When Reality TV Creates Reality: How "Copaganda" Affects Police, Communities, and Viewers[*]

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Abstract

Television shows with police officer protagonists are ubiquitous on American television. Both fictional shows and reality shows portray a world where criminals are nearly always apprehended. However, this is a distortion of reality, as crimes mostly go unsolved and police officers infrequently make arrests. What does the omnipresence of this genre mean for the general public's conception of police, for the practice of policing, and for the communities being policed? I use department-level and officer-level arrest data to find that arrests for low-level, victimless crimes increase by 20 percent while departments film with reality television shows, concentrated in the officers actively followed by cameras. These arrests do not meaningfully improve public safety and come at the cost of the local public's confidence. I then document quasi-experimentally and experimentally that these shows – particularly their overrepresentation of arrests – improve non-constituent viewer attitudes towards and beliefs about the police. The results are consistent with "copaganda" shows inflating trust in police nationally while subjecting some to harsher but not more effective enforcement. I consider the implications for police reform.

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We've all grown up on television shows in which the police are superheroes.
They solve every problem; they catch the bad guys;
they chase the bank robbers; they find the serial killers.
But this is all a big myth. This is not what police actually do.
- Alex Vitale, Policing and Social Justice Project[1]

I Introduction

Public discussions about policing practices have been frequent and heated following the protests against police killings of Michael Brown, George Floyd, Breonna Taylor, and many others.[2] Police and the unique power they wield to incapacitate their fellow civilians are under the microscope. Confidence in the police is at its lowest point in thirty years (Gallup 2023). Media portrayals of police have similarly come under fire, leading to renewed calls to cancel some police procedurals and reality shows that partner with police departments (Sukhan 2021).

Televised stories told from the perspective of the police and often with the explicit cooperation of the police – known as "copaganda" – have been ubiquitous in the United States for decades and popular among both police and viewers. Reality television shows that follow real police as they do their jobs are a particularly popular subgenre. Hundreds of police departments and sheriffs' offices have filmed with these shows, first on the show *COPS* beginning in 1989, and now on its popular spin-offs and successors. In a survey, I find that 90 percent of Americans have seen at least one of these reality shows and that they interpret them as accurate portrayals of policing. However, these shows have sent an inaccurate message: that police constantly solve crimes and make arrests. Since 2000, the police featured on *COPS* have made arrests in 89 percent of segments. In fact, crime and arrest data show that crimes are much more likely to go unsolved than not, and that most police officers infrequently make arrests.[3]

[1] Uetricht (2020)

[2] Police are responsible for over 1,000 civilian deaths per year – around 5 percent of all homicides.

[3] According to FBI UCR data, the share of reported crimes that are solved (the clearance rate) has been

Identifying the effects of such portrayals on viewers and on the institution of policing is a complicated empirical challenge. The genre was born in an era of law and order rhetoric, rising crime, and the ramp-up of the war on drugs in the late 1980s and early 1990s. Viewers also select into watching these shows and thus simply measuring their attitudes towards police may reflect a predisposition towards supporting police rather than the effect of the shows themselves. Disentangling to what extent copaganda is a symptom versus a cause of attitudes towards police and of policing practices is fundamentally difficult.

I focus on *Live PD*, the 21st century spin-off of *COPS*, because (1) its popularity has since surpassed *COPS*, (2) its 2016 premiere allows me to examine its causal effects in a variety of data sources, and (3) its almost-live format allows me to examine its real-time effects on the police officers it follows. *Live PD*'s viewership quickly outpaced that of its predecessor, averaging over 2 million viewers each week. It became the top unscripted crime show on TV and the top original cable program on Friday and Saturday nights. I use both quasi-experimental and experimental methods to identify the effects of the program on both sides of the cameras: on viewers, the police officers filmed, and the communities those officers serve.

I find that, rather than reflecting the reality of policing, these cameras create a new reality. First, I use data on *COPS'* segments over the last 30 years to establish the ways that the genre distorts the realities of policing, portraying police as far more efficient soldiers in the war against crime than they are in reality. Next, using arrest data and variation in the timing and location of *Live PD* filming, I find that police use their discretion to perform the role of the TV cop. Police engage in more proactive policing, in the form of stops and arrests for quality-of-life[4] crimes, while reality TV cameras are following them. I use a difference-in-differences strategy utilizing variation in the location and timing of *Live PD* camera presence and find that cameras significantly increase quality-of-life arrests by nearly 20 percent. The results for the affected jurisdictions suggest that many hundreds

well below 50 percent since at least 1970, hovering around 25 percent for the last fifty years. In data collected for this project and discussed in Section II, I show that a conservatively high estimate for the number of arrests made by a typical police officer is 12-14 per year.

[4] This term refers to arrests for victimless crimes that reflect the quality of life of the arrestee, including drug possession, curfew/loitering, disorderly conduct, drunkenness, liquor, and vagrancy.

of arrests occurred that otherwise would not have during each month of camera presence, most commonly for drug possession, totaling thousands of arrests overall. I use officer-level arrest data obtained through the Freedom of Information Act from several filming jurisdictions to find that the largest distortions come from officers being actively followed by cameras, but that these distortions spill over to their non-filmed colleagues as well.

Interpreting shifts in arrest behavior requires insight into the mechanisms behind such arrests. Arrests are not only affected by police enforcement decisions but also by criminal activity and constituent reporting of crimes. In reported crime data and in new datasets obtained through the Freedom of Information Act on stops and 911 calls, I rule out alternative explanations for the shift in arrests (namely, increases in criminal activity or reporting) and show that they can be attributed to shifts in police behavior caused by cameras alone.

I then examine a wide variety of outcomes, including crime data and opinion data, to better understand the implications of this copaganda-induced rise in enforcement for the affected communities. The impacts depend on whether any public safety benefits of such arrests outweigh the direct costs of the incapacitation and any negative impacts on arrested individuals' and the communities' outcomes.[5] I find that these marginal quality-of-life arrests do not improve policing quality, as measured by clearance rates[6] and use of force, nor do they reduce reported crime. Constituents' confidence in police falls, driven particularly by those who identify as ideologically moderate or liberal.

Finally, using a difference-in-differences/instrumental variables strategy to address the endogeneity of viewership and microdata on attitudes towards police, I find that confidence in the police rises in response to this distorted portrayal in high-viewership areas. In an accompanying survey experiment, I find that Americans have generally inflated expectations of police productivity, and that *Live PD*'s overrepresentation of arrests further inflates

[5] See, for example, Dobbie et al. (2018) on how a few days in jail can alter economic trajectories and Agan et al. (2023) on how misdemeanor prosecution can increase criminal offending. Ongoing work with Amanda Agan and David Autor in collaboration with a large background company investigates the very real consequences of having an arrest on your background check for employment even if that arrest never led to a conviction.

[6] The rate at which reported crimes are "cleared" through arrests or exceptional circumstances.

viewers' beliefs about police productivity.

This paper has broader implications for recent polarization in opinions about the police and the larger public conversation about police reform. The differential effects of the show on confidence in police for viewers versus constituents suggest that what people think they want from the police for an abstract, far-off community may not actually match up with what they would want in their own community. At a minimum, it suggests that how the police interpret viewers' desires does not result in effective or confidence-inspiring policing for their constituents.

These results contribute to a new and growing literature in economics suggesting that these marginal arrests for quality-of-life crimes may not meaningfully improve public safety and may be quite costly. Cho et al. (2023) examine marginal arrest pullbacks after officer fatalities and find no meaningful public safety effects. Chalfin et al. (2022) use funding increases and hiring to examine shifts in arrest types and find that quality-of-life arrests raise racial gaps in policing and their public safety payoff is unclear. Other work examines shifts in stop behavior through changes in command staff (Bacher-Hicks and de la Campa 2020), court-driven reforms (Tebes and Fagan 2022), and DOJ investigations (Campbell 2023). This paper uses a quasi-experimental shift that more directly affects officer behavior and salience to the public without shifting other aspects of policy or funding.

This paper also contributes to the literature on how media can affect viewers' and key decision-makers' beliefs, attitudes, and even actions. This paper is the first to examine the effects of television about public servants, as well as the implications of reality television on both sides of the camera: for those filmed and those watching. Past studies, such as Philippe and Ouss (2018), find that news coverage of crime can affect judicial decision-making. Ang (2023) finds that exposure to racist propaganda affects racial violence and hate crimes for over one hundred years. Bursztyn et al. (2023) show how infotainment television programming affected COVID-19 outcomes. Ash and Galletta (2023) demonstrate how cable news affects electoral outcomes and government budgets. Gentzkow (2006) and DellaVigna and Kaplan (2007) show how television and cable news affect voting, respectively. La Ferrara et al. (2012) examine the effect of Brazilian soap operas on fertility, and Kearney and Levine (2015) examine the effects of U.S.-based reality television show *16 and Pregnant* on teen births. Gentzkow and Shapiro (2008) and Kearney and Levine (2019) examine the effects of television on educational outcomes.

This work also demonstrates a new way in which psychological and non-pecuniary factors can influence employee (and specifically police) performance. Mas (2006) shows that wage fairness norms play a role in policing effort. The labor economics literature has theorized and shown that various forms of monitoring and oversight can reduce shirking and enhance productivity (Nagin et al. 2002; Pascual-Ezama et al. 2015). A recent literature has sprung up on the effect of oversight and transparency via body-worn cameras, with most rigorous studies finding insignificant impacts on arrest behavior (Yokum et al. 2019; Cubitt et al. 2017; Lum et al. 2020).[7] The creators of *Live PD* pitch the show as similar to body-worn cameras in its goals: "We felt like we could offer some kind of transparency: What is actually happening on patrol on a given night?" (Frederick 2018). However, this paper demonstrates that who is doing the monitoring and for what purpose affects how employees respond.[8]

The remainder of this article proceeds as follows. Section II describes the copaganda genre and its distorted portrayals of police. Section III explores the effects of copaganda cameras on arrests. Section IV explores the implications of such shifts for the local community. Section V explores the effects of copaganda on viewers' attitudes and beliefs about police. Section VI concludes.

[7] One exception is Kim (2019), which finds reductions in civilian fatalities.

[8] I test this directly in Appendix A for the subset of departments that reported their 2016 body-worn camera adoption status to the Bureau of Justice Statistics.

II Copaganda and its Distortions

A. *COPS*

Live PD follows in the now-long tradition of the reality TV copaganda genre, born in 1989 with *COPS*. These shows have become known to many as "copaganda" due to the cooperation between producers and police agencies in the form of filming contracts, the agencies' ultimate veto power over what airs, and the stories being told entirely from the perspective of the police.[9]

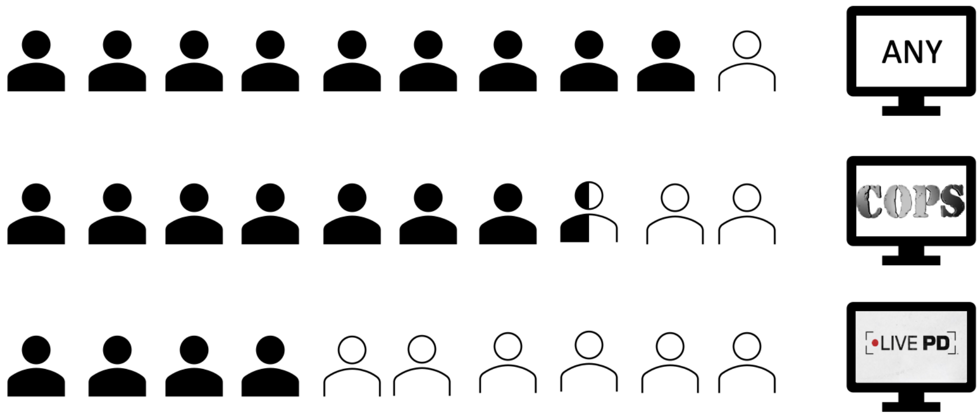
COPS invented the genre of reality TV during the 1988 Writers Guild of America strike, realizing that they could film actual cops to create entertaining footage without the need to pay for writers, actors, or sets. The show was briefly canceled after George Floyd's death at the hands of police in May 2020, but has since returned. *COPS*' 35th season began in 2023 and it retains its mighty reputation as the longest running crime/legal show on American television. *COPS* is most commonly consumed as re-runs rather than live, as it has been in syndication for nearly 30 years. *Live PD* surpassed it in live viewership numbers when it premiered in 2016, but *COPS* remains the show people have been most exposed to cumulatively (Horton 2022).

COPS, *Live PD*, and shows modeled after them are ubiquitous in the United States. As shown in Figure 1, I conducted a nationally representative survey of nearly 500 US-based adults on Prolific and found that 90% have seen *COPS*, *Live PD*, or a related reality TV show about police. This probability is consistent regardless of age, educational attainment, race, ethnicity, or gender. *COPS* is such a universal cultural touchstone that it has been parodied endlessly on shows like *The Simpsons*, and even in media meant for children, such as *Shrek 2*.

The show has been criticized over the years for its invasion of privacy, its portrayals of

[9]These shows are part of a larger ecosystem of copaganda coming directly from police departments through public relations teams (Karakatsanis 2022) and from Hollywood in the form of fictional police procedural television and movies (Rosenberg 2016).

Figure 1: Share of U.S. Adults Who Have Ever Seen Cop-Focused Reality TV



Notes. This figure depicts the share of 465 surveyed U.S. adults who have ever seen (1) any reality TV about police, (2) *COPS*, or (3) *Live PD*. The survey was conducted on Prolific in 2023.

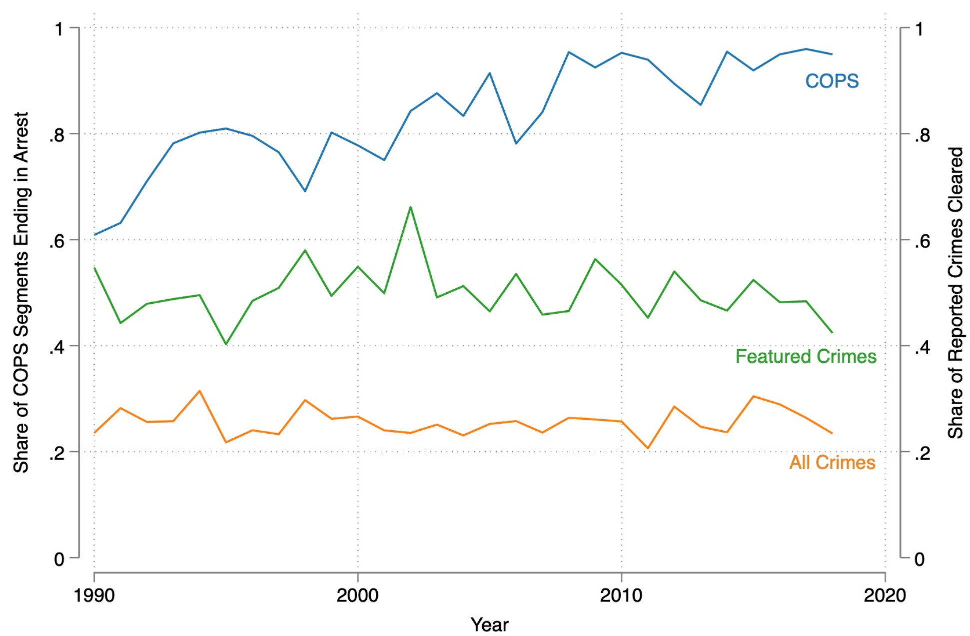
race, and its over-emphasis on violent crime, all of which its creators have responded to in some form. Creators have always claimed that anyone featured on the show signed a consent form.[10] The officers featured on the show have become more diverse and the suspects featured on the show have become more representative as well. Similarly, the show's most recent seasons portray violent crimes in similar proportion to crime data.

