ESSAYS ON LABOR ECONOMICS

by

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ABSTRACT

This dissertation is composed of three essays on labor economics. First, I examine the effect of the rapid rise in binge watching on reported crime. I use conditionally exogenous variation in the runtime of newly released Netflix Originals to identify the effect of binge watching on reported crime. I find that binge watching reduces crime contemporaneously and in the first three days that the new content is available. I find no evidence that binge watching reduces total crime reported over a nearly two week period after new content becomes available. Second, I replicate a well-known paper by Card and Dahl (2011) which examines the effect of emotional cues on violent crime. I confirm their baseline result while using their original study design from 1995–2006. I expand on their analysis by expanding the time series of their original data and using new data. I find their baseline result is not robust using out-of-sample data from 2007–2019. Third, I estimate the effect of cell phones on traffic accidents by using the expansion of the Lifeline Assistance Program as an exogenous shock to the stock of cell phones, I use a difference-in-differences quasi-experimental design to find that cell phones causally increase traffic fatalities when those cell phones are made available in states with no restrictions to cell phone use while driving and states that ban texting while driving and require hands-free calling. In addition, I find that additional cell phones have no effect when states have only one restriction on cell phone use while driving—implying that the optimal policy to reduce traffic fatalities is to ban texting while driving.

1. NETFLIX AND CRIME

1.1 Introduction

Binge watching—watching multiple episodes of the same series in the same sitting—is a pop cultural phenomenon (Marsh 2014). Binge watching is especially prevalent in the youngest generations: 91 percent of Generation Z (born 1997–2003) and 86 percent of Millenials (born 1983–1996) have binge watched. Generation Z and Millenials watch the most episodes per sitting: at least seven on average (Wescott et al. 2018, 12). A seven-episode binge of a scripted drama equals watching nearly three two-hour films back-to-back-to-back. A more extreme consumption behavior, binge racing—watching an entire season within the first 24 hours of availability, has grown from 200,000 accounts worldwide to over 5 million from 2013 to 2017 on Netflix alone (Dwyer 2017).

Criminal offenders, similar to binge watchers, are frequently concentrated among the youngest generations. In 2018, over 58 percent of persons arrested in the United States were between the ages of 15 and 34 (Federal Bureau of Investigation 2019), but 15–34-year-olds account for 27 percent of the population (U.S. Census Bureau 2019). Crime is concentrated in the evening hours. Similarly, most binge watching occurs at night (Marsh 2014). Crime has long interested policy makers and researches due to its large economic cost. Anderson (2012) estimates the annual economic cost of crime as over 3.2 trillion in 2012 dollars.

The binge watching offers an intersection with potential offenders *and* potential victims that may influence crime. Reductions in crime could come directly from potential offenders binge watching a series as opposed to engaging in criminal activities—the incapacitation effect. Binge watching could also remove potential victims from places with a high probability of crime since most binge watching occurs at home on a Friday or Saturday night instead of say, a bar. Ninety-eight percent of binge watching occurs at home (Marsh 2014). Binge watching could also create an increase in crime. While binge watching is usually a solitary activity, 38 percent of binge watchers prefer to watch with a significant other (Marsh 2014). If binge watching is a social activity, it would fulfill a necessary condition of domestic violence by creating a social event that may not have existed otherwise. With potential mechanisms influencing the effect of binge watching on crime both positively and negatively, determining the net effect of binge watching becomes an empirical exercise.

This paper provides the first evidence linking binge-watching behavior and crime. I use runtime of newly-released Netflix Original Series as a conditionally exogenous shock on binge watching to identify the intent-to-treat effect of binge watching on reported crime incidents. Netflix releases all episodes from new seasons of their original series immediately at midnight Pacific time (i.e., 00:00 PT) on the announced release date. Release dates are often announced at least six weeks in advance. Viewers may prepare for a content shock in response to this announcement. Netflix never includes the runtime of any episode or season itself in any series release date announcements. Therefore, viewers are unable to observe the intensity of the content shock until after the season is released.

I use data from Federal Bureau of Investigation's (2020b) National Incident-Based Reporting System (NIBRS). I find reported crime is decreased by one percent on an average runtime release day. Reductions in crime are concentrated in crimes against persons and property crime. Crimes against persons decrease by a larger percentage away from a residence than in residence. Property crime, however, have a larger percentage decrease in a residence than away from a residence. Reductions in reported crimes against property do not depend on whether the season is from a new series or a continuation of an existing series. Reported crimes against persons, however, have a much larger reduction when the new seasons are continuations of existing series.

I expand the baseline specification into a finite distributed lag model to show the intertemporal effects of binge watching. I show reported crime responds in anticipation of a release by finding reported crime decreases in the evening immediately preceding a release. It is possible that binge watching is merely shifting crime away from release dates. By examining an 12-day period after release dates, I fail to reject the hypothesis that binge watching shifts crime. There is evidence to suggest that net reported crimes are reduced three days after a release, which is the the period that generates the most viewership.

