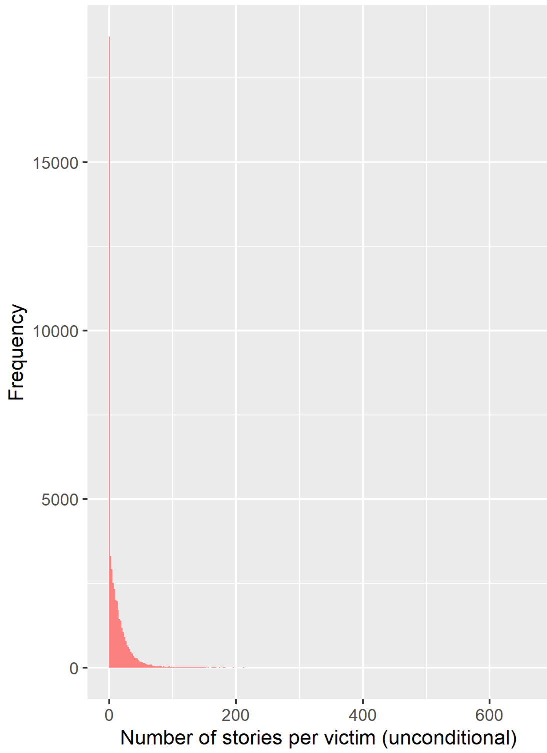
Table A.4: Robustness in Gaps in News Coverage

	(1)	(2)
Race Composition Indicator	Panel A: Quantile Regression	
WV-NWS	0.290***	0.331***
	(0.079)	(0.081)
NWV-WS	0.157*	0.158
	(0.086)	(0.096)
NWV-NWS	0.225***	0.254***
	(0.063)	(0.078)
	Panel B: Negative Binomial	
WV-NWS	0.104***	0.110***
	(0.019)	(0.019)
NWV-WS	0.060***	0.065***
	(0.020)	(0.022)
NWV-NWS	0.076***	0.072***
	(0.016)	(0.018)
	Panel C: Any Story with Incident Controls	
WV-NWS	0.055***	0.056***
	(0.012)	(0.011)
NWV-WS	0.0304**	0.0300**
	(0.013)	(0.013)
NWV-NWS	0.051***	0.055***
	(0.016)	(0.016)
	Panel D: IHS(stories) with Incident Controls	
WV-NWS	0.265***	0.274***
	(0.050)	(0.047)
NWV-WS	0.165***	0.163***
	(0.054)	(0.050)
NWV-NWS	0.173***	0.207***
	(0.068)	(0.063)
Sample	With Suspect	With Suspect
DMA FE	✓	✓
Victim Controls	✓	
Suspect Controls	✓	
Incident Controls	✓	
Neighborhood Controls	✓	
Observations	25,865	25,152

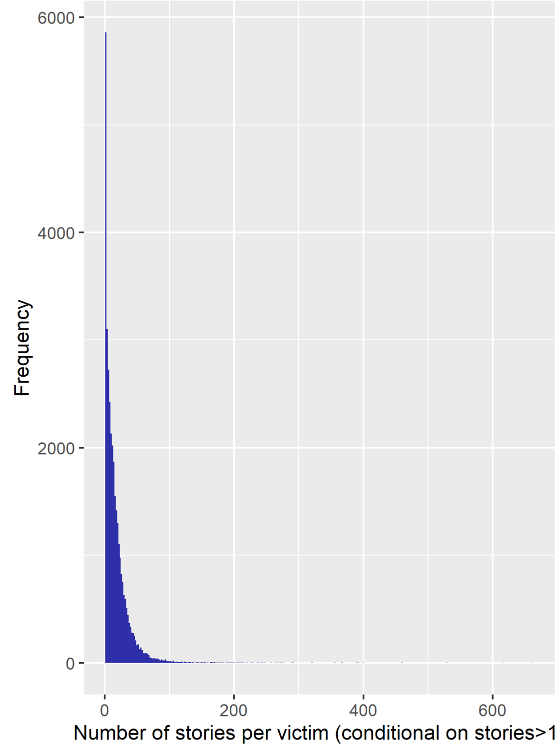
Notes: This table reports coefficients similar to those in equation 7, which estimates the coefficient on a race composition indicator to measure gaps in coverage compared to homicides with a white victim and a white suspect. Panel A shows the results from running a quantile regression at median instead of an OLS one. Panel B shows the result from an equivalent negative binomial model. Panel C and D add to the main controls incident flags and homicide rates by race and age within that tract. Clustered s.e. by DMA. (*** $p < 0.01$;** $p < 0.05$; * $p < 0.1$).

B Additional Figures

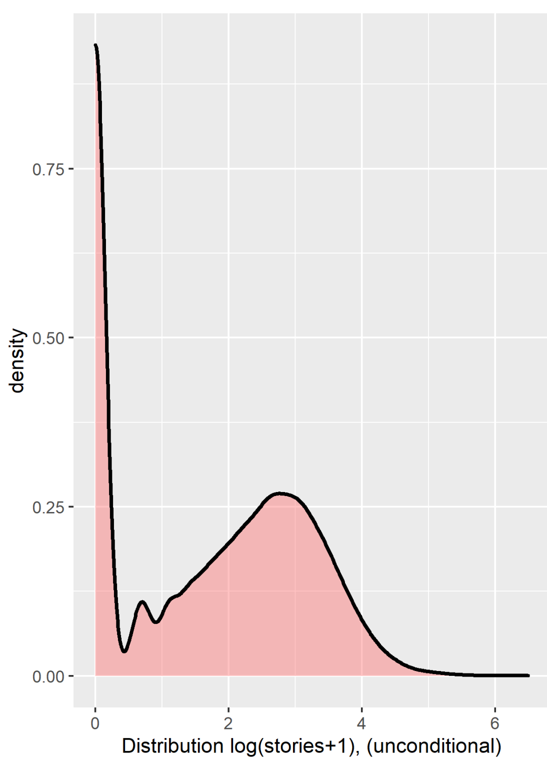
Figure B.1: Distribution of Stories and log(Stories) per Homicide



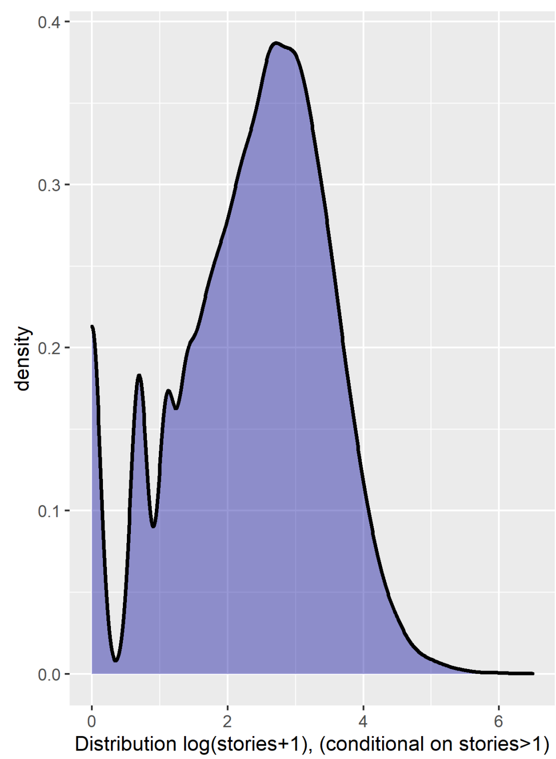
(a) Stories [Unconditional]



(b) Stories [Conditional stories>0]

