



Made available through Montana State University's [ScholarWorks](#)

Effects of Violent Media Content: Evidence from the Rise of the UFC

Jason M. Lindo, Isaac D. Swensen, Glen R. Waddell

© This manuscript version is made available under the CC-BY-NC-ND 4.0 license <https://creativecommons.org/licenses/by-nc-nd/4.0/>

Effects of Violent Media Content: Evidence from the Rise of the UFC

Jason M. Lindo, Texas A&M University, NBER, and IZA

Isaac D. Swensen, Montana State University

Glen R. Waddell, University of Oregon and IZA

March 2022

Abstract

We document the effect of violent media on crime. Specifically, we evaluate the effects of The Ultimate Fighter, a hit TV show that features fighters competing in violent mixed martial arts and which brought Ultimate Fighting Championship into the mainstream. We estimate the effect of exposure to the show's earliest episodes using panel data from police agencies across the United States and a strategy that uses network ratings prior to the show's premier as an instrumental variable. We show that this exposure significantly reduced crime: these effects are particularly evident for assault, began in the month the show premiered, and persisted for many years. These estimates do not reflect systematic differences across geographic areas in their trends in crime rates prior to 2005. To complement our main results, we also investigate the effects of "UFC Main Events," which air in bars and on Pay-Per-View. This analysis additionally suggests reductions in violence caused by viewership.

JEL Codes: K42, L82, L83

* Lindo (jlindo@tamu.edu) is a Professor at Texas A&M University, a Research Associate at NBER, and a Research Fellow at IZA; Swensen (isaac.swensen@montana.edu) is an Associate Professor at Montana State University; and Waddell (waddell@uoregon.edu) a Professor at the University of Oregon and a Research Fellow at IZA. We gratefully acknowledge being included as one of the teams given access to Nielsen data through the NBER-Nielsen Collaboration, led by Andrew Sweeting, Matt Gentzkow, and Jesse Shapiro. Thanks to Dan Feenberg for his invaluable assistance in extracting Nielsen data for our analysis. David Pritchard provided stellar research assistance. We thank Stefano DellaVigna, Matthew Gentzkow, Phil Levine, Patrick Markey, Jesse Shapiro, and Michael Ward for helpful discussions. We also thank the MSU Initiative for Regulation and Applied Economic Analysis for financial support.

1 Introduction

There is a long history of social-science research on the effects of violent media content on anti-social behavior. Laboratory experiments conducted by psychologists, which account for a large share of the rigorous work on this topic, have demonstrated that violent media content increases measures of aggression in the short run.¹ This supports the notion that media violence contributes to social problems, which is often cited as the consensus among professionals.² That said, this type of laboratory-based evidence is unable to account for the fact that, in their every day lives, individuals make choices about the media they consume and their behavior is affected by numerous other personal and contextual factors.³

The fact that individuals typically choose what media they consume is a challenge for researchers who seek to estimate the causal effects of media violence. Reverse causality is a major threat to estimates of the relationship between violent-media consumption and violent behavior, whether they are based on cross-sectional, time-series, or panel data. For this reason, obtaining credible estimates of the *causal* effect of media violence requires an external force that alters the media that individuals consume—a force that is otherwise unrelated to violence. The power of laboratory studies is that the experimenter can play this role. That said, laboratory studies have three key disadvantages: (i) they study the effects of media content that individuals may choose not to consume in their every day life, (ii) they do not observe individuals in their natural setting, and (iii) they typically evaluate subjects only over a very short period of time. The pioneering study of Dahl and DellaVigna (2009) overcomes these challenges by using field data and exploiting variation in the consumption of violent media across the United States and over time that is driven by blockbuster movie releases.

¹ We discuss this literature in Section 2.1.

² In 2000, a joint statement from several professional groups concluded that “viewing entertainment violence can lead to increases in aggressive attitudes, values and behavior.” In 2009, the American Academy of Pediatrics issued a statement that the evidence is sufficiently convincing that violence in the media “represents a significant risk to the health of children and adolescents” and that “the debate should be over.”

³ Some researchers have also criticized the methodology used in laboratory-based studies and meta-analyses based on such studies. For example, see Ferguson et al. (forthcoming).

This earlier work finds no evidence that a violent blockbuster movie increases crime following its release.⁴ In similar spirit, Markey et al. (2015) and Cunningham et al. (2016) examine crime rates around the release of violent video games, finding no evidence that they increase crime in the short run.

Despite the important contributions of these earlier studies, there are many unanswered questions about the effects of media violence on anti-social behavior. Perhaps most important among them is how little we know about the longer-run effects.⁵ This is made more critical as researchers often highlight “incapacitation effects” as a likely channel through which violent media content might reduce violent behavior. Put simply, media consumption reduces violent behavior because it involves the substitution of time towards an activity during which the probability of violent behavior is low (e.g., watching a program) from activities during which probabilities are higher. We therefore pursue our question with an interest in considering the effects of repeated exposure to violent content over a sustained period of time. For example, one could easily imagine a scenario in which consuming violent media initially reduces crime due to incapacitation, but increases crime in the longer run due to the potential psychological effects that are themselves persistent, or which accumulate through repeated exposure.⁶

We take a step towards doing so by considering the effects of early exposure The Ultimate Fighter (TUF), a hit reality-TV show that features fighters training for and competing in violent mixed martial arts bouts. This show marked the first time that such fights were shown on cable TV and has been credited with bringing Ultimate Fighting Championship (UFC) into the mainstream. In a spirit similar to Kearney and Levine’s (2015) study of the effects

⁴ Interestingly, they find evidence that violent-movie releases *reduce* crime in the evening hours when individuals are likely watching the movie and also in the subsequent nighttime hours. They attribute these reductions to an incapacitation effect and reduced alcohol consumption.

⁵ The identification strategies used in Dahl and DellaVigna (2009), Markey et al. (2015), and Cunningham et al. (2016) preclude analyses of longer-run effects.

⁶ For similar reasons, existing evidence from the lab and the field are not necessarily at odds with one another. Lab-based studies are arguably seeking to identify psychological effects on propensities to engage in anti-social behavior *holding context constant*. Field-based studies identify the effects on propensities to engage in anti-social behavior inclusive of effects on context that may result from changes in how individuals choose to spend their time.

of the show 16 and Pregnant on teen childbearing, we exploit variation in TUF viewership generated by viewership patterns prior to TUF’s premier. In particular, we use Spike TV ratings from the Monday time slot in which TUF would later air (which is based on content that is quite different from TUF) as an instrumental variable for a county’s exposure to TUF. Following Kahn-Lang and Lang (2019), we also control for Spike TV ratings across all days and time slots prior to TUF’s premier to address the possibility that differences in Spike TV viewership may correlate with crime trends or levels. We argue that the variation induced by this instrument, combined with this control variable in particular, offers an intuitively appealing research design that leverages idiosyncratic variation in viewership. We also provide empirical evidence to support this idea.

Our results indicate that early exposure to UFC content via the first season of TUF significantly reduced anti-social behavior, as measured by monthly reports of crimes filed by police agencies across the United States. Interestingly, the effects are particularly evident for assaults, which is the crime that is most closely related to the content of the show itself, though we find qualitatively similar effects on rape and on property crimes. Moreover, these effects began in the month the show premiered and persisted for many years afterwards. In support of our causal interpretation of these estimates, we show that the variation we exploit is unrelated to pre-existing crime trends *and levels*. We also find estimates very close to zero in “placebo tests” for effects prior to TUF’s premier. Further, using hourly crime data from police agencies across the United States, we provide evidence that similar content depresses violent crime in an analysis of “UFC Main Events,” which air in bars and on Pay-Per-View.

While we have no way of determining exactly which mechanisms explain our main results, our analysis of “UFC Main Events” is helpful towards this end in the sense that it demonstrates reductions in violent crime during the time when individuals were consuming UFC-related content. Naturally, our main results may be driven in part by reductions in crime when individuals were watching new episodes of TUF. However, we would emphasize that the explosion of UFC’s popularity caused by TUF leads us to believe that our estimates likely

capture much broader impacts on time-use. In particular, we think of TUF viewership as something that led to more-general interest in UFC-related content among its viewers and their peers. As such, the effects of TUF we document may be explained by direct effects and/or peer effects on time-use outside of the time when new episodes of TUF aired (e.g., watching re-runs on Friday nights and weekends, watching UFC content available from video rental stores, watching UFC Main Events on Saturday nights, or learning/practicing martial arts).⁷ Separately or in conjunction with any such time-use effects, our results may also be explained by changes in the underlying psychological characteristics that affect an individual’s propensities to engage in violence (or crime) in a given activity. For example, there could be “catharsis” or “relief” effects whereby viewing UFC content (or engaging in related activities) leads to temporary reductions in propensities to engage in anti-social behavior (for individuals with preferences for such behavior).⁸ It is also possible that preferences could be altered by the UFC’s glorification of violence by its realistic portrayal of serious injuries that can result from physical altercations.⁹

In addition to contributing to the literature on the determinants of anti-social behavior, our study also contributes to the growing literature on the real-world impacts of television-media content. In one of the first rigorous studies in this literature, Gentzkow (2006) shows that the introduction of television accounts for up to half of the decline in American voter turnout since the 1950s. More-recent studies have found that some types of television exposure improve children’s outcomes (Gentzkow and Shapiro, 2008; Kearney and Levine, forthcoming; Cornelson, 2018) whereas other types of television impair children’s outcomes

⁷ It is quite possible that various amateur and professional fighting organizations have experienced rising popularity as a result of TUF and UFC, or that they have been created in part because of such heightened interest. Perhaps most notably, the North American Allied Fight Series, which almost exclusively held events in Ohio for a decade, held its first event in September of 2005 and Belator had its first event in 2009.

⁸ Researchers have similarly implicated catharsis or relief effects in suggesting that pornography could reduce sex crimes (Donnerstein et al., 1975; Posner, 1992).

⁹ Such effects could affect all forms of violence in which individuals’ behaviors are determined to some degree by a weighing of costs and benefits (Becker, 1968). Violence is often categorized as instrumental violence motivated by a desire for personal gain, virtuous violence in the service of a societal moral good (Ginges and Atran, 2009; Fiske and Rai, 2014), and impulsive violence which involves a loss of self-control (Dollard et al., 1939; Baumeister and Heatherton, 1996; Card and Dahl, 2011).

(Hernæs et al., forthcoming), that television access affects voters’ preferences (Enikolopov et al., 2011; Ellingsen and Hernæs, 2018), and that specific types of media content can have significant impacts on birth rates (La Ferrara et al., 2012; Trudeau, 2016; Kearney and Levine, 2015).¹⁰ To our knowledge, our study is the first to document the causal impacts of television content on crime.¹¹

2 Background

2.1 Violent media content and behavioral outcomes

That witnessing violence can increase anti-social tendencies is often thought of through the lens of social learning theory (Bandura, 1977). In this developmental framework, individuals observe the behaviors of others (referred to as models) and consider imitating what they observe based on their own characteristics, the characteristics of the models, and the observed outcomes of the behavior. As a framework for understanding violence, social learning theory finds support from decades of research. While also implicating the roles of parenting, peers, and role models, for example, this literature also includes studies that document correlations between aggression and violent media content. Though the largest part of this literature focuses on violence in television and film, recent efforts have focused on violent video games, often citing concerns surrounding the dramatic increases in time spent on gaming and the intense violence that exists in many popular video games.¹²

¹⁰ For a survey of the literature on the impact of exposure to the media see DellaVigna and La Ferrara (2015).

¹¹ Studies examining the impacts of other forms of media content have found that it can incite racial violence. For example, Yanagizawa-Drott (2014) shows that radio programming in Rwanda calling for the extermination of the Tutsi minority had a significant impact on participation in killings by militia groups and ordinary civilians; Adena et al. (2015) shows that Nazi radio propaganda incited anti-Semitic acts by ordinary citizens; and Ang (2020) shows that the fictional portrayal of the KKK in the film *The Birth of a Nation* caused lynchings and race riots in the United States in the early 20th century. Internet expansion has also been shown to influence sex crime in Europe (Bhuller et al., 2013).

¹² This sub-area of study also includes experimental and survey evidence on the internet, music videos and lyrics, television news, initial television access, and other forms of media. Additional studies explore heterogeneity by viewer characteristics, social environments, and various forms of violent media content

In cross-sectional and longitudinal survey data, the measurable correlation between viewing violent content and violent, aggressive, or otherwise anti-social behavior is consistently positive (Eron et al., 1972; Johnson et al., 2002; Anderson et al., 2003; Gentile et al., 2004; Anderson et al., 2010). Importantly, though, this correlation has multiple contributors, one can imagine, some of which are sure to include latent preferences for violent content among individuals with anti-social tendencies, or confounding factors associated with both the viewing of violent media and anti-social behaviors—we think of socio-economic factors, neighborhood characteristics, peer effects, or parental influences, for example. As such, it is unlikely that these studies offer valid estimates of causal effects.

That said, existing laboratory studies employing randomized controlled trials offer evidence that viewing violent content *increases* aggressive tendencies.¹³ Such studies typically take place in laboratories or schools where subjects are randomly assigned varying degrees of exposure to violent content via a short video, film, or through active participation in a violent video game. After being exposed to the content, subjects are asked to interact with each other. While earlier studies allowed direct physical aggression among youth after exposure to violent content (Parke et al., 1977; Josephson, 1987), more-recent studies focus on less-harmful or alternative measures of anti-social tendencies. For example, these include white-noise shocks, electric shocks, physiological arousal, empathy towards others, verbal aggression, and stated attitudes toward violence and hostility (Anderson et al., 2003, 2010; Greitemeyer and Mügge, 2014). To be clear, outcomes in experimental settings are typically far removed from violent crimes such as assault, rape, and murder. As such, laboratory studies provide limited insight into the extent to which violent media affects more serious forms of violent behavior.

Another limitation of laboratory studies is that they can only measure effects over a short time horizon, typically the length of time that subjects are observed in the lab. Likewise, the typical experiment measures immediate effects shortly after a single exposure, and even

within each genre (Anderson et al., 2003).

¹³ We note that the robustness of this evidence has been called into question based on concerns about replicability, publication bias, and study design. For example, see Ferguson et al. (forthcoming).

experimental studies that involve cumulative exposure are still limited to relatively short windows. The degree to which repeated exposure affects behavior over a longer timeframe remains an important outstanding question.