This paper relates to the broad literature on media consumption and crime. The previous literature can be described in two parts. First, the behavioral response to violent content literature originated by Dahl and DellaVigna (2009) and followed by Cunningham, Engelstätter, and Ward (2016) and Lindo, Swensen, and Waddell (2020). The first two papers use violent media as a treatment on high-frequency national violent crime outcomes. Both of these papers use variation over time to identify short-run effects. Lindo, Swensen, and Waddell (2020) use violent media as a treatment as well but use agency-month crime outcomes. They identify the long-term effect of violent media by using an instrumental variable strategy made available by the variation over time and across counties. Second, the consumption externality literature started by Kendall (2007) and followed by Bhuller et al. (2013) and Diegmann (2019). These papers focus on broadband internet access to examine the effect of internet consumption on crime, particularly sex crimes. These papers use regional variation over time for identification. This paper aims to bridge these two sets of papers by examining the consumption externalities that result from binge watching using high-frequency outcomes and an identification strategy that does not use regional variation. In line with the prior research on violent media and crime, I find that violent media decreases reported crime incidents. However, I also find that there is no statistical difference between the reductions in crime from high-violence media and the reductions in crime from low-violence media.

This paper also relates to prior literature on non-incarceration incapacitation. Much of this literature focuses on policies that mandate where youth can be and when. Anderson (2014); Billings, Deming, and Rockoff (2013); Jacob and Lefgren (2003) and Luallen (2006) examines policies related to schools. Carr and Doleac (2018) investigate a juvenile curfew law. Chalfin, Danagoulian, and Deza (2019) explore the responses to seasonal and regional variation in pollen counts. The common theme across these papers is that their treatments impose restrictions. Binge watching, however, offers a choice that may increase the opportunity cost of crime.

This paper contributes to the literature in at least three ways. First, I provide the first evidence of the effect of binge watching on crime. Second, I find that crime responds in anticipation of a media release. Third, I add additional evidence that violent content does not increase violent crime.

The paper proceeds as follows. Section 1.2 provides background on binge watching and Netflix. Section 1.3 describes data sources and empirical sample construction. Section 1.4

lays out empirical methods and associated results. Section 1.5 discusses empirical results, potential mechanisms, and then concludes.

1.2 Background

Since the rapid expansion of television content creation and television set ownership in the 1940s and 1950s, consumption of newly released scripted television programming has remained largely unchanged. New programming has been restricted to over-the-air carriers and fixed to a periodic release schedule, typically one new episode per week. This release model restricted consumers to scheduling their television consumption around release times—appointment viewing.

Until the 1970s, all content was broadcast over-the-air and remained appointment viewing only. Opportunities to binge watch came when networks ran series marathons—scheduling many episodes of the same series back-to-back. The arrival of videocasette recorders (VCRs) in the 1970s and the rise of video home system (VHS) tapes added additional ways to watch content after its original airing, but was not widely adopted as a method to introduce new episodes of scripted series to consumers. VCRs provided the first opportunity for consumers to avoid appointment moving by time shifting their television consumption. VCRs also provided the first chance to regularly binge watch. Even though binge watching was possible, it was often impractical. For example, *Star Trek: The Next Generation*'s first season was released on 25 VHS tapes—one for each episode. In order to binge watch *Star Trek: TNG* on VHS, a viewer would need to change tapes between every episode. Digital video discs (DVDs) were introduced in the 1990s. To binge viewers, DVDs were a strictly superior VHS tape. Instead of 25 VHS tapes, *Star Trek: TNG*'s first season was released on 7 DVDs. DVDs are also easier to store than VHS tapes, with the first season box set for *Star Trek: TNG* occupying similar shelf space to just two VHS tapes.

The 1990s brought two additional important technology advances: digital video recorders (DVRs) and video on demand (VOD). DVRs became available in 1999 and significantly reduced costs associated with binge watching. DVRs are typically offered as a multi-purpose device included with a paid cable television subscription. DVRs often combine digital recording capability with the hardware used to decode the encrypted television single sent over cable infrastructure. DVRs eliminated the need to change or store tapes or discs by storing all episode recordings on an internal hard disk drive. Since DVRs are multi-purpose devices, a viewer could switch between watching over-the-air broadcasts and his or her recorded content through a simple interface. DVRs also allowed for an interaction between the television schedule and the recording device, which significantly reducing the cost of recording an over-the-air broadcast to watch later. The introduction of VOD was the most important to binge watchers. Similar to DVRs, VOD was first offered as an additional service to paid cable television subscribers and utilized a convenient interface on a multi-purpose device. VOD requires no physical storage but requires the trade-off of requiring constant cable network connectivity. Unlike DVRs, VOD delivers content immediately upon viewer request with no prior requirements. After the introduction of VOD, few limitations to binge viewing remained. Content was limited to previously aired content.

Netflix launched with a direct-to-consumer DVD rental product in April 1998. Netflix's core business remained effectively unchanged until February 2007 when Netflix expanded their content delivery options with a streaming video-on-demand (SVOD) service. Netflix's initial streaming service was limited in two ways. First, consumers were given a finite amount of streaming hours (e.g., 18 streaming hours were granted to holders of the $18 per month subscription plan). Second, consumers were restricted to viewing content via a personal computer (Anderson 2007; Helft 2007). The former restriction was abandoned by January 2008. The latter restriction started to be relaxed in 2008 as Netflix began to partner with consumer electronics firms to expand their streaming service to more internet connected devices. By 2010, Netflix Streaming had reached all types of internet connected consumer electronic devices (e.g., gaming systems, TV set-top boxes, Blu-ray disc players, internet-connected televisions, cell phones, tablets) (Netflix 2019). Netflix was a content re-distributor until February 2013 when they released *House of Cards*'s first season. Netflix releases all their original content at midnight Pacific time (i.e., 00:00 PT).[1] By releasing all

1. ↑*House of Cards*'s third season was released at 3 a.m. Pacific time. This appears to be the only exception.

the episodes in the first season simultaneously, Netflix granted their viewers a never-before-offered consumption opportunity: to binge watch newly available content.