However, the show provides an increasingly inaccurate portrayal of arrest behavior. *COPS* portrays a world of impossibly effective crime fighters. Each episode features 3 segments covering an incident in one jurisdictions. Figure 2 shows that in the first five seasons of the show, cops made arrests in 70 percent of segments, which was already high relative to actual clearance rates. By the 30th season, that share was 95 percent. In reality, police officers make far fewer arrests on a regular basis than these shows depict, and the probability that a crime is solved has not risen. As shown in Figure 2, the clearance rate for crimes in the featured jurisdictions was, in reality, only 23 percent for all crimes and only 43 percent for the distribution of crime types shown on the show.[11]

[10]Though it is often litigated and discussed to what extent individuals in handcuffs and/or under the influence of drugs or alcohol who sign a consent form truly do so with full information and free of coercion.

[11]For example, property crimes are common, solved at low rates, and are rarely shown on *COPS*.

Figure 2: Clearance Rate on *COPS* versus Reality



Notes. The blue line depicts the share of segments ending in arrest by season of *COPS* based on data hand-coded by the *Running from COPS* reporting team. The orange line depicts the actual clearance rate for all crimes reported to police in each season's filming jurisdictions based on FBI UCR data in those same years. The green line re-weights the clearance rate by the crime types shown on each season. The clearance rate is calculated as the number of reported crimes cleared through arrests or exceptional means (where they have identified the perpetrator but have not made an arrest) divided by the number of reported crimes.

Data on officer-level arrests can also provide a window into how often one could expect an officer to make an arrest. In the officer-level data collected for this project across 4 jurisdictions, as shown in Appendix Figure D1, the median number of annual arrests for an active sworn officer is 12.[12] To extend to a larger sample that includes more large cities, one can take all arrests made in the FBI UCR database and divide by every sworn law enforcement officer in the database, though this is inevitably skewed upward by high-

[12]Because I lack HR data on officers, I conservatively define their active periods as between any two months in which they made at least one arrest, which means I inevitably undercount zeroes.

arrest cities. This calculation yields 14 arrests on average per officer in the most recent pre-pandemic year.[13] Yet another way to bound the exercise with a high number would be to examine the distribution of arrests by patrol officers in New York City during the era of stop-and-frisk policing around the turn of the century. A NYPD lieutenant conducted a survey in 2002 of active patrol officers and found that most made 0 or 1 arrest in their most recent full-time month of patrol (Linn 2008). Regardless of the source, data strongly suggest that arrests are not nearly as common as portrayed on reality cop shows.

This distorted portrayal of real policing is facilitated by heavy editing. New York Times coverage suggests a ratio of 50 hours of filming to one hour of content (O'Connor 1989). Raw footage unearthed in lawsuits also provides insight into the cutting room floor; for example, an hour-and-thirty-seven minute-long interaction was condensed into seven minutes on air (Taberski 2019). That 95 percent of segments now end in arrest suggests producers jettison footage that does not contain arrests, giving the inaccurate impression that police nearly always make the arrest.

B. *COPS*, but Live

In the modern era of declining confidence in police, *Live PD*'s main innovation on the *COPS* model was to provide even more verisimilitude with the "appearance of" no editing, providing a more direct counter to body-worn camera footage circulating the news and social media. Police departments are filmed by TV crews and the footage is aired almost immediately.[14] The show began in 2016 and was the top unscripted crime show on TV and the top original cable program on Friday and Saturday nights. Its six hours of content each week were watched by an average of 2.4 million people (Horton 2022). Like *COPS*, it was pulled from the air temporarily following George Floyd's death in mid-2020. *Live PD* viewers boycotted the channel, A&E, causing channel ratings to drop by half (Flint

[13] FBI UCR reported 10,085,207 arrests and 697,195 sworn officers in 2019. The number of total arrests was much smaller in 2020 and 2021 in part due to the pandemic.

[14] Usually with anywhere from a 10-minute to several day delay to allow departments up to 48 hours to review footage before it's aired, though some cases have been aired weeks later.

2020; Pagones 2020). In 2022, the channel Reelz brought the show back under the name *On Patrol: Live* (Starr 2022).

Before its hiatus, *Live PD* cameras rode along with up to 20 police departments per season, airing footage "almost live" from incidents filmed earlier that day or earlier that week beginning in October 2016. The show's hosts and guest police commentators sit surrounded in the studio by many screens with live footage. They show clips of the most interesting footage and then provide narration and feedback in the style of sports commentary. Appendix Figure D2 provides a picture of the studio.

The list of participating jurisdictions changed each season, with 48 total police departments participating before its hiatus, covering a wide variety of geographies, department types (sheriffs, police, and state patrols), and department sizes. Appendix Figure D3 provides a map of participating jurisdictions and Appendix Table E1 contains additional information about filming departments. The contracts negotiated between Big Fish Entertainment – *Live PD*'s parent company – and police departments enumerate the non-pecuniary benefits to departments of participating: "'the appearance of' no editing, and the feeling as if content is coming straight from the street to living rooms across America"; capturing "the 'real-time' perspective and diversity within the department and the city;" and an opportunity to "showcase the officers" in a "real-time communications and outreach effort."[15] The department agrees to give film crews access to a handful of key officers, who may be filmed simultaneously or at different times, on Friday and Saturday nights and for at least 1-2 additional shifts each week. Officers are chosen by their department and must provide written permission to be featured. In exchange, *Live PD* gains access to department facilities and personnel, and ultimately owns all footage.

These contracts typically need to be approved by elected officials. In the case of a police department, a mayor or city council typically signs off. In the case of a sheriff's office, the sheriff must sign off. No money changes hands as part of these contracts; rather, they reflect a mutually beneficial public relations agreement. Departments have the opportunity to review all footage and to request anything not be aired due to any perceived "safety or

[15] See, for example, the second contract between Tulsa Police and Big Fish Entertainment (Dehnart 2020).

security risk." Freedom of Information Act requests in Appendix B reveal many instances of police asking producers not to air footage for an array of other reasons, such as racist language, violations department policy, or concerns about the optics of too many clips featuring non-white "suspects." Producers did not air these segments.

Given the TV cop archetype established by *COPS* in which an arrest is nearly always made, there is an incentive for officers who want to be featured on the show to initiate contact with constituents that they may not have otherwise, or to escalate a contact into an arrest. As one of the officers featured on the first season said, "because you've got 'an audience' there, kind of the pressure's on you to try to produce something because you want the fans (you know the people who are watching the show), you want them to have something exciting to see" (FOP 2017). Officers are certainly aware of the archetype of the reality TV cop and the distance between that expectation and the realities of their actual job.

Like on *COPS*, *Live PD* clips typically feature arrests, though the scope for editing is more limited with it's "almost live" nature. In a random sample of 50 clips from *Live PD*'s four seasons, 64 percent ended in arrests.

III Effects on Policing

What happens when the cameras show up to film actual police officers and the producers and viewers expect to watch them make arrests? On a show like *COPS*, selective editing plays a large role in maintaining the distorted view of police effectiveness. But when the show is "live," how do producers, police chiefs, sheriffs, and officers meet the expectation that they make arrests? Given the inaccurate portrayal of reality shown on these television programs, I now turn to how these distorted expectations affect police themselves. I examine effects on arrest behavior at the department level and at the officer level to better understand the dynamics of how cameras affect policing.

A. Data & Methods

I employ a similar empirical strategy in multiple datasets to identify the causal effect of cameras on arrests.[16] First, I employ a difference-in-differences identification strategy utilizing variation in the location and timing of treatment (*Live PD* filming) across police departments nationally.

To avoid the interpretation pitfalls of two-way fixed effects (TWFE) models with potential dynamic treatment effects and staggered adoptions, I have built a stacked department-level dataset to ensure that each *Live PD* cohort (i.e. Season 1 only, Seasons 1-2, Seasons 2-3, etc.) is compared only to never-featured and much-later-featured departments in case of dynamic treatment effects, following Deshpande and Li (2019) and Cengiz et al. (2019). No already-treated units are ever used in the comparison group. All specifications include fixed effects for each dataset within the stacked dataset and include dataset-by-department clustered standard errors to ensure no departments are double-counted in the estimates or standard errors. In addition, I compare my results to several alternate TWFE/DiD estimators in Appendix Figure D4, with very similar results.

At the national level, I use FBI Uniform Crime Reporting (UCR) detailed arrest data at the department-by-month level to investigate whether departments that are filmed by *Live PD* see a change in arrests and/or arrest composition during filming that similar non-filming departments do not (Kaplan 2021). These data include 3,549 UCR-reporting departments that consistently report monthly detailed arrest information between 2014 and 2020 to ensure that all included departments have at least two years of pre-*Live PD* data.[17] Departments have been excluded if they have known errors, do not report sub-types of arrests, or report data intermittently.

Quality-of-life arrests include arrests for drug possession, curfew/loitering, disorderly conduct, drunkenness, liquor, and vagrancy.[18] I examine the natural logarithm of arrests to

[16] See Appendix Table E2 for detailed descriptions of the data.

[17] See Appendix Table E1 on the appearance of *Live PD* filming departments in the data and Appendix Table E2 for information on the data sources.

[18] This definition is based on previous literature, including Premkumar (2021), Cho et al. (2023), and

account for underlying differences in numbers of arrests across departments and to focus on percentage changes in arrests.[19] Because these quality-of-life arrests are common and left to the discretion of individual officers, the model in Appendix C predicts that these are the types of arrests that will respond to the incentive to make an arrest on camera. However, I also examine effects on other types of arrests.

Following past literature, I fit a local polynomial function for total arrests and quality-of-life arrests for each department and mark values outside the 99.9% confidence interval as outliers.[20] I consider specifications where these outliers are set to missing and specifications where these outliers are imputed using the local polynomial function's predicted value to show that my results hold regardless of the treatment of outliers.

The event study specification to examine pre-trends and potential for dynamic treatment effects in balanced panel between the quarters before camera arrival and the quarters during filming is:

$$ln(A^c_{dt}) = \lambda_{-1} + \lambda_t EverLivePD_d * [\sum_{t=-10}^{-2} lags_t + \sum_{t=0}^{2} leads_t] + \varepsilon \tag{1}$$

The dummy for the quarter before camera arrival is omitted. Thus, each lag tests the difference between each pre-period quarters's treatment effect relative to t=-1 and each lead tests the difference between each treatment period quarter's treatment effect relative to t=-1.

The basic difference-in-differences specification, which averages together the pre-treatment periods and treatment periods into a single dummy variable, *DuringFilming*, is:

$$ln(A^c_{dt}) = \gamma_0 + \gamma_1 EverLivePD * DuringFilming_{dt} + \gamma_2 EverLivePD_d + \gamma_3 DuringFilming_t + \varepsilon \tag{2}$$

Chalfin et al. (2022).

[19] I add 1 to each month's arrest total to avoid the natural logarithm being undefined in the rare case of a zero.

[20] I base this on Evans and Owens (2007), Mello (2019), and Premkumar (2021).

The preferred specification that includes two-way fixed effects to soak up additional variation is:

$$ln(A^c_{dt}) = \beta_0 + \beta_1 EverLivePD * DuringFilming_{dt} + \mu_d + \lambda_t + \varepsilon_{dt} \tag{3}$$

Where A^c_{dt} represents arrests of crime type c in department d in month t, $EverLivePD * DuringFilming_{dt}$ represents time periods during and after active filming in a department, μ_d are department fixed effects (which absorb *EverLivePD*), and λ_t are month-by-year fixed effects (which absorb *DuringFilming*). The coefficient β_1 is the primary coefficient of interest.

Before proceeding, I examine the identifying assumptions for the difference-in-differences method. The critical assumption is parallel trends: that the allocation of the intervention is unrelated to the pre-treatment outcome trend and that the post-treatment shift in trends can be attributed to the treatment. In the case of *Live PD* selection, it is likely that the decision of which departments to feature on a given season may be driven by overall arrest levels, though likely not by the composition of arrest types, and almost certainly not by anticipated changes in certain types of arrests. The same likely goes for the selection of featured officers within a department. This assumption is bolstered by parallel pre-trends shown later in Figure 3.

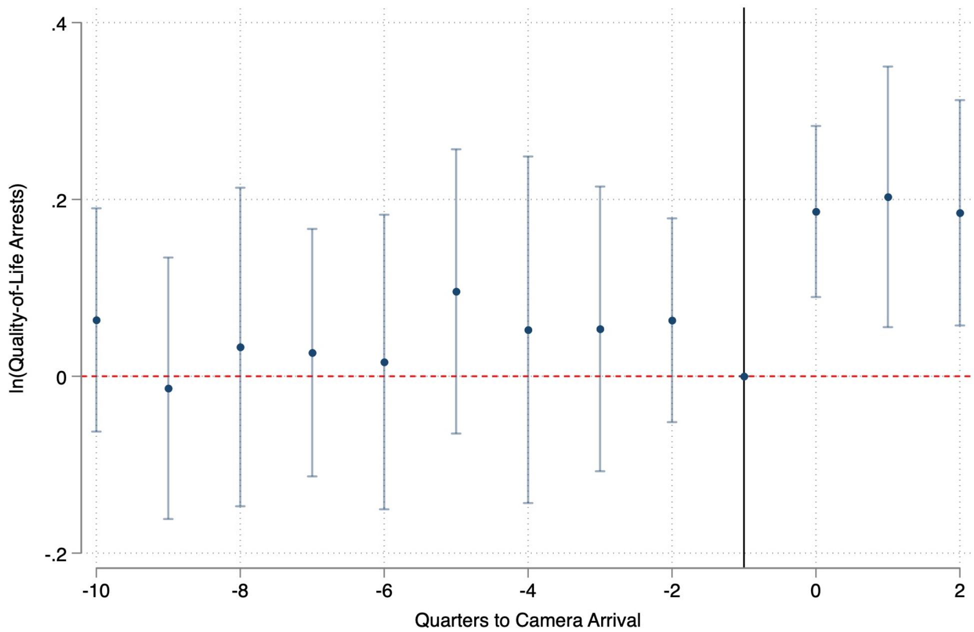
One potential threat to difference-in-differences validity is any anticipatory changes in arrest behavior before filming begins. For example, *Live PD* contracts are often finalized months before the arrival of cameras, and the officers (particularly those who want to be featured) may learn about the potential contract early in the negotiation process. If department leadership selects featured officers in part based on recent arrest history, officers may alter their behavior to maximize the chance that they will be featured. Some of this anticipatory ramp-up in arrests is visible at the officer level shown in Section III-C, which biases me against finding a positive effect, but is not present at the department level.

Stable Unit Treatment Value Assumption (SUTVA) requires that the response of each treated unit depends only on its treatment assignment, not the assignments of other units. This seems likely to hold, because it is extremely rare for bordering or overlapping departments to be featured on the show, which could result in spillovers between treated units. There may be spillovers into bordering or overlapping control (non-*Live PD*) jurisdictions, which would, if anything, bias against finding an effect of the treatment.

B. National Department-Level Analysis

The results of the main specifications for quality-of-life arrests and all other arrests suggest that police respond to *Live PD* cameras by making more arrests, particularly those involving low effort and high discretion, as predicted in the model in Appendix C.