As a whole, though they have likely overstated these effects (Hilgard et al., 2017), these laboratory studies seem to provide compelling evidence that viewing violent media can increase aggressive anti-social tendencies. This has led some researchers and health organizations to make strong statements, concluding that “the debate is over” regarding the effects of violent media content.¹⁴ That said, experimental analyses in the lab do not replicate real-world choices to view media content, choices that may be influenced by alternative activities, content-specific contextual factors, possible coping mechanisms, and wide-ranging content-specific preferences over media consumption. In short, the controlled approach that lends to a causal interpretation of results obtained in the lab, and which allows researchers to measure psychological characteristics, may limit the generalizability of the findings to real-world effects of violent-media consumption on behavior.

Attempting to fill this critical gap in the literature and overcome many of the limitations of the lab, Dahl and DellaVigna (2009) uses a quasi-experimental approach to analyzing violent crime around blockbuster movie releases. They find that blockbuster movies with relatively high violent content decrease assaults the night of the movie, with especially large declines in the hours following the conclusion of normal showtimes. Exploring mechanisms, they provide evidence that incapacitation reduces assaults during the movie, and substitution away from alternative activities (e.g., going to the bar), likely drive the larger reduction in assaults after the movie. These results highlight that the effects of choices to view violent content in real-world contexts may be very different from the effects found in laboratory settings. Notably, Dahl and DellaVigna (2009) finds no effects in the three weeks following exposure, though they acknowledge that their empirical approach is not well suited to estimate

¹⁴ See, for instance, Anderson et al. (2003) and Bushman and Anderson (2015), and the 2000 joint statement from the American Academy of Pediatrics on the Impact of Entertainment Violence on Children.

longer-run effects. It is also not well suited to estimating the effects of repeated exposure to violent content. In contrast, the treatment we consider (early exposure to UFC content) is understood to have generated the sort of fandom that entails frequent repeated exposure over a sustained period of time.

Also offering evidence on the effects of violent media content, Cunningham et al. (2016) uses an instrumental-variables strategy to estimate the effect of popular video-game releases on violent crime. With time-series variation in retail sales of the 30 most-popular video games, they instrument for sales with measures of quality and weeks on the market, ultimately describing their results as indicating “no evidence of an increase in crime associated with video games and perhaps a decrease.” Markey et al. (2015) provides suggestive evidence that violent video games reduce violent crime using variation in video game release dates, video game sales, and keyword searches for violent video game guides.¹⁵ As with Dahl and DellaVigna (2009), these estimates are *not* consistent with the idea that violent media content increases violent behavior, and thus further suggest a tension between the positive effects found in the large lab-based literature and negative effects found in quasi-experimental studies.

As with most studies that evaluate the behavioral effects of individual choices to consume violent media content, a key limitation of our work is that we cannot determine whether observed effects are driven by the sorts of psychological mechanisms that have been the focus of lab-based research or whether they are the effects of changes in time-use. By considering the paths of criminal behavior coming out of the “TUF natural experiment” for a longer period of time than is typical, we at least hope to encourage further investigation into what might cause such persistently different paths. Ultimately, separately identifying confounders here can only be mitigated with richer data on time use and psychological characteristics.¹⁶

¹⁵ In a related context, Ward (2011) finds lower growth in crime rates in counties with relatively high rates of growth in video-game stores. Ward (2199), instrumenting for own video-game playing with peer video-game playing, finds no support in Add Health surveys for a causal relationship running from video game playing to aggressive outcomes.

¹⁶ Dahl and DellaVigna (2009) infers the “voluntary incapacitation effects” based on the fact that violent

2.2 The Ultimate Fighter and the rise of the UFC

It's amazing to think how close we came to not being here today. If it weren't for what these guys did, I don't know if there would even be a UFC.

-UFC President, Dana White, at the *The Ultimate Fighter* "Season 1 Reunion" three years after it aired.

The Ultimate Fighter (TUF) is widely credited as having brought mixed-martial arts, and the Ultimate Fighting Championship (UFC) in particular, into the mainstream. The UFC began organizing and promoting mixed-martial-arts events, which aired on Pay-Per-View, in the early 1990s. These fighting tournaments had minimal rules, no weight classes, took place in a cage, and were famously referred to as "human cockfighting" by Senator John McCain in 1996 who lobbied to have the events banned. As a result, the events were banned in every state with an athletic commission, making it difficult for them to be held in major markets. During this period of time, UFC held events in Iowa, Mississippi, Louisiana, Wyoming, and Alabama. In 1998 the UFC lost its Pay-Per-View distribution. The UFC subsequently modified its rules and eventually secured sanctioning from California and New Jersey in 2000, thus allowing the company to hold events in these states under the supervision of their athletic control boards. Despite these steps towards legitimacy, the UFC was sold for just \$2 million in January 2001.

New ownership did not lead to an immediate turnaround for UFC. UFC returned to Pay-Per-View in late 2001 but events averaged only 150,000 Pay-Per-View buys in the following three years. By 2004, the new ownership group had lost \$34 million since 2001. The ownership group subsequently doubled down on their investment in UFC, spending \$10 million dollars to produce the reality TV show, *The Ultimate Fighter*.¹⁷

TUF premiered on a Monday night (January 17, 2005) on Spike TV, which was promoted as "The First Network for Men." New episodes aired weekly on Monday nights (11pm-midnight) and each new episode re-aired the following Friday night (midnight), Saturday

movie releases significantly increase time spent in theaters and the fact that individuals are extremely unlikely to commit crimes while attending a movie.

¹⁷ Previously, the same ownership group had success in promoting their casinos through the reality TV series, *American Casino*.

evening (7-8pm), and Sunday evening (5-6pm). New episodes aired in the time-slot following the wrestling entertainment program, WWE Raw. The Spike TV content airing in November 2004 was for the most part quite different from TUF, which is desirable because we would question whether additional violence was actually a part of the “treatment” if TUF was merely replacing something similarly violent. In particular, the shows airing in November 2004 in what would later become TUF’s Monday time slot included a comedy show featuring comedic actors narrating footage from a 1980s Japanese game show featuring regular people attempting (and almost always failing) to navigate an obstacle course (Most Extreme Elimination Challenge, five times), a professional wrestling show (WWE Velocity, one time), a reality show about young men quitting their jobs to pursue their dreams (I Hate My Job, two times), a show about a video game (Need for Speed: Underground 2, one time), and a reality show featuring a video game superfan (Ultimate Gamer, one time). Between November 2004 and the premier of TUF, the shows airing in this time slot were Most Extreme Elimination Challenge, Ultimate Gamer, 2004 Video Game Awards, The World is Not Enough (1999 James Bond film), and Most Amazing Videos.

That said, it might be tempting to suggest that treatment here is merely one additional hour or so of violent content each week. However, we see the advent of UFC on Spike as a significant inducement mechanism into exposure to violence and inducing a scale of fandom that is still impressive. In the two years after the show aired on Spike TV, UFC’s revenue increased 1,258%, including a 1,700% increase in Pay-Per-View sales.¹⁸ The media’s representation of the period is also decidedly one sided, with claims such as “Nothing really goes back to ‘normal’ after The Ultimate Fighter. Not Spike. Not the UFC.”¹⁹ We return to this point below.

What may have made the difference for the TUFs success is that it followed a format

¹⁸ Source: <https://www.theguardian.com/sport/2016/mar/04/the-fight-game-reloaded-how-mma-conquered-world-ufc>

¹⁹ Source: <https://www.bloodyelbow.com/2017/2/14/14600570/inside-the-ultimate-fighter-why-the-reality-show-was-is-so-important>

similar to other reality shows, bringing together contestants to live and interact in the same residence under nearly constant surveillance. TUF content is distinct from other reality shows in its focus on fighters training for and participating in violent and often brutal mixed-martial-arts bouts that occur in each one-hour episode as a part of a season-long tournament for a “six-figure UFC contract.” These bouts as well as the preceding training and sparring display frequent injuries, blood, choking, and similar content typical of mixed-martial-arts events where the goal is to knock out or submit your opponent.²⁰ It also features UFC stars as coaches and guests.

TUF was considered an instant ratings success, as 1.7 million viewers tuned into the initial episode. This represented a 36 percent increase over the shows that aired on the same day and time slot in the previous year. The show continued to be popular throughout the season and ultimately drew three-million viewers to its finale. TUF was considered Spike TV’s first hit show and led to the production of UFC Unleashed, which aired fights from past UFC events, UFC Fight Night, which aired fights live, and UFC Countdown specials, which promoted upcoming events that were scheduled to air on Pay-Per-View. Naturally, it also spawned many subsequent seasons of TUF—two annually—and is now in its 27th season. As indicated by the UFC President’s quote at the beginning of this section, TUF is widely viewed as a turning point for UFC’s popularity. A Yahoo Sports article describes TUF as having “broke the UFC through to the masses.”²¹

²⁰ While TUF centers on the fights, it also features the backgrounds of fighters in addition to drama, aggression, and conflict among competing fighters living together. While the structure of the show varies over its 27 seasons, the usual format divides between 14 and 32 contests into two teams as they live together, train together, and compete in fights. Each episode features at least one fight, the loser of which is eliminated from the tournament while the winner advances to the next round of fights. TUF has sparked careers for a large number of fighters, including many who did not win the competition. As such, TUF fights are central to the show not just because they determine who wins the competition in a given season, but also because they provide a unique opportunity for participants to earn recognition and credibility from UFC executives and a national television audience which can promote their professional careers. Eighteen TUF participants (including seven who did not win their TUF season) have fought for a title and eight have become UFC champions according to <https://tvtropes.org/pmwiki/pmwiki.php/Series/TheUltimateFighter>. The coaches for each team are typically UFC star fighters who are scheduled to fight against each other in the featured bout in the UFC Pay-Per-View event following the conclusion of the TUF season. These fights are heavily promoted as the TUF season finale approaches.

²¹ <https://sports.yahoo.com/ufcs-25th-anniversary-look-25-slideshow-wp-080028231/photo-p-2005-ultimate->

This sentiment appears to be borne out based on an analysis of UFC’s Pay-Per-View events, which continued to be held intermittently throughout the year, and which individuals could pay to view through their cable provider. In Panel A of Figure 1 we report annual Pay Per View buy-rates (across all events), which ranged between 278,000 and 415,000 between 2002 and 2004, doubled in 2005, and then exploded to more than five million for many years thereafter.²²

An analysis of Google searches between 2004 and the end of 2016—the results of which are shown in panels B and C of Figure 1—also demonstrate the sizable and sustained interest in UFC that followed the premier of TUF. Indeed, search activity for UFC grew after the TUF premier such that it was comparable to searches for Oprah and Beyonce by the middle of 2006, and such that it outstripped search activity for CSI, which was the highest rated television show when TUF premiered and which was typically among the highest rated shows throughout the 2000s. In contrast, peak search activity for the show *16 and Pregnant*, which has been credited with reducing the United States teen birth rate by 4.3 percent in the 18 months after it began airing in 2009 (Kearney and Levine, 2015), is an order of magnitude lower. Searches for UFC also grew to be comparable to searches for other cultural touchstones appealing to young men, including *Call of Duty* (the highest grossing video-game franchise in the first-person shooter genre of all time) and the NHL (the fourth most searched professional sport in the United States) while usually outstripping searches for LeBron James and Tom Brady (who were arguably the most popular athletes in the NBA and NFL, respectively, during the 2000s). As a whole, the results shown in Figure 1 highlight the cultural importance of the UFC, which gives credence to the notion that the show responsible for its popularity (TUF) may have had significant impacts on outcomes such as crime.²³

fighter-reality-photo-080028393.html.

²² Data available from <https://thesportsdaily.com/2018/02/16/all-time-ufc-ppv-sales-data-fox11>.

²³ Based on survey data from 2012, interest in mixed martial arts was highest among young men at the time, but also quite substantial among and older age groups. For men, 67 percent of those aged 18-34 report being fans, versus 49 percent of those aged 35-54 and 28 percent of those aged 55+. For women, 44 percent of those aged 18-34 report being fans, versus 28 percent of those aged 35-54 and 15 percent of those aged 55+. Though other less-prominent organizations featured female fighters, the UFC did not do so until 2013. See <https://thesportsdaily.com/2018/02/16/all-time-ufc-ppv-sales-data-fox11>.

To investigate the possibility that the sharp increase in UFC interest beginning in 2005 may be spurious, perhaps reflect increasing interest in violent media content more broadly, or that TUF may have generated spillover effects that expanded interest in related content, we have conducted similar analyses of WWE “professional wrestling” which Wikipedia defines as a “form of wrestling and performance art involving matches whose progress and outcome are planned in advance, typically between performers with established character roles.”²⁴ The results of these analyses are shown in Appendix Figure A1. Pay-Per-View buy-rate data indicate that interest in WWE fell from 2002 to 2004, rose from 2004 to 2005, then fell fairly steadily thereafter.²⁵ As such, 2002-to-2004 changes are not consistent with the notion that TUF premiered in the midst of a secular increase in interest in related content. The 2004-to-2005 increase *could* reflect this sort of phenomenon, but could also reflect spillovers from heightened interest in TUF and UFC or something else. Notably, WWE buy-rates fell fairly steadily thereafter, in contrast to UFC buy-rates which again jumped dramatically in 2006 and then rose again from 2007 to 2010. The results from our analysis of Google searches—note that these are monthly instead of annual aggregates—also shows some evidence of increased interest in WWE 2005. However, it actually fell at the *beginning* of 2005 when TUF premiered and search interest for UFC grew. As such, these provide further support for the popular idea that TUF (as opposed to changing interest in related content) was

[//www.sportsbusinessjournal.com/Journal/Issues/2012/10/01/Research-and-Ratings/Up-Next.aspx](http://www.sportsbusinessjournal.com/Journal/Issues/2012/10/01/Research-and-Ratings/Up-Next.aspx). Based on survey data from 2021, interest in UFC is also widespread across race and ethnicity. Specifically, 34 percent of white people report being a casual or avid fan of UFC, versus 58 percent for Black people, 45 percent for Hispanic people, and 50 percent for those reporting other race or ethnicity. Similar patterns are evident for avid fandom. See <https://www.statista.com/statistics/1122545/ufc-interest-ethnicity/>. Moreover, UFC and TUF have long featured successful fighters from a wide range of backgrounds. The first UFC event featured four African American fighters. TUF Season 1 included two African Americans amongst the 16 competitors, including Josh Koscheck who was prominently featured and went on to have an extensive career in the UFC, including a title bout in 2010, despite losing in the TUF semi-final to the eventual winner. Many of the most famous UFC fighters of all time are Mexican Americans, including its first middleweight champion (Frank Shamrock), and many other champions, including Tito Ortiz, Cain Velasquez, and Carlos Condit. Moreover, the TUF Season 1 winner was Diego Sanchez, a Mexican American.