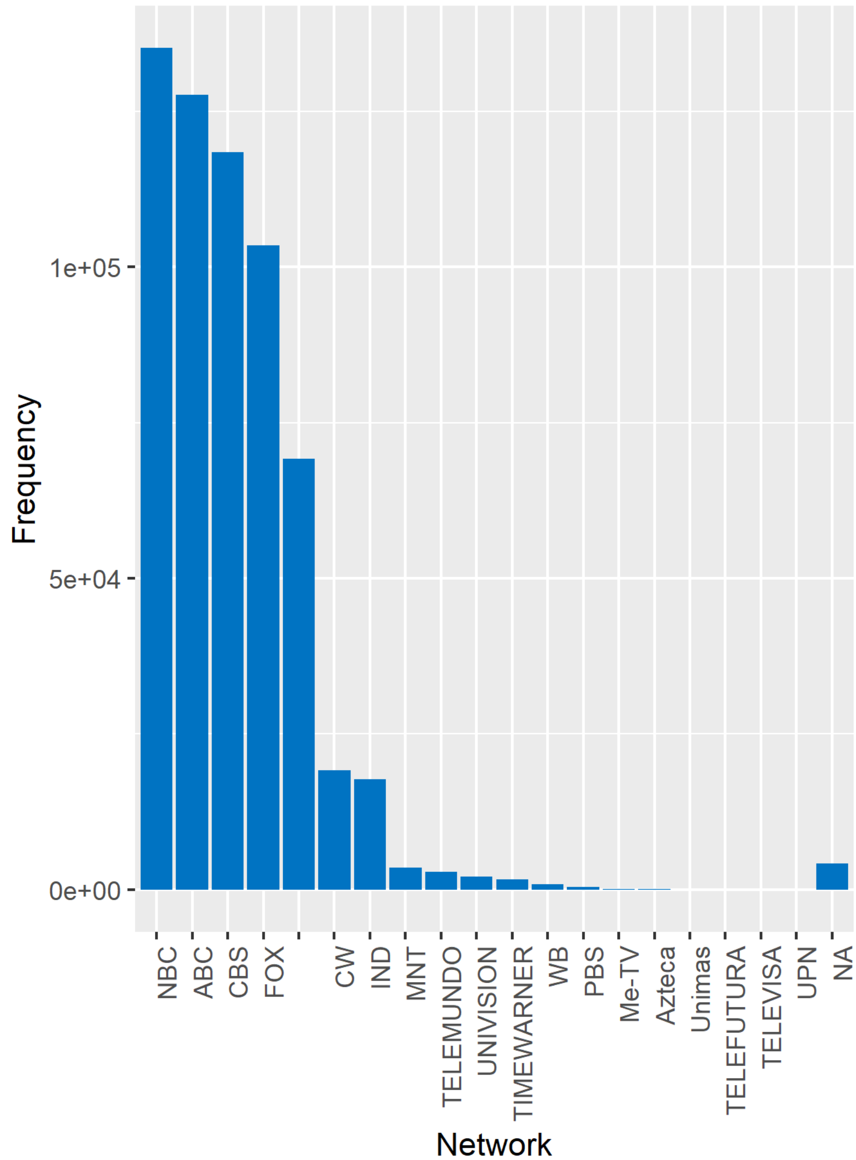


(c) Log (Stories+1) [Unconditional]



(d) Log (Stories+1) [Conditional stories>0]

Figure B.2: Rank of Networks by number of stories in sample



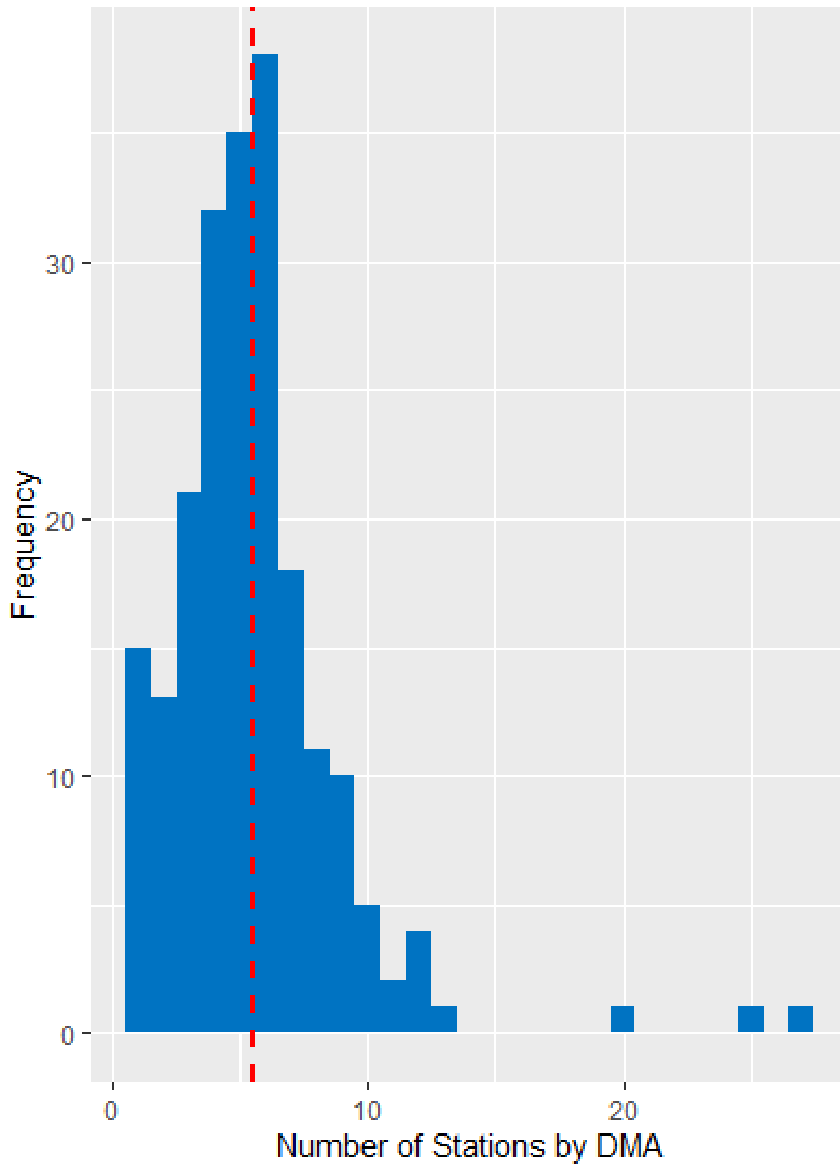
C Data Details

C.1 Merging victims with news stories

Sample size: 624,000 news stories about 49,000 victims

- Select only killed victims
- Find stories that include name or address, plus terms 'killed', 'homicide', 'shooting', and so on, within 7 days of crime.

Figure B.3: Distribution of Number of Stations by DMA



Example:

From 'FOX 44 NEWS @ NINE':

"In tonight's crime tracker, the search is on for the person police say, KILLED 2 people last night. Investigators say PERSON NAME and *SECOND VICTIM* were found with bullet wounds, inside an apartment on GARDERE LANE in Baton Rouge. PERSON NAME was pronounced dead on scene. *SECOND VICTIM* was taken to the hospital where he later died. If you have information that can help investigators, call crime stoppers at 344-stop."

C.2 GVA Data

Following categories for incident characteristics were grouped into dummies:

Accidental: "Accidental Shooting - Death", "Accidental Shooting - Injury", "Accidental Shooting at a Business", "Accidental Shooting", "Accidental/Negligent Discharge"

Defensive Use: "Defensive Use - Crime occurs, victim shoots subject/suspect/perpetrator", "Defensive Use - Good Samaritan/Third Party" , "Defensive use - No shots fired" , "Defensive Use - Shots fired, no injury/death", "Defensive Use - Stand Your Ground/Castle Doctrine established", "Defensive Use - Victim stops crime", "Defensive Use - WITHOUT a gun", "Defensive Use"

Domestic Violence: "Domestic Violence"

Gang Involvement: "Gang involvement", "Shootout (where VENN diagram of shooters and victims overlap)"

School related:"School Incident", "School Shooting - elementary/secondary school", "School Shooting - university/college", "Gun at school, no death/injury - elementary/secondary school"