Consumer demand is driving the growth in binge-watching behavior.[2] The majority of SVOD users want to binge watch and avoid appointment viewing (Nielsen 2013). Seventy-three percent of viewers report having positive feelings toward binge watching. Seventy-nine percent of viewers say binge watching increases their enjoyment of what they watch (Netflix 2013). Binge-watching behavior may be further encouraged by the fear of missing out (FOMO) (Conlin 2015, 21; Conlin, Billings, and Averset 2016). Przybylski et al. (2013) define FOMO "as a pervasive apprehension that others might be having rewarding experiences from which one is absent". In the case of seasons where episodes are released all at once, viewers with FOMO would begin binge watching a new season shortly after the release date. This appears to be the case with Netflix viewers: audiences are 25 times larger in the first three days after a release than they are in the following two months (Flomenbaum 2016).

Streaming services have embraced the trend toward binge watching. Netflix employs an immediate release model for their original series to allow their viewers to binge watch immediately. "Our viewing data shows that the majority of streamers would actually prefer to have a whole season of a show available to watch at their own pace," said Ted Sarandos, Netflix's chief content offer (Jurgensen 2013). Amazon Prime Video and Hulu also have released some, but not all, new seasons under the all-at-once model. Several streaming services have changed to a different series production model to encourage binge watching. Series are created with episodes ending in cliffhangers to draw viewers immediately into the next episode rather than use suspense to encourage viewers to return later to watch the next episode (Conlin 2015, 3).

Amazon Prime Video, Hulu, and Netflix do not release viewership numbers. Nielsen launched a SVOD measurement system in October 2017. Nielsen's SVOD measurement system tracks American viewers that watch on a television through audio capture technology (Nielsen 2017). The second season of *Stranger Things* was the first release to be covered. Nielsen reported that 15.8 million viewers, with 11 million viewers between the ages 18–49,

2. ↑See Conlin (2015) for more details.

had watched the first episode of the second season within three days after its release (Otterson 2017). But this is still an undercount. Seventy percent of Netflix viewing happens on televisions (Kafka 2018).

In order to provide a comparison with a film release, we can do a back-of-the-envelope exercise.[3] The first two episodes of the second season of *Stranger Things* have a combined runtime of 104 minutes, close to a typical film release. Nielsen reported that 13.7 million viewers had watched the first two episodes within three days (Otterson 2017). After adjusting for the television-only viewership numbers, an estimated 19.6 million watched. These 19.6 million viewers are equivalent to the second-most viewers in the first three days of any film released in 2017. The first three-day viewership of *Stranger Things*'s second season is between the estimated first three-day viewership of *Star Wars: Episode VIII - The Last Jedi* and the live-action remake of *Beauty and the Beast* (Box Office Mojo 2018).[4]

Stranger Things season two comprises of nine episodes, totaling 464 runtime minutes. 361,000 people watched the complete second season of *Stranger Things* in the first 24 hours after its release (Otterson 2017). After adjusting for the television-only viewership, about 515,000 people allocated almost eight hours of their day to be captivated by the show.

Nielsen also reports that young adults watch a lot on the first day. Young adults watched an average of 113 runtime minutes, 225 runtime minutes, and 168 runtime minutes watched for *Fuller House* season three, *The Defenders* season one, and *House of Cards* season five, respectively, in the first day of release (Levin 2017).[5]

1.3 Data

I construct a sample beginning in 2007—the year that Netflix Streaming was introduced—and ending in 2018—the last year in which NIBRS data is available. Netflix Original series releases began in 2013. Thus, I observe six years with treatment and six years without

3. ↑I use data on the first three days of gross revenue for domestic films from Box Office Mojo (2018) and the average ticket price of $8.97 in 2017 (National Association of Theatre Owners 2019).

4. ↑*Star Wars: Episode VIII - The Last Jedi* had an estimated 24.5 million viewers in the first three days. *Beauty and the Beast* had an estimated 19.4 million viewers in the first three days.

5. ↑Levin (2017) reports young adults watched an average of 4.4 episodes, 4.6 episodes, and 3.2 episodes on the first day of *Fuller House* season three, *The Defenders* season one, and *House of Cards* season five, respectively. I assumed these averages cover the first episodes available and calculated the estimated runtime.

treatment. I aggregate crime incidents on an incident level to the county level. Then using county identifiers, I link the data with time zones and daily weather data. Lastly, I aggregate the sample into a daily national time series.

1.3.1 Series Data

I construct the treatment variable using the runtime of Netflix Original series releases. Netflix, Hulu, and Amazon Prime Video have all released seasons where all the episodes released were made immediately available, but Netflix is the only platform to maintain this release model for all of their original series. Netflix is also the preferred steaming choice for many consumers. A recent survey (Marsh 2014) showed that of the 35% of viewers that use preferred to binge watch through a streaming service where 25 percentage points chose Netflix as their platform opposed to 3 percentage points and 1 percentage point for Hulu and Amazon Prime respectively.[6]