²⁴ WWE stands for “World Wrestling Entertainment” and was previously called WWF, which stands for “World Wrestling Federation.” Recall that the television program WWE Raw aired in the time-slot prior to TUF.

²⁵ Data on these buy-rates are from <http://www.2xzone.com/wwe/buyrates.shtml>.

responsible for increases in interest in UFC content after its premiere.²⁶

3 Data

In order to measure TUF viewership, we use data collected by Nielsen, the primary source for measuring exposure to television content in the United States. Nielsen collects show ratings data from an array of sources including diaries, in-home metering and audio-recording meters worn by participating individuals. They do so primarily during specific “sweeps months” of the year, not throughout the entire year. We use their county level data on: viewership of new episodes of Season 1 TUF airing Monday nights in February 2005; Spike viewership in the same time slot in November 2004 (prior to TUFs premier); and overall Spike TV viewership measured during November 2004. Our data extracts from Nielsen allow us to calculate ratings for 55 percent of the United States population.²⁷

Our main source for crime data is the “Offenses Known and Cleared by Arrests” segment of the Uniform Crime Reports (UCR), which are a compilation of monthly crime statistics contributed to by law-enforcement agencies across the United States to the FBI.²⁸ We focus on known offenses in order to capture crimes that come to the attention of law enforcement, as opposed to restricting crimes to those that have been cleared by arrest. We make this choice out of concern for the possibility that police-officer behavior may also be affected by TUF viewership, in which case arrests may be affected independently of impacts on crime. UCR data also include annual estimates of the populations covered by each agency, which we use to calculate agency specific crime rates, per 10,000 residents. We restrict our sample to

²⁶ The same figure also shows search intensity for the video game Call of Duty. This time series exhibits spikes that coincide with new game releases. It also shows declining search intensity leading up to and during the first season of TUF.

²⁷ Due to the structure of the data request process, which required us to identify specific networks, dates, and time periods, and because the show aired at slightly different times in different time zones, we do not have accurate measures for all of these variables outside of the Pacific, Central, and Eastern time zones. Data are also missing for counties from which Nielsen did not collect data in a given sweeps period.

²⁸ The UCR Offenses Known data used in this study were collected and compiled by the Inter-University Consortium for Political and Social Research (ICPSR).

municipal police agencies and agency-years with 12 months of submitted crime reports.²⁹

We then link agency-by-month UCR data with county level Nielsen ratings data, keeping agencies for which these Nielsen data are available.³⁰ Note that municipal agencies are nested within counties. Nonetheless, we do not aggregate the agency level data to the county level, because agencies in a given county do not necessarily report data for all (or the same) years—changes over time within county-aggregated data can therefore reflect variation in the composition of contributing agencies. Employing agency level data (along with our inclusion of agency fixed effects in regression models) allows us to avoid this problem, while allowing for clustering at the county level when estimating standard errors accounts for errors that may be correlated across agencies within a county.³¹ The resulting dataset includes monthly crime statistics from 8,750 municipal agencies in 41 states, spanning 2001–2016. In Table 1 we present summary statistics for this sample, with breakdowns of violent crime (assault, rape, murder) and property crimes (theft, motor-vehicle theft, robbery).

4 Empirical strategy

Our identification strategy exploits the fact that people exhibit some degree of habit persistence in television viewing, and this habit persistence causes some individuals to watch TUF when it began airing who otherwise would not have watched the show (perhaps until learning about it later). To fix ideas, consider someone who frequently watches Spike TV on Monday nights in 2004. We would expect this person to be more likely than others, all else equal, to watch TUF when it begins airing in January 2005 on Spike TV on Monday nights. This is the sort of variation we seek to exploit, in a spirit similar to the Kearney and Levine (2015) analysis of the effect of MTV’s 16 and Pregnant on teen childbearing using “pre-treatment” MTV

²⁹ As we discuss in greater detail below, we have investigated estimates based on various subsets of this data set that are aimed at having a more balanced panel, the results of which are similar to our main results.

³⁰ Ratings data is available for 74 percent of agencies.

³¹ For an extensive discussion of these issues with county level UCR data, see Maltz and Targonski (2002).

viewership in the relevant time slot as their instrument.

Just as 16 and Pregnant was very different from the content that made up Kearney and Levine’s instrument, which helps to address concerns about selection bias, the Spike TV content airing in November 2004 was for the most part quite different from TUF (as we described in Section 2.2). That said, a skeptical reader may still be concerned that crime propensities are correlated with watching Spike TV on Monday nights in 2004. After all, men are more likely to commit crimes than women and Spike TV was promoted as “The First Network for Men.”³²

For this reason, we focus specifically on variation driven by 2004 Spike TV viewership in the relevant time slot (in which new episodes of TUF would later air) *controlling for overall 2004 Spike TV viewership*. As such, the thought experiment is a comparison of individuals who watch the same amount of Spike TV in 2004 overall, but one set of individuals typically watches in the (future) TUF time slot while the other set of individuals watches at other times. As a result of habit persistence, initial rates of TUF viewership will be higher for the first set of individuals than for the second set of individuals—that is, there will be a “first-stage” effect on early exposure to TUF. Under the assumption that crime propensities are the same for the two sets of individuals, we could estimate the effect of early exposure to TUF by comparing crime rates across the two groups after TUF begins airing, and inflating this reduced-form estimate by the estimated first stage. With data on crime rates prior to TUF, however, we can relax this assumption by comparing *changes* in crime rates across the two sets of individuals. The validity of this approach rests on the common trends assumption generally required by difference-in-differences designs.

To operationalize this strategy, data availability requires us to use county level ratings

³² It is along these lines that Jaeger et al. (2016, 2020) critique the Kearney and Levine (2015) analysis of the effects of MTV’s 16 and Pregnant. Specifically, Jaeger *et al.* highlight that there are significant level differences in outcomes associated with the instrument and that there is also evidence of differential trends in outcomes associated with the instrument. In response, Kearney and Levine (2016) makes the case that evidence of differential trends during an extended pre-treatment period does not imply that there are differential trends over the relevant period of time.

data and crime data from police agencies that are nested within counties. As such, we cannot think about these data as offering the two convenient sets of individuals described in the thought experiment outlined above. Nonetheless, the same sort of variation can be exploited via two-stage least squares.

While we calculate two-stage least squares estimates—and standard errors associated with these estimates—in the standard manner, here we discuss the first- and second-stages to convey the intuition behind this approach. The first-stage regression equation corresponding to our two-stage least squares estimates is:

$$RateTUF_{ac} = \psi_0 RateTimeSlot04_{ac} + \psi_1 RateSpike04_{ac} + u_{ac} , \quad (1)$$

where $RateTUF_{ac}$ is the Season 1 TUF rating for agency a 's county c , $RateTimeSlot04_{ac}$ is the 2004 rating for the time slot in which TUF would later air (the excluded instrument), and u_{ac} is a random error. $RateSpike04_{ac}$ is the 2004 rating for Spike TV across all days and time slots—prior to TUF's premier—which addressees the possibility that differences in Spike TV viewership may correlate with crime trends or levels. Moreover, controlling for overall viewership also frames the interpretation of estimated treatment effects more precisely. By identifying the effects only off of *independent* variation in viewership in the future TUF time slot, the inducement toward violent content is therefore from a conglomeration of the shows that aired in the same time slot before TUF premiered in 2005.³³

The second-stage regression equation illustrates how variation in $\widehat{RateTUF}_{ac}$, the predicted Season 1 TUF rating, is used to identify the effects of TUF viewership on crime in the 40 quarters following its premier (and to produce placebo estimates for the eight quarters prior

³³ Recall, these shows include Most Extreme Elimination Challenge, WWE Velocity, I Hate My Job, Need for Speed: Underground 2, Ultimate Gamer, Most Extreme Elimination Challenge, the 2004 Video Game Awards, The World is Not Enough, and Most Amazing Videos. See Section 2.2 for more background discussion.

to its premier). Specifically, the second-stage equation is

$$\begin{aligned}
y_{act} = & \sum_{j=-8}^{40} \beta_j \mathbb{1}(QuartersFromPremier_t = j) \widehat{RateTUF}_{ac} + \\
& \sum_{j=-8}^{40} \theta_j \mathbb{1}(QuartersFromPremier_t = j) RateSpike04_{ac} + \\
& \theta_{ac} + \delta_t + \epsilon_{act} ,
\end{aligned} \tag{2}$$

where y_{act} is the natural log of the crime rate for police agency a in county c at time t , $\widehat{RateTUF}_{ac}$ is the first-stage predicted Season 1 TUF rating, $RateSpike04_{ac}$ is defined the same way as in Equation (1), θ_{ac} are agency fixed effects that capture any fixed differences across agencies with time-invariant impacts on crime, δ_t are time-period fixed effects that capture any time-varying shocks to crime that are common across agencies, and ϵ_{act} is a random error term.³⁴

Without the $RateSpike04_{ac}$ interaction terms, Equation (2) would be identical to the two-way fixed effects model that is commonly used for circumstances in which researchers seek to exploit variation in treatment intensity to estimate treatment effects under a difference-in-differences-like identifying assumption: that the variable generating treatment intensity ($RateTimeSlot04_{ac}$) is unrelated to trends in the outcome. By including $RateSpike04_{ac}$ interaction terms in the model, we relax this assumption. Specifically, these interactions capture the expected changes in crime rates over time for areas with different levels of overall 2004 Spike TV viewership. As such, the inclusion of this term makes it such that our estimates are identified by comparing changes in crime for areas with relatively high ratings in the TUF time slot before it aired to changes in crime for counties with relatively low ratings in

³⁴ For simplicity, we refer to the outcome variable in the text as the log of the crime rate. However, to address the fact that the number of crimes may be zero in some instances, making the natural log undefined, we transform counts using the inverse hyperbolic sine transformation. As such, the outcome variable is defined as $\ln(\frac{count + \sqrt{count^2 + 1}}{population})$. We take this approach in order to use all of the data but it has little influence on the resulting estimates because the observations for which it is relevant are given little weight in our (WLS) regressions. Moreover, the results are similar extremely similar if we use alternative transformations.

the TUF time slot before it aired *adjusted for the differences in crime that are expected over time across areas with different levels of overall 2004 Spike TV viewership*.

We take this approach, additionally controlling for $RateSpike04_{ac}$ interaction terms, to address potential concerns that there may be differences in trends associated with 2004 Spike TV ratings for the relevant time slot. It is motivated by Kahn-Lang and Lang (2019) discussion of the exchange between Kearney and Levine (2015, 2016) and Jaeger et al. (2016, 2020), which highlights the importance of examining what factors might explain differences in pre-treatment outcome levels *even if such differences are differenced out in a difference-in-difference design*. Specifically, they argue: “[i]f we understand why the experimental and control groups differ in levels, we may better understand whether to anticipate common or divergent trends.” They go on to explain that we can be relatively “confident that we have solved the problem with nature’s design of the experiment” if we are able to identify a control variable that eliminates initial differences in outcomes (as well as any differences in pre-treatment outcome trends) associated with treatment. In the next section, we show how controlling for Spike TV’s overall rating in 2004 accomplishes this in our setting.

A few additional details of our analysis are worth mentioning. First, in order to improve efficiency we use weighted-least squares, with agency jurisdiction populations as weights. Second, the standard-error calculations allow errors to be correlated within counties over time.³⁵ Third, in addition to the disaggregated event-study type of model described here, we also estimate a more parsimonious model to summarize the estimated effects.

³⁵ Because the police agencies we consider are nested within counties, this implies that we are also allowing errors to be correlated across police agencies in the same county. Our choice to cluster the standard errors at the county level is motivated by the fact that the variation we exploit is at the county level and a desire to address “design-based uncertainty” with regards to the natural experiment we exploit (Abadie et al., 2020). Because the outcomes we analyze (crime rates) are based on all known crimes rather than a sample of crimes, we anticipate little “sampling-based uncertainty” in our data, driven only by the fact that population counts are typically measured with error. Nonetheless, we have also estimated standard errors allowing for clustering at the Nielsen Designated Market Areas (DMA) level, which combine counties into roughly 200 areas. This approach, which would accommodate errors that are correlated within each DMA, produces extremely similar estimates.

5 Evidence for the validity of the research design

In this section we begin by showing that we have a strong first stage and then show evidence to support the exclusion restriction required by our instrumental variables empirical strategy.

5.1 First-stage estimates

In Figure 2 and Table 2 we demonstrate the strength of the first stage. Specifically, the first graphic in Figure 2 plots Season 1 TUF ratings (measured in February 2005) against Spike TV ratings for the same time slot before TUF premiered (measured in November 2004), with market sizes that vary with the population of the area it represents. The weighted-least-squares estimate for this relationship, shown in Column (1) of Table 2, indicates that a one-percentage-point higher rating in the relevant time slot before TUF premiered is associated with a 0.62 percentage-point higher TUF rating.

As we discussed in detail in Section 4, this relationship may simply reflect relatively high rates of TUF viewership in areas where a relatively large share of the population watched Spike TV prior to TUF’s premier. This is not the sort of variation we seek to exploit. Our thought experiment is a comparison of individuals who watch the same amount of Spike TV prior to TUF’s premier, but one set of individuals typically watches in the (future) TUF time slot while the other set of individuals watches at other times. The aggregate-data analogue is to exploit differences in pre-TUF ratings across days and times among areas with the same overall Spike TV ratings.