Others: "Animal shot/killed", "Armed robbery with injury/death and/or evidence of DGU found", "Assault weapon (AR-, AK-, and ALL variants defined by law enforcement)", "ATF/LE Confiscation/Raid/Arrest", "Attempted Murder/Suicide (one variable unsuccessful)", "Bar/club incident - in or around establishment", "BB/Pellet/Replica gun", "Brandishing/flourishing/open carry/lost/found", "Car-jacking", "Child injured (not child shooter)", "Child injured by child", "Child Involved Incident", "Child killed (not child shooter)", "Child killed by child", "Child killed self", "Child picked up and fired gun", "Concealed Carry License - Perpetrator", "Concealed Carry License - Victim", "Criminal act with stolen gun", "Drive-by (car to street, car to car)", "Drug involvement", "Ghost gun", "Gun buy back action", "Gun range/gun shop/gun show shooting", "Gun shop robbery or burglary", "Gun(s) stolen from owner", "Guns stolen from law enforcement", "Hate crime", "House party", "Hunting accident", "Implied Weapon", "Institution/Group/Business", "Kidnapping/abductions/hostage", "LOCKDOWN/ALERT ONLY: No GV Incident Occurred Onsite", "Mistaken ID (thought it was an intruder/threat, was friend/family)", "Non-Aggression Incident", "Non-Shooting Incident", "Pistol-whipping", "Playing with gun", "Police Targeted", "Political Violence", "Possession (gun(s) found during commission of other crimes)", "Possession of gun by felon or prohibited person", "Road rage", "Self-Inflicted (not suicide or suicide attempt - NO PERP)", "Sex crime involving firearm", "Shot - Dead (murder, accidental, suicide)", "Shot - Wounded/Injured", "Shots Fired - No Injuries", "Shots fired, no action (reported, no evidence found)", "ShotSpotter", "Spree Shooting (multiple victims, multiple locations)", "Stolen/Illegally owned guns recovered during arrest/warrant", "Suicide - Attempt", "Suicide", "Thought gun was unloaded", "Under the influence of alcohol or drugs (only applies to the subject/suspect/perpetrator)", "Unlawful purchase/sale", "Workplace shooting (disgruntled employee)"

Home Invasion: "Home Invasion - No death or injury", "Home Invasion - Resident injured", "Home Invasion - Resident killed", "Home Invasion - subject/suspect/perpetrator injured", "Home Invasion - subject/suspect/perpetrator killed", "Home Invasion"

Mass murder or shooting: "Mass Murder (+ deceased victims excluding the subject/suspect/perpetrator , one location)", "Mass Shooting (+ victims injured or killed excluding the subject/suspect/perpetrator, one location)"

Murder and Suicide: "Murder/Suicide"

Officer involved: "Officer Involved Incident - Weapon involved but no shots fired", "Officer Involved Incident" "Officer Involved Shooting - Accidental discharge - no injury required", "Officer Involved Shooting - Bystander killed", "Officer Involved Shooting - Bystander shot", "Officer Involved Shooting - Officer killed", "Officer Involved Shooting - Officer shot", "Officer Involved Shooting - Shots fired, no injury", "Officer Involved Shooting - subject/suspect/perpetrator killed", "Officer Involved Shooting - subject/suspect/perpetrator shot", "Officer Involved Shooting - subject/suspect/perpetrator suicide at standoff", "Officer Involved Shooting - subject/suspect/perpetrator suicide by cop", "Officer Involved Shooting - subject/suspect/perpetrator surrender at standoff", "Officer Involved Shooting - subject/suspect/perpetrator unarmed"

Terrorism: "Terrorism Involvement"

C.3 Imputation of race

Race imputation occurs in two stages. In the first stage, I use the package WRU in R based on Imai & Khanna (2016) and Khanna *et al.* (2017) to estimate the posterior probability of any individual belonging to white or non-white race group conditional on their last name and the tract where the homicide happened. This method uses both the Census demographic distribution of the tract, along with the U.S. Census Surname List of 2010, to estimate the $Pr(Race_i|Surname_i, tract_i)$ for each subject (victim and suspect). In the few cases in which the last name is not observed, the posterior probability is only estimated with the demographic distribution of the tract. In the second stage, following Collinson *et al.* (2023), I choose a threshold of 0.5 in the posterior probability to impute the race group of the individual.

Below are presented the bivariate density of the posterior probabilities of race of suspect and victim by incident. It would be concerning if the posterior probabilities are close to the threshold value used (in this case, 0.5). As it can be corroborated in the Figures, this is not the case here, as the mass of the density is concentrated in the corners (0,0) and (1,1) - further corroborating that most crime is intra-racial instead of inter-racial.

Figure C.1: Density of posteriors of race - Heatmap

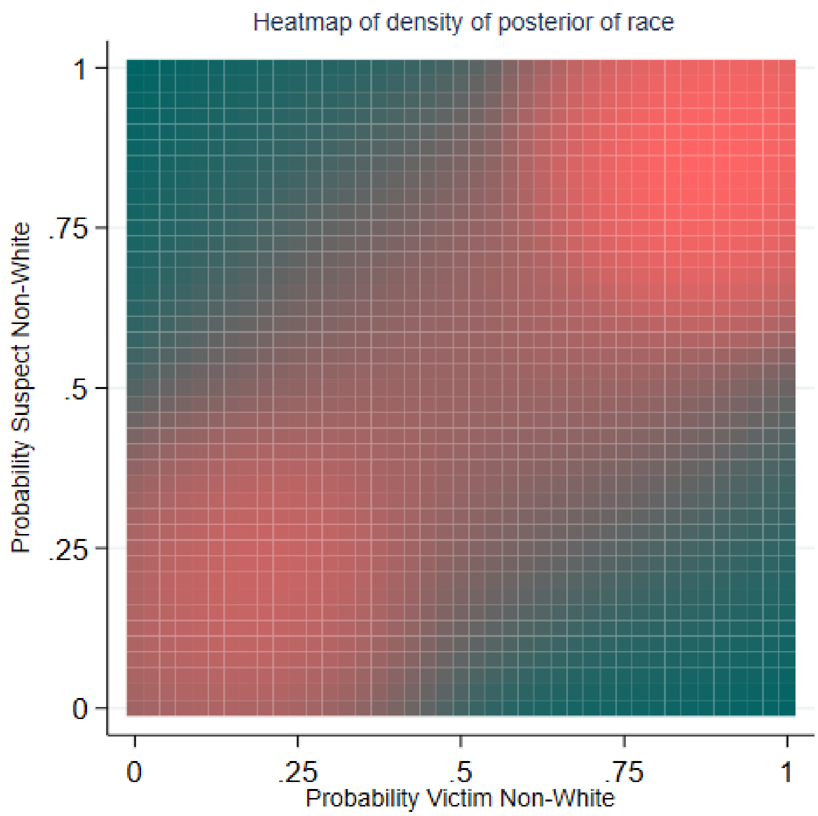
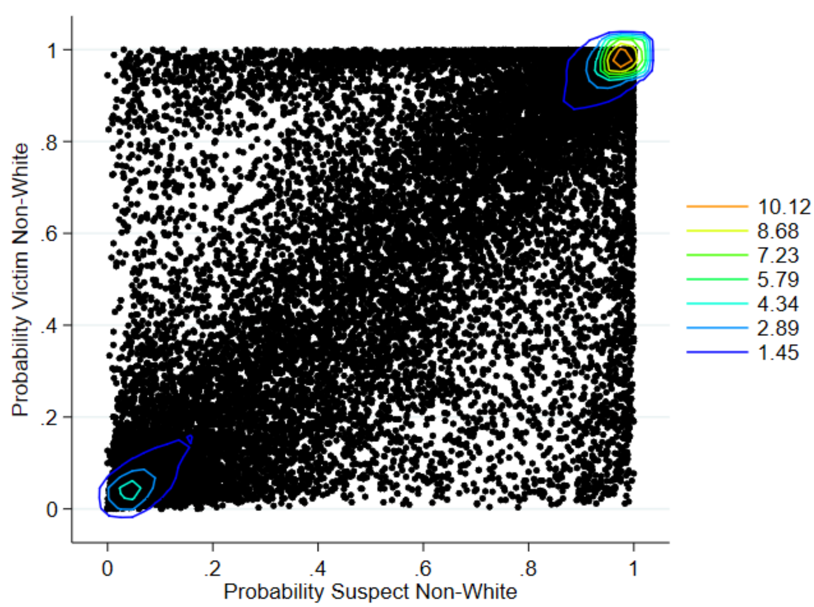