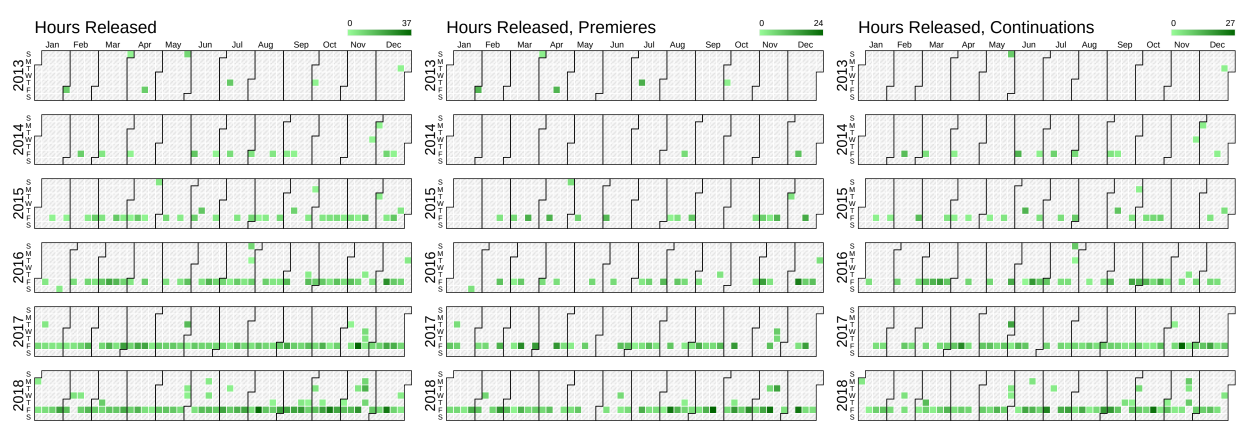
I build a release schedule for Netflix Original Series produced in English that meet the following criteria: multiple episodes from a single season are made available at once and the United States market release is the worldwide premiere.[7] The former condition ensures that the content is an immediately binge-watchable series. The latter condition ensures that the content is not available through other distribution channels, such as peer-to-peer file-sharing networks. I assign an indicator for whether the release is the first time the series is available (premiere) or if the release is a continuation of an already aired program (continuation). Continuations include all season beyond the first season (e.g., *House of Cards* season 2 and beyond), revivals (e.g., *Arrested Development* season 4 and beyond, all seasons of *Fuller House*), and initial series where the major character was introduced in another series (e.g., *Luke Cage* season 1 and beyond). From 2013–2018, Netflix released 424 seasons—182 premieres and 242 continuations—from 232 different titles on 223 different dates, covering 4 Sundays, 5 Mondays, 12 Tuesdays, 7 Wednesdays, 15 Thursdays, 179

6. ↑The remaining six percentage points was shared between HBO Go and Showtime Anytime.
7. ↑I use Wikipedia as a guide: https://en.wikipedia.org/wiki/List_of_original_programs_distributed_by_Netflix.

Fridays, and 1 Saturday. Conditional on there being a release, the conditional mean of the treatment variable, runtime hours, is 9.7.

I supplement the release schedule with user-submitted data from IMDb.com, Inc. (2020). IMDb—Internet Movie Database—is an online database that contains summary information on films, television series, and internet videos, among other things that has been submitted by its users. Information submitted to IMDb may be submitted by anyone, including viewers, those involved in production, or other content creaters. I use episode-level runtime minutes to determine the runtime hours of a season. If the user-submitted episode runtime is missing, I assign the minimum observed value across all series episodes as the imputed value. I use user-submitted review ratings as a proxy for quality of release. Due to quality issues in user-submitted ratings, I use all IMDb series ratings to construct a within-year above and below median rating variable. This procedure assigns one value per series to all episodes in the sample.

Figure 1.1 displays a heat map of runtime hours for Netflix Original Series by release date from 2013 to 2018 that remain in our sample. Darker shades indicate more content was released on that day. The diagonal hex pattern indicates that there was no release that day. We can see that the number of release dates and the average number of hours released per release date has increased each year.



(a) All releases. (b) Premiere releases. (c) Continuation releases.

Figure 1.1. Hours Released of Netflix Original Series by Day, 2013–2018. Each cell represents a day. Darker shades indicate more hours of new content was made available.

In order to categorize series by content, I add data from Common Sense Media (2020). Common Sense Media (CSM) is a nonprofit organization that aims to help parents manage the media options available to children. Among other objectives, CSM has experts review films, television series, and internet videos across several content categories by using an 0–5 rating with a zero rating indicating the lack of an element and a five rating indicating an abundance. For television programs, reviewers rank content in the following categories: overall rating; educational value; positive messages; positive role models and representations; violence; sex; language; consumerism; and drinking, drugs, and smoking. Reviewers also suggest a minimum appropriate age for a viewer. CSM reviews are more granular than the government-mandated content reviews provided by the TV Parental Guidelines Monitoring Board. Gabrielli et al. (2016) show that the content ratings from the TV Parent Guidelines Monitoring Board were ineffective in identifying violence, sex, and substance abuse. Since only series-level reviews are available, I assign the content ratings to all seasons. Fortunately, CSM experts update their reviews in response to new releases of a series. Figure 1.7 is the violence rating description from CSM's review of *Stranger Things* (Slaton 2020). *Stranger Things* receives a three out of five indicating a moderate amount of violence present in the series. The reviewer describes several specific violent sequences and places the latter sequences in the third season. CSM reviewed 210 of the 238 in-sample Netflix Original Series released between 2013 and 2018, leaving 31 of 438 seasons in-sample without any content review information. Appendix Tables 1.18–1.25 show all in-sample Netflix Original Series with their TV Parent Guidelines Monitoring Board ratings, CSM reviewer ratings, CSM violence rating, IMDb ratings median group, the first year the series had a premiere on Netflix Streaming, and an indicator for whether any of the series's seasons are categorized as a continuation during the sample period.