Figure 3: Department-Level *Live PD* Camera Arrival Event Study



Notes. This figure depicts an event study specification for a balanced panel between 10 quarters before and 2 quarters after camera arrival. Each coefficient tests the difference between each quarter's treatment effect on the log of quality-of-life arrests relative to t=-1 (months -3 to -1). Standard errors clustered at dataset-by-department level.

First, the event study chart in Figure 3 suggests that *Live PD* camera presence led to consistently higher rates of quality-of-life arrests during a season on the show. There are no visually noticeable or statistically significant pre-trends, which bolsters the use of the difference-in-differences method for this outcome. An event study figure for agencies that filmed for more than one season, which allows for a longer balanced panel, can be found in Appendix Figure D5.

The preferred two-way fixed effect specification for the main outcome in column 2 of Table 1 suggests that *Live PD* filming raises quality-of-life arrests by around 19 percent during filming, after accounting for underlying differences in these kinds of arrests across different departments and over time. Without adding in these fixed effects, standard errors

Table 1: National, Department-by-Month Level Effect of Live PD on Arrests

	Quality-of-Life Arrests		Drug Possession Arrests		Other Arrests	
	(1)	(2)	(3)	(4)	(5)	(6)
	DiD	TWFE	DiD	TWFE	DiD	TWFE
EverLivePD* DuringFilming	0.212*	0.188***	0.211	0.180**	0.104	0.060*
	(0.120)	(0.067)	(0.142)	(0.075)	(0.077)	(0.031)
Observations	1,389,936	1,389,936	1,389,936	1,389,936	1,389,936	1,389,936
R^2	0.002	0.821	0.001	0.779	0.003	0.915
Dataset FE	✓		✓		✓	
Dataset-by-Dept FE		✓		✓		✓
Dataset-by-Time FE		✓		✓		✓

Notes. This table shows the main difference-in-difference and TWFE specifications for 3 categories of arrests (quality-of-life arrests, drug possession arrests, and other arrests). Drug possession arrests are a subset of quality-of-life arrests. All outcomes are measured in logarithms and can be interpreted as percent changes. Odd numbered columns show the γ_1 treat*post coefficient from the difference-in-difference equation 2. Even numbered columns show the β_1 treat*post coefficient from the two-way fixed effect equation 3. Standard errors clustered at dataset-by-department level. *** p<0.01, ** p<0.05, * p<0.1.

are large, though the point estimate magnitudes are similar. Appendix Table E3 shows the results look similar using the version of arrest variables with outliers set to missing rather than imputed.

Drug possession arrests are the most common subcategory of these quality-of-life arrests. Column 4 of Table 1 shows that *Live PD* filming is associated with an 18 percent increase in drug possession arrests in the preferred specification. This is consistent with the data available from *COPS* showing a disproportionate emphasis on drug-related arrests.[21]

Notably, column 6 of Table 1 shows that these increases in quality-of-life arrests are not mirrored as much in other kinds of arrests, as predicted by the model in Appendix C. The preferred specification suggests an insignificant change in other types of arrests on the order of 6 percent. This suggests that officers substitute from not making arrests to making arrests, rather than from one type of arrest to another. Within other kinds of arrests, the

[21] For example, in Season 30, 38 percent of featured arrests were for drugs, while only 15 percent of arrests overall in those jurisdictions during that time were for drugs.

only significant changes are in lower-level arrests deemed "Part 2" by the FBI rather than in the more serious Part 1 index crime arrests, consistent with the hypothesis that officers are making more discretionary arrests.

Next, I examine whether additional controls and heterogeneity analyses can explain or alter these results. Column 1 of Appendix Table E4 includes controls for time-varying county characteristics like crime, drug-related deaths, and population do not meaningfully change the point estimate. Column 2 of Appendix Table E4 shows that departments featured on the show for more than one season have a larger treatment effect point estimate but it is not statistically significantly different from the single-season departments. Column 3 of Appendix Table E4 shows that the main national-level results seem largest among smaller sized departments, but, again, this is not statistically significantly different. These can be thought of as departments where treatment dosage was higher, as a greater share of their officers were exposed to cameras. In these smaller departments, an average of 17 percent of their officers were featured on the show, versus an average of 6 percent in larger departments. Appendix Table E5 shows that shifts in arrests were similar for arrestees of different races, in line with the genre's more careful treatment of race in recent years. Finally, as shown in Appendix Figure D4, I use alternative difference-in-difference and two-way fixed effects methodologies and find similar results.

The preceding analyses use all available variation in *Live PD* status within each department but do not include post-*Live PD* time after filming has ended. In Table 2, I include post-*Live PD* data and examine directly the effects during the period up to two years after cameras have left. The effect sizes after cameras have left are approximately halved. However, the large standard errors make it impossible to distinguish these effects from either zero or the main (during filming) effect sizes. This provides suggestive evidence that the shift is a lasting one, and may reflect a shock to policing culture in addition to a short-term change in incentives.

Table 2: National, Department-by-Month Level Effect by Period

	(1) Quality-of-Life Arrests	(2) Drug Possession Arrests	(3) Other Arrests
EverLivePD*DuringFilming	0.181***	0.173**	0.060*
	(0.065)	(0.074)	(0.031)
EverLivePD*AfterFilming	0.096	0.068	0.068
	(0.080)	(0.094)	(0.048)
Observations	1,595,026	1,595,026	1,595,026
R^2	0.815	0.773	0.909
Dataset-by-Dept FE	✓	✓	✓
Dataset-by-Time FE	✓	✓	✓

Notes. This table shows the TWFE specifications for 3 categories of arrests (quality-of-life arrests, drug possession arrests, and other arrests), adding in post-filming data in binary variable EverLivePD*AfterLivePD. Drug possession arrests are a subset of quality-of-life arrests. All outcomes are measured in logarithms and can be interpreted as percent changes. Standard errors clustered at dataset-by-department level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

C. Within-Jurisdiction Officer-Level Analysis

I now employ a stacked difference-in-differences strategy at the officer level within several *Live PD* filming jurisdictions, utilizing different treatment assignments and timing at the officer level. It is critical to note that the comparison group (non-filmed officers in filmed jurisdictions) are still partially treated by the presence of the cameras, though less directly than the treatment group (filmed officers), and were part of the treatment group in the department-level analysis. This officer-level strategy helps us better understand where any distortions may be coming from within the departments and whether they are limited only to filmed officers. I sent Freedom of Information Act requests to 13 randomly chosen filming jurisdictions asking for officer-level arrest data in the years before, during, and after filming. I received usable arrest data covering 2,779 officers from Pasco County, FL; Tulsa, OK; St Tammany Parish, LA; and Walton County, FL. These represent the more than 11,000 officers in jurisdictions that filmed with *Live PD*.[22] The main officer-level specification looks essentially the same as at the national level, but at the officer rather than department level and with the number of arrests as the main outcome.[23] α_1 is the primary coefficient of interest:

$$A^c_{it} = \alpha_0 + \alpha_1 EverLivePD * DuringFilming_{it} + \mu_i + \lambda_t + \varepsilon_{it} \tag{4}$$

The Tulsa, OK Police Department filmed in seasons 1 and 4, taking a two-season break in between. The Walton County, FL Sheriff's Office filmed in season 1 only. The Pasco County, FL Sheriff's Office filmed in seasons 2 and 3. I include all months during which a department was featured on the show as treatment months in regression specifications, which biases downward the effect size if the cameras were not present throughout the entire season.[24] The main outcome variables are now counts of arrests or binary indicators of any arrest given the large number of zeroes at the officer level.

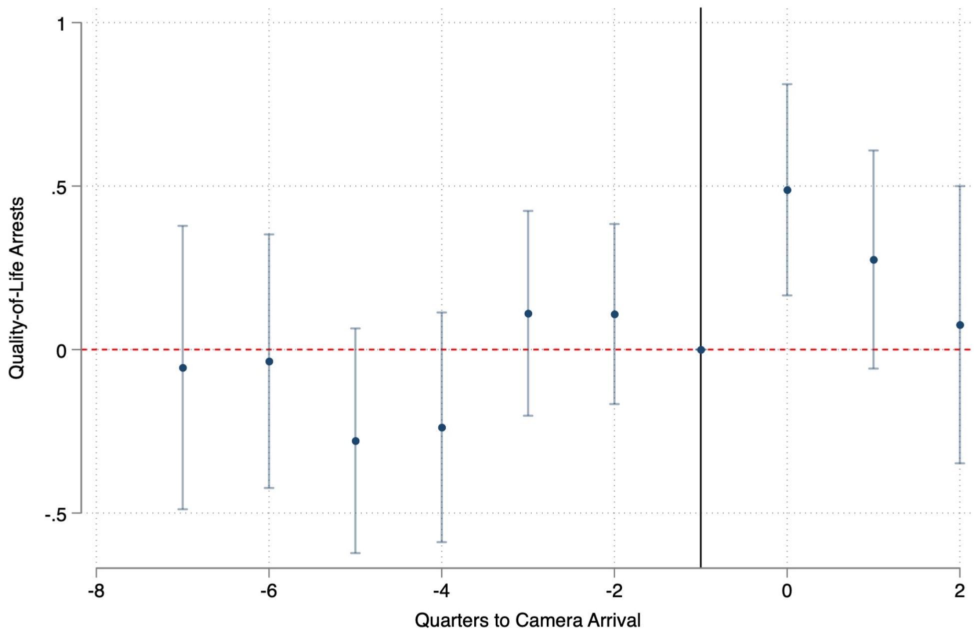
Figure 4 shows a balanced panel in event time starting at approximately 1.5 years before camera arrival through 6 months on the show, based on data availability and filming periods. The regressions that use all available time and geographic variation in the data can better determine whether the jump at the time of camera arrival is statistically meaningful. There is some suggestion of a noisy pre-trend visible, with a jump in quality-of-life arrests starting around 3 quarters before camera arrival in treated relative to untreated officers. Since post-camera quality-of-life arrests fall relatively for treated officers, this suggests some anticipatory ramp-up in arrests that is related to the cameras. This could mean either that the departments choose officers based on their pre-trends or that officers essentially auditioned to be featured on the show by ramping up these kinds of arrests. Since the

[22] Based on FBI UCR police employee data for UCR-reporting departments, which will undercount the total due to non-reporting. Over 124,000 officers were employed in jurisdictions that filmed with *COPS*.

[23] The most common number of monthly arrests for an active officer is zero, which would make the natural logarithm frequently undefined.

[24] For example, contracts and emails obtained through the Freedom of Information Act suggest cameras left Tulsa in January 2017, but they were still featured on the show using previously filmed footage through April 2017.

Figure 4: Officer-Level *Live PD* Camera Arrival Event Study



Notes. This officer-level event study includes the pre-*Live PD* period and the during *Live PD* period for a balanced panel in event time (leaving out Walton County, FL due to the limited 9 months of available pre-period data) from 7 quarters before camera arrival to 2 quarters after camera arrival (during filming). The outcome is the number of quality-of-life arrests. Each coefficient tests the difference between each quarter's treatment effect on quality-of-life arrests relative to t=-1. Standard errors are clustered at the dataset-by-officer level.

contract negotiation periods are fairly public and long, it is likely that officers are aware that the cameras are coming long before they arrive, supporting the latter explanation.

The overall officer-level regression results in Table 3, which use all variation in the data including up to 2 years pre-*Live PD* and to up 3 years after camera arrival, look similar to the national department-level analysis. Officers filmed by *Live PD* increase their quality-of-life arrests during *Live PD* filming months. The pre-period comparison group average is 0.3 arrests per month, so a 0.3 arrest increase amounts to a very meaningful shift in arrest behavior. It is likely that officers are substituting downtime or other duties for these

Table 3: Officer-by-Month Effect of *Live PD* Filming

	Quality-of-Life Arrests		Drug Possession Arrests		Other Arrests	
	(1)	(2)	(3)	(4)	(5)	(6)
	DiD	TWFE	DiD	TWFE	DiD	TWFE
Ever Live PD*During Filming	0.195	0.318**	0.186	0.257**	−0.770	0.321
	(0.134)	(0.132)	(0.118)	(0.119)	(0.469)	(0.364)
During Filming	0.029***		0.025***		0.040	
	(0.009)		(0.007)		(0.048)	
Ever Live PD	0.711***		0.505***		3.958***	
	(0.092)		(0.066)		(0.726)	
Observations	187,362	187,362	187,362	187,362	187,362	187,362
R^2	0.015	0.279	0.017	0.221	0.026	0.396
Dataset FE	✓		✓		✓	
Dataset-by-Officer FE		✓		✓		✓
Dataset-by-Time FE		✓		✓		✓

Notes. This table shows the main difference-in-difference and two-way fixed effects specifications for 3 outcomes (quality-of-life arrests, drug arrests, and other arrests) at the officer level. All outcomes are the number of arrests that fall into each category for each officer-month. Odd numbered columns show the treat*post coefficient for the traditional difference-in-difference equation. Even numbered columns show the treat*post coefficient for two-way fixed effect equation as in equation 4. Standard errors clustered at dataset-by-officer level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

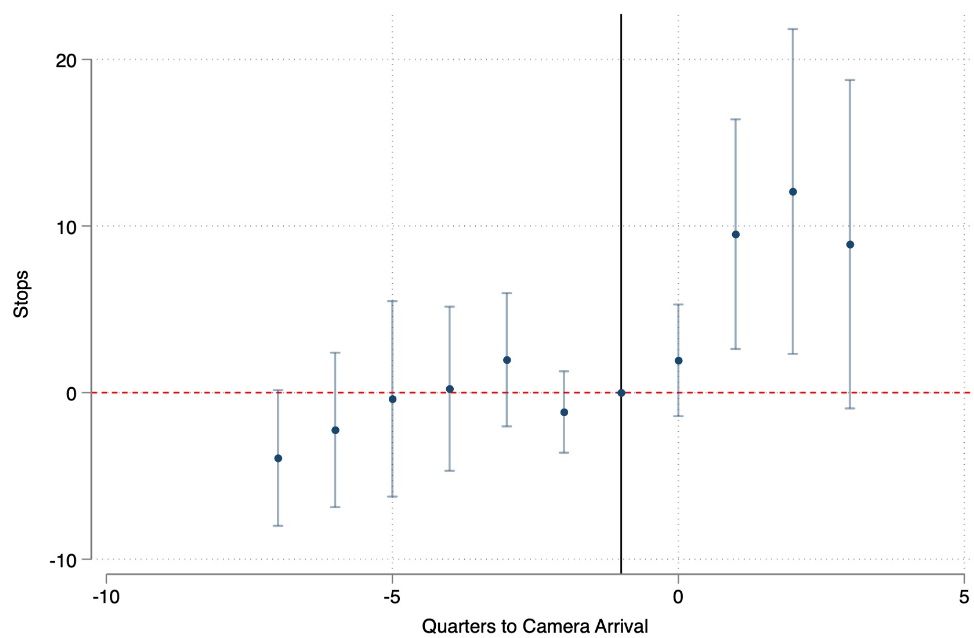
arrests or working overtime, rather than substituting one kind of arrest for another, since other arrest types do not change significantly.