Figure C.2: Density of posteriors of race - Contour map

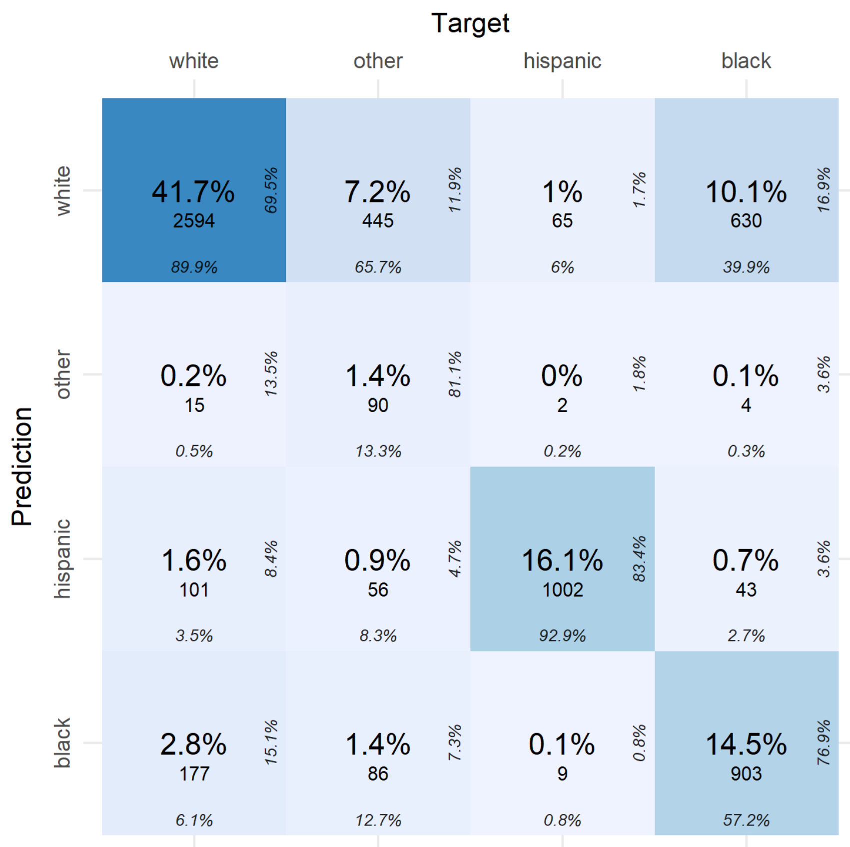


Fremont *et al.* (2016) show that predicting health outcomes with the predicted probability is essentially indistinguishable than using self-reported data. They cite that the agreement between self-reported race and this the predicted probability is typically between 90 and 96% for the four largest groups: Blacks, Asian/Pacific Islanders, Hispanics and Whites. Accuracy is lower for American Indians/Alaskan Natives and Multi-racial individuals.

C.3.1 Race classification error

To test how well the algorithm is performing, I use a database of victims in which the race is known: the Mapping Police Violence (MPV). The MPV dataset is composed by individuals killed by the police, and is similar to the GVA in that it includes names, age and gender, but also race. Doing the imputation on this set, I can test for the different rates of type 1 and type 2 error. Figure C.3 below presents the confusion matrix for the prediction of race for the MPV sample.

Figure C.3: Confusion Matrix for Race Imputation



Let r be the true or target race, and r^* be the predicted race. The rows of the matrix represent the predicted race, while the columns indicate the actual race. The large numbers in each cell represent the overall percentage of the sample for that class. Type 1 error for the white/non-white classification is the share of classified whites that are not white. That is, Type 1 error is 31.5%. Type 2 error is given by the rate at which whites are classified as non-whites, which is 13.3%.

Indeed, we can obtain also the probabilities of being classified as white while being non-white: $pr(r^* = w|r = nw) = 0.3418$; and the probability of being classified as non-white while being white: $pr(r^* = nw|r = w) = 0.111$.

In general, it is well known that when classification error for binary variables is not too large, there is attenuation bias in OLS, so that the OLS estimate is a lower bound for the true coefficient.[15] In these cases, we cannot have classical measurement error as the correlation between measurement error and the dummy is always negative. Aigner (1973) shows that in this case, if there are no other controls in the regression, then the OLS estimate of the effect of race on coverage n is $\beta^{OLS}_{n,r^*} = \beta_{n,r}[1 - pr(x^* = 1|x = 0) - pr(x^* = 0|x = 1)]$.

In this case, based on the coefficients from Table A.3, we would have that the real difference in coverage across race of victim $\beta_{n,r} = 0.057/(1 - 0.3418 - 0.111) = 0.1042$. That is, the estimates we have presented are a lower bound, and if the sample from police shootings is informative on the rates of misclassification, the true effects are approximately double those presented in the main tables.

C.4 Zillow data filters

Beginning with the full Zillow Transaction and Assessment Dataset (ZTRAX), I implement the following sequential screens:

1. **Arm's-length condition**. Remove observations flagged as gifts, non-arm's-length transfers, or with a reported sale price of $0 or missing.

2. **Conveyance type**. Retain only records coded as *deed transfer*, *deed with concurrent mortgage*, *foreclosure*, or *mortgage*.

3. **Property-use code**. Keep parcels whose use is classified as single-family residence (SFR or inferred SFR), townhouse, cluster home, condominium, cooperative, row house, planned-unit development (PUD), bungalow, patio home, or garden home.[16]

4. **Duplicate-day filter**. If the same parcel identifier (`zpid`) appears multiple times on the same calendar day, drop all but the first observation to avoid within-day flips.

5. **Price trimming**. Exclude sales with nominal prices below $10,000 (likely non-arm's-length or data errors) and above $900,000. I do so for two reasons. First, essentially all houses above this price do not have a homicide in the relevant radius, and thus, are not part of the common support for the comparison with houses that do have a homicide. The second reason, is to avoid these outlier observations from driving the results presented.

6. **Common-support filter**. Discard parcels in census tracts that register *no* homicides

[15]This is in case that $pr(x^* = 1|x = 0) + pr(x^* = 0|x = 1) < 1$. As we will see, this is the case here.

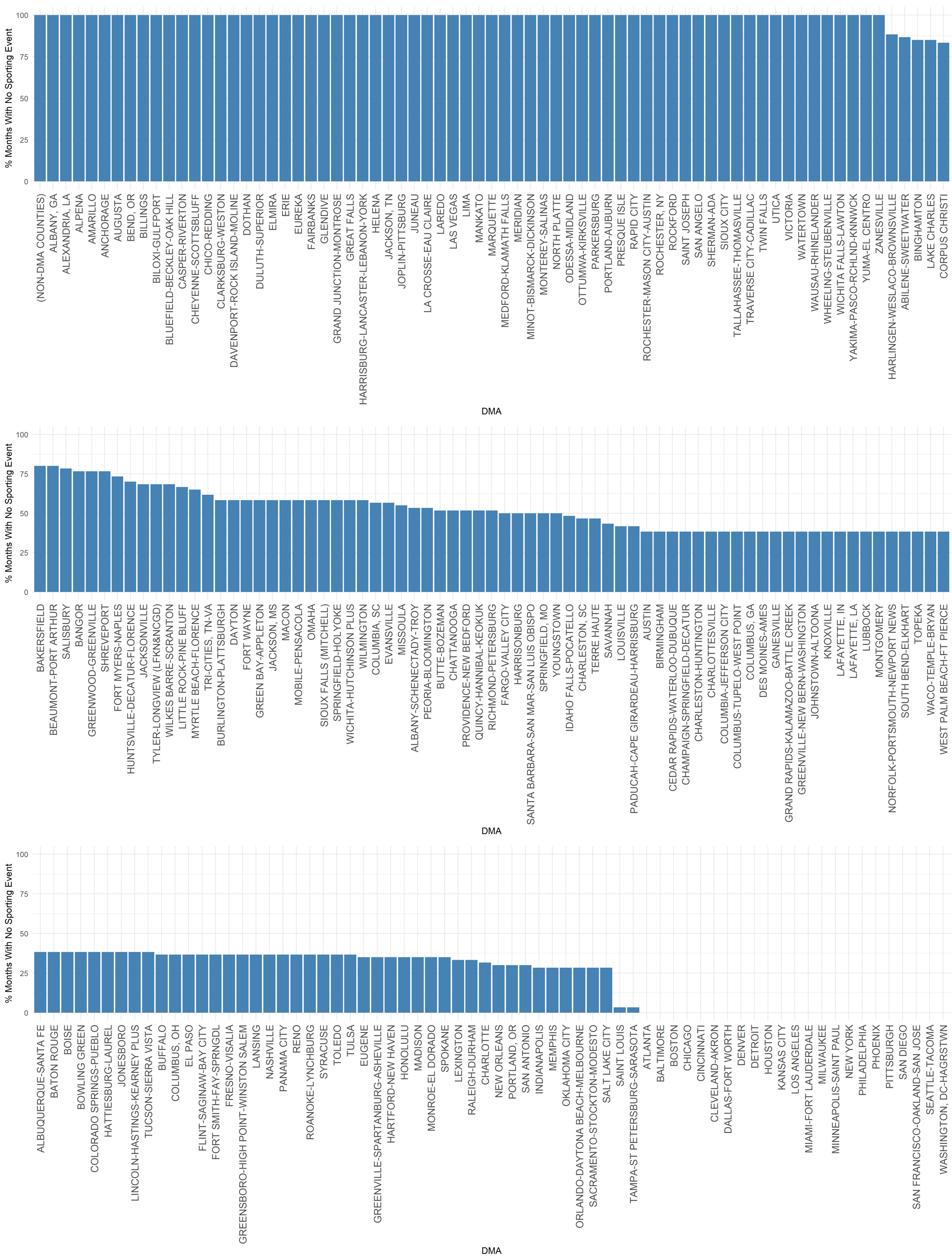
[16]These codes correspond to `property_use_standardized` values `RR101`, `RR102`, `RR103`, `RR104`, `RR105`, `RR106`, `RR107`, `RR108`, `RR109`, and `RR110` in the public ZTRAX codebook.