I split series into two types three separate times. First, I separate all series into high violence or low violence based on the CSM reviewer's violence rating. I categorize a series as high violence for ratings of 3–5 and low violence for ratings 0–2. Next, I assign quality ratings based on either the IMDb median group or the CSM reviewer's overall rating. For the IMDb-based categories, high-quality series have an above-median within-year rating in the series's premiere year. For CSM overall rating categories, due to the right skew of the observed ratings, I assign high quality to ratings 4–5 and low quality to ratings 0–3.[8]

1.3.2 Crime Data

I obtain crime data from NIBRS, which is part of the Uniform Crime Reporting (UCR) Program. NIBRS is the most detailed crime data currently available that covers multiple states. NIBRS is not nationally representative. Agencies report data to NIBRS either voluntarily or due to state-level mandates.[9] In 2016, NIBRS covered 33 percent of the population and accounted for 28 percent of all crimes reported to the UCR Program (Federal Bureau of Investigation 2017).

NIBRS collects data for 52 different offenses at the incident level—Group A offenses—with 10 additional offenses at the arrest level—Group B offenses. I use Group A offenses only. Offenses can be separated into three categories based on victim classifications: crimes against persons, crimes against property, and crimes against society. Crimes against persons are crimes in which persons are injured or restricted. Crimes against property are crimes in which property is illicitly obtained or damaged. Crimes against society include crimes like drug use and prostitution, where there is a negative externality on society itself. The modal reported offenses are assault, theft, and drug offenses for crimes against persons, crimes against property, and crimes against society, respectively. Appendix Table 1.17 lists all offenses and their respective offense code, description, and groupings.

Agencies in NIBRS report detailed crime data at the incident level. Each incident report includes the reported offense codes as well as the date, time, location, and a reporting flag for if the record was created on the actual incident date. Further information such as data on victims and offenders is reported if available at the time of the report.

Akiyama and Nolan (1999) show that aggregating the data at the incident level, offender level, and victim level can produce different counts of reported crime. I aggregate 2007–2018 incident-level data to the national level. Before aggregating, I restrict the sample to local and county reporting agencies. I keep incidents that are reported on the day in which

8. ↑Nearly half the series in the sample are assigned an overall rating of 4 by their respective CSM reviewer.
9. ↑A federal mandate to report incident-level crime data to NIBRS begins January 1, 2021.

they occurred from agency-years where the agency reports to NIBRS for 12 months. I create binary indicators for incident types before aggregating to prevent over counting offenses. For example, an incident that reports a robbery, an assault, and a motor vehicle theft has offense codes related to two crimes against persons and two crimes against property. However, the incident would be coded as one crime against person incident and one crime against property incident. In order to align exposure to Netflix releases, I convert reported incident times from local time into Pacific time. I create days based on the 24-hour period after midnight Pacific time. I drop Arizona agencies since Arizona does not observe daylight saving time and has very few reporting agencies during the study period. After the time adjustment, I drop December 31 in all years since there are less than 24-hour reporting periods for agencies that are not in the Pacific time zone and do not report incidents in the following year.[10] Figure 1.2 shows NIBRS coverage by county for 2007–2018. Each shaded county contains at least one reporting agency-year in the estimation sample. The sample contains reporting agencies from 40 states, mostly from the eastern United States and covering the Eastern and Central time zones. Each year more reporting agencies meet the sample selection criteria. In 2007, the first year of our sample, 3,645 local agencies in 33 states covering 69.49 million people reported crime incidents. In the last year of our sample, 2018, 5,191 local agencies in 40 states covering 105.68 million people reported crime incidents. Figure 1.8 shows NIBRS coverage by county for each year in the sample separately, as well as describing the growth of reporting agencies, the number of states covered, and total population covered.

Appendix Figure 1.9 shows heat calendars for Group A crime incidents, reported crimes against persons, reported crimes against property, and reported crimes against society after sample restriction and time zone adjustment. Darker shades indicate more daily reported incidents. Seasonal patterns in reported incidents are apparent. Monthly reported incidents are highest in the summer months and lowest in the winter months. Friday has the highest number of reported incidents, while Sunday has the lowest. Reported incidents are high on the first day of the month, as well as New Year's Day. Reported incidents are lowest on Thanksgiving and Christmas Day. Appendix Figures 1.10–1.11 show heat calendars for each

10. ↑December 31 is New Year's Eve. New Year's Eve often has the most reported incidents in a year. However, dropping these observations has no quantitative effect on the results presented in this paper.