This analysis also helps us better understand whether the changes in arrest behavior are confined to officers who are filmed or whether there are spillovers to the rest of the department. The *Live PD* effects are strongest for those being filmed. Chosen officers make more arrests than other officers at baseline, which is consistent with the department selecting officers they expect will likely make on-camera arrests. Even so, there is a clear added effect of *Live PD* camera presence on arrests. However, the difference-in-differences specifications in Table 3 show that non-filmed officers also see significant increases in quality-of-life arrests during filming of smaller magnitudes, suggesting spillovers in the tendency to make more discretionary arrests to the department at large. These effects on both filmed and non-filmed officers help explain the department-level increases.

D. Mechanisms Behind Arrest Shifts

Why are quality-of-life arrests rising? Is it only due to a shift in enforcement behavior and police proactivity, or are civilians' criminal behavior or reporting behavior shifting in response to cameras?

Figure 5: Officer-Level Stops Event Study



Notes. This officer-level event study shows the pre-Live PD period and the during *Live PD* period for a balanced panel in event time in Fort Bend and Williamson Counties in Texas. Each coefficient tests the difference between each quarter's treatment effect on the number of stops relative to t=-1. Standard errors are clustered at the dataset-by-officer level.

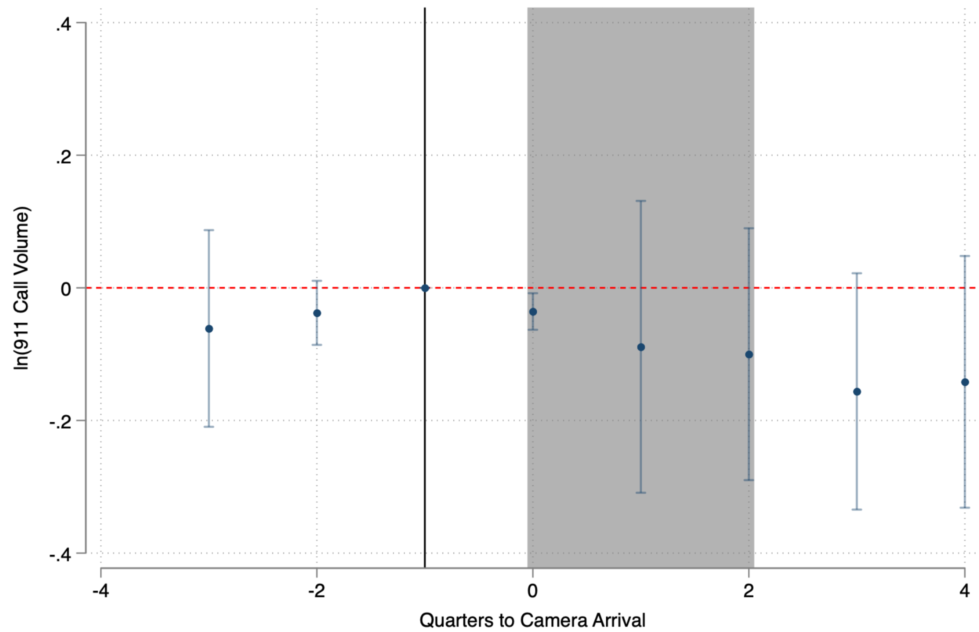
To assess whether we should think of these as shifts in proactive policing activity, I examine stop data provided by two additional filming departments through FOIA requests: Williamson County, TX and Fort Bend County, TX. Both jurisdictions were featured for a full season (10 months) on the show. Analysis shows that filmed officers significantly increased stops of pedestrians and vehicles during filming relative to non-filmed officers, strongly suggesting a shift in proactivity and additional civilian contact. The event study in Figure 5 shows a large uptick in stops around the time cameras began filming.[25] The pre-period comparison group average number of stops in a month is 3.4 and the pre-period treatment group average number of stops in a month is 5.5. The average treatment effect of 8.7 stops therefore constitutes a very meaningful increase in stops, with filmed officers nearly doubling their already higher stop numbers.

This is the case despite the fact that the comparison group of non-filmed officers is also making significantly more stops (45% more) than in pre-camera months. Results look similar with alternative outcome measures of stops, such as binary measures of whether any officer made any stop, as shown in Appendix Table E6.

Next, I assess whether criminal activity or reporting behavior shifted systematically in 911 data. I obtained 911 dispatch data through FOIA for one large filming jurisdiction (Tulsa, OK). I compare changes in call behavior in Tulsa to three other jurisdictions with public 911 data and similar call volumes in the same time period: Dallas, TX; Charleston, SC; and New Orleans, LA. As seen in Figure 6, there are no jumps in call volumes in Tulsa relative to other places during filming. If anything, call volumes may have fallen slightly, especially post-filming, as shown in the regressions in Appendix Table E7.

[25] Williamson County was first featured in October 2018, one month after Season 3 filming began. Fort Bend was first featured in February 2018, four months into Season 2 filming.

Figure 6: 911 Call Volume Event Study



Notes. This city-level event study shows the pre-*Live PD* period, the during *Live PD* period, and post-*Live PD* period for a balanced panel in event time in Tulsa relative to Charleston, Dallas, and New Orleans. Cameras were filming in approximately the period denoted by the shaded region (months 0 through 6) in Tulsa. Each coefficient tests the difference between each month's treatment effect on the log of 911 call volume relative to quarter=-1. Standard errors are clustered at the city level.

IV Effects on the Filmed Communities

Having established the large shifts in proactive, quality-of-life arrests caused by cameras, I now try to understand the community-level implications of police ramping up these marginal arrests. I do so by examining clearance rates, crime, use of force, and civilian confidence. More than 6 million people lived in jurisdictions where their police filmed with *Live PD*.[26]

[26]Based on FBI UCR data on covered populations from UCR-reporting departments, which will undercount the total due to non-reporting. More than 46 million people lived in jurisdictions where their police

First, are the results above consistent with an improvement in policing quality, typically measured by clearance rates (the share of reported crimes that are cleared by arrests)? If police are clearing reported crimes through these increased arrests, this would raise the clearance rate mechanically.

Table 4: Effect of *Live PD* on Clearances, Crime, and Civilian Fatalities

	Clearances	Crime	Any Fatality
EverLivePD*	0.062	0.035	0.017
DuringOrAfterFilming	(0.053)	(0.033)	(0.012)
Observations	1,595,799	1,595,799	1,595,799
R^2	0.89	0.95	0.12
Dataset-by-Dept FE	✓	✓	✓
Dataset-by-Time FE	✓	✓	✓

Notes. This table shows the reduced form effect of *Live PD* at the department level on clearances, crime, and civilian fatalities. Outcomes are the logarithm of clearances for all crimes, the logarithm of reported crimes, and a binary indicator for any fatality, respectively. The coefficients for log outcomes can be interpreted as percent changes. The coefficient for the binary indicator can be interpreted as percentage point changes in the likelihood of a civilian fatality. Standard errors clustered at dataset-by-department level. *** p<0.01, ** p<0.05, * p<0.1.

I examine the numerator (clearances) and the denominator (reported crimes) separately to better identify the dynamics at play. Table 4 shows that the shift in enforcement does not have a significant relationship to clearances. Given the prior results showing increases in primarily quality-of-life arrests, this makes sense, since the kinds of circumstances that result in such discretionary arrests tend to involve proactive policing rather than a response to reported crime (only the latter of which contributes to the clearance rate numerator of cleared crimes and denominator of reported crimes). However, the large standard errors mean that clearances may have risen, but by much less than 1-to-1 with arrests.

The reduced form equation for crime in Table 4 shows that there is no significant effect of *Live PD* presence on reported crime, which suggests the results we have seen so far are not driven by residents increasingly reporting crimes during *Live PD* filming periods, nor are the arrests themselves reducing crime through deterrence or incapacitation. In fact, the

filmed with *COPS.*

point estimate is positive. Its 95 percent confidence interval rules out any crime reduction more than 0.2% for a 1% increase in quality-of-life arrests.

Next, I examine the effect of *Live PD* presence on police violence – specifically, fatal encounters. This outcome is a frequent occurrence at over 1,000 per year nationally,[27] but statistically rare enough at the department level that I am unlikely to detect effects unless they are quite large. Qualitative data suggest this is an important outcome to examine nonetheless. *Live PD* crews have filmed several high-profile civilian deaths.[28] In addition, reporting suggests that violent encounters with police may have risen during periods of *Live PD* filming in some places (Plohetski and Chang 2020). Due to a lack of systematically collected data on use of force by the federal government, I use crowdsourced and verified national department-level fatal encounter data, gathered by Mapping Police Violence (MPV 2020). These data capture known cases where civilians are killed by police.

Column 3 of Table 4 shows that the probability of a fatal encounter with police seems to be positively related to *Live PD* presence, and potentially of a meaningful magnitude, but – as expected – the estimate is imprecise and not significant. The reduced form effect suggests *Live PD* may have doubled the likelihood of a fatal encounter, but this is not significant.

I next examine how *Live PD* filming and concurrent changes in policing enforcement affect civilian attitudes towards the police. I utilize Gallup microdata on attitudes towards police in counties with departments that filmed with *Live PD* versus those that did not since 2010.

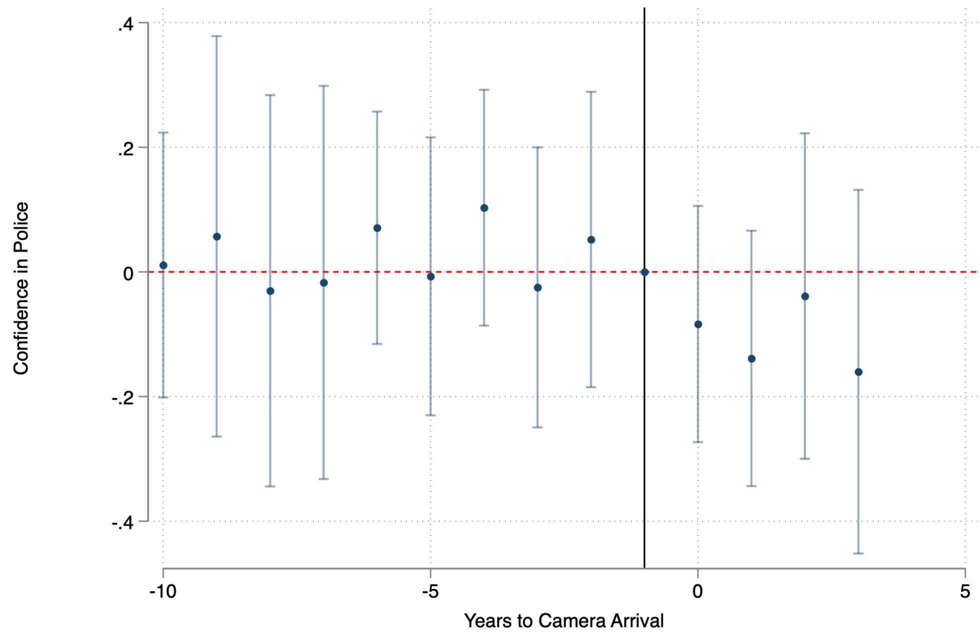
The event study in Figure 7 suggests negative effects, but each individual year's effect relative to t=-1 is not significant. The pre-trends in confidence in police are not significantly different from zero across filming and non-filming counties.

Difference-in-difference regressions pooling the pre- and post periods show that confidence in police was falling overall in the post-*Live PD* period, but fell more so in *Live PD* filming counties. Column 1 in Table 5 shows that *Live PD* filming counties were 12 percentage points less likely to report confidence in police relative to the trend in non-filming

[27] This means that police are responsible for approximately 5 percent of all homicides.

[28] See, for example, Fleming (2020).

Figure 7: Confidence in Police Event Study Among Filming Counties



Notes. This figure depicts an event study specification for filmed counties relative to non-filmed counties (leaving out high-viewership counties) in the years before and after *Live PD* filming. Each coefficient tests the difference between each year's treatment effect on confidence in police relative to t=-1. Standard errors clustered at the dataset-by-county level.

counties.

This reduction in confidence in the police is driven by moderates and liberals in filming jurisdictions, as seen in columns 2 and 3 of Table 5. The point estimate for conservatives in column 2 is negative but insignificant and cannot be differentiated from either zero or the point estimate for moderates and liberals. This decline in confidence provides additional insight into the community-level implications of this shift in policing culture, and adds to the previous results suggesting that the shift did not meaningfully improve public safety. This also helps explain the negative effects on 911 call volume in Tulsa, since confidence and trust in police tend to go hand in hand with crime reporting behavior.

Table 5: Effect of *Live PD* on Confidence in Police

	(1) ALL	(2) Conservatives	(3) Moderates & Liberals
EverLivePD* DuringorAfterFilming	-0.119^{***}	-0.064	-0.132^{***}
	(0.044)	(0.099)	(0.046)
Observations	38,181	14,154	24,027
R^2	0.005	0.006	0.017
Dataset FE	✓	✓	✓

Notes. This table shows the treat*post coefficient from the diff-in-diff equation testing whether filming history of the police department(s) and/or sheriff's office(s) in a respondent's county affects confidence in police. Confidence in police is a binary variable for whether the respondent has quite a lot or a great deal of confidence in police, or less confidence. Effects on confidence in police can be interpreted as a percentage point shift in the probability of having confidence in the police. Columns 2 and 3 look at subsets of respondents based on political ideology. Regressions use Gallup microdata weights. Standard errors clustered at dataset-by-county level. *** p<0.01, ** p<0.05, * p<0.1.

This decline may be caused by a combination of reactions to the increased enforcement, the lack of improvement (and potential worsening) of public safety outcomes, a backlash to filming, and/or a disconnect between what is portrayed on television and the policing actually experienced on the ground. To avoid confounding the effects of show viewership and the effects of constituency in a filming jurisdiction, I also try specifications where I remove the highest viewership counties from the analysis and see the same result. Overall, the effects suggest that constituents do not find what they are seeing or experiencing from their police confidence-inspiring.

V Effects on Viewers

Who are the non-constituent viewers of copaganda shows, are they selecting into viewership based on their existing beliefs about police, and do these shows change how they conceive of the police? I use quasi-experiments and experiments to understand both selection into viewership and the causal effect of the show itself on viewers. I find that regular viewership improves attitudes towards the police, and that watching even a single clip in which officers make an arrest raises viewers' estimate of police productivity.

A. Who Are the Viewers?

To better understand selection into viewership, I combine Simmons Local county-levelvdata on *Live PD* viewership with Gallup microdata on attitudes about crime and policing across geographies. I look at pre-show attitudes in areas that would eventually have high viewership rates of the show in 2019 relative to other counties[29].

As shown in Table 6, high-viewership counties are significantly more conservative, more concerned about crime, and more likely to oppose marijuana legalization than other counties. This may reflect a preference for more arrests among viewers relative to non-viewers, particularly for drug possession. Most of the popular segments from the show feature arrests, particularly involving individuals who seem to be under the influence of drugs or alcohol. One of the most popular clips from *Live PD* on YouTube, garnering over 37 million views, is a compilation of drug bust footage, including several individuals under the influence of substances, suggesting these kinds of arrests are of particular interest to viewers (A&E 2020). However, high-viewership counties and other counties have similar levels of confidence in the police at baseline.

B. Quasi-Experimental Effects on Viewers

How do the opinions of the viewers change in response to *Live PD*'s portrayal of police? The public's confidence in their police directly affects the police's ability to do their job, as the public needs to believe in the legitimacy and effectiveness of the police to deter criminal behavior, encourage reporting of crimes, and cooperate with investigations.[30]

[29] These are counties at or above the 75th percentile of viewership, where around 9% or more of a county's respondents or more reported watching the show in the previous week in 2019. Results look very similar with other definitions, such as above-average viewership or a standard deviation above average viewership.