The second graphic in Figure 2 depicts this variation, by plotting Season 1 TUF ratings adjusted for pre-TUF overall Spike TV ratings against Spike TV ratings for the same time slot before TUF premiered also adjusted for pre-TUF overall Spike TV ratings. While a bit weaker than the relationship depicted in the unadjusted plot, it again shows a strong positive relationship that is additionally evident in the weighted-least-squares estimate shown

in Column (2) of Table 2. This first-stage estimate indicates that a one percentage-point higher rating in the relevant time slot before TUF premiered—holding overall Spike TV ratings constant—is associated with a 0.52 percentage-point higher TUF rating (with an F-statistic of 40). While we cannot determine whether this is due to habit persistence or advertising, this estimate supports our hypothesis that TUF would have relatively high rates of initial viewership in areas where a relatively large share of the population watched Spike TV in its time slot before it premiered even after controlling for overall Spike TV ratings.

5.2 Exclusion restriction, common trends, common levels

Given our difference-in-differences identification strategy, in order to be valid the instrument must be unrelated to changes in violent crime following TUF’s premier that would be expected in the absence of the show (conditional on covariates). Given that we are conditioning on overall Spike TV ratings, our instrument can be thought of as capturing the degree to which residents of an area tended to watch Spike TV in the future TUF time slot versus watching Spike TV at other times among areas where residents watch the same amount of Spike TV overall. While we cannot say that this is random, we believe it is the sort of idiosyncratic difference that makes for a convincing instrument if it can also hold up to closer scrutiny.

As a first pass at examining the degree to which this variation appears to be “as good as random,” in Figure 3 we show a map of the United States that depicts this variation. It demonstrates significant variation all across the country—importantly, it does not appear as if the instrument takes particularly large values in any particular region, or in places that we would expect to have particularly high (or low) crime rates or trends in crime rates.

We more formally examine the degree to which the variation we exploit is related to pre-TUF crime rates in Figure 4. Specifically, in Panel A we focus on 2004 log of violent crime rates and in Panel B we focus on the change in the log of violent crime rates from 2001 to 2004. Although the graphics labeled “Unadjusted” show that the levels and the

trends of our outcome measure are positively related to pre-TUF Spike TV ratings in the relevant time slot, the graphics labeled “Adjusted” demonstrate that controlling for pre-TUF Spike TV ratings overall eliminates these relationships. In other words, the slope coefficients and standard errors provided in the figure reveal statistically significant estimates in the “Unadjusted” plots, while the “Adjusted” plots reveal no such significance. Using the language of Kahn-Lang and Lang (2019), that we have a control variable that eliminates both the initial difference and differential trends allows us to feel “confident that we have solve[d] the problem with nature’s design of the experiment.” Thus, it provides support for the untestable assumption that the variation we exploit is unrelated to the crime trends that would have been observed in the absence of TUF.³⁶

5.3 Main results

Having provided evidence in support of the research design, we now present estimates of the causal effects of early viewership of The Ultimate Fighter (TUF)—early in time, not youth—in order to speak to the effects of exposure to violent media content on crime. We exploit variation in TUF viewership generated by geographic differences in viewership *prior* to TUF’s premier, and its role in explaining the variation in first-season viewership. Specifically, we use Spike TV ratings in the same Monday time slot in which TUF would later air as an instrumental variable for a county’s early exposure to TUF.

In Figure 5 we plot second-stage coefficient estimates from the event-study type of specification corresponding to our identification strategy, evaluating the effects on violent crimes.³⁷ Specifically, for each quarter we plot separate estimates of the effect of TUF on

³⁶ Another identifying assumption not discussed in detail here is that pre-TUF Spike TV ratings in the relevant time slot not have any direct impacts on crime rates after the TUF premier. Given the content that was typically in that time slot—a variety of game shows and adult animated comedy, for example—we think this a reasonable assumption. This position is reinforced by the fact that our analysis does not find any evidence of impacts on crime prior in the year leading up to the TUF premier and we find evidence of an immediate impact on crime when TUF begins airing.

³⁷ Our measure of violent crime includes assault (simple and aggravated), rape, and murder.

crime in the years overlapping with seasons 1 through 20, from 2005 through to the end of 2014.³⁸ We also plot estimated “effects” for each quarter of the years prior to TUF’s premier, which serves as a set of placebo tests for effects prior to TUF’s premier. The omitted time periods represented by the “pre” period are those in 2001 and 2002. As discussed above, in addition to the indicator variables capturing time periods leading and lagging TUF’s premier interacted with its Season 1 rating (which we instrument for using pre-TUF ratings in the relevant time slot), the model also controls for the same indicator variables interacted with pre-TUF Spike TV ratings overall in addition to agency fixed effects and time fixed effects.

There are three main takeaways from the set of violent crime estimates depicted in Figure 5. First, the variation in early exposure to TUF that we exploit was unrelated to the evolution of violent crime rates leading up to the show’s premier. In all categories of violent crime, the placebo estimates for the quarters leading up to the show’s premier are all very close to zero and exhibit no evidence of any relevant differences in pre-existing trends. This evidence is consistent with the results presented in Figure 4, which demonstrated that violent-crime-rate trends and levels were unrelated to the instrument after controlling for pre-TUF Spike TV ratings overall.

Second, we see an immediate effect on the violent crime rate with the commencement of Season 1 (Panel A). Indeed, if one considers the estimated effects for all 48 time of the periods depicted in Panel A of Figure 5, the largest change in the estimated effect from one period to the next occurs exactly when TUF began airing (in the first quarter of 2005). This is indicative of an effect that is highly statistically significant based on the notion of “in-time placebo tests” as discussed in Heckman and Hotz (1989) and Abadie et al. (2015). This pattern is also evident for assaults (Panel B) and to a lesser degree for rapes (Panel C), while there is no evidence of effects on murder (Panel D). In Appendix Figure A2, we present an

³⁸ We also plot one pooled estimate for observations in 2015 through 2016, overlapping with seasons 21 through 24, though they do not deviate from zero if we separately identify each of them. In nearly all instances, seasons spanned three months and there were three months between seasons—this pattern was only interrupted by some slight changes following Season 16.

even more disaggregated set of estimates that demonstrates that the effect on violent crimes appears in the first month of TUF’s airing and that there is no evidence of such declines in the three months leading up to the premier. That the effect is so precipitous with the show’s premier lends confidence to the validity of our research design and the statistical significance of the effect because it is hard to imagine what sort of confounder could lead to such results.

The third takeaway from the set of estimates depicted in Figure 5 is that the effects persist beyond the period of time spanned by TUF’s first season (Q1 2005). Indeed, the effects appear to persist into the “off season” after the first season was completed and before the second season began (Q2 2005) and all the way through at least 2012 (i.e., through eleven or twelve seasons of TUF and the off seasons in between). This pattern is likewise evident for the subcategories of assault and rape—evidence of effects that are immediate and only seem to attenuate some five years after the show’s premier.

These results naturally raise the question: why would persistent effects eventually fade out? We believe this is likely the result of two counteracting mechanisms. Intuitively, the initial effects may be sustained or even grow if individuals affected by the instrument (or their friends who live in the same county) consume more and more UFC content over time, either because their fandom grows or because UFC produced more and more content. At the same time, while the instrument generates variation in who becomes a UFC fan in early 2005, nearly everyone will eventually hear about the UFC and those who are inclined to like it will become fans. Thus, the effect of early exposure to UFC content via TUF’s first season should be expected to die out at some point.

Another feature of the pattern of estimates depicted in Figure 5 is that the effects appear larger in the first and third quarters of each year. This may be due to the fact that these quarters closely overlapped with new seasons of the show as opposed to off seasons in which new episodes were shown, but it may also be a statistical aberration.³⁹

³⁹ Reruns of prior seasons would typically be aired when new episodes were not shown.

In Figure 6 we report results for property crimes. This figure show a very similar pattern as Figure 5—it raises no concerns about differential trends prior to TUF’s premier and suggest an immediate and persistent effect on crime after the show begins airing. The effects are also similar for the separate categories of theft, motor-vehicle theft, and robbery.⁴⁰ Notably, however, estimates of the effect on property crime are more imprecise.⁴¹

While the estimated effects shown in Figure 5 and Figure 6 are informative, we are knowingly giving up precision in this figure to be able to estimate a large number of parameters that help us to assess the research design and to get a sense of the timing of the treatment effects. Having provided this evidence, in Table 3 we present estimates from more-parsimonious models that pool together the time periods spanned by the first four years following TUF’s premier and, separately, the time periods afterwards. In discussing the effects, we focus on implied effects of a *tenth* of a percentage-point increase in TUF Season 1 ratings which is more reasonable than a one-percentage point increase given that the mean rating is 1.59 (with a standard deviation of 1.55). We also note that these percent effects are calculated using our regression estimates and data to evaluate how predicted outcomes change in response to a (hypothetical) *tenth* of a percentage-point increase in TUF Season 1 ratings.⁴²

The estimate in Column (1) of Panel A indicates that a 0.1 percentage-point increase in TUF Season 1 ratings reduces violent crime by 0.55% over four years. Across subcategories of violent crime, shown in columns (2) through (4) of Panel A, we find that the estimated effects are largest for assaults (-0.54%), somewhat smaller for rape (-0.23%), and close to zero for murder. Consistent with our prior evidence that the effects eventually fade away, the estimated effects are never statistically significant for the time period after 2008. Despite suggestive evidence of effects on property crime in Figure 6 and point estimates are consistent with

⁴⁰ Property crime includes theft (larceny and burglary), motor-vehicle theft, and robbery.

⁴¹ Dahl and DellaVigna (2009) also finds statistically insignificant reductions in property crimes associated with violent media consumption (in the form of blockbuster movie releases).

⁴² We do so out of respect for the fact that coefficient estimates do not always closely approximate percent effects when an outcome variable has been transformed using the inverse hyperbolic sine function.

that evidence, pooled estimates of the effect on rates of property crime are not statistically significant in the aggregate or in any subcategory.

To put the estimated effects into perspective, note that our measure of viewership implies that a 0.1 percentage-point difference in TUF Season 1 viewership corresponds to 1,202 additional individuals watching newly aired episodes of the show in February in the average county.⁴³ Naturally, many more individuals may have been induced by the instrument into watching the show at other times when it aired (e.g., re-airings on Friday nights, Saturday evenings, and Sunday evenings), and were likely induced into consuming other forms of UFC content (e.g., videos available at rental stores and main events airing on selected Saturdays). Thus, exposure may well be underestimated in our first stage. Moreover, given the explosion of UFC’s popularity caused by TUF, it is likely that those initially induced into watching had impacts on other individuals—direct viewings that escaped the measurement of Nielsen (e.g., friends watching together, viewing parties) or peer effects that moved others to consume similar UFC content—each of these would also have us underestimate the first-stage response, inflating the the two-stage-least-squares estimate. In any case, the estimate in Panel A of Table 3 indicates that the measurable increase in *new*-episode viewership corresponds to a 0.55 percent reduction in violent crimes (95% CI: 0.01, 1.08), or 7.5 fewer violent crimes per month in the average county (95% CI: 0.14, 14.7). Another important piece of context for this estimate is the target audience for both the network (Spike TV) and UFC were the same as the demographic group responsible for a disproportionate share of violent crimes—young men.⁴⁴ In any case, we suggest that readers exercise caution with regard to the magnitude of these estimated effects. While the estimates indicate a statistically significant effect on violent crime, the 95% confidence interval includes effects that would be considered small in

⁴³ For context, note that a 0.1 percentage-point change in TUF viewership corresponds to a 9.4 percent increase in viewership (in light of the population-weighted mean rating of 1.06). The calculation of 1,202 additional viewers is based on the (population-weighted) average county size represented in our analysis, which is 1.202 million.

⁴⁴ In the overlap of UCR arrest data and Cancer-SEER age- and gender-specific population data for the counties in our analysis, men aged 15–29 represent 11 percent of the population but account for 36 percent of arrests for violent crimes.

magnitude and effects that would be considered large in magnitude.

In summary, using variation in pre-TUF viewership in the time slot that would later become the time slot for new episodes of TUF—while controlling for differences in overall Spike TV viewership—we identify systematic reductions in violent crime associated with viewership of the show’s first season. These reductions are driven by the category one might anticipate being most responsive given that TUF (and UFC) essentially features assaults sanctioned by athletics commissions.⁴⁵ These effects begin with the show’s premier, and persist for at least five years. In support of a causal interpretation of these estimates, the estimated effects are very close to zero in “placebo tests” for effects prior to TUF’s premier and estimates that are routinely negative for a long period of time thereafter.

We have also conducted many robustness checks that further support the validity of this conclusion, the results of which are shown in Table A1 in the Appendix. In particular, we find results with the same pattern and similar levels of statistical significance when focusing on reduced-form effects of the instrument in an approach that is otherwise the same as the one we used to obtain 2SLS estimates reported in Table 3. Moreover, the results are similar if we control for time-varying county covariates capturing demographics and economic conditions. The results are also similar if we focus on a completely balanced panel of police agencies reporting in every month throughout our 16-year panel. We have also confirmed that we evidence the same pattern of results and similar levels of statistical significance using alternative approaches to constructing the outcome variable to account for zeros, or if we instead estimate the effects using a Poisson model.⁴⁶ That said, we note that the estimated effects from 2005 to 2008 are smaller in magnitude based on otherwise similar

⁴⁵ Though we think it is reassuring for the validity of the research design that the estimated effects are most prominent for assault, we think it important to note that nothing precludes TUF viewership from having impacts on many different forms of anti-social behavior. Thus we think it inappropriate to think of estimated effects on other crime outcomes as “placebo tests.”

⁴⁶ Recall, our main results evaluate the “log of the crime rate” which is constructed as $\ln(\frac{\text{count} + \sqrt{\text{count}^2 + 1}}{\text{population}})$. As alternatives, we consider adding one to counts before taking the natural log and also the quartic root of the outcome. The quartic root closely follows the natural log function for positive values and allows for the value of zero.

Poisson models. These estimates are roughly half the magnitude of WLS estimates, implying that a 0.1 percentage-point difference in TUF Season 1 viewership (which corresponds to 1,202 viewers) corresponds to a 0.14 percent reduction in violent crimes from 2005 to 2008, or 1.9 fewer violent crimes per month in the average county. The estimated effects are also smaller when we use OLS rather than WLS, which is indicative of larger effects in more populous areas (Solon et al., 2015).