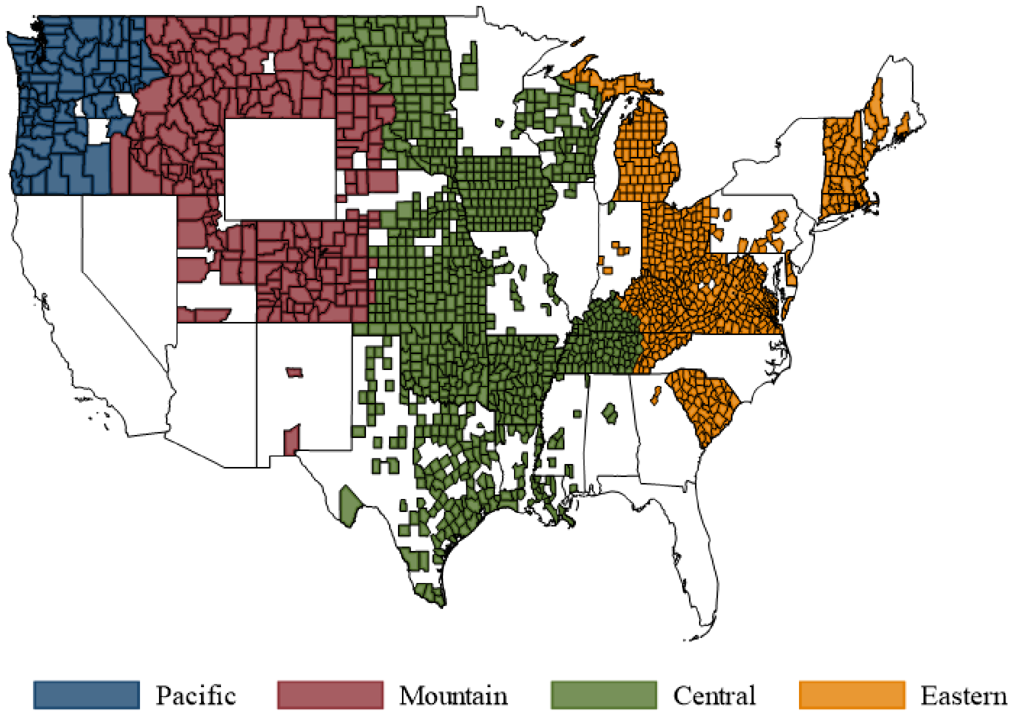


Figure 1.2. NIBRS Coverage by County, 2007–2018. Each shaded county has at least one reporting agency that reports crime incidents for each month in at least one year.

crime category separated by location type. Appendix Figures 1.12–1.15 show heat calendars for each crime category separated by time bins. Each way of classifying reported incidents displays distinct and strong seasonality.

1.3.3 County-level Data

County-level time zones were gathered from the National Weather Service (2019). Counties with multiple listed time zones were assigned the first listed time zone.

Daily weather data was obtained from Schlenker and Roberts (2006, 2009). Schlenker and Roberts (2006) model daily weather by using monthly estimates from the PRISM Climate Group and daily data from weather recording stations to extrapolate daily weather. PRISM utilizes models to assign minimum temperature, maximum temperature, and precipitation at a fine level while accounting for complex terrain and other environmental factors. Since Schlenker and Roberts (2006) model daily weather, weather is observed for every county-day regardless of the lack of equipment, equipment malfunctions, or recording errors while also accounting for spatial differences in weather stations. Following Dahl and DellaVigna (2009), I create seven indicators for daily county weather. I code any day with more than one tenth of an inch of precipitation as a rainy day. I create three indicators for hot days based off maximum temperature and three indicators for cold days based off minimum temperature. Specifically, the hot indicators are for maximum temperature in Fahrenheit between 80 and 90 degrees, 90 and 100 degrees, and more than 100 degrees. The cold indicators are for minimum temperature in Fahrenheit between 20 and 32 degrees, 10 and 20 degrees, and less than 10 degrees. I match the weather indicators to counties at the agency-day level and use agency population coverage as a weight to aggregate the weather indicators into continuations weather variables at the national level.

1.4 Empirical Results

I present a baseline model to estimate the contemporaneous effect of binge watching on crime. I expand the model to split the released runtime into different categories to explain the baseline effect. I present a finite distributed lag model to investigate intertemporal effects. Summing the coefficients of the finite distributed lag model provides the short-run effect when the model is estimated in levels. All models are estimated in levels in order to consistently compare results with the finite distributed lag model.[11]

All effects presented are intent-to-treat effects since I do not observe binge watching on an individual level. Due to the heterogeneity in binge watching and variation in runtime hours across release dates, a back-of-the-envelope calculation to estimate the treatment-on-the-treated is hard to defend. It is best to think about the estimates as lower bounds on the treatment-on-the-treated.

11. ↑Models are estimated in Stata 16.1 using the command reghdfe (Correia 2017).

1.4.1 Contemporaneous Effects on Crime

First, I look into the contemporaneous effects of a Netflix release on daily reported crime incidents through the following regression

$$y_t = \beta \text{Netflix}_t + \psi \text{weather}_t + \alpha_s + \gamma_t + \varepsilon_t, \tag{1.1}$$

where y_t is the number of reported incidents (e.g., crimes against persons) on day t; Netflix_t is hours released of new series on Netflix Streaming on day t ; α_s is a set of seasonal fixed effects; γ_t is a set of holiday fixed effects; weather_t is a set of population-weighted weather controls for hot, cold, and rain; and ε_t is an idiosyncratic error term. I estimate (1.1) using ordinary least squares. Seasonal fixed effects include day-of-week fixed effects, daylight savings time fixed effects, month $\times$ day-of-month fixed effects, and year $\times$ month fixed effects. Day-of-week fixed effects control for correlation between days of the week, most importantly of which is that Friday is the modal day for Netflix releases and the modal day for reported crime incidents. Daylight savings fixed effects include an indicator for the period covered by daylight savings time and separate indicators for the start day of daylight savings time and the end day of daylight savings time. Daylight savings fixed effects account for changes in daylight exposure as well as the shorter and longer days associated with the switch to and away from daylight savings time.[12] Month $\times$ day-of-month fixed effects control for within-day seasonality. We previously discussed such month-day specific seasonality that persists year-over-year (e.g., the first day of the month, and constant day holidays such as Christmas Day and New Year's Day) and show in Appendix Figures 1.9–1.15. Year $\times$ month fixed effects account for month-long shocks that may affect crime during the study period. For example, the events related to the shooting of Michael Brown Jr. in Ferguson, Missouri in 2014 may have had an short-run shock to crime level as well as a persistent effect in the following months or years. Since I require that reporting agencies report the full 12 months in a year to be included in the sample, year $\times$ month fixed effects also account for the movement of agencies in and out of the sample. Holiday fixed effects include all holidays that repeat on an regular