[30] See, for example, Tyler and Fagan (2008) and Papachritos et al. (2012) on the theory and empirics of these relationships.

Table 6: Pre-*Live PD* Preferences and Beliefs

	High-Viewership Counties	Other Counties	p-value
How would you describe your political views?			
Conservative or Very Conservative	0.446	0.393	0.000***
	(0.497)	(0.488)	
Moderate	0.377	0.376	0.901
	(0.485)	(0.484)	
Liberal or Very Liberal	0.177	0.231	0.000***
	(0.382)	(0.422)	
Number of Respondents	3,870	10,390	
How would you describe the problem of crime?			
Very or Extremely Serious	0.605	0.529	0.000***
	(0.489)	(0.499)	
Moderately Serious	0.364	0.422	0.000***
	(0.481)	(0.494)	
Not Too Serious or Not Serious At All	0.031	0.049	0.000***
	(0.173)	(0.215)	
Number of Respondents	4,061	10,792	
Should the use of marijuana be made legal?			
Yes	0.450	0.521	0.000***
	(0.498)	(0.500)	
Number of Respondents	1,450	3,920	
How much confidence do you have in the police?			
A great deal	0.250	0.257	0.323
	(0.433)	(0.437)	
Quite a lot	0.308	0.324	0.026**
	(0.462)	(0.468)	
Some	0.300	0.296	0.592
	(0.458)	(0.457)	
Very little	0.124	0.106	0.000***
	(0.329)	(0.307)	
None	0.013	0.012	0.444
	(0.114)	(0.109)	
Number of Respondents	6,925	10,308	

Notes. This table shows group averages for Gallup survey questions. Estimates include all years 2000-2015 (pre-*Live PD*) in which the question was asked. High-viewership counties are at or above the 75th percentile of *Live PD* viewership in 2019. These categories are mutually exclusive and both exclude filming counties. *** p<0.01, ** p<0.05, * p<0.1.

Gallup asks annually about respondents' level of confidence in the police in their Confidence in Institutions survey. I convert the Gallup Likert scale into a binary variable for whether the respondent feels "quite a lot" or "a great deal" of confidence in the police versus less confidence (some, little, or none).[31] I remove *Live PD* filming counties from this analysis so as not to confound viewership and camera presence.

First, I plot an event study specification to examine pre-trends and potential for dynamic treatment effects, comparing confidence in police over time between high viewership and low viewership counties, using the premiere of *Live PD* as the event at t=0:

$$ConfidenceInPolice_{ct} = \kappa_{-1} + \kappa_t HighViewership_c * [\sum_{t=-10}^{-2} lags_t + \sum_{t=0}^{4} leads_t] + \varepsilon \quad (5)$$

In the event study, $HighViewership_c$ binarizes county-level viewership into a value of 1 at the 75th percentile of viewership and above and a value of 0 at the 25th percentile of viewership and below.[32]

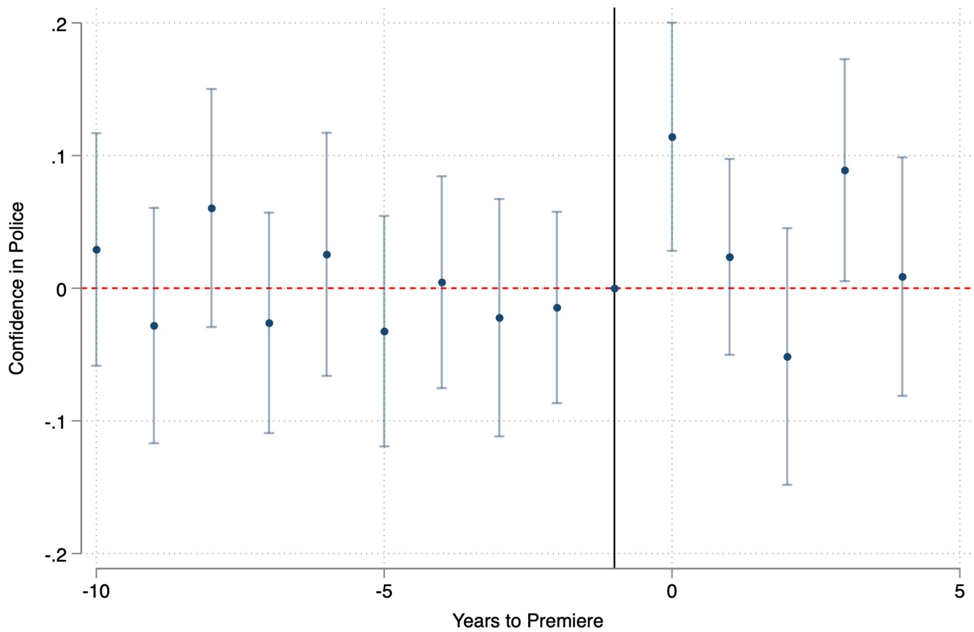
The event study in Figure 8 shows flat pre-trends in higher-viewership counties relative to low-viewership counties and a relative increase in confidence in police most notably in the year of *Live PD*'s premiere. However, using only a subset of counties to binarize the treatment means the estimates for individual years are noisy, especially after the premiere year, and it is still difficult to know whether selection into *Live PD* viewership would have led to similar shifts even without *Live PD*.

To isolate a more quasi-random component of viewership, I use channel (A&E) viewership rates in 2015, before the premiere of *Live PD* was announced, to predict later *Live PD* viewership rates. Compliers isolated in the DDIV specification are those who opted into *Live PD* viewership due to their habit of already watching the channel it eventually premiered on.

[31] Results look very similar using other definitions, such as converting into standard deviation units or converting each answer into a linear numeric scale.

[32] The event study looks similar using other definitions of high and low viewership.

Figure 8: County-Level Event Study of *Live PD* Viewership on Confidence in Police



Notes. This figure depicts an event study specification with treatment defined as 1 among counties with viewership at or above the 75th percentile and 0 among counties with viewership at or below the 25th percentile. Each coefficient tests the difference between each year's treatment effect on confidence in police relative to t=-1. Standard errors clustered at the county level.

Naive Difference-in-Difference

$$ConfidenceInPolice_{ct} = \alpha_0 + \alpha_1 WatchLivePD_c + \delta_2 Post_t + \delta_3 WatchLivePD_c * Post_t + \upsilon_{ct} \tag{6}$$

First Stage

$$WatchLivePD_c = \beta_0 + \beta_1 WatchA\&E_c + \varepsilon_c \tag{7}$$

Reduced Form

$$ConfidenceInPolice_{ct} = \delta_0 + \delta_1 WatchA\&E_c + \delta_2 Post_t + \delta_3 WatchA\&E_c * Post_t + \upsilon_{ct} \quad (8)$$

DDIV

$$ConfidenceInPolice_{ct} = \rho_0 + \rho_1 \widehat{WatchLivePD}_c + \rho_2 Post_t + \rho_3 \widehat{WatchLivePD}_c * Post_t + \eta_{ct} \quad (9)$$

In these specifications, *Post* is after the *Live PD* premiere in 2016, $WatchLivePD_c$ is the share of respondents in a county who reported watching *Live PD* in the previous week in 2019, and $WatchA\&E_c$ is the share of respondents in a county who reported watching the *A&E* channel in the previous week in 2015. Both variables are transformed into standard deviation units of county-level viewership.[33]

The key identifying assumption of this DDIV approach is that A&E ratings in the period before *Live PD* aired are unrelated to trends in attitudes towards police except due to *Live PD*. Parallel trends in the event study specification bolsters this assumption, though it is fundamentally untestable. The exclusion restriction – that A&E viewership affects trends in attitudes towards police during this era only through *Live PD* seems reasonable since it was the only show about policing that came on the air during this era and was the channel's most popular show by far. It is also important that the instrument (A&E viewership) is relevant and has a monotonic effect on the treatment (*Live PD* viewership). Relevance is established by the very strong first stage shown in Table 7. Monotonicity is strongly suggested by the binscatter plot in Appendix Figure D6 showing continuous increases in *Live PD* viewership rates across the support of A&E viewership rates.

Table 7 shows across all specifications that counties with higher viewership of *Live PD* reported relatively higher confidence in the police in the years after *Live PD*'s introduction than pre-*Live PD* trends would have predicted. The DDIV coefficient suggests that moving one standard deviation up in county-level *Live PD* viewership raises the probability of voicing confidence in the police by 5.5 percentage points, more than countering the

[33]Results look very similar using other definitions, such as using raw percentages of viewership.

Table 7: Effect of *Live PD* on Confidence in Police Among Viewers

	OLS	First Stage	Reduced Form	DDIV
	Confidence in Police	Watch Live PD	Confidence in Police	Confidence in Police
Watch Live PD * Post	0.041***			0.055***
	(0.010)			(0.015)
Watch Live PD	−0.001			−0.009
	(0.006)			(0.009)
Post	−0.052***		−0.052***	−0.052***
	(0.011)		(0.011)	(0.011)
Watch A&E * Post			0.039***	
			(0.011)	
Watch A&E		0.694***	−0.006	
		(0.023)	(0.006)	
Observations	18,691	18,691	18,691	18,691
R^2	0.004	0.476	0.003	0.004
Kleibergen-Paap F-Stat		444.667		

Notes. This table shows the naive OLS regression and the IV stages. The naive OLS diff-in-diff shows the effect of higher *Live PD* viewership rates on confidence in police before and after its premiere. The first stage shows the effect of 2015 A&E watching on 2019 Live PD watching. The reduced form shows the effect of 2015 A&E watching on confidence in police. The DDIV shows the effect of predicted 2019 Live PD watching on confidence in police. Watch variables have been transformed into standard deviation units. Confidence in police is a binary variable for whether the respondent has quite a lot or a great deal of confidence in police, or less confidence. Effects on confidence in police can be interpreted as a percentage point shift in the probability of having confidence in the police. Standard errors clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

overall decline in confidence in police over this period. Watching *Live PD* seems to have insulated viewers from the general decline in confidence in police since 2016. The slightly higher coefficient on the 2SLS specification relative to the OLS specification suggests that compliers who were pulled into watching *Live PD* by their prior A&E viewership may have been particularly influenced, though this is not statistically distinguishable from the OLS coefficient.

The improvement in attitudes among far-off viewers coupled with the declines in confidence among constituents and non-improvement (or potential deterioration) in public safety suggests that entertaining and catering to the preferences of far-off viewers may come at the cost of the local community.

C. Experimental Effects on Viewers

To better understand the effects of the specific content of the shows on a wider set of attitudes and beliefs, as well as to isolate a cleaner causal effect of watching these shows, I turn to experimental evidence.

I ran a survey experiment on Prolific in spring 2023 to better understand how US-based individuals think about policing and about reality TV shows focused on policing. A total of 683 U.S.-based adults participated. One-third were randomly assigned to a placebo condition and two-thirds to an experimental condition. In the experimental condition, respondents saw one of 10 randomly selected clips from *Live PD* on YouTube. Six of these clips ended in an arrest, two ended in a non-arrest detention, and two ended with no one taken into custody. The heterogeneity was meant to reflect the true distribution of segments on the show, and allows me to identify the effects of the different content (e.g. arrest versus no arrest). In the placebo condition, respondents saw one of 10 randomly selected clips from a reality TV show about a different profession from the same channel (A&E) and time period.[34] All respondents were then asked questions about their beliefs about police and asked to estimate various aspects of police work (e.g. how many arrests are made in a year).

Respondents were very familiar with reality TV shows about cops; 90% reported having seen *COPS*, *Live PD*, or another similar show, as previously showin in Figure 1. The pervasiveness of these shows and their portrayal of the constant-arrest cop may be one reason why Americans overestimate the number of arrests that a typical cop makes in a year by an order of magnitude. As shown in Figure Appendix D7, respondents overestimated the

[34] Alternative professions shown included exterminators, truckers, and individuals who bid on storage unit auctions.

number of arrests made by a typical police officer in a year by an order of magnitude,[35] regardless of their experimental condition. The median guess was around 100 and the mean was around 200 arrests per year. Despite rewarding respondents for accuracy, Appendix Figure D8 shows that they also overestimated the rate at which police solve crimes with an average guess over 40 percent, though the true overall clearance rate was 28 percent. Overall, respondents' high level of exposure to these shows and their beliefs about the accuracy of these shows, as shown in Appendix Figure D9, may contribute to these distorted beliefs.

Prolific respondents were hired in batches to make the sample as representative of the US population as possible. The F-statistic on the balance table for the experimental versus placebo group, column 1 of Appendix Table E8, suggests that randomization was successful, with balance between the placebo and experimental groups on race, ethnicity, education, ideology, age, and likelihood of interacting with police as a victim or potential suspect. I include gender fixed effects in regressions comparing the placebo and experimental groups to ensure comparability. The F-statistic on the balance table within the experimental group for those who saw clips with an arrest versus no arrest (column 2 of Appendix Table E8) was also not significant at the 5% level. However, the significance on ideology and Hispanic ethnicity led me to include political identity and ethnicity fixed effects in the regressions comparing the arrest clip versus no arrest clip groups to ensure comparability.

Perhaps unsurprisingly given the high level of exposure to these shows even among the control group, Appendix Table E9 shows that watching a single clip of *Live PD* did not move beliefs about police or crime significantly. However, those who watched *Live PD* instead of another reality show rated it as a significantly more accurate portrayal of the profession shown. 83 percent of respondents said they believed *LivePD* was a moderately, very, or extremely accurate portrayal of policing. Interestingly, respondents found it slightly less entertaining on average than the placebo shows.

Within the group assigned to watch a *Live PD* clip, the content of the clip shown did matter.

[35] Recall the earlier analysis suggesting a high estimate of 12-14 arrests per year.

Table 8: Experimental Effect of Clip Ending in Arrest Within Experimental Group

	(1) Confidence in Police	(2) Crime Concern	(3) Spending Preference	(4) # Arrests Estimate	(5) Clearance Rate Estimate	(6) Clip Accuracy	(7) Clip Entertaining
Arrest in Clip	−0.07*	0.11	−0.06	33.74	3.90**	−0.01	0.21**
	(0.04)	(0.09)	(0.08)	(22.81)	(1.84)	(0.09)	(0.09)
Constant	0.37***	−0.09	−0.01	153.01***	41.57***	0.23***	−0.18***
	(0.03)	(0.07)	(0.06)	(17.59)	(1.42)	(0.07)	(0.07)
Observations	465	465	465	465	465	465	465
R^2	0.12	0.10	0.20	0.02	0.02	0.09	0.04
Ideology FE	✓	✓	✓	✓	✓	✓	✓
Ethnicity FE	✓	✓	✓	✓	✓	✓	✓

Notes. Each column shows a different regression testing the effect of being assigned to a Live PD clip containing an arrest versus not containin an arrest (within the experimental group). *Confidence in Police* is an indicator for whether the respondent has at least quite a lot of confidence in police. *Crime Concern* is a Likert scale response for how serious a concern crime is to the respondent converted into standard deviation units. *Spending Preference* reflect how much the respondent wants the government to spend on the police relative to the status quo, converted into standard deviation units. *# Arrests Estimate* is the respondent's guess for the annual number of arrests made by a typical police officer. *Clearance Rate Estiamte* is the respondent's guess for the share of all reported crimes that are cleared by arrests. *Clip Accuracy* and *Clip Entertaining* are Likert scale responses for how accurately the clip represents the profession and how entertaining the clip is, respectively, converted into standard deviation units. All regressions contain ideology and ethnicity fixed effects. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8 shows that those who watched police make an arrest guessed the true clearance rate for all crime was significantly higher than those who did not see an arrest. This suggests that viewers seem to internalize the arrests on the show as reflective of reality and as arrests that respond to reported crimes.[36] However, the analysis in this paper suggests neither of these is accurate. Interestingly, viewers who saw an arrest found the clip more entertaining than those who did not, which may speak to the demand for such content as a driver of the distortions.