during the study period so that every observation belongs to a tract in which treatment status (exposure to a nearby homicide) can, in principle, vary.

C.5 Instrument variation

The instrument for coverage is the share of homicides around a house being sold that occurs right before a game day of a team in the same media market. That implies that the share can be different from 0 only in places with teams in the NBA, MLB, NCAA basketball, or NCAA football. It turns out that not all media markets have a team playing in either of those leagues. Thus, the exogenous variation that the instrument is leveraging is actually coming from those markets with at least some games in those leagues. Figure C.4 below shows the distribution of the share of months by media market that is missing any sporting event in my data. Overall, 30% of all media markets do not have any sporting events. As might be expected, these are fairly small media markets overall. Thus, I exclude from the main results all media markets without at least one month with games in my data.

Figure C.4: Share of months missing sporting events in my data by DMA



These figures plot the share of months with no sporting events by teams in each DMA, for matches in the NBA, MLB, NCAA basketball, and NCAA football. The DMAs with no sporting events throughout the sample account for 6% of all transactions in the final sample.

D Simple Hedonic model with uncertainty

This appendix presents a unified modeling framework that reconciles the standard hedonic model under certainty (as in Rosen (1974)) with an extension that incorporates uncertainty and subjective expectations about non-marketable amenities, such as crime. By placing both scenarios in a single framework, we clarify how the standard hedonic regression coefficient on realized crime may fail to recover the true marginal willingness-to-pay (MWTP) for reductions in crime risk. In the presence of uncertainty and imperfect information, the hedonic price function and MWTP must be understood in terms of subjective probabilities rather than ex-post realizations. This perspective illuminates the source of potential bias when using realized crime levels as a proxy for households' valuations of crime avoidance.

D.1 Under Uncertainty

We begin with a household that chooses housing characteristics x, anticipating a non-marketable amenity C that may or may not occur (e.g., a high-crime environment). The household also consumes a numeraire good z, and total income is y:

$$\max_{x,z} E_{C|N}[u(x, C, z; h)] \quad \text{s.t.} \quad y = z + P(x, E_{C|N}[C]).$$

Here, C is a random variable taking values in $\{0, 1\}$, and $E_{C|N}[C] = \lambda_{C|N}$ denotes the household's subjective probability of experiencing $C = 1$ given signal N. The term $P(x, \lambda_{C|N})$ is the hedonic price function that depends on both x and the household's expectation of the amenity.

In the standard no-uncertainty model, C is known, and thus $\lambda_{C|N} = C$. The household solves:

$$\max_{x,z} u(x, C, z; h) \quad \text{s.t.} \quad y = z + P(x, C).$$

In this simpler setting, the first-order conditions imply:

$$MWTP_C = \frac{\partial u/\partial C}{\partial u/\partial z} = \frac{\partial P(x, C)}{\partial C}.$$

This is the classic Rosen (1974) result, where the slope of the hedonic price function with respect to the amenity equals the household's MWTP.

D.2 Deriving MWTP Under Uncertainty

Under uncertainty, the household's expected utility is:

$$E_{C|N}[u(x, C, z; h)] = \lambda_{C|N} u(x, 1, z; h) + (1 - \lambda_{C|N}) u(x, 0, z; h).$$

Using a Lagrange multiplier λ for the budget constraint, the first-order conditions give:

$$\frac{\partial E_{C|N}[u]}{\partial z} - \lambda = 0, \quad \text{and} \quad \frac{\partial E_{C|N}[u]}{\partial \lambda_{C|N}} - \lambda \frac{\partial P(x, \lambda_{C|N})}{\partial \lambda_{C|N}} = 0.$$

Combining these, we get:

$$\frac{\partial E_{C|N}[u] / \partial \lambda_{C|N}}{\partial E_{C|N}[u] / \partial z} = \frac{\partial P(x, \lambda_{C|N})}{\partial \lambda_{C|N}}.$$

The left-hand side is the household's MWTP for a marginal change in the *perceived* probability of crime, while the right-hand side is the corresponding slope of the hedonic price function. Thus, the equilibrium condition under uncertainty is perfectly analogous to the certain case, except it is defined in terms of $\lambda_{C|N}$ rather than a known C.

If households are fully informed, then their subjective probability matches the objective probability, $\lambda_{C|N} = \lambda_C^0$, and using λ_C^0 in the hedonic regression would identify the true MWTP for avoiding crime. In practice, households' beliefs may diverge from the objective probability due to imperfect information, media bias, or behavioral factors.

D.3 Bias in Reduced-Form Hedonic Regressions with Realized Crime

Empirically, researchers often regress observed prices on *realized* crime outcomes C_i:

$$\ln P_i = \alpha_0 + \alpha_1 C_i + x_i' \beta + \epsilon_i.$$

However, C_i is an ex-post realization, not the household's ex-ante perceived probability $\lambda_{C|N}$ at the time of purchase. Since the hedonic equilibrium (and thus MWTP) reflect beliefs rather than realizations, using C_i directly can lead to biased estimates of the true MWTP. Formally:

$$\alpha_1 \approx \frac{\partial P(\cdot)}{\partial \lambda_{C|N}} \frac{d\lambda_{C|N}}{dC_i},$$

and the ratio $\frac{d\lambda_{C|N}}{dC_i}$ need not be one. Households might vastly overreact (or underreact) to certain signals relative to the eventual crime outcomes, distorting the interpretation of

α_1 as a measure of MWTP.

D.4 Conclusion

By framing both the certain and uncertain cases within a single approach, we reveal that what matters for hedonic equilibrium and MWTP estimation is households' subjective probabilities. The standard hedonic model recovers MWTP under perfect information, but with uncertainty and imperfect information, the straightforward use of realized crime measures C_i in hedonic regressions will not generally yield unbiased estimates of the true MWTP.

E Analysis of Coverage on Listings as Mechanism

In Subsection 3.2 I presented evidence that the housing prices decrease up to 8% whenever there is a covered homicide in the area and that the price effect of a non-covered homicide is indistinguishable from zero. But what are the mechanisms driving these results? In this Section I evaluate what happens to both the number of listed houses up for sale, and to the price of those listed units, in an area that has a covered or non-covered homicide.