5.4 Supplementary analysis of “UFC Main Events”

In the prior section, we noted that the effects of early exposure to TUF become apparent in the very month in which the show premiered. In this section, we take an additional step towards understanding how the sort of content featured on TUF—violent mixed martial arts bouts—affects behaviors in the very short run. We do so by analyzing the effects of “UFC Main Events” using hourly crime data from police agencies across the United States. These events air live in bars and on Pay-Per-View and showcase fights involving top UFC fighters, usually including a title fight for one or more weight divisions. The fact that these events are scheduled irregularly throughout the year is a distinct advantage that is critical to the empirical strategy that we use in this section, which compares how rates of crime change around the time of these events relative to the same times on otherwise-similar days.

For this analysis, we identify start times for 91 UFC Main Events, spanning August 2010 through December 2016.⁴⁷ To measure hourly crime rates, we use data from the National Incident Based Reporting System (NIBRS) collected by the Federal Bureau of Investigation (FBI). NIBRS is a voluntary program that collects detailed information on crime incidents from law-enforcement agencies across the United States. Though there are far fewer agencies included in NIBRS than in the UCR program, NIBRS identifies the date and hour of incidents known to municipal law enforcement agencies, facilitating our analysis of short-run changes

⁴⁷ Data available from ufc.com, by clicking “events” followed by “all events” followed by “past.” Our sample includes UFC 117 to UFC 207, retrieved on 26 Nov 2016.

in crime that occur around UFC Main events.⁴⁸ As all but one UFC Main event in our time frame occurred on a Saturday, we restrict our sample to Saturdays—specifically, we consider hours from Saturdays at 6am to Sundays at 5:59am, the following morning. This results in a data set including 15,144,168 agency-hour observations based on 3,771 municipal law enforcement agencies in 34 states. We focus on assault, the crime outcome for which we saw the clearest evidence of effects in our main results, and on assaults in bars given that bars typically air these events.

To identify the effect of UFC Main Events, we employ the following model:

$$y_{aymh} = \sum_{j=-10}^{16} \beta_j \mathbb{1}(HoursFromEvent_{aymh} = j) + \theta_{ay} + \delta_{am} + \gamma_{sh} + \epsilon_{aymh}, \quad (3)$$

where y_{aymh} is the log of the number of reported offenses per 10,000 residents in agency a in year y in month m in hour h .⁴⁹ The set of parameters β_j traces out the effects during the hours leading up to, during, and after the event begins. (In the model’s notation, the first hour of the event corresponds to $HoursFromEvent_{aymh}=0$.) We include 10 lead terms and 17 contemporaneous/lag terms, which is all possible leading/lagging indicators given the data and time zones represented therein.⁵⁰ The agency-by-year fixed effects, θ_{ay} , capture differences across agencies that vary year by year; agency-by-month fixed effects, δ_{am} , address seasonality in crime rates specific to each agency; state-by-hour fixed effects, γ_{sh} , address the potential for crime to vary across hours of the day in a given state; and ϵ_{at} are random error terms that we allow to be correlated across time for each agency. Given this specification,

⁴⁸ We do not use these NIBRS data in analyses like those in the prior sections because agency participation in NIBRS is especially limited and sporadic in the early-to-mid 2000s, which frequently leads to unusual data patterns that are unlikely to be random (since they likely relate to agency operations and organization such as staffing and digitization). With such uneven and irregular agency participation, we think NIBRS is more convincingly used for analyses that focus on more recent years and/or that focus on abrupt changes over short periods of time.

⁴⁹ Similar to our main analysis we transform counts using the inverse hyperbolic sine transformation to address the fact that the number of crimes are zero in many agency-hours, making the natural log undefined. As such, the outcome variable is $\ln(\frac{count + \sqrt{count^2 + 1}}{population})$.

⁵⁰ We do not report the estimates for the earliest (latest) lead (lag) terms, which have very large standard errors because they are only identified off of a subset of the data in the latest (earliest) time zones.

the estimated effects are identified through within-agency comparisons of hourly patterns of crime leading up to and following UFC events on event-day Saturdays to the patterns on non-event Saturdays, after controlling for agency-specific differences across months and years, and state-specific hours of the day.

The results of this analysis are shown in Figure 7, with Panel A showing estimated effects on assaults and Panel B showing the estimated effects on assaults in bars. Notably, the largest estimated effect that we find for assaults (Panel A) is in the hour after the event has begun airing, though this estimate is not statistically significant at the five-percent level. For assaults in bars (Panel B), the one statistically significant effect that we find is in the second hour after the event begins—this estimate indicates approximately 0.5 percent fewer assaults in bars during that hour.⁵¹ As a whole, this set of estimates provides support for the idea that the sort of content featured on TUF reduces violent crime.⁵²

6 Conclusion

Our results indicate significant reductions in violent behavior caused by violent media content.

Our estimation strategy relies on the fact that people tend to exhibit habit persistence in

⁵¹ Recall that these events feature many fights. Generally, the fights are ordered such that lesser-known and/or lower-ranked fighters feature in the earliest fights and better-known and/or higher-ranked fighters feature in later fights. The last fight of the event involves the “headliner” which is often a championship fight or some other fight involving famous and/or highly ranked fighters. Fights often end in a knockout or submission. Otherwise, they end in a judges decision after three five-minute rounds (or five five-minute rounds for championship fights). Given variation in the number of fights and the length of fights, there is substantial variation in the length of the event. They can last for more than three hours, though the typical duration is approximately two hours. For more information, see the discussion of the “main card” on wayofmartialarts.com/how-long-does-a-ufc-event-last/.

⁵² We have also investigated effects on domestic violence, assaults involving victims under age eighteen, assaults involving offenders under age eighteen, and assaults involving victims and offenders both under age eighteen. We cannot rule out that the effects on these outcomes are the same as the effects we estimate for all assaults and assaults in bars—the estimated effects are sometimes negative for the 1-2 hours after an event begins, as they are in our analyses of all assaults and assaults in bars. However, estimates are generally imprecise and we sometimes find similarly sized effects on these outcomes in the hours before an event. Thus, we are inclined against making strong claims and instead see the variation in the data as inconclusive. Separately, we have also investigated effects on incidents in which the offender is known to the victim and those in which the offender is a stranger. The results of these analyses are consistent with both types of incidents being affected in a similar manner.

television viewing, which causes some individuals to watch *The Ultimate Fighter* when it begins airing. Intuitively, we estimate the effects by comparing counties where individuals watch the same amount of Spike TV overall (in 2004, prior to TUF’s premiere), but vary in the amount of viewership in TUF’s future time slot.

Our results suggest that viewership of TUF’s first season significantly reduced violence among individuals induced to watch via this mechanism, in the months the show first premiered and for many years thereafter. These reductions are driven by the categories of crime that one might anticipate being responsive (given that TUF features sanctioned assaults, in some sense) and not by those that might constitute a stretch (i.e., murder). In support of our causal interpretation of these estimates, we find estimates very close to zero in “placebo tests” for effects prior to TUF’s premier. We also demonstrate short-run responsiveness to mixed martial arts events depicting similar content that air in bars and on Pay-Per-View, which we also interpret as supportive of our causal interpretation.

We believe that it is particularly informative that our results reinforce the findings of prior well-identified field studies (Dahl and DellaVigna, 2009; Markey et al., 2015; Cunningham et al., 2016). As a whole, this evidence suggests that we should be skeptical about policy prescriptions that are based on the wealth of laboratory-based studies, given that the short-run effects that might be expected from those studies have not been found in rigorous studies of individuals in their natural environments. Indeed, this evidence indicates that violent media content can reduce violence. That said, we think it is appropriate to suggest caution in making any strong policy prescriptions regarding violent media content. Each study provides but one point of context, and contributes to a very complicated question that implicates a wide and diverse set of content, all of which is typically lumped together as “violent media content.” Moreover, the effects identified in each study are specific to the set of individuals induced into violent media consumption by the specific circumstances under consideration. As the literature grows, it will be critical for researchers to consider whether violent media content is violent crime-reducing in general, or whether the effects vary according to the

specific characteristics of the content or the individuals induced into consuming the content.

References

- Abadie, Alberto, Susan Athey, Guido Imbens, and Jeffrey Wooldridge. 2020. "Sampling-based vs. Design-based Uncertainty in Regression Analysis." *Econometrica*, 88.
- Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2015. "Comparative Politics and the Synthetic Control Method." *American Journal of Political Science*, 59(2): 495–510.
- Adena, Maja, Ruben Enikolopov, Maria Petrova, Veronica Santarosa, and Eka-terina Zhuravskaya. 2015. "Radio and the Rise of The Nazis in Prewar Germany." *The Quarterly Journal of Economics*, 130(4): 1885–1939, DOI: <http://dx.doi.org/10.1093/qje/qjv030>.
- Anderson, C. A., A. Shibuya, N. Ihori, E.L. Swing, B.J. Bushman, A. Sakamoto, H. R. Rothstein, and Muniba Saleem. 2010. "Violent Video Game Effects on Aggression, Empathy, and Prosocial Behavior in Eastern and Western Countries: A Meta-Analytic Review." *Psychological Bulletin*, 136(2): 151–173.
- Anderson, Craig A., Leonard Berkowitz, Edward Donnerstein, L. Rowell Huesmann, James D. Johnson, Daniel Linz, Neil M. Malamuth, and Ellen Wartella. 2003. "The Influence of Media Violence on Youth." *Psychological Science in the Public Interest*, 4(3): 81–110.
- Ang, Desmond. 2020. "The Birth of a Nation: Media and Racial Hate." *HKS Faculty Research Working Paper Series*, RWP20-038.
- Bandura, Albert. 1977. *Social Learning Theory*. Oxford, England: Prentice-Hall.
- Baumeister, Roy F., and Todd F. Heatherton. 1996. "Self-Regulation Failure: An Overview." *Psychological Inquiry*, 7(1): 1–15.
- Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy*, 76(2): 169–217.
- Bhuller, Manudeep, Tarjei Havnes, Edwin Leuven, and Magne Mogstad. 2013. "Broadband Internet: An Information Superhighway to Sex Crime?" *Review of Economic Studies*, 80(4): 1237–1266.
- Bushman, Brad J., and Craig A. Anderson. 2015. "Understanding Causality in the Effects of Media Violence." *American Behavioral Scientist*, 59(14): 1807–1821.
- Card, David, and Gordon B. Dahl. 2011. "Family Violence and Football: The Effect of Unexpected Emotional Cues on Violent Behavior." *The Quarterly Journal of Economics*, 126(1): 103–143.
- Cornelson, Kirsten. 2018. "Media Role Models and Black Educational Attainment: Evidence from The Cosby Show." *Mimeo*.

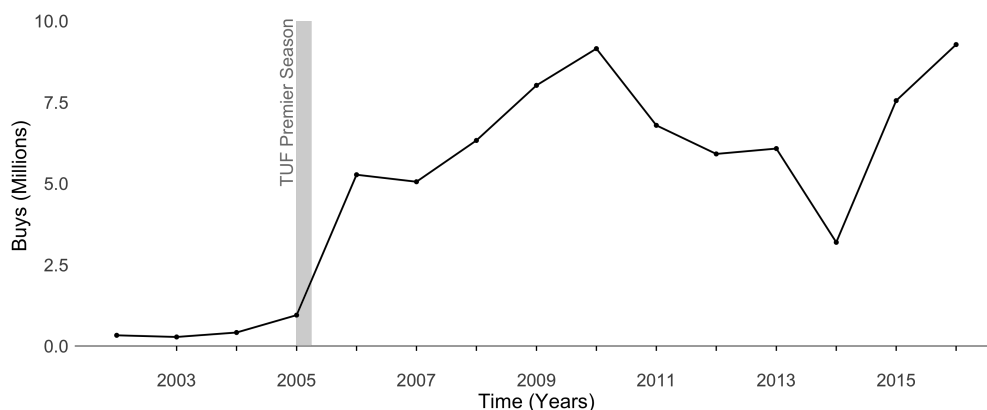
- Cunningham, Scott, Benjamin Engelstätter, and Michael R. Ward.** 2016. "Violent Video Games and Violent Crime." *Southern Economic Journal*, 82(4): 1247–1265.
- Dahl, Gordon, and Stefano DellaVigna.** 2009. "Does Movie Violence Increase Violent Crime?" *The Quarterly Journal of Economics*, 124(2): 677–734.
- DellaVigna, Stefano, and Eliana La Ferrara.** 2015. "Chapter 19 - Economic and Social Impacts of the Media." In *Handbook of Media Economics*. eds. by Simon P. Anderson, Joel Waldfogel, and David Strömberg: North-Holland, 723 – 768.
- Dollard, John, Neal E. Miller, Leonard W. Doob, O. H. Mowrer, and Robert R. Sears.** 1939. *Frustration and aggression.*: Yale University Press.
- Donnerstein, E., M. Donnerstein, and R. Evans.** 1975. "Erotic Stimuli and Aggression: Facilitation or Inhibition." *Journal of Personality and Social Psychology*, 32, p. 237–244.
- Ellingsen, Sebastian, and Øystein Hernæs.** 2018. "The impact of commercial television on turnout and public policy: Evidence from Norwegian local politics." *Journal of Public Economics*, 159 1 – 15.
- Enikolopov, Ruben, Maria Petrova, and Ekaterina Zhuravskaya.** 2011. "Media and Political Persuasion: Evidence from Russia." *American Economic Review*, 101(7): 3253–85.
- Eron, L. D., L. R. Huesmann, M. M. Lefkowitz, and L. O. Walder.** 1972. "Does Television Violence Cause Aggression?" *American Psychologist*, 27, p. 253–263.
- Ferguson, Christopher J., Allen Copenhaver, and Patrick Markey.** forthcoming. "Reexamining the Findings of the American Psychological Association's 2015 Task Force on Violent Media: A Meta-Analysis." *Perspectives on Psychological Science*, 0(0): .
- Fiske, Alan Page, and Tage Shakti Rai.** 2014. *Virtuous Violence: Hurting and Killing to Create, Sustain, End, and Honor Social Relationships.*: Cambridge University Press, , DOI: <http://dx.doi.org/10.1017/CBO9781316104668>.
- Gentile, Douglas A, Paul J Lynch, Jennifer Ruh Linder, and David A Walsh.** 2004. "The Effects of Violent Video Game Habits on Adolescent Hostility, Aggressive Behaviors, and School Performance." *Journal of Adolescence*, 27(1): 5 – 22, Video Games and Public Health.
- Gentzkow, Matthew.** 2006. "Television and Voter Turnout." *The Quarterly Journal of Economics*, 121(3): 931–972.
- Gentzkow, Matthew, and Jesse M. Shapiro.** 2008. "Preschool Television Viewing and Adolescent Test Scores: Historical Evidence from the Coleman Study." *The Quarterly Journal of Economics*, 123(1): 279–323.
- Ginges, Jeremy, and Scott Atran.** 2009. "What motivates participation in violent political action: selective incentives or parochial altruism?" *Annals of the New York Academy of Sciences*, 1167 115–23.