Overall, the experiment provides evidence of the wide reach of these shows, viewers' sense of their verisimilitude, as well as their potential to alter some beliefs about the realities of policing in a matter of minutes. However, the effects should be thought of as the short-

[36] This tendency of viewers to neglect potential editing and selection into what is shown is closely related to the availability heuristic and Enke (2020)'s "what you see is all there is" heuristic.

term effect of watching a single clip from the show on top of the cumulative effects of past exposure, given the near-universal previous exposure to these shows among respondents.

VI Conclusion

Taken as a whole, these results suggest that (1) the distortions embedded in the copaganda genre alter viewers' attitudes towards and beliefs about the police, and that (2) the presence of reality TV cameras - and the distorted expectations that come with them - incentivize police to make more arrests for crimes with low social costs such as drug possession, loitering, and vagrancy. These arrests reflect a shift in enforcement by police rather than a change in the incidence of crime or reporting. This shift in enforcement does not meaningfully improve public safety, and opinion data showing loss of confidence in the police suggest the costs of such enforcement are high. This result joins the recent literature suggesting that marginal arrests for quality-of-life crimes are not an effective public safety measure and may be actively harmful to constituents.

These findings also demonstrate that reality TV shows about police are not providing their "promise of 'transparency' and 'clarity'" (Stanhope 2017). They are not a neutral window into the realities of policies. Rather, they provide a distorted view of American policing to their millions of viewers. My quasi-experimental results suggest that this distortion significantly inflates their confidence in the police. My experimental results suggest that the overrepresentation of arrests on these shows further inflates viewers' already inaccurate beliefs about police productivity. This may alter voting, crime reporting, cooperation in criminal investigations, and other behaviors affected by trust and confidence in the police. Future research should investigate these potential downstream effects.

These results have important implications not only for police departments and sheriffs' offices considering partnering with *Live PD*'s new iteration, *On Patrol: Live*, or similar shows, but also for media covering the police and for policymakers designing accountability mechanisms for police. These estimates suggest that police respond strongly to non-financial incentives and expectations built into accountability mechanisms, whether or not it is in the public's best interest to incentivize that behavior. Who is watching the police and what their expectations are matter a great deal.

This paper also demonstrates that what people think they want from the police (i.e. arrests) may not match up with the reality of how police try to meet those expectations on the ground. Effective policing tactics that reduce crime and inspire civilian confidence may not result in many (if any) arrests for quality-of-life crimes. For example, practices from procedural justice and community policing such as initiating positive, non-hostile, and informal contacts with constituents, adopting a "guardian" over a "warrior" mindset to constituent interactions, diverting individuals with substance use disorders to treatment, or deescalating interactions without using force do not make for particularly entertaining television but may result in more effective policing.

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Appendix A. Body-Worn Cameras

Live PD's hosts state that "*Live PD* is an extension, to some degree, of body cameras," and they hope that "body cameras and [*Live PD*] cameras lead to better policing" (Stanhope 2017). There are several reasons to think that reality TV filming may differ from body-worn cameras or other forms of oversight. First, police departments have veto power over what footage makes it to the air, which may reduce or counteract any potential transparency effect. Second, the filming is explicitly for entertainment purposes, and the footage is only aired if it is deemed entertaining enough by producers. Third, if the footage is aired, it will be seen by large numbers of viewers, whereas the vast majority of body-worn camera footage is not easily accessible or made available for viewing.

Table A1: Effect of Live PD on Arrests in Departments with and without Body-Worn Cameras

	(1) Quality-of-Life Arrests	(2) Drug Possession Arrests	(3) Other Arrests
EverLivePD*DuringFilming	0.138**	0.144*	0.061
	(0.069)	(0.078)	(0.050)
EverLivePD*DuringFilming*BWC	0.011	−0.019	−0.006
	(0.176)	(0.198)	(0.072)
Observations	430,363	430,363	430,363
R^2	0.876	0.826	0.945
Dataset-by-Dept FE	✓	✓	✓
Dataset-by-Time FE	✓	✓	✓

Notes. This table shows the department-level TWFE specification interacted with indicator *BWC* (whether the department had already rolled out body-worn cameras in 2016), for the log of three categories of arrests. Only the subset of departments that reported BWC status to the Bureau of Justice Statistics are included. Standard errors clustered at the dataset-by-department level. *** p<0.01, ** p<0.05, * p<0.1.

I test whether the main results differ based on whether the jurisdiction had already deployed body-worn cameras in 2016, when the show began, to see whether the effects of *Live PD* are counteracted or diminished by body-worn camera use. If *Live PD* is simply a substitute for body-worn cameras, offering the same transparency effects, then there should be no effects of *Live PD* in jurisdictions that have deployed body cameras.

I have merged in body-worn camera use in 2016 from the Bureau of Justice Statistics' Law Enforcement Management and Administrative Statistics Body-Worn Camera Supplement. The results in Appendix Table A1 show that, among the jurisdictions that completed the survey, those with body-worn cameras do not experience a significantly different effect of *Live PD* cameras on quality-of-life arrests relative to those without body-worn cameras. This suggests that *Live PD* cameras are not a substitute for body-worn cameras.

Appendix B. Emails Obtained Through Freedom of Information Act Requests

A. Slidell Police Express Concern about Racist Language in Clip

Daniel Seuzeneau

From: Daniel Seuzeneau
Sent: Friday, May 4, 2018 1:12 PM
To: Ashlee Souza
Cc: Ashlee Souza; Tammy Zohar; [redacted] Sheila Cabano; Paul Gordon; Crystal Kaufman; Jennifer L Rettig; Tom Caruso; Kara Kurcz*
Subject: Re: Slidell-Package for Approval (Domestic Pursuit)

Around 1;02;45, Officer Peck says, "Now you got half of I-10 shutdown boy." At least I think he said boy? If we can take that out. I know someone will take that as racist.

Detective Daniel M. Seuzeneau
Slidell Police Department
Public Information Officer

[redacted] Office
Cell
Dispatch
Fax

Follow us on Facebook!
http://facebook.com/SlidellPD

Follow us on the Web!
http://www.slidellpd.com

Follow us on Twitter
http://twitter.com/SlidellPolice

Sent from my iPhone

On May 4, 2018, at 12:26, Ashlee Souza [redacted] wrote:

> Please use this updated link:
>
> https://www.dropbox.com/s/ndq4x75n9iymt1c/SL_DOMESTIC%20PURSUIT_5.3.18_v2.Copy.01.mov?dl=0
>
> > On May 4, 2018, at 10:52 AM, Ashlee Souz[redacted]@bigfishusa.com> wrote:
> >
> > Hi Det. Seuzeneau! Please see the link below for your approval! Thank you, Ashlee
> >
> > SL_Domestic
> > Pursuit 4/28 https://www.dropbox.com/s/0b5vm2wk9k81hb5/SL_DOMESTIC%20PURSUIT_5.3.18_v2.Copy.01.mov?dl=0

B. Wakulla County Expresses Concern about Policy Deviations in Clip

From:	"Chris Savary" [redacted]
To:	"Ashlee Souza" [redacted]
	"Ashlee Souza" [redacted]
Date:	7/19/2017 12:04:51 PM
Subject:	RE: Wakulla Co-Packages for Approval (Shed-Lord Dispute & Machete ManDan)

Hi Ashlee

The first package with Sgt. Delbeato is good to go.

The second package with Hanks has a couple of problems that the Sheriff, Captain Kemp, and I identified. At the 1:43 mark Deputy Hanks starts a discussion with the subject in reference to description. In this conversation Deputy Hanks makes reference to the subject looking like he smokes crack. Then at the 2:30 mark Deputy Hanks tells the camera that "Daniel is one of our local drunks". The subject also has blood on him and we handle the situation without using universal precautions without gloves. Finally Deputy Hanks allows the subject to handle large knives while he is in such close proximity.

Now I know that these points might seem minor but several factors are involved here....first the Sheriff, Captain Kemp and I all agree that this interaction between Hanks and the subject is one that reflects poorly on us. However most importantly we are going to be in the middle of our Accreditation inspection by the FCAC, Florida Commission for Accreditation. They could possibly see this as a violation of some of our policies and that would reflect negatively on our evaluation.

We would request that the "Machete Man" with Deputy Hanks package not be aired at all. Thanks and I hope you can understand our concerns as explained.

Chris Savary
Public Information Officer
Wakulla County Sheriff's Office
[redacted]

-----Original Message-----
From: Ashlee Souza [redacted]
Sent: Tuesday, July 18, 2017 3:19 PM
To: Ashlee Souza
Cc: Chris Savary; Erika Paige Barnette; Kara Kurcz; Tammy Zohar; [redacted] Sheila Cabano; Paul Gordon; Crystal Kaufman; Tom Caruso
Subject: Wakulla Co-Packages for Approval (Shed-Lord Dispute & Machete Man Dan)

Hi Lt. Savary! Please see the link below for your approval! Thank you! Ashlee

https://www.dropbox.com/sh/vtkwu57h8cm2vn9/AAAlHD6uiKllHDjlKe44NRaWa?dl=0

C. Tulsa Police Chief Expresses Concern About Racial Dynamics of Clips

From: John Zito <john.zito@bigfishusa.com>
Sent: 11/4/2019 2:50:31 PM
To: Larkin, Sean <slarkin@cityoftulsa.org>;Rick Hankey <rick.hankey@bigfishusa.com>
Subject: Re: Tulsa packages

Thanks, Sean.

This is what I feared

On Nov 4, 2019, at 3:44 PM, Larkin, Sean <slarkin@cityoftulsa.org> wrote:

Please see the below email from my Chiefs Office

Sent from my iPhone

Begin forwarded message:

From: "Dalgleish, Eric" <edalgleish@cityoftulsa.org>
Date: November 4, 2019 at 1:36:20 PM MST
To: "Larkin, Sean" <slarkin@cityoftulsa.org>
Subject: RE: Tulsa packages

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Sean- The Chief is not a fan of the idea. He expressed concerns of the compilation bringing back the negative attention focused on the OGU episodes as they related to minorities. His point is 5 of the 6 involve minorities and although the police work is commendable, recovering F/Aâs etc.. it will renew voices of dissent around our participation in the show. He is not sure how the mayor will react, regarding our continued participation, if that occurs. We all agreed it will not serve the overall LIVEPD/TPD partnership well.

From: Larkin, Sean
Sent: Wednesday, October 30, 2019 10:00 PM **To:** Dalgleish, Eric <edalgleish@cityoftulsa.org>
Subject: Fwd: Tulsa packages

Thank you and enjoy!!

Sent from my iPhone

Begin forwarded message:

From: Rick Hankey <rick.hankey@bigfishusa.com>
Date: October 30, 2019 at 7:44:26 PM CDT
To: "Larkin, Sean" <slarkin@cityoftulsa.org>
Cc: Nicole Karczewski <nicole.karczewski@bigfishusa.com>
Subject: Tulsa packages

ï»¿
Hey Sean - Thank you so much for doing this. Feel free to delete the titles when you send. They're just for internal reference.

Talk to you soon! Rick

HIDDEN LOCKER
https://www.dropbox.com/s/xkikpx9eqiye3x4/HIDDEN%20LOCKER.mov?dl=0

RUN AND HIDE
https://www.dropbox.com/s/dkfypltc2yuu1x5/RUN%20AND%20HIDE.mov?dl=0

Appendix C. Model of Arrest Behavior

I create a model to demonstrate why I focus on quality-of-life arrests in this paper. The model expands our understanding of police arrest activity by acknowledging that not all arrests are created equal, and that only certain kinds of arrests are likely to flexibly and quickly respond to changing incentives.

In this model of police utility, an officer maximizes utility by determining the amount of effort (e) to exert in making arrests on both the extensive and intensive margin. The level of effort differs along the spectrum of crime severity. For tractability, I collapse crimes into two categories ($c = L, H$), which represent crimes with low social costs of crime victimization and which typically require low effort on the part of the officer to make, and crimes with high social costs of crime victimization and which typically require higher effort on the part of officers to make ($e_L < e_H$).[37]

Officers earn a baseline wage (w) without making any arrests. Their utility function (f_c) monotonically increases in the number of arrests (A_c) they make of each type, which itself is a function of crime type-specific effort and the number of reported or observed crimes of that type (c).

$$\max_{e_L, e_H} = w + [f_L(A_L(e_L, L)) - \frac{1}{2}e_L^2] + [f_H(A_H(e_H, H) - \frac{1}{2}e_H^2]$$

Making arrests may increase compensation directly through overtime hours, higher likelihood of promotion, or (in some departments) bonuses. Arrests may simply endear the officer to their supervisor or colleagues, or enhance their own self-image.

High-level arrests in crime category H include violent offenses and serious felonies such as murder, armed robbery, or aggravated assault. These require higher effort in part because these types of crimes are more rare ($H < L$) and in part because solving them typically

[37] See McCollister et al. (2010) for an overview of the literature calculating costs of various types of crimes.

requires investigatory effort ($e_H > e_L$), but can come with higher reward. For example, a high-effort arrest for a high social cost crime may involve needing to receive a tip from the public, investigate, track down suspects, confront suspects in a potentially dangerous situation, etc.

Quality-of-life arrests in crime category L include drug possession arrests, public drunkenness, and other crimes that are either victimless, low in social cost, or – in the case of drug possession – can viewed more as a public health problem rather than a public safety problem. These kinds of arrests require low levels of effort for an officer because they are common and often habitual (e.g. they may involve known constituents or a set of known locations). Officers typically have high levels of discretion over whether to seek out or make these kinds of arrests.[38]

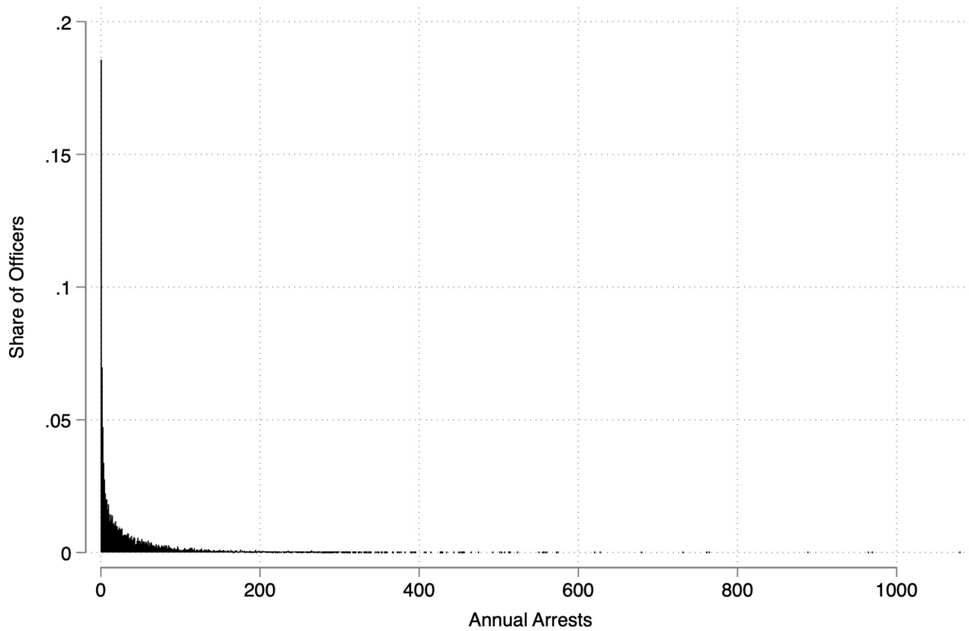
Reality TV filming may raise the marginal benefit of making any arrest by making an officer's work salient to their supervisor and to the public (including both their constituents and viewers). Making an arrest raises an officer's probability of appearing on the show on the extensive margin (at all) and on the intensive margin (on any given night), since footage is only aired if deemed sufficiently interesting. Based on analysis for *COPS* footage and clips available from *Live PD*, arrests are of particular interest to and expected by viewers. The camera presence may thus incentivize officers to make arrests they otherwise would not have, as they are certainly aware they are being watched – potentially by millions of people.[39] An officer's likelihood of making a quality-of-life arrest on any given day or in any given week is much higher than a high-level arrest, and thus we would expect the camera's presence to increase quality-of-life arrests significantly more than high-level arrests.