Supply: On the one hand, it is possible that the coverage of the homicide shifts demand downwards. This is plausible if most potential home buyers do not find out about the homicides occurring in a specific location unless the news media covers them. That shift will be reflected in prices so long as supply does not change with the coverage of a homicide. On the other hand, it is possible that once a homicide in an area occurs, supply shifts outwards if some homeowners had overly optimistic beliefs about the security in the area. Notice however, that if the decreases in prices are working mainly through the supply side, one should expect to see a decrease in prices both for houses around covered homicides as well as for non-covered ones, which is not what I found in the previous section.

An alternative, more direct, way to evaluate effects on the supply side is to estimate the change in the overall number of listings in areas that have a covered homicide and compare them with areas that have a non-covered homicide. Although Zillow does not provide information on individual listings, they do report the median number of listings in a given month at the ZIP level for all zip-months that have at least 10 listings in it. I merge these data at the zip-by-month level with the GVA and NE data to obtain the number of homicides and the coverage of those homicides at the same level of aggregation. I take the sample between 2014 to 2018, as this overlaps with my main specification in the previous section. With these data, I can test if the median number of listed units in a zip code with a covered homicide changes compared to zip codes with homicides that are not covered. If there are no differences between the two, that would be evidence that the lower prices paid for houses close to a covered homicide are mainly driven by downward shifts in demand.

Listed Prices for Housing Units: A related question is if the decrease in transacted prices for houses close to a covered homicide is driven by immediate decreases in the listed price of the units, or if the potential buyers are driving down the price from the listed one to the transacted one during the bargaining process for the sale. Again, one would expect the latter to be the case if indeed it is the coverage itself informs more of the demand side than the supply. Zillow also reports the median price of listings by zip code and month (if there are more than 10 listings in the zip code). I merge this data with the GVA and NE data as well.

Specification and TWFE estimate: With these data at hand, I want to evaluate the effect of homicide coverage on the median number of listings at the zip-month level, as well as the median listed price per square foot. I start proposing a difference-in-difference specification, in which treatment can be defined by the group of zip codes homicides that get covered, while control is made out of zip codes with non-covered homicides. The event marking the before and after would be the homicide itself. This comparison would allow us to recover the effect of coverage itself and separate it from the non-TV media effect of homicides on listings. Notice, however, that the cleanest sample to be used for this approach would be to only consider zip codes that have exactly one month with homicides across the whole sample, as those would have only one event defining a pre- and post-period.[17]

Equation E.1 presents the proposed model to evaluate the effect of coverage on listings and prices for the sample of zip codes with only one month with homicides in the sample. The treatment group is defined by zip codes with one month of homicides throughout the sample, and at least one of those homicides gets covered in the news. The control group, on the other hand, is defined by zip codes with one month of homicides in the sample, but that homicide is not covered in the news. I can evaluate the extent to which the zip codes in the control and treatment differ from one another pre and post-homicide. I consider up to 4 quarters before, and up to 12 quarters after the homicide. The proposed model is

[17] Alternatively, one would need to define the maximum length that a covered and uncovered homicide would have in listings or prices. Without that, the effect of either cannot be identified. See Chaisemartin *et al.* (2022).

thus:

$$
\begin{aligned}
y_{zmq} = & \lambda_z + \mu_m + \sum_{q=-4}^{-1} \pi_q Coverage_{zq} + \\
& \sum_{q=0}^{12} \alpha_q Coverage_{zq} + X_{zqm}\Gamma + \epsilon_{zmq}
\end{aligned} \tag{E.1}
$$

where y is either the log of the median number of listings in the month or the log of the median listed price in zip code z and month-by-year m. The homicide occurs in quarter $q = 0$. λ_z and μ_m represent zip code and month-by-year fixed effects, respectively. I estimated a weighted model by zip population and cluster standard errors by zip code to allow for serial correlation. The parameters of interest are the effects of homicide coverage up to 12 quarters (3 years) after the homicide ($\{\alpha_q\}_1^{12}$), and the differences in trends between control and treatment zip codes leading up to the homicide ($\{\pi_q\}_{-4}^{-1}$). This strategy has several advantages for identification: first, it allows defining a state-absorbing treatment as occurring once in time for each zip code; second, it allows for a fully non-parametric control for the number of months with homicides by selecting places that only have one such month.

In this case, the treatment effect of coverage on outcomes, α_q, can be estimated for the treated group (ATT) by using a Two Way Fixed Effects (TWFE) model under two main assumptions. The first is that zip codes in which the homicide is covered would have followed the trend of zip codes with a non-covered homicide in the absence of coverage. That is, the counterfactual of the number of listings (or the price of said listings) for zip codes in which the homicide gets covered is only correctly specified if it would have followed the same trend in time as did the number of listings (or listed price) for those zip codes in which the homicide did not get covered. Although the parallel trend assumption after the homicide occurs is not testable, the parallel trend in the lead-up to the homicide can be tested by verifying if π_q is indistinguishable from zero. The second assumption is that the treatment effect of coverage is not heterogeneous by the timing of treatment. This seems like a sensible assumption given that the panel is relatively short in terms of time (covering 48 months). This assumption can be relaxed, however, as I present below.

The TWFE estimates of Equation E.1 are presented in Figure E.1, with the effects of crime coverage in the median number of listings in Figure E.1a and in the median listed price per sq ft in Figure E.1b. Both figures show that the zip codes in which the homicide was covered follow the same trajectory in terms of the number of listings and price per square feet for the median listing up to the quarter in which the homicide takes place. Indeed all the differences in the pre-period are precisely estimated zeroes. Furthermore, Figure E.1a shows that there is essentially no change in the supply of houses for the first three quarters after the homicide, and a steady decline starting in the fourth quarter up to the 8th, in which there is a decrease of 4.8% in the median monthly number of listings. This decrease of between 3 and 4 % in the supply seems to continue throughout the third year as well.

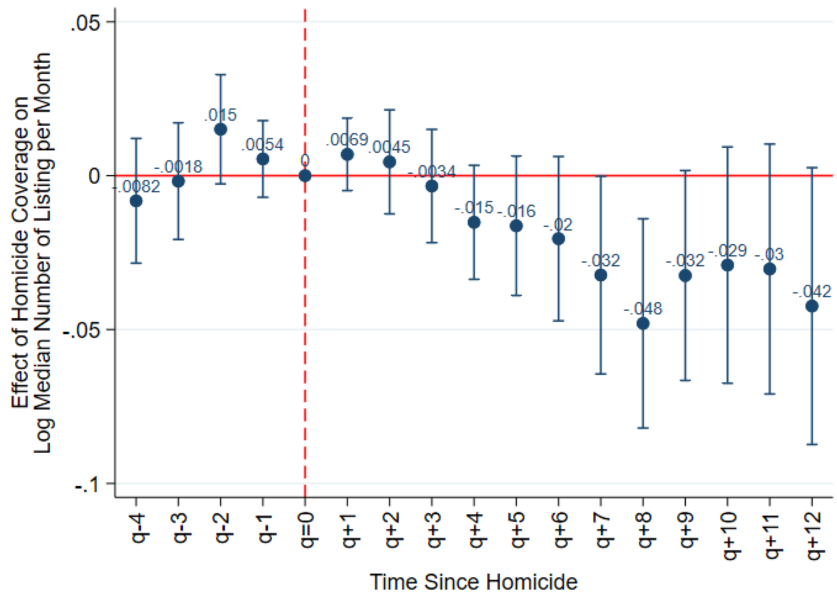
When evaluating the effect of coverage in the price per square feet at the zip code level, Figure E.1b suggests that there is an immediate and steady decline in listed prices for at least three quarters after the homicide occurs. Although noisy, these estimates show a decrease in the average median listed price of between 3 and 5 % for those zip codes in which the homicide was covered compared to those in which it was not. There only starts to be a closing of the price gap by the end of the third year.