- Greitemeyer, Tobias, and Dirk O. Mügge.** 2014. "Video Games Do Affect Social Outcomes: A Meta-Analytic Review of the Effects of Violent and Prosocial Video Game Play." *Personality and Social Psychology Bulletin*, 40(5): 578–589.
- Heckman, James J., and V. Joseph Hotz.** 1989. "Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training." *Journal of the American Statistical Association*, 84(408): 862–874.
- Hernæs, Øystein, Simen Markussen, and Knut Røed.** forthcoming. "Television, Cognitive Ability, and High School Completion." *Journal of Human Resources*.
- Hilgard, Joseph, Christopher R. Engelhardt, and Jeffrey N. Rouder.** 2017. "Overstated evidence for short-term effects of violent games on affect and behavior: A reanalysis of Anderson et al. (2010)." *Psychological Bulletin*, 143(7): 757–774.
- Jaeger, David A., Theodore J. Joyce, and Robert Kaestner.** 2016. "Does Reality TV Induce Real Effects? On the Questionable Association Between 16 and Pregnant and Teenage Childbearing." IZA Discussion Papers 10317, Bonn.
- Jaeger, David A., Theodore J. Joyce, and Robert Kaestner.** 2020. "A Cautionary Tale of Evaluating Identifying Assumptions: Did Reality TV Really Cause a Decline in Teenage Childbearing?" *Journal of Business & Economic Statistics*, 38(2): 317–326.
- Johnson, Jeffrey G., Patricia Cohen, Elizabeth M. Smailes, Stephanie Kasen, and Judith S. Brook.** 2002. "Television Viewing and Aggressive Behavior During Adolescence and Adulthood." *Science*, 295(5564): 2468–2471.
- Josephson, W. L.** 1987. "Television Violence and Children's Aggression: Testing the Priming, Social Script, and Disinhibition Predictions." *Journal of Personality and Social Psychology*, 53(5): 882–890.
- Kahn-Lang, Ariella, and Kevin Lang.** 2019. "The Promise and Pitfalls of Differences-in-Differences: Reflections on 16 and Pregnant and Other Applications." *Journal of Business & Economic Statistics*, 0(0): 1–14.
- Kearney, Melissa S., and Philip Levine.** forthcoming. "Early Childhood Education by Television: Lessons from Sesame Street." *American Economic Journal: Applied Economics*.
- Kearney, Melissa S., and Phillip B. Levine.** 2015. "Media Influences on Social Outcomes: The Impact of MTV's 16 and Pregnant on Teen Childbearing." *American Economic Review*, 105(12): 3597–3632.
- Kearney, Melissa S., and Phillip B. Levine.** 2016. "Does Reality TV Induce Real Effects? A Response to Jaeger, Joyce, and Kaestner (2016)." *IZA Discussion Paper 10318*.
- La Ferrara, Eliana, Alberto Chong, and Suzanne Duryea.** 2012. "Soap Operas and Fertility: Evidence from Brazil." *American Economic Journal: Applied Economics*, 4(4): 1–31.

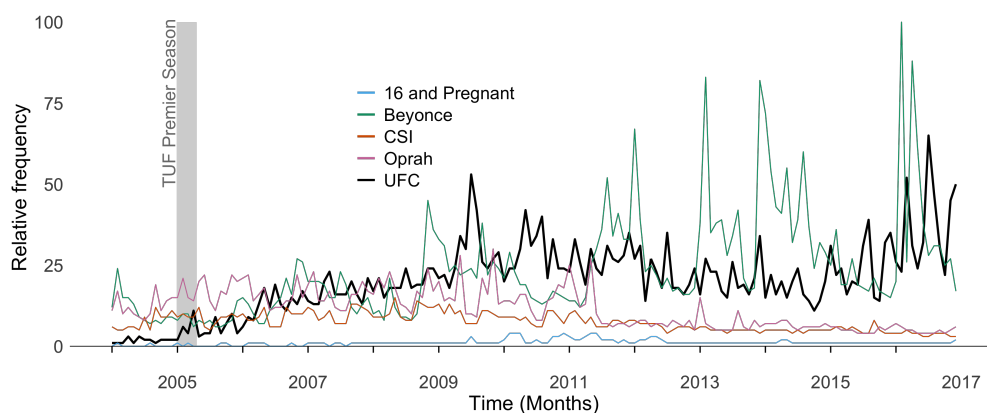
- Maltz, Michael D., and Joseph Targonski.** 2002. "A Note on the Use of County-Level UCR Data." *Journal of Quantitative Criminology*, 18, p. 297–318.
- Markey, P. M., C.N. Markey, and J.E. French.** 2015. "Violent Video Games and Real-World Violence: Rhetoric Versus Data." *Psychology of Popular Media Culture*, 4(4): 277–295.
- Parke, R D, L Berkowitz, J P Leyens, and S West.** 1977. "Some Effects of Violent and Nonviolent Movies on the Behaviour of Juvenile Delinquents."
- Posner, Richard A.** 1992. *Sex and Reason*. Cambridge, MA: Harvard University Press.
- Solon, Gary, Steven Haider, and Jeffrey Wooldridge.** 2015. "What Are We Weighting For." *Journal of Human Resources*, 50(2): 301–316.
- Trudeau, Jennifer.** 2016. "The Role of New Media on Teen Sexual Behaviors and Fertility Outcomes—The Case of 16 and Pregnant." *Southern Economic Journal*, 82(3): 975–1003.
- Ward, Michael R.,** "Adolescent Video Game Playing and Fighting over the Long-term." *Contemporary Economic Policy*, n/a(n/a): .
- Ward, Michael Robert.** 2011. "Video Games and Crime." *Contemporary Economic Policy*, 29(2): 264–271.
- Yanagizawa-Drott, David.** 2014. " Propaganda and Conflict: Evidence from the Rwandan Genocide." *The Quarterly Journal of Economics*, 129(4): 1947–1994, DOI: <http://dx.doi.org/10.1093/qje/qju020>.

Figure 1
Ultimate Fighting Championship popularity over time

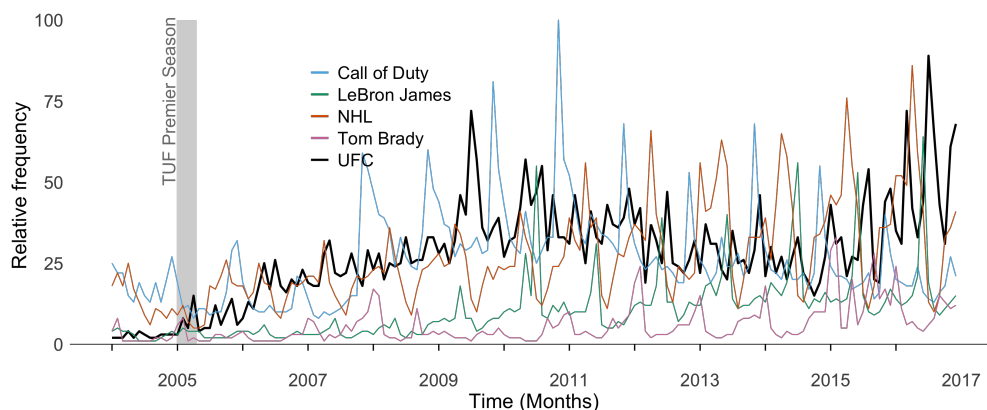
Panel A: As measured by total main event PPV buys



Panel B: As measured by “unrelated” Google searches

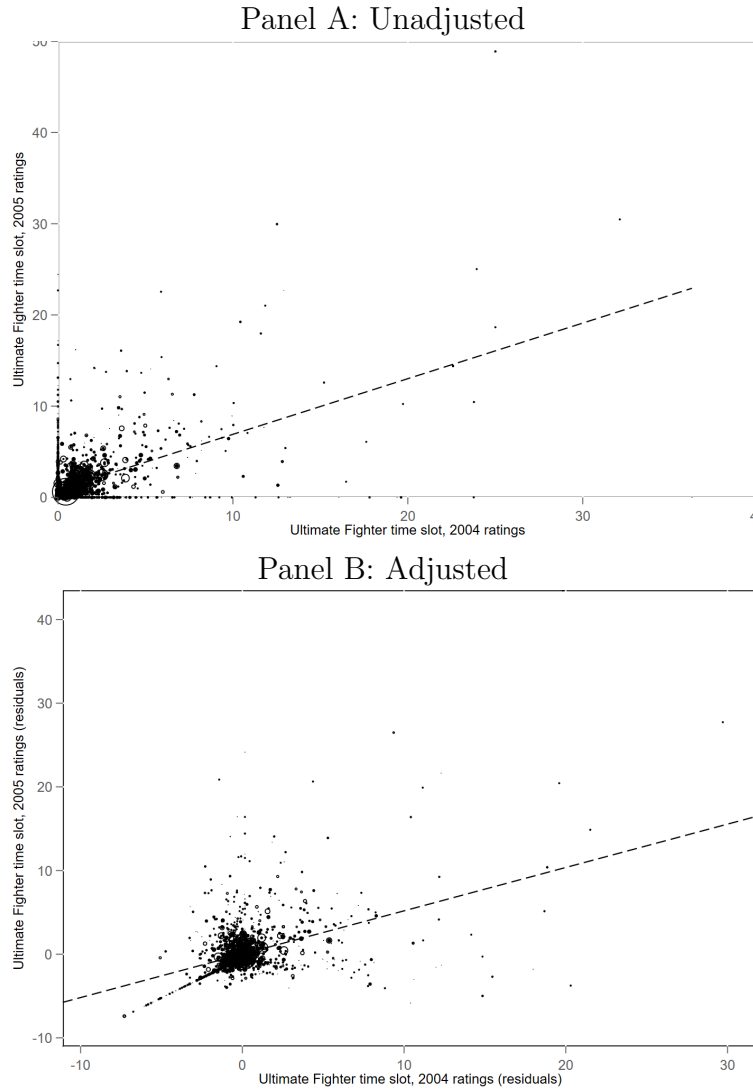


Panel C: As measured by “related” Google searches



Notes: In Panel A, we plot the number of Pay-Per-View buys annually, available from <https://thesportsdaily.com/2018/02/16/all-time-ufc-ppv-sales-data-fox11/>. In panels B and C, we plot Google search results, which are always proportionate to the time and location of a query. (To be able to compare relative popularity, each data point is divided by the total searches of the geography and time range it represents. The resulting numbers are then scaled on a range of 0 to 100 based on a topic's proportion to all searches on all topics.) In our case, we requested data for all available months, but originating in the United States.

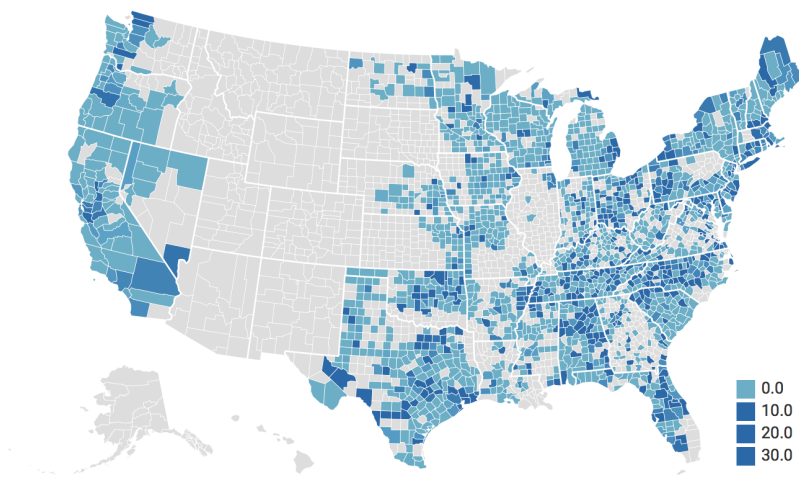
Figure 2
Graphical depiction of the first stage



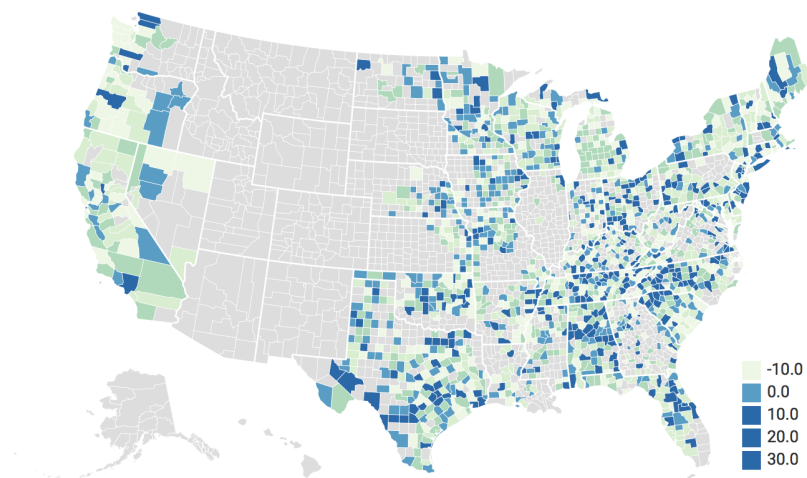
Notes: In this figure we plot Season 1 TUF ratings (February 2005) and Spike network ratings in the same time slot before TUF premiered (November 2004). The graph labeled “adjusted” plots the residuals of these ratings, each adjusted for overall Spike viewership before TUF premiered (November 2004).