[38] Some departments, such as Metropolitan Police Department in Washington, DC, explicitly call these arrests "discretionary" for their officers. The existence of such a policy has been mentioned by both MPD officers and staff, though I was unable to find a publicly available written policy.

[39] There is likely a kind of Hawthorne effect at play, where officers want to do more of the action(s) that watchers interpret as productive while being observed: in this case, making an arrest.

Appendix D. Figures

Figure D1: Distribution of Annual Arrests at Officer Level

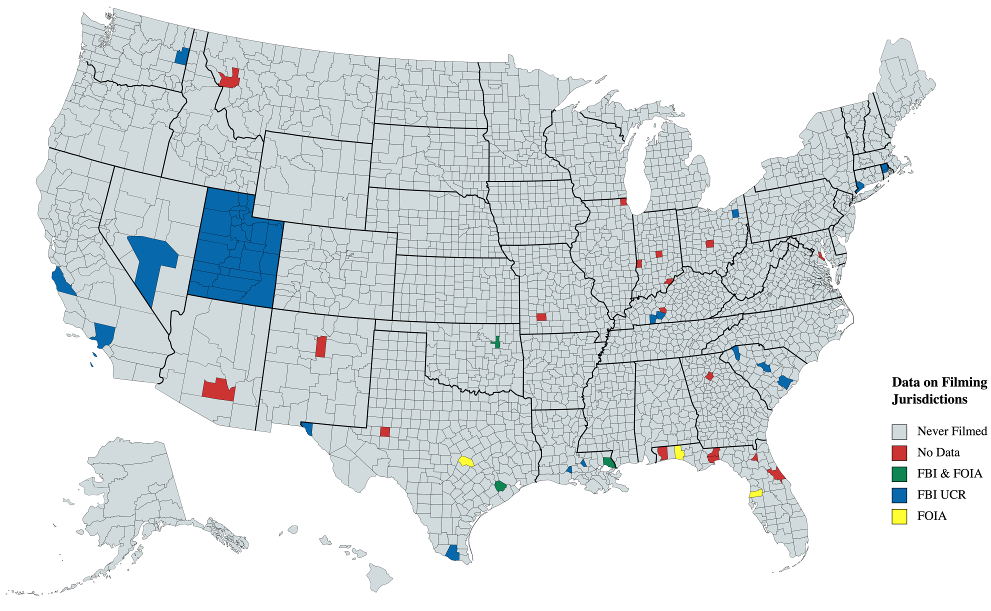


Notes. This shows the distribution of annual arrests at the officer-year level among active sworn officers in the officer-level data obtained through the Freedom of Information Act from Pasco County, FL; Tulsa, OK; St Tammany Parish, LA; and Walton County, FL. To be included in the universe of officers, the officer must have made at least one arrest in the period covered by the data. Active periods are defined as between any two months in which they made at least one arrest.

Figure D2: Picture of the *Live PD* Studio

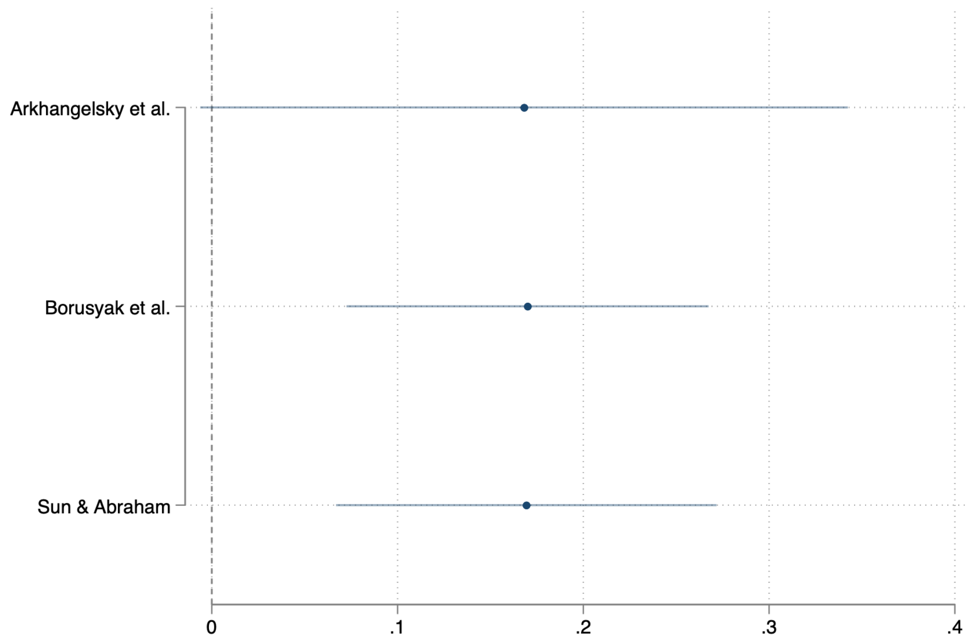
Notes. This picture shows the inside of the *Live PD* studio, with its hosts in the foreground and several screens with footage playing behind them. Source: Nakamura (2020).

Figure D3: Map of *Live PD* Filming Jurisdictions



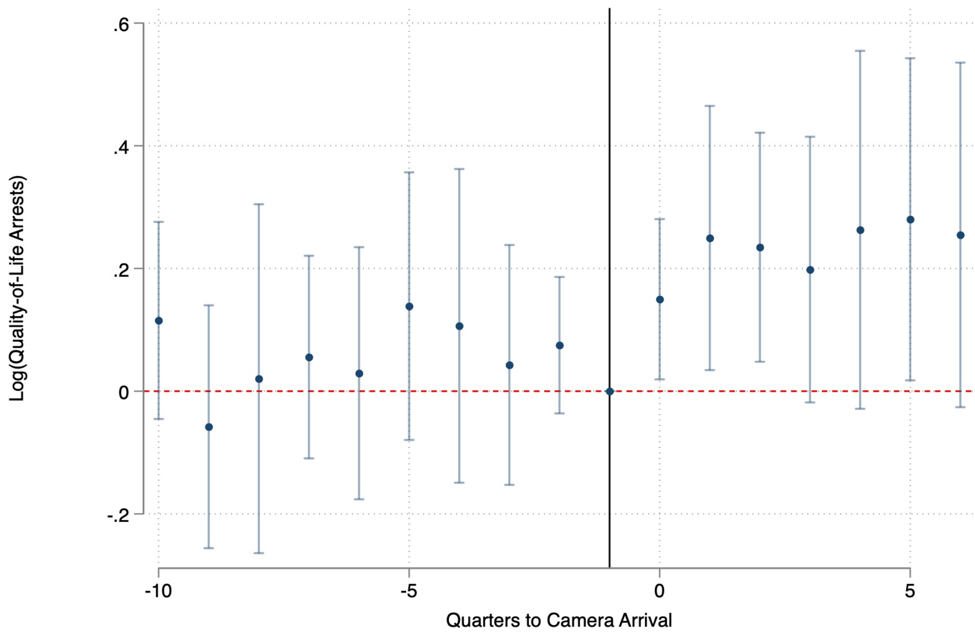
Notes. This map shows the jurisdictions that filmed with *Live PD* over its four seasons and the data sources for each used in this paper.

Figure D4: Alternate TWFE/Difference-in-Differences Estimators for Quality-of-Life Arrests



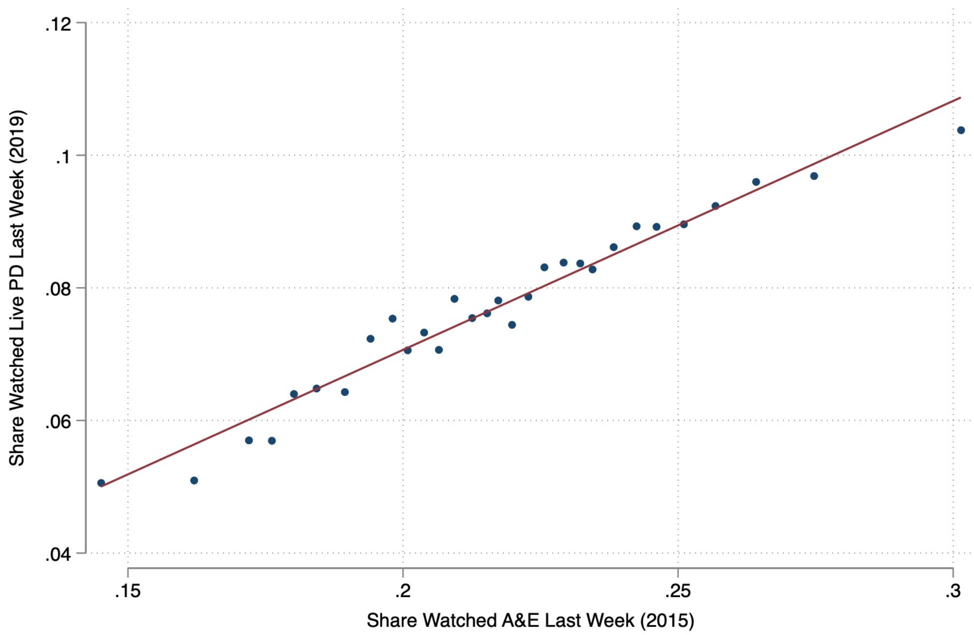
Notes. Alternate estimators include Arkhangelsky et al. (2021)'s synthetic difference-in-differences estimator, Borusyak et al. (2022)'s staggered adoption design difference-in-differences imputation, and Sun and Abraham (2021)'s interaction weighted event study estimator using Sun (2022) code.

Figure D5: National, Department-Level Event Study for Quality-of-Life Arrests in Multi-Season Filming Departments



Notes. This figure depicts an event study specification for a balanced panel between 30 months before and 20 months after camera arrival for the multi-season filming treated agencies. Each coefficient tests the difference between each quarter's treatment effect on the log of quality-of-life arrests relative to t=-1 (months -3 through -1). Standard errors clustered at dataset-by-department level.

Figure D6: County-Level Viewership Rates of A&E Channel and *Live PD* Show



Notes. This figure shows a binscatter plot of county-level viewership rates of the channel A&E in 2015 on the x-axis (before *Live PD*), and of county-level viewership rates of *Live PD* in 2019 on the y-axis.

Figure D7: Distribution of Respondent Estimates:
Annual Arrests at Officer Level

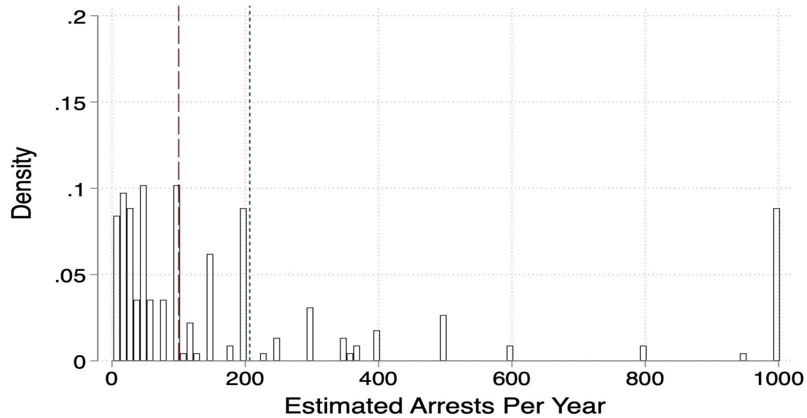


Figure D8: Distribution of Respondent Estimates:
Clearance Rate

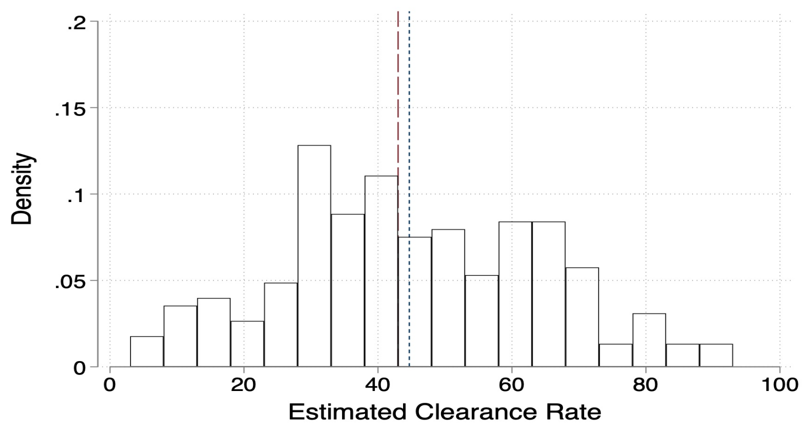
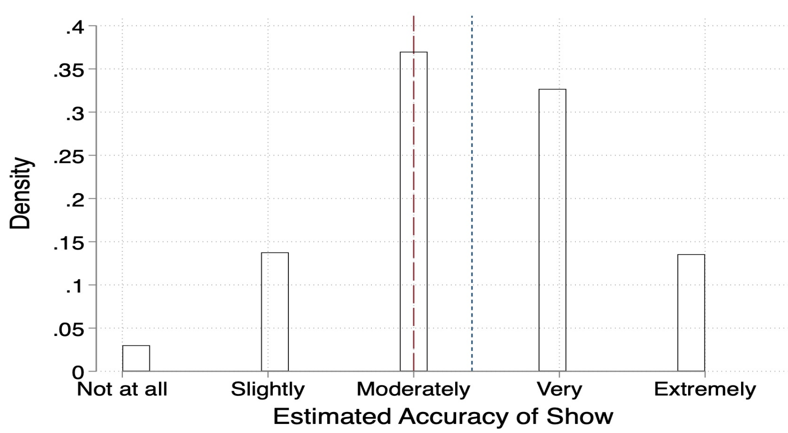


Figure D9: Distribution of Respondent Estimates:
Accuracy of Show's Portrayal



Notes. In all figures, the median is shown with a red long-dashed line and the mean is shown with a navy short-dashed line. Figure D7 shows the distribution of estimates for how many arrests a typical police officer makes in a year among the placebo group. This variable is topcoded at 1,000 for visualization. Figure D8 shows the distribution of estimates for the clearance rate for all crimes in 2019 among the placebo group. Figure D9 shows the distribution of estimates for how accurately the show portrays the profession of policing. The scale is converted into a linearly increasing scale (1-5) to calculated the median and mean.