Time Heterogeneous Treatment Effects: The previous result captures coverage's effect on the number of listings and listed prices for the median listing by zip code. There are two important things to note about those estimates: first, they capture the effect of coverage, but do not present the non-coverage-related effects of a homicide, as the comparison group also had a homicide; second, the TWFE estimate only captures a weighted average of the treatment effects of coverage if there is no heterogeneity in treatment effects through time, as the TWFE is estimated in a staggered treatment model (see Chaisemartin *et al.* (2022)). Below I estimate now a model that both allows for heterogeneity in treatment effects through time and directly compares places with no homicides with those that have a homicide that was covered, as well as those places with a non-covered homicide. The estimation follows the model with individual and time-heterogeneous treatment effects and estimation based on the imputation approach proposed by Borusyak *et al.* (2021).

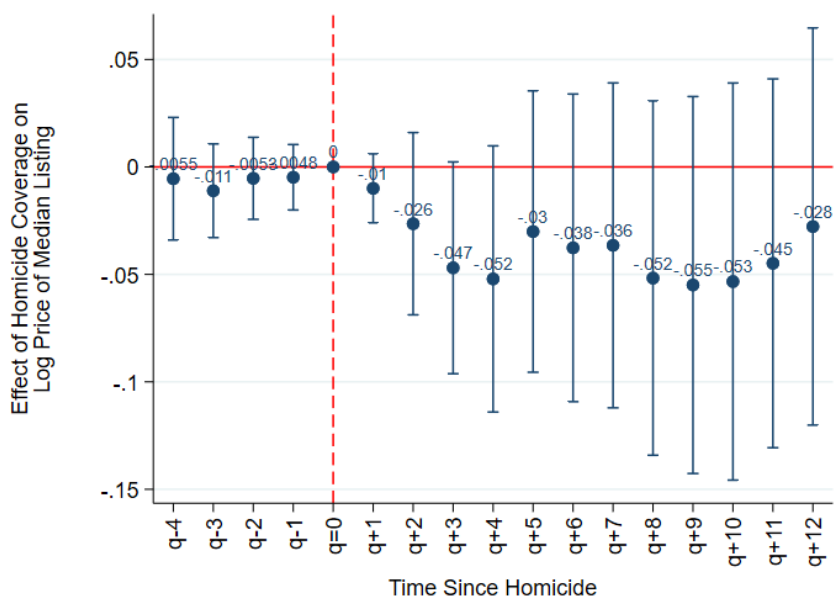
In both instances, the comparison is done now with zip codes that have no homicides in the sample. This model allows for full heterogeneity in treatment effects α_{zm} by zip code and month. The results are in Figure E.2 (with the outcome being the number of listings) and Figure E.3 (with the outcome being the log price of the median listing). For both figures panels (a) represent the effect of the covered homicide, and (b) that of a non-covered homicide.

Figure E.1: Dynamic difference-in-difference results

(a) Effect of Covered Homicide on Number of Listings

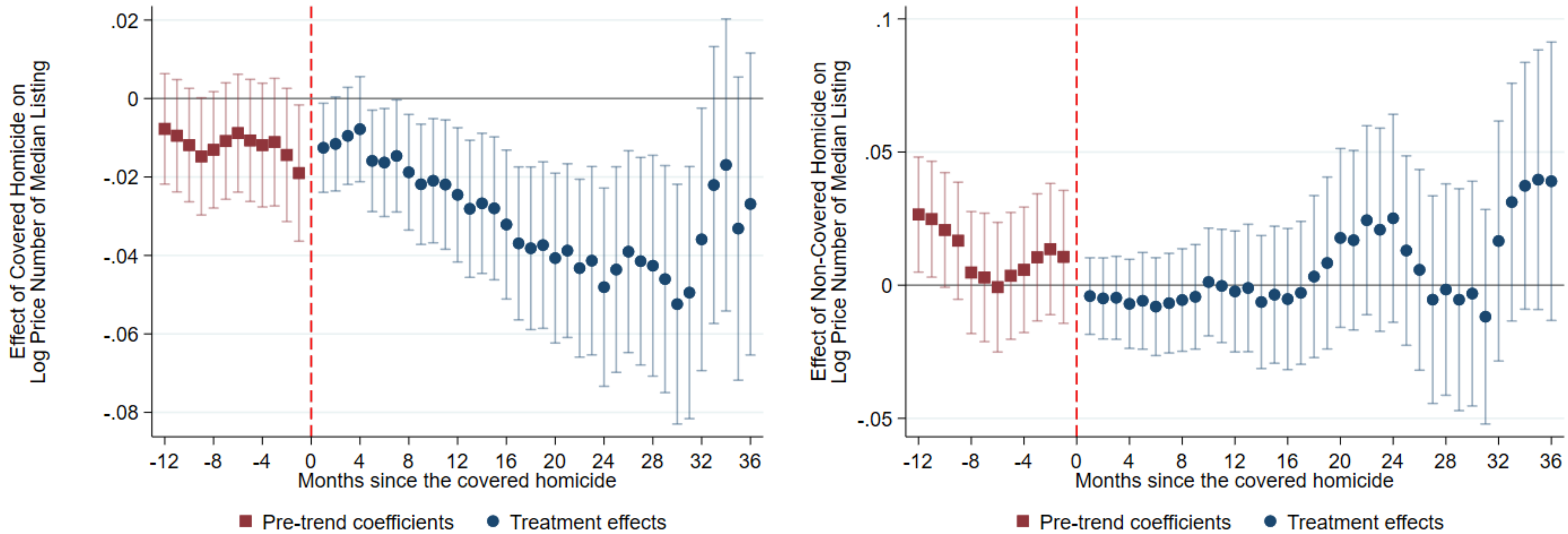


(b) Effect of Covered Homicide on Listed Prices



This Figure presents the dynamic difference-in-difference estimate from Equation E.1 of the effect of coverage on: (a) the log of the median number of listings in a month; and (b) the log of the median listed price per sq ft in a month. It compares ZIP codes that only have one homicide that gets covered, with those ZIP codes that have only one homicide that does not get covered. The bar represents the 90% confidence interval based on standard errors clustered at the ZIP code level.

Figure E.2: DiD Estimates of Effects of Coverage and Non-coverage of Homicides on Number of Listings By Zip code-Month

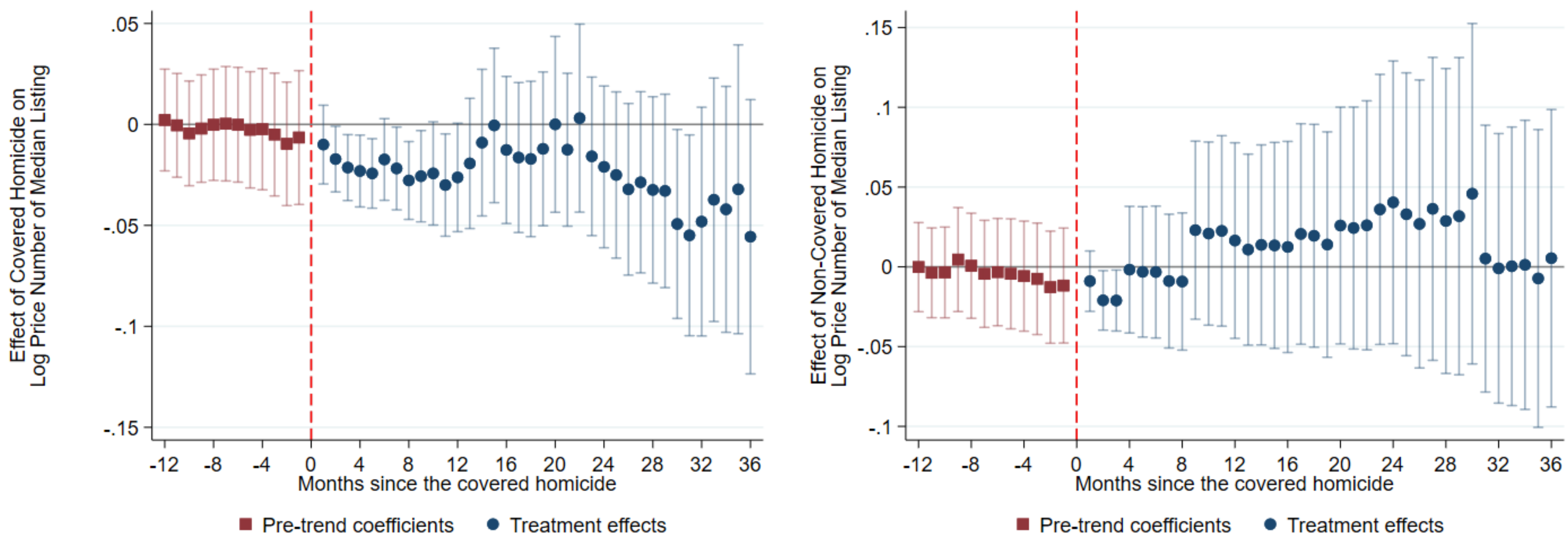


(a) Effect of Covered Homicide

(b) Effect of Non-Covered Homicide

Note: The above figure presents the imputation estimate proposed by Borusyak *et al.* (2021). It shows the effect on the median number of posted listings at the zip code by month of two different treatments. Subfigure (a) shows the effect of having a covered homicide; Subfigure (b) shows the effect of having a non-covered homicide.