Figure 3
Viewership in The Ultimate Fighter's time slot (in 2004) before its premier (in 2005)

Panel A: Unadjusted



Panel B: Adjusted for overall Spike TV viewership (2004)

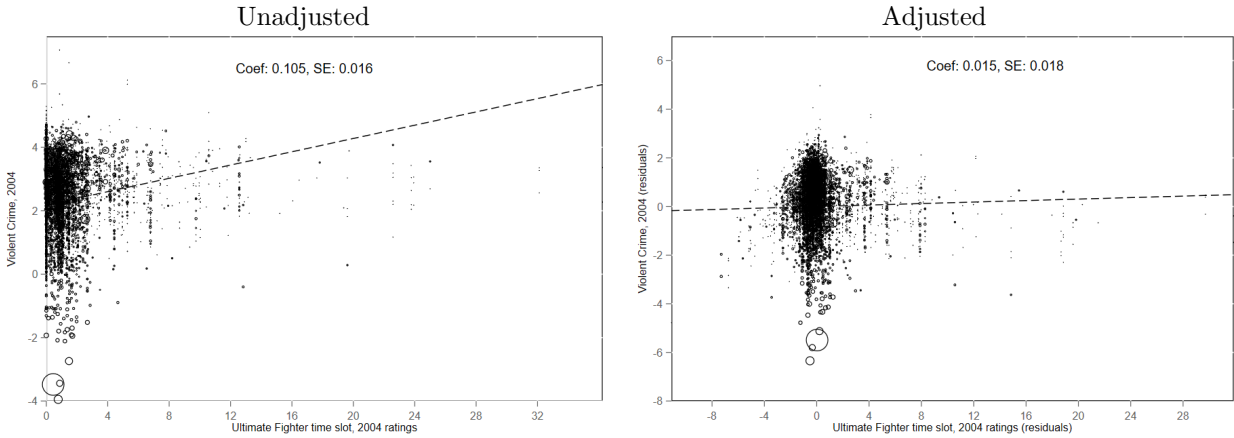


Notes: County level ratings data from Nielsen. Spike TV Viewership in TUF's time slot is measured in November 2004. Season 1 TUF viewership is measured during February 2005. Overall Spike TV viewership is during November 2004. See Section 3 for additional details.

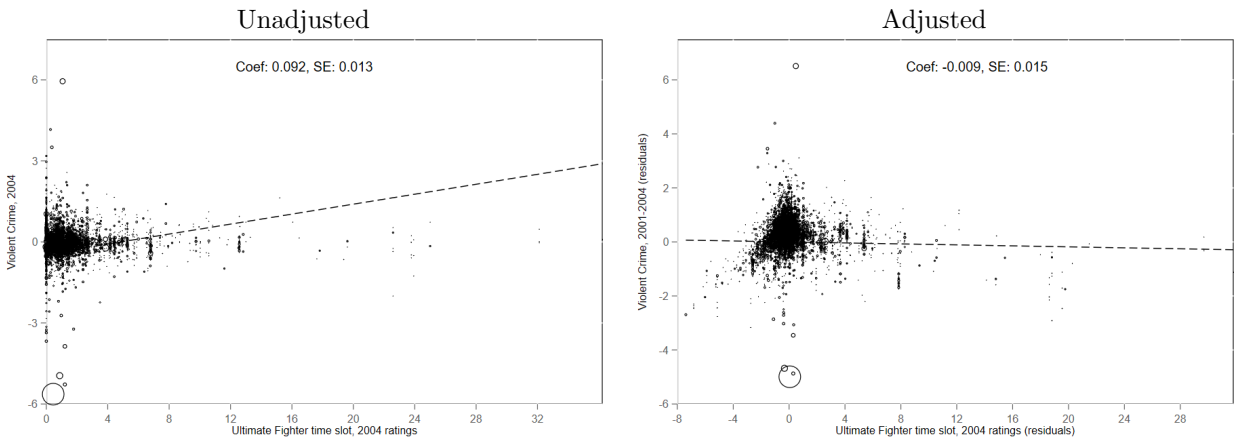
Figure 4

How controlling for pre-treatment Spike TV overall ratings eliminates the links between the instrument and pre-treatment violent crime rate levels and trends

Panel A: 2004 log violent crime rates vs 2004 rating in TUF time slot

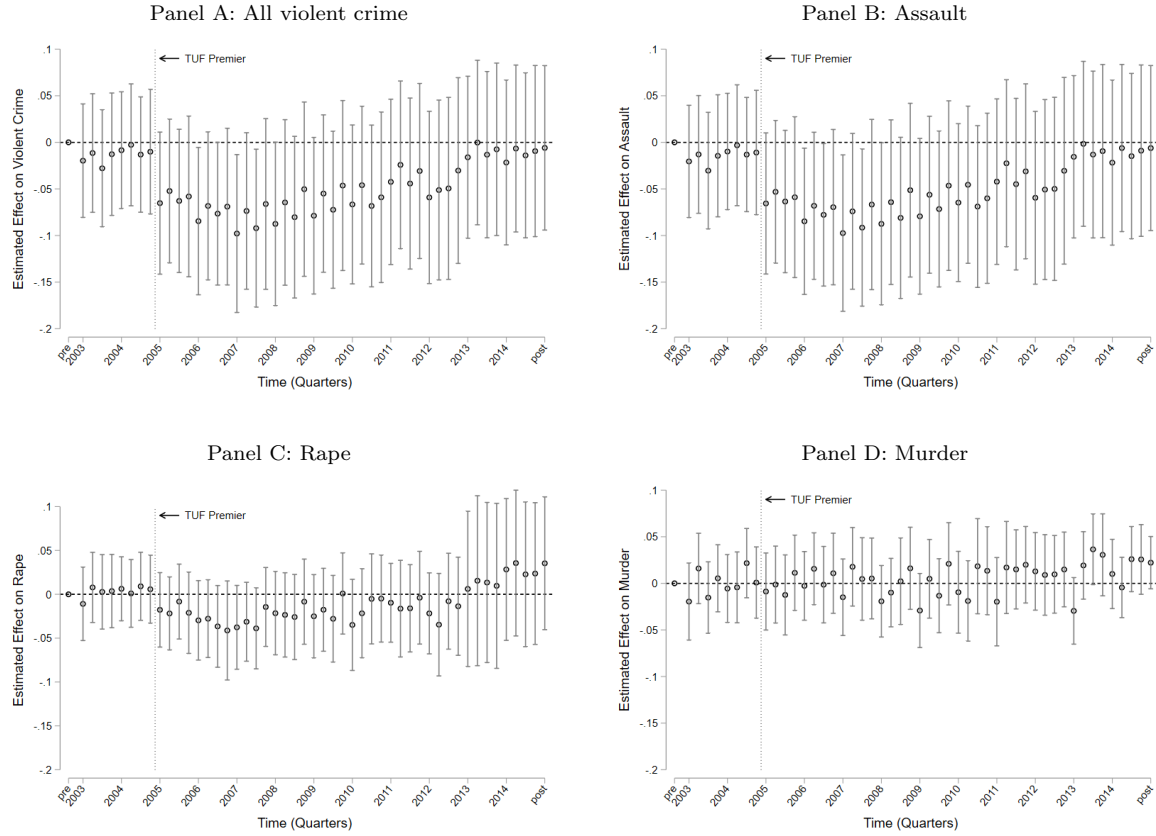


Panel B: 2001-2004 change in log violent crime rates vs 2004 rating in TUF time slot



Notes: In Panel A, the “unadjusted” graphic plots the log of the violent crime rate in 2004 and The Ultimate Fighter time slot ratings in 2004 while the “adjusted” graphic plots the residuals from the log of the violent crime rate in 2004 and the Ultimate Fighter time slot ratings in 2004 both adjusted for overall Spike viewership in 2004. Panel B is similar but uses the 2001–2004 change in the log of the violent crime rate instead of the 2004 level. Each observations is weighted (indicated by size) by agency jurisdiction population.

Figure 5
2SLS estimates of the effect of Season 1 TUF viewership on violent crimes



Notes: Estimates are based on 1,372,296 monthly observations spanning 2001–2016 for 8,750 law-enforcement agencies participating in the Uniform Crime Reports: Offenses Known Segment. The figure plots coefficients and 95 percent confidence intervals of quarter indicators interacted with average Ultimate Fighter viewership ratings from an instrumental variables model where the instrument is the 2004 viewership ratings for the the same time slot and network as TUF would air (in 2005). The outcome variable is the log of the number of reported offenses per 10,000 residents in the agency jurisdiction. The models include year-by-month and agency fixed effects. The regressions are weighted by agency jurisdiction population. Standard-error estimates allow for clusters at the county level.

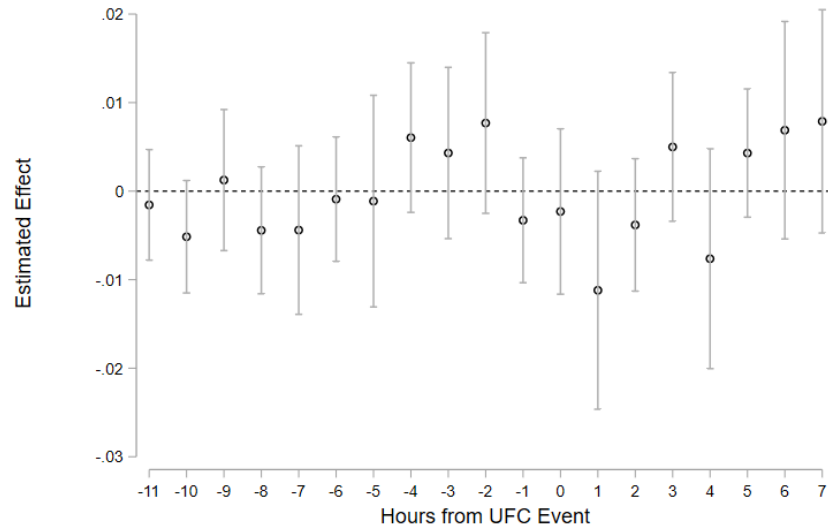
Figure 6
2SLS estimates of the effect of Season 1 TUF viewership on property crimes



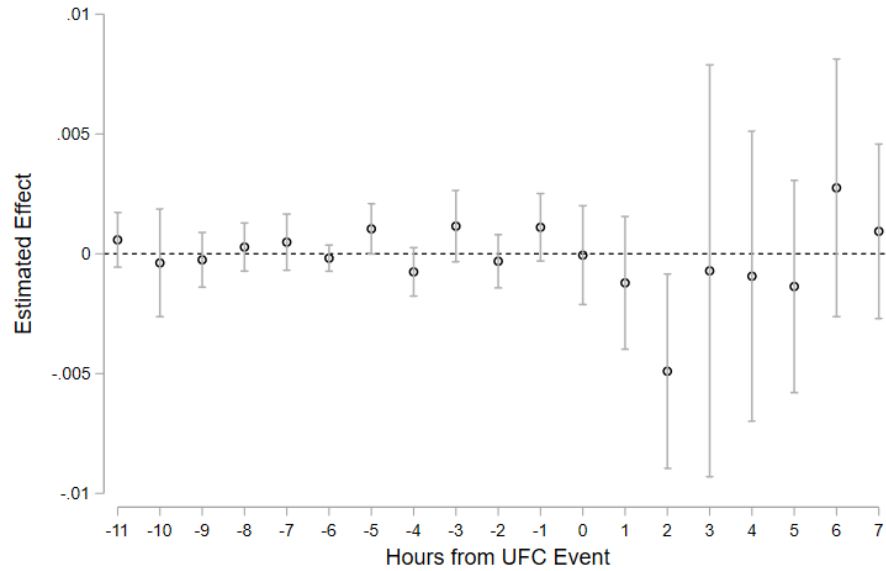
Notes: See Figure 5.

Figure 7
Estimated effects of UFC “Pay-Per-View Main Events”

Panel A: Assault



Panel B: Assault in bars



Notes: Estimates are based on 15,144,168 hourly observations on Saturdays spanning 2010–2015 for 3,771 municipal law-enforcement agencies participating in the FBI’s National Incident Based Reporting System (NIBRS). The figure plots coefficients and 95 percent confidence intervals of hourly indicators leading up to and following the start of Pay-Per-View UFC Events. The outcome variable is the log of the number of reported offenses per 10,000 residents in the agency jurisdiction. The models include agency-by-month, agency-by-year, and state-by-hour fixed effects. The regressions are weighted by agency jurisdiction population. Standard errors are clustered at the agency level.

Table 1
Summary statistics for UCR and Nielsen Ratings data

Violent Crime	11.33
Assault	11.01
Rape	0.27
Murder	0.05
Property Crime	30.34
Theft	25.71
Motor-Vehicle Theft	3.15
Robbery	1.48
Ultimate Fighter time slot	
2005 ratings	1.59
2004 ratings	1.06
Ultimate Fighter network, 2004 ratings	0.36
Number of Agencies	8,750
N	1,372,296

Notes: Means of crime outcomes are the number of reported offenses per 10,000 residents in the agency jurisdiction.

Table 2
First-stage estimates for The Ultimate Fighter ratings

	(1)	(2)
Ultimate Fighter time slot, 2004 ratings	0.616*** (0.084)	0.523*** (0.082)
Ultimate Fighter network, 2004 ratings		1.458*** (0.370)
Fstat	53.76	40.68
N	1372296	1372296

Notes: Observations are at the agency level to be consistent with our analysis of crime outcomes (which are at the agency-month level). Ratings are measured at the county level. The regressions are weighted by agency jurisdiction population. Standard-error estimates allow for clusters at the county level. The reported F-statistic is for the exclusion of 2004 ratings in The Ultimate Fighter's future time slot. *, **, and ***, indicate statistical significance at the ten-, five-, and one-percent levels, respectively.