Appendix E. Tables

Table E1: List of Participating *Live PD* Jurisdictions

List of Participating Agencies	Season 1 (10/2016-8/2017)	Season 2 (10/2017-8/2018)	Season 3 (9/2018-8/2019)	Season 4 (9/2019-5/2020)	Originating Agency Identifier (ORI)	FIPS State + County Code	In FBI UCR Data	In FOIA Data
Jefferson County, AL				✓	AL00100	1073		
Pinal County, AZ		✓			AZ01100	4021		
Dept of Public Safety, AZ	✓				AZCCHP✓	4013		
Pomona, CA				✓	CA01955	6037	✓	
Salinas, CA			✓	✓	CA02708	6053	✓	
Bridgeport, CT	✓				CT00015	9001	✓	
Bradford County, FL				✓	FL00400	12007		
Clay County, FL				✓	FL01000	12019		
Tallahassee, FL				✓	FL03703	12073		
Pasco County, FL		✓	✓		FL05100	12101		✓
Santa Rosa County, FL	✓				FL05700	12113		
Volusia County, FL				✓	FL06400	12127		
Wakulla County, FL	✓				FL06500	12129		
Walton County, FL	✓				FL06600	12131		✓
Gwinnett County, GA		✓			GA06700	13135		
Lake County, IL	✓	✓			IL04900	17097		

Clark County, IN	✓	✓			IN01000	18019		
Jeffersonville, IN	✓	✓			IN01003	18019		
Lawrence, IN			✓	✓	IN04902	18097		
Terre Haute, IN				✓	IN08401	18167		
Edmonson County, KY	✓				KY03100	21061		
Logan County, KY	✓				KY07100	21141	✓	
Warren County, KY	✓				KY11400	21227	✓	
Lafayette, LA			✓	✓	LA02803	22055	✓	
St Tammany Parish, LA	✓				LA05200	22103	✓	✓
Slidell, LA		✓	✓		LA05202	22103	✓	
West Baton Rouge, LA				✓	LA06100	22121		
Calvert County, MD	✓				MD00500	24009		
Greene County, MO		✓	✓	✓	MO03900	29077		
Missoula, MT				✓	MT03200	30063		
US Marshal Service, NY			✓		N/A	N/A		
Santa Fe, NM			✓		NM02601	35049		
Nye County, NV		✓	✓	✓	NV01200	32023	✓	
Franklin County, OH			✓		OH02500	39049		
Streetsboro, OH		✓			OH06712	39133	✓	
Tulsa, OK	✓			✓	OK07205	40143	✓	✓

Oklahoma Highway Patrol, OK			✓		OKOHP00	40109		
Warwick, RI		✓	✓		RI00203	44003	✓	
East Providence, RI			✓	✓	RI00404	44007	✓	
Berkeley County, SC				✓	SC00800	45015	✓	
Greenville County, SC	✓	✓			SC02300	45045	✓	
Richland County, SC	✓	✓	✓	✓	SC04000	45079	✓	
El Paso, T✓		✓	✓		TX07102	48141	✓	
Fort Bend County, T✓		✓			TX07900	48157	✓	✓
Mission, T✓	✓	✓	✓	✓	TX10810	48215	✓	
Midland County, T✓	✓				TX16500	48329		
Williamson County, T✓			✓	✓	TX24600	48491		✓
Utah Highway Patrol, UT	✓	✓			UTUHP00	49035	✓	
Spokane County, WA	✓	✓			WA03200	53063	✓	

Table E2: Data Description

Data Source	Dataset	Freq-uency	Year(s)	Description	Access Site
FBI & Jacob Kaplan	Uniform Crime Reporting Detailed Arrests by Age, Sex, and Race (ASR) Data	Monthly	2012-2020	Highly granular data on the number of people arrested for a variety of crimes by covered law enforcement agencies in a given month and year	https://doi.org/10.3886/E102263V10
FBI & Jacob Kaplan	Uniform Crime Reporting Offenses Known and Clearances by Arrest	Monthly	2012-2020	Compilation of offenses reported to law enforcement agencies in the United States for those crimes which people are most likely to report to police and those crimes which occur frequently enough to be analyzed across time	https://doi.org/10.3886/E100707V15
BJS	Law Enforcement Management and Administrative Statistics Body-Worn Camera Supplement	One Time	2016	Survey of agencies' body-worn camera and other technological equipment deployment as of 2016	https://doi.org/10.3886/ICPSR37302.v1

Wikipedia	*LivePD* Season & Episode Information	N/A	2016-2020	List of participating agencies and officers by season and episode	https://en.wikipedia.org/wiki/Live_PD
Simmons LOCAL	Cable/Television /Radio Viewership Data	Annual	2016-2020	Viewership of A&E and Live PD at the county level	https://simplyanalytics.com
NACJD	Law Enforcement Agency Identifiers Crosswalk, United States, 2012 (ICPSR 35158)	Updated Regularly	2012-present	Crosswalk between agency names, ORIs, and FIPS codes	https://www.icpsr.umich.edu/web/NACJD/studies/35158
Mapping Police Violence	2013-2020 Police Killings	Daily	2013-2020	Fatal encounters with police with agency identifiers	https://mappingpoliceviolence.org/s/MPVDatasetDownload.xlsx
CDC	Underlying Cause of Death Database	Annual	2012-2020	Underlying cause of death data based on death certificates at the county level. Drug overdose deaths are deaths with an ICD-10 underlying cause code of X40-X44, X60-X64, X85 or Y10-Y14	https://wonder.cdc.gov/ucd-icd10.html

Gallup Survey Microdata	Confidence in Institutions Survey	Annual	1993-2021	Cross-sectional survey about confidence in institutions conducted annually in June	https://guides.library.harvard.edu/public_opinion/gallupmicro
Vera Institute 911 Data	911 Dispatch Data for Charles, Dallas, and New Orleans	Daily	2016-2021	911 dispatch data standardized across several cities	https://github.com/tsdataclinic/Vera/tree /master
Running from *COPS* Reporting Team	*COPS* Episode Content Data	Each episode	1989-2018	Hand-coded information on the content of each segment of each episode created by the Pineapple Street reporting team	
Fort Bend County, TX Sheriff	Open Records Stop Data	Daily	2015-2020	Stop data with officer identifiers made available by a public records request	
Pasco County, FL Sheriff	Open Records Arrest Data	Daily	2015-2019	Arrest data with officer identifiers made available by a public records request	
St Tammany Parish, LA Sheriff	Open Records Arrest Data	Daily	2015-2019	Arrest data with officer identifiers made available by a public records request	

Tulsa, OK Police Department	Open Records Arrest Data	Daily	2009-2020	Arrest data with officer identifiers made available by a public records request	
Tulsa, OK Police Department	Open Records 911 Dispatch Data	Daily	2016-2018	911 dispatch data made available by a public records request	
Walton County, FL Sheriff	Open Records Arrest Data	Daily	2016-2019	Arrest data with officer identifiers made available by a public records request	
Williamson County, TX Sheriff	Open Records Stop Data	Daily	2017-2020	Stop data with officer identifiers made available by a public records request	

Table E3: National, Department-by-Month Level Effect Using Missing Arrest Data

	Quality-of-Life Arrests		Drug Possession Arrests		Other Arrests	
	(1) DiD	(2) TWFE	(3) DiD	(4) TWFE	(5) DiD	(6) TWFE
EverLivePD*	0.214*	0.184***	0.211	0.180**	0.106	0.059*
DuringFilming	(0.119)	(0.067)	(0.142)	(0.075)	(0.076)	(0.031)
Observations	1,389,242	1,389,241	1,389,937	1,389,936	1,388,193	1,388,192
R^2	0.002	0.821	0.001	0.779	0.003	0.915
Dataset FE	✓		✓		✓	
Dataset-by-Dept FE		✓		✓		✓
Dataset-by-Time FE		✓		✓		✓

Notes. This table shows the main difference-in-difference and TWFE specifications for 3 categories of arrests (quality-of-life arrests, drug possession arrests, and other arrests) with outliers set to missing. Drug possession arrests are a subset of quality-of-life arrests. All outcomes are measured in logarithms and can be interpreted as percent changes. Odd numbered columns show the γ_1 treat*post coefficient from the difference-in-difference equation 2. Even numbered columns show the β_1 treat*post coefficient from the two-way fixed effect equation 3. Standard errors clustered at dataset-by-department level. *** p<0.01, ** p<0.05, * p<0.1.

Table E4: Alternative Specifications for National, Department-by-Month Level Effect of Live PD on Arrests

	Covariates	# Seasons	Size
	(1)	(2)	(3)
	Quality-of-Life Arrests	Quality-of-Life Arrests	Quality-of-Life Arrests
EverLivePD*DuringFilming	0.191***	0.157	0.145**
	(0.069)	(0.117)	(0.058)
EverLivePD*DuringFilming*Multi		0.057	
		(0.144)	
EverLivePD*DuringFilming*Small			0.158
			(0.173)
Observations	1,009,112	1,009,112	1,1,009,112
R^2	0.841	0.839	0.839
Covariates	✓		
Dataset-by-Dept FE	✓	✓	✓
Dataset-by-Time FE	✓	✓	✓

Notes. This table shows three separate alternative TWFE specifications. All outcomes are measured in logarithms and can be interpreted as percent changes. The first row shows the treat*post coefficient from each version of the TWFE equation 3. Column 1 adds time-varying covariates for crime, population, and drug deaths. Crime data come from monthly reports in the FBI UCR. County-level drug deaths come from the CDC WONDER database. Following Chalfin and McCrary (2018) and Premkumar (2021), I fit a local polynomial function to smooth out the department-level annual population variable from the FBI UCR on a monthly basis. Column 2 separates the main coefficient for departments that filmed for one season verus multiple. *Multi* is a binary variable capaturing whether a treatment department was on for more than one season of the show. Column 3 separates the main coefficient for departments by size. *Small* is a binary variable capturing whether the department had 150 officers - the median size of treated departments - or fewer in any year. Standard errors clustered at dataset-by-department level. *** p<0.01, ** p<0.05, * p<0.1.

Table E5: National, Department-by-Month Level Effect of Live PD on Arrests by Race

	Quality-of-Life Arrests	
	White	Non-White
EverLivePD*DuringFilming	0.160**	0.140**
	(0.081)	(0.063)
Observations	1,389,826	1,389,826
R^2	0.805	0.814
Dataset-by-Dept FE	✓	✓
Dataset-by-Time FE	✓	✓

Notes. This table shows the TWFE specifications for low-level arrests for white and non-white arrestees. Coefficients are the β_1 treat*post coefficient from the two-way fixed effect equation 3 run separately by race. Standard errors clustered at dataset-by-department level. *** p<0.01, ** p<0.05, * p<0.1.

Table E6: Effect of *Live PD* on Officer-Level Stops

	DiD	TWFE	DiD	TWFE
	(1)	(2)	(3)	(4)
	# Stops	# Stops	Any Stop	Any Stop
EverLivePD*DuringFilming	8.664***	8.664***	0.262***	0.262***
	(3.338)	(3.341)	(0.068)	(0.069)
Observations	17,174	17,174	17,174	17,174
R^2	0.036	0.454	0.017	0.476
Dataset FE	✓		✓	
Dataset-by-Dept FE		✓		✓
Dataset-by-Time FE		✓		✓

Notes. This table shows the main officer-level difference-in-difference and TWFE specifications for the number of stops and for the binary indicator of whether any stop was made. Columns 1 and 3 show the treat*post coefficient from the difference-in-difference equation. Columns 2 and 4 show the treat*post coefficient from the two-way fixed effect equation. Standard errors clustered at dataset-by-officer level. *** p<0.01, ** p<0.05, * p<0.1.

Table E7: Effect of *Live PD* on City-Level 911 Calls

	DiD	TWFE
	(1)	(2)
	911 Calls	911 Calls
EverLivePD*DuringFilming	−0.025	−0.046*
	(0.023)	(0.026)
EverLivePD*AfterFilming	−0.099***	−0.107***
	(0.030)	(0.034)
Observations	100	100
R^2	0.107	0.995
Year FE	✓	
City FE		✓
Month-by-Year FE		✓

Notes. This table shows the treat*post coefficient from the city-level difference-in-difference specification in column 1 and the TWFE specification in column 2. The outcome is the logarithm of monthly 911 call volume and coefficients can be interpreted as percentage changes. Standard errors clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

Table E8: Randomization Balance Table

	(1) Experimental Group	(2) Arrest in Clip	(3) Control Group Mean
Share White	0.032	-0.089	0.774
	(0.045)	(0.058)	(0.419)
Share Conservative	0.029	-0.119^{**}	0.350
	(0.039)	(0.049)	(0.478)
Share Hispanic	0.071	-0.167^{**}	0.080
	(0.061)	(0.074)	(0.271)
Share Male	0.081^{**}	-0.032	0.449
	(0.038)	(0.049)	(0.498)
Age	-0.001	-0.001	41.150
	(0.001)	(0.002)	(13.359)
Share with Some College Education or More	0.077^{*}	-0.028	0.757
	(0.045)	(0.058)	(0.430)
Share Interacted with Police as a Victim	0.039	0.012	0.350
	(0.040)	(0.050)	(0.478)
Share Interacted with Police as a Suspect	0.040	-0.054	0.173
	(0.048)	(0.059)	(0.379)
Observations	683	458	226
R^2	0.019	0.033	
F-stat	1.64	1.91	
Prob > F	0.102	0.056	

Notes. Column 1 shows the effect of demographic groups on the probability of assignment to the experimental group (relative to the placebo group). Column 2 shows the effect of demographic groups on the probability of assignment to a clip containing an arrest within the experimental group. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Column 3 shows the control group mean and standard deviation for each characteristic.

Table E9: Experimental Effect of Watching a *Live PD* Clip

	(1) Confidence in Police	(2) Crime Concern	(3) Spending Preference	(4) # Arrests Estimate	(5) Clearance Rate Estimate	(6) Clip Accuracy	(7) Clip Entertaining
Treatment	−0.01	−0.03	−0.15*	−28.32	−0.49	0.70***	−0.17**
	(0.04)	(0.08)	(0.08)	(20.65)	(1.57)	(0.08)	(0.08)
Constant	0.34***	0.02	0.11*	200.19***	44.75***	−0.47***	0.11*
	(0.03)	(0.07)	(0.07)	(16.89)	(1.29)	(0.06)	(0.07)
Observations	682	682	682	682	682	682	682
R^2	0.00	0.01	0.01	0.01	0.01	0.11	0.01
Gender FE	✓	✓	✓	✓	✓	✓	✓

Notes. Each column shows a different regression testing the effect of being assigned to the experimental group (relative to the placebo group). *Confidence in Police* is an indicator for whether the respondent has at least quite a lot of confidence in police. *Crime Concern* is a Likert scale response for how serious a concern crime is to the respondent converted into standard deviation units. *Spending Preference* reflect how much the respondent wants the government to spend on the police relative to the status quo, converted into standard deviation units. *# Arrests Estimate* is the respondent's guess for the annual number of arrests made by a typical police officer. *Clearance Rate Estiamte* is the respondent's guess for the share of all reported crimes that are cleared by arrests. *Clip Accuracy* and *Clip Entertaining* are Likert scale responses for how accurately the clip represents the profession and how entertaining the clip is, respectively, converted into standard deviation units. All regressions contain gender fixed effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.