Figure E.3: DiD Estimates of Effects of Coverage and Non-coverage of Homicides on Median Listed Price Per Sqft By Zip code-Month



(a) Effect of Covered Homicide

(b) Effect of Non-Covered Homicide

Note: The above figure presents the imputation estimate proposed by Borusyak *et al.* (2021). It shows the effect on the median listed price per square foot at the zip code by month of two different treatments. Subfigure (a) shows the effect of having a covered homicide; Subfigure (b) shows the effect of having a non-covered homicide.

Once more, the results show a similar pattern. First, notice that for all outcomes, there is no pre-trend that can be detected statistically up to 8 months before the homicide takes place both for zips with and without coverage. Furthermore, the effect of non-covered homicides is not different from zero either for prices or the number of listings, except for months 2 and 3 after in the median listed price per sqft, which shows a very modest reduction of about 2%. The effects of covered homicides on the other hand are much more consistent: there seems to be a reduction in the median listed price of about 2 percent starting the month after the homicide and continues for about a year after it takes place. Although noisy, the point estimates suggest a persistent decline in the median listed price of up to 5% 30 months after the homicide. The effect on the number of listings is even more striking: there is no effect for the first 4 months after the homicide, but starting on the 5th month, there is a decline of about 2% in the number of listings, which gradually increases up to a decrease of 5% two years after the homicide takes place.

F Model of preference externalities

Consumers type: Under the framework of Anderson & Waldfogel (2015), consider two types of consumers (or potential viewers): “white" $[w]$, “non-white" $[n]$. Each individual consumer type has a preference for bias in the coverage they would like to see reflected in the news x_i^w. There are three main reasons why consumers might have preferences for some bias in the crime news they consume. First, individuals might get a higher return from a private action with more information. This encompasses cases such as the decision of what house to buy, how much to pay for it, what commute to take to work, etc. In principle, if the news organization has no limit on the information that it can communicate, under the private action motive a consumer would prefer unbiased crime news. Under limited space for news communication, however, a rational consumer might prefer some degree of bias in the news consumed by others if that means that the news they care about is always reflected accurately.[18] Second, coverage of crime news might have an entertainment value, if consumers have preferences for salaciousness, for example. Lastly, consumers might get ‘psychological utility’ from biased crime news if they would like to see their priors validated in the news cycle. This might occur if consumers have a stereotype about a group of individuals, and they would get utility from seeing their beliefs

[18] Imagine there are two homicides, one that is more relevant to a white consumer (with WV and WS) and another more relevant to a non-white consumer (with NWV and NWS). If the TV station can only report on one of the two homicides because of limited space, each consumer would prefer some degree of bias in the news presented to the other group (e.i., that the TV station does not present that story and instead presents the one they are interested in).

reflected in the media. All three sources of value from the news is compatible with preferences for some sort of bias in consumer crime coverage.

Consider, in this setting, a one-dimensional type of bias. Both groups have different uniform distributions of preferences for bias, so $x_i^w \sim U[0,1]$ and $x_i^n \sim U[z, 1+z]$ with densities $f_n < f_w$, where $z \in [0,1]$, so the overlap in distributions is $1-z$ (see Figure F.1)[19]. In a Hotelling-style model, each individual consumer decides to tune into one station or another, depending on the distance of the bias selected by the TV station to their preferred bias. This is captured as a probability of watching the news $d(t|x_i - x_s|)$, with $d(\cdot)$ being a decreasing function. t represents the transportation cost of not consuming their ideal bias.

Advertisers: Most local TV stations are free-to-air, and thus, do not charge subscription prices to access their content. TV stations' revenue comes from what advertisers pay to reach the audience tuning in. Assume a continuum of advertisers of mass $A = 1$ in each market. All advertisers have a common willingness to pay (WTP) to reach consumers from each type but differ in the WTP to reach consumers across types. In particular, I assume that the WTP to reach consumers of type 'white' (ϕ_w) is larger than to reach the 'non-white' type (ϕ_n), based on average higher purchasing power by type w. Given these considerations, each advertiser selects in which TV station to advertise.

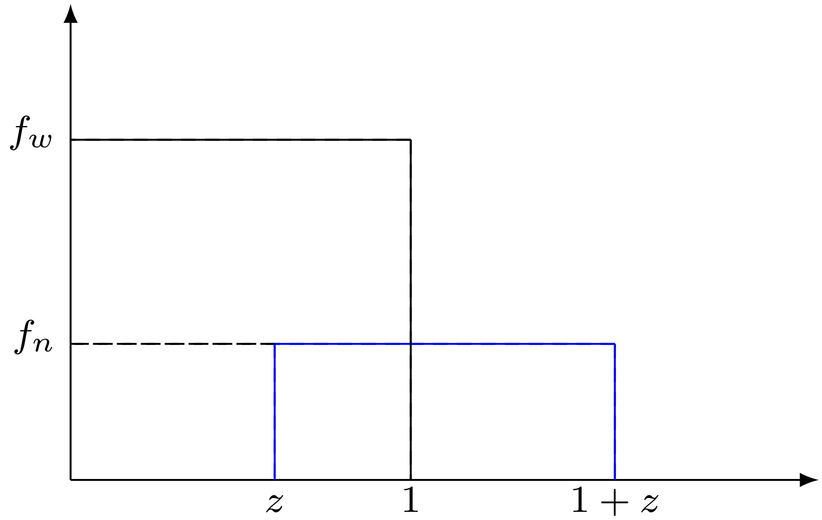
Station: TV stations receive revenue based on the share of individuals of each type that tunes into their signal, weighted by the WTP that each advertiser pays to reach that type. I initially consider the case of a representative TV station in the market with a profit function, so the problem the station solves is defined as:

$$\max_{x_s} \Pi^M = \max_{x_s} \left[\phi_w \int_0^1 d(t|x_i^w - x_s|) dx_i^w + \phi_n \int_z^{1+z} d(t|x_i^n - x_s|) dx_i^n \right] \tag{F.1}$$

Equilibrium under a representative news provider: The equilibrium of this simple model is given by the representative news outlet catering more towards the white-type, as they are larger in size and the advertisers pay more to reach them. Consider the case where $\phi_w = \phi_n$ and $f_w = f_n$, then both groups are identical and the firm would choose their bias to be $x_s^* = (1+z)/2$. On the other hand, if $\phi_n = 0$, then the firm does not consider the tastes of the non-white type consumers at all, and $x_s^* = 1/2$, catering perfectly to the median consumer of the white-type. It can be shown that $\frac{\partial x_s^*}{\partial f_n} > 0$, so the larger the size

[19]To be precise, f_n and f_w do not have to be densities, as they do not need to integrate to 1. They can be thought of as scaled densities by population shares.

Figure F.1: Distribution of ideal bias for white (f_w) and non-white (f_n) types



of n, the more content caters towards their preference for bias. Similarly, $\frac{\partial x_s^*}{\partial \phi_n} > 0$, and thus larger purchasing power of n implies that the observed bias will be more reflective of their preferences. The simple comparative statics of an increase in the population of n is depicted in Figure F.2. In it, the news outlet selects bias x_s^M when the population of non-white types is f_n, and it shifts to the right $x_s^{M'}$ when the population increases to f_n'.

Figure F.2: As share non-white grows, TV station caters more to median non-white