12. ↑Doleac and Sanders (2015) and Umbach, Raine, and Ridgeway (2017) both show that daylight savings time affects reported crimes.

schedule but not a constant day (i.e, Martin Luther King Jr. Day, Presidents' Day, Memorial Day, Labor Day, Columbus Day, Easter, Thanksgiving, and Mother's Day). I expand the set of holiday indicators to include the typical holiday observance periods around the associated holidays (see Appendix 1.B.1 for details). I also add an indicator for Super Bowl Sunday. Weather controls include the population-weighted continuous variables for hot, cold, and rain described in the data section.[13] I report heteroskedastic and autocorrelation consistent (HAC) standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994).

In order to further explain the contemporaneous effects, I split Netflix_t in (1.1) into multiple variables. Specifically, (1.1) becomes

$$y_t = \sum_{j \in J} \beta_j \text{Netflix}_{j,t} + \psi \text{weather}_t + \alpha_s + \gamma_t + \varepsilon_t, \tag{1.2}$$

where J is a set such as {premiere, continuation}, {high violence, low violence}, or {high quality, low quality} and the rest of the variables are as defined in (1.1).

Table 1.1 panel A shows the baseline results for daily reported Group A offenses estimated from (1.1). Column 1 is the naïve regression without any controls. The naïve regression shows a positive effect with each hour of content released increasing crime by 167 reported incidents. The difference in coefficients from columns 1 to 2 shows the positive correlation between hours released and reported incidents that is accounted for by adding seasonal fixed effects. After adding seasonal fixed effects in column 2, binge watching reduces reported incidents by 14 incidents per hour released. Once seasonal fixed effects are added, the effect of released runtime hours on reported incidents is stable with the addition of holiday fixed effects (column 3) and weather controls (column 4). The coefficient in columns 2–4 are all about -14 and statistically significant at the one-percent level. For an average runtime release, this effect represents contemporaneous reduction of about 136 reported incidents per release day or a one-percent reduction relative to the mean.[14]

13. ↑Weather is correlated with crime. See Jacob, Lefgren, and Moretti (2007) or see Murataya and Gutierrez (2013) for a meta-analysis.

14. ↑-14.02 reported incidents per hour released times 9.7 hours released on an average release day divided by $12,770.6$ reported incidents per day $= 0.0106$.

Table 1.1. The Effect of Binge-watchable Hours Released on Reported Group A Crime Incidents

	(1)	(2)	(3)	(4)
A. *All seasons*				
Hours released	166.88^{***}	-14.21^{***}	-14.29^{***}	-14.02^{***}
	(13.05)	(3.27)	(3.39)	(3.37)
B. *First season and continuations separated*				
Hours released, premiere	135.84^{***}	-18.32^{***}	-17.83^{***}	-14.31^{***}
	(16.08)	(5.24)	(4.87)	(4.26)
Hours released, continuation	193.76^{***}	-10.56^{**}	-11.14^{**}	-13.75^{***}
	(15.09)	(4.38)	(4.51)	(4.15)
Seasonal FE		✓	✓	✓
Holiday FE			✓	✓
Weather controls				✓
$\overline{y}$	12,770.6	12,770.6	12,770.6	12,770.6
H_0: premiere = continuation	0.002	0.262	0.288	0.911

Notes: Each column in each panel is a separate regression. Seasonal FE include fixed effects for day-of-week, daylight savings time, month × day-of-month, and year × month. Weather controls include variables for hot, cold, and rain. Holiday FE include fixed effects for each of the following days: Super Bowl Sunday, Martin Luther King Jr. Day, Presidents' Day, Memorial Day, Labor Day, Columbus Day, Easter, Thanksgiving, Mother's Day, and the extended weekends associated with Martin Luther King Jr. Day, Presidents' Day, Memorial Day, Labor Day, Columbus Day, and Thanksgiving. $\overline{y}$ is the mean of the dependent variable. H_0: premiere = continuation is a p-value from a Wald test related to the coefficients in Panel B. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

There may be a stronger contemporaneous effect in response to new releases that continue an existing narrative. In Table 1.1 panel B, I investigate by separating runtime hours from premiere seasons and runtime hours from continuation seasons then estimating (1.2). The coefficients in columns 1–4 follow the same pattern as the main specification presented in Panel A. After we account for seasonality in column 2, the coefficients for premiere seasons and continuation seasons are all negative and not statistically different from each other. However, while not statistically different, the coefficients in columns 2 and 3 show that the overall reduction in reported incidents loads onto premiere seasons. After weather controls, however, the difference in coefficients for premiere seasons and continuation seasons is muted. These changes suggest that viewers are more likely to watch a premiere season if the weather is poor (i.e., extreme hot, extreme cold, and/or rainy) whereas they're more likely to be exposed to crime incidents if the weather is nice (i.e., temperate and/or no rain). Further, the change in the effect for continuation season suggests that viewers are not concerned with weather changes when choosing to binge watch a continuation.