Table 3
2SLS estimates of the effect of Season 1 TUF viewership

Panel A: Violent crimes				
	Violent Crime	Assault	Rape	Murder
	(1)	(2)	(3)	(4)
RateTUF05 $\times$ 1(2005-2008)	-0.0654** (0.0318)	-0.0652** (0.0315)	-0.0274* (0.0148)	0.0008 (0.0110)
RateTUF05 $\times$ 1(2009-2016)	-0.0240 (0.0370)	-0.0235 (0.0371)	0.0031 (0.0241)	0.0109 (0.0108)
Agencies	8,750	8,750	8,750	8,750
N	1,372,296	1,372,296	1,372,296	1,372,296
% effect of 0.1-unit higher rating, 2005-2008	-0.55	-0.6	-0.23	0.008
% effect of 0.1-unit higher rating, 2009-2016	-0.2	0.03	0.1	0.01

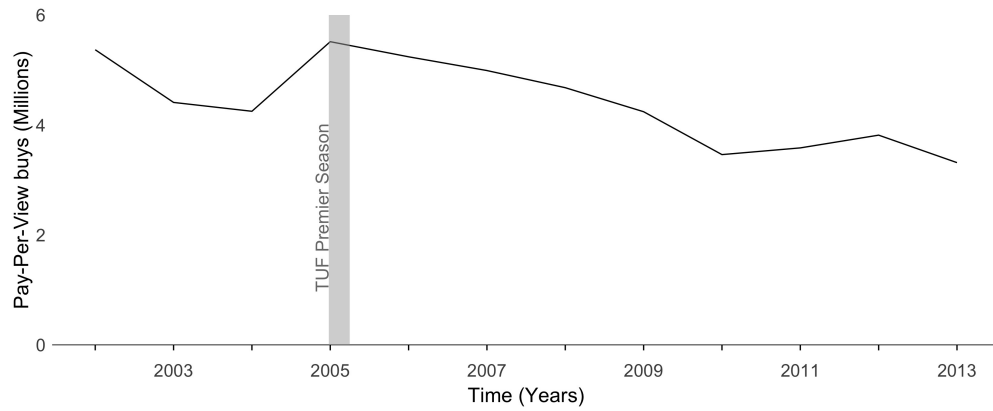
Panel B: Property crimes				
	Property Crime	Theft	MV Theft	Robbery
	(1)	(2)	(3)	(4)
RateTUF05 $\times$ 1(2005-2008)	-0.0555 (0.0396)	-0.0535 (0.0385)	-0.0328 (0.0298)	-0.0413 (0.0256)
RateTUF05 $\times$ 1(2009-2016)	-0.0144 (0.0396)	-0.0141 (0.0380)	-0.0116 (0.0325)	-0.0115 (0.0207)
Agencies	8,750	8,750	8,750	8,750
N	1,372,296	1,372,296	1,372,296	1,372,296
% effect of 0.1-unit higher rating, 2005-2008	-0.48	-0.39	-0.26	-0.35
% effect of 0.1-unit higher rating, 2009-2016	-0.13	-0.12	-0.11	-0.1

Notes: Estimates are based on monthly crime reports for law-enforcement agencies participating in the Uniform Crime Reports: Offenses Known Segment. Estimated coefficients are from an instrumental variables model where the instrument for average Ultimate Fighter viewership ratings is the 2004 viewership ratings for the the same time slot and network as TUF. The outcome variable is the log of the number of reported offenses per 10,000 residents in the agency jurisdiction. The models include year-by-month and agency fixed effects. The regressions are weighted by agency jurisdiction population. Standard-error estimates allow for clusters at the county level. *, **, and ***, indicate statistical significance at the ten-, five-, and one-percent levels, respectively.

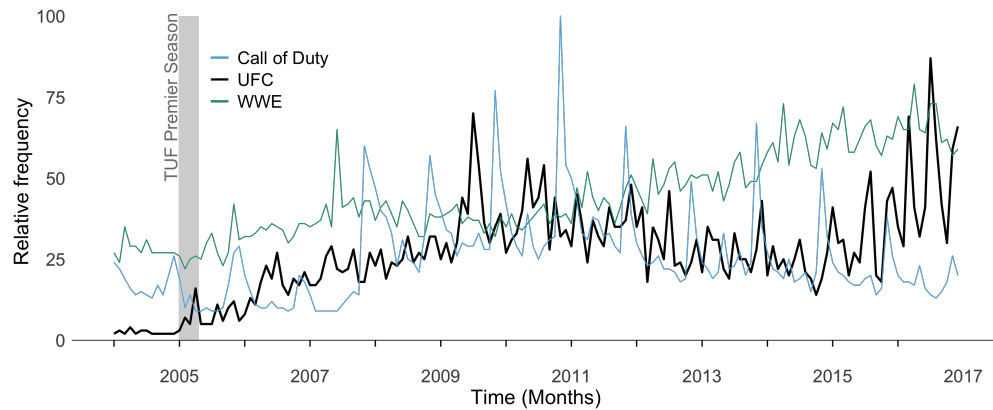
7 Appendix

Figure A1
Analysis of Interest in “Professional Wrestling” (WWE)

Panel A: Pay-Per-View Buys



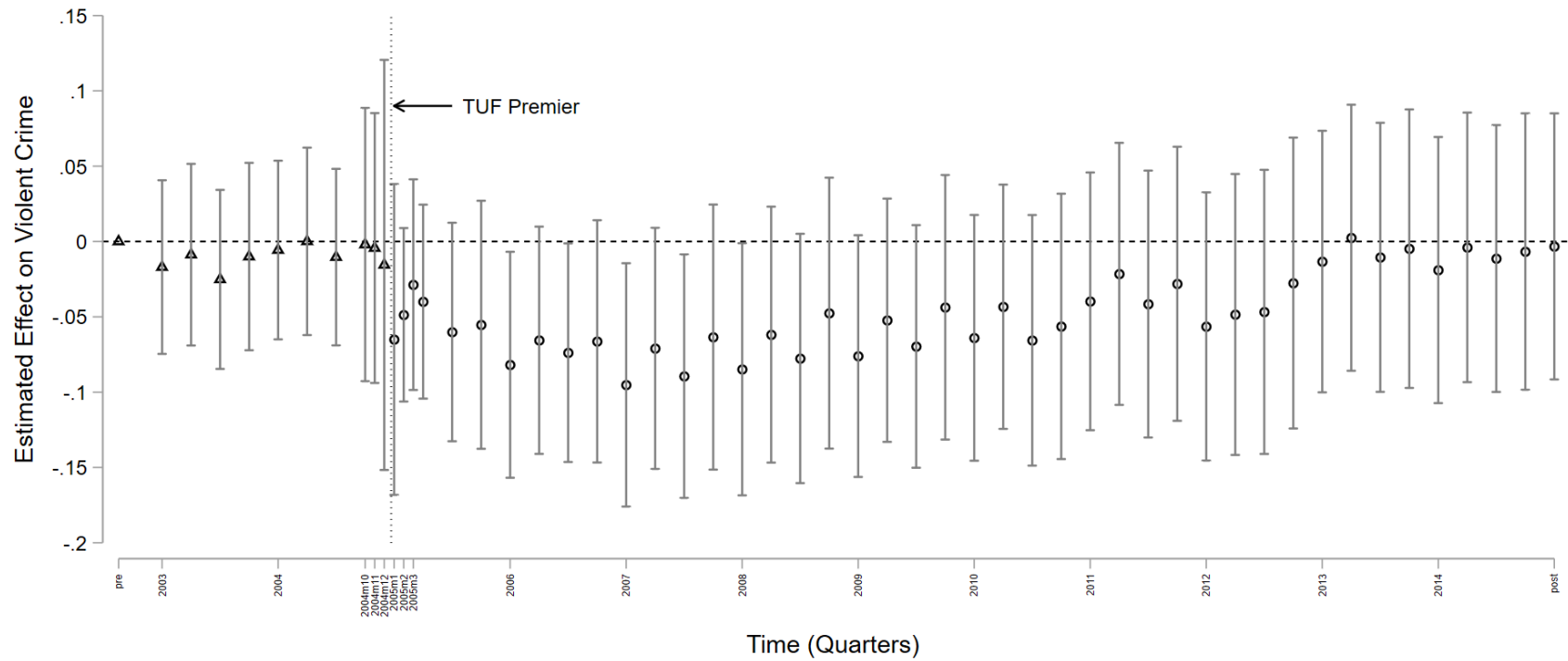
Panel B: Google Trends



Notes: In Panel A we plot the number of WWE (and WWF) Pay-Per-View buys annually, available from <http://www.2xzone.com/wwe/buyrates.shtml>. In Panel B we plot Google search results, which are always proportionate to the time and location of a query. (To be able to compare relative popularity, each data point is divided by the total searches of the geography and time range it represents. The resulting numbers are then scaled on a range of 0 to 100 based on a topic's proportion to all searches on all topics.) In our case, we requested data for all available months, but originating in the United States.

Figure A2

2SLS estimates of the effect of Season 1 TUF viewership on violent crime
with separate estimates for each month during the quarter before and after its premier



Notes: This figure is the same as the graphic for violent crime in Figure 5, except here we present separate estimates for each month in the quarter before and the quarter after TUF began airing. For visual clarity, we depict the estimated effects prior to TUF as triangles and the estimated effects after TUF began airing as circles.

Table A1
Estimated reduced-form effects of the instrument on crime

Estimation Method Outcome Adjustment for zeros Covariate Adjustment Balanced Sample	Panel A: Violent crimes															
	WLS	WLS	WLS	WLS	WLS	WLS	WLS	WLS	WLS	WLS	WLS	OLS	OLS	OLS	OLS	OLS
	IHS No Yes (1)	IHS No Yes (2)	IHS No Yes (3)	IHS No Yes (4)	LN(+1) No Yes (5)	LN(+1) No Yes (6)	QRroot No Yes (7)	QRroot No Yes (8)	IHS No Yes (9)	IHS No Yes (10)	LN(+1) No Yes (11)	LN(+1) No Yes (12)	QRroot No Yes (13)	QRroot No Yes (14)	Poisson n/a No Yes (15)	Poisson n/a No Yes (16)
RateTUFslot04 $\times$ 1(2005 – 2008)	-0.0342** (0.0137)	-0.0349** (0.0137)	-0.0312** (0.0150)	-0.0292* (0.0151)	-0.0308** (0.0120)	-0.0280** (0.0135)	-0.0118*** (0.0039)	-0.0125*** (0.0039)	-0.0008** (0.0031)	-0.0128*** (0.0039)	-0.0056** (0.0026)	-0.0106*** (0.0033)	-0.0045** (0.0020)	-0.0082*** (0.0024)	-0.0137*** (0.0051)	-0.0145*** (0.0053)
RateTUFslot04 $\times$ 1(2009 – 2016)	-0.0122 (0.0187)	-0.0104 (0.0203)	-0.0175 (0.0148)	-0.0151 (0.0172)	-0.0100 (0.0168)	-0.0147 (0.0134)	-0.0036 (0.0068)	-0.0098* (0.0056)	-0.0040 (0.0059)	-0.0156*** (0.0053)	-0.0034 (0.0049)	-0.0132*** (0.0045)	-0.0021 (0.0039)	-0.0087*** (0.0032)	-0.0080 (0.0075)	-0.0147* (0.0079)
Agencies	8,737	8,737	3,984	3,984	8,737	3,984	8,737	3,984	8,737	3,984	8,737	3,984	8,737	3,984	8,603	39,83
N	1,371,636	1,369,308	764,928	764,196	1,371,636	764,928	1,371,636	764,196	1,371,636	764,196	1,371,636	764,196	1,371,636	764,196	1,365,192	764,004
% Effect of 0.1-unit higher rating, 2005-2008	-0.29	-0.33	-0.26	-0.29	-0.33	-0.29	-0.29	-0.26	-0.06	-0.11	-0.06	-0.11	-0.14	-0.22	-0.14	-0.14
% Effect of 0.1-unit higher rating, 2009-2016	-0.10	-0.10	-0.16	-0.15	-0.11	-0.15	-0.09	-0.21	-0.04	-0.14	-0.04	-0.14	-0.06	-0.24	-0.08	-0.15
Panel B: Assault																
RateTUFslot04 $\times$ 1(2005 – 2008)	-0.0341** (0.0135)	-0.0348** (0.0135)	-0.0313** (0.0149)	-0.0292* (0.0150)	-0.0307** (0.0119)	-0.0280** (0.0134)	-0.0118*** (0.0039)	-0.0125*** (0.0039)	-0.0006** (0.0031)	-0.0127*** (0.0039)	-0.0055** (0.0026)	-0.0106*** (0.0033)	-0.0043** (0.0020)	-0.0082*** (0.0024)	-0.0139*** (0.0052)	-0.0147*** (0.0054)
RateTUFslot04 $\times$ 1(2009 – 2016)	-0.0119 (0.0187)	-0.0102 (0.0203)	-0.0174 (0.0149)	-0.0151 (0.0172)	-0.0097 (0.0169)	-0.0147 (0.0135)	-0.0036 (0.0068)	-0.0099* (0.0056)	-0.0040 (0.0059)	-0.0156*** (0.0053)	-0.0034 (0.0049)	-0.0132*** (0.0045)	-0.0022 (0.0039)	-0.0088*** (0.0032)	-0.0081 (0.0077)	-0.0150* (0.0080)
Agencies	8,750	8,750	3,985	3,985	8,750	3,985	8,750	3,985	8,750	3,985	8,750	3,985	8,750	3,985	8,599	3,983
N	1,372,296	1,369,968	765,096	764,364	1,372,296	765,096	1,372,296	764,364	1,372,296	764,364	1,372,296	764,364	1,372,296	764,364	1,364,892	764,004
% Effect of 0.1-unit higher rating, 2005-2008	-0.28	-0.33	-0.27	-0.29	-0.33	-0.29	-0.29	-0.26	-0.06	-0.11	-0.06	-0.11	-0.13	-0.229	-0.14	-0.14
% Effect of 0.1-unit higher rating, 2009-2016	-0.1	-0.10	-0.15	-0.15	-0.1	-0.15	-0.1	-0.21	-0.03	-0.14	-0.04	-0.14	-0.07	-0.24	-0.08	-0.15

Notes: Estimates are based on monthly crime reports for law-enforcement agencies participating in the Uniform Crime Reports: Offenses Known Segment. Estimated coefficients are from regressions in which the years indicated are interacted with 2004 viewership ratings for the same time slot and network that TUF would be on in 2005. The models include year-by-month and agency fixed effects. Column 1 shows the reduced-form effect of the instrument corresponding to the 2SLS estimates shown in Table 3. The outcome variable is the log of the number of reported offenses per 10,000 residents in the agency jurisdiction, with different transformations to account for zeros indicated in the second row of the table. Standard-error estimates allow for clusters at the county level. *, **, and ***, indicate statistical significance at the ten-, five-, and one-percent levels, respectively.