We can further explore the effects of binge watching by taking advantage of the rich crime data provided by NIBRS. All reported incidents can involve four types of people: (1) offender, (2) officer, (3) victim, and (4) bystander. All reported incidents must have an offender who commits the offense and an officer that writes the incident report. By definition, crimes against persons must have a victim that suffers the offense. All incidents could be reported to the police by any type of person. Crimes against property and crimes against society are exposed to one less agent than crimes against persons. All types of people can binge watch.

Table 1.2 presents the regression results for (1.1) and (1.2) in panels A and B, respectively, for Group A crimes, crimes against persons, crimes against property, and crimes against society with the column title listing the type of reported crime used as the dependent variable. All models use the full specification with weather controls, seasonal fixed effects, and holiday fixed effects. Column 1 is the same as Table 1.1 column 4 and is reproduced for convenience. The remaining three columns are a decomposition of reported Group A offenses.[15] We see a reduction of 4.25 incidents per runtime hour for crimes against persons in panel A column 2. Further, panel A column 3 shows a reduction in property crime incidents by 10.86 per runtime hour. These effects are reductions of 1.4 percent and 1.2 percent on an average release day, respectively, relative to their means; each above the baseline effect of a 1 percent decline. Panel A column 4 shows that the effect of runtime hours on crimes against society is positive and not statistically significant. Panel B provides results that split the runtime hours into premiere and continuation seasons. We see that reduction in reported incidents for crimes against persons is larger for continuation seasons and statistically different than the lesser effect for premiere seasons. Crimes against property incidents have the same response to premiere seasons and continuation seasons. Surprisingly, crimes against society have a different signed effect for premiere seasons and continuation seasons, with the positive effect

15. ↑Robberies create a minor overlap between reported crimes against persons and crimes against property.

for continuation seasons being statistically-significant at the five-percent level. Even though we have established that there is an overall reduction in aggregate crime incidents in response to runtime hours released, we see that there are heterogeneous effects present by crime type. In order to explain these differences, we must further categorize reported crime incidents.

Table 1.2. The Effect of Binge-watchable Hours Released on Reported Crime Incidents, Specific Crimes

	(1) Group A	(2) Person	(3) Property	(4) Society
***A.** All seasons*				
Hours released	-14.02^{***}	-4.25^{***}	-10.86^{***}	0.27
	(3.37)	(1.28)	(2.20)	(0.69)
***B.** First season and continuations separated*				
Hours released, premiere	-14.31^{***}	-2.02	-11.56^{***}	-1.46
	(4.26)	(1.42)	(2.64)	(1.32)
Hours released, continuation	-13.75^{***}	-6.24^{***}	-10.24^{***}	1.81^{**}
	(4.15)	(1.43)	(2.85)	(0.92)
$\overline{y}$	12,770.6	2,925.0	8,438.7	1,586.8
H_0: premiere = continuation	0.911	0.005	0.688	0.064

Notes: Each column in each panel is a separate regression with the column title describing the type of reported crime used as the dependent variable. Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. H_0: premiere = continuation is a p-value from a Wald test related to the coefficients in Panel B. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** p $<$ 0.01, ** p $<$ 0.05, * p $<$ 0.1

We are interested in the effects on crime by location since most binge watching happens at home and crimes types vary by location. In the sample, reported incidents occur away from a residence 56 percent, 40 percent, 58 percent, and 79 percent of the time for Group A offenses, crimes against persons, crimes against property, and crimes against society, respectively.

Table 1.3 shows results of (1.1) including the seasonal fixed effects, holiday fixed effects, and weather controls with the column title listing the type of crime incident used as the dependent variable. Panel A shows the effects of binge watching on the different types of crime in all locations. This panel is the same as Table 1.2 panel A and is reproduced for convenience. Panel B shows results for reported crimes away from a residence. Panel C shows results for reported crimes that happen in a residence. Column 1 shows the decreases in reported Group A offenses away from a residence and in reported Group A offenses away from a residence are close in magnitude. Relative to the mean, however, Group A offenses in a residence have a larger percentage decrease in reported incidents on an average runtime release day than Group A offenses away from a residence (1.4 percent against 0.8 percent). We see the same pattern holds for crimes against property in regards to magnitude and percentage changes relative to the mean. Similarly we see that reported incidents away from a residence and in a residence are close in magnitude for crimes against persons. However, unlike Group A crimes and crimes against property, crimes against persons away from a residence has a larger percentage decrease in incidents on an average runtime day relative to mean than crimes against persons in a residence (2.0 percent against 1.0 percent). Lastly, we see that there is a marginally statistically significant increase in crimes against society away from a residence and a statistically significant decrease in crimes against society in a residence. The reduction in crimes against society in a residence indicate a 2.2 percent decline on an average runtime release day relative to the mean, which is the largest reduction observed.

We see that since crimes against property are the majority of reported crime incidents away from a residence and in a residence, it is no surprise that Group A crimes shares a pattern with crimes against property. The larger percentage of reported incidents declining in a residence suggests two possibilities. First, potential criminals are watching the new release instead of exploring criminal opportunities—the incapacitation effect. Second, potential victims of property crime have less exposure to these incidents on release dates. If potential victims stay home to watch a series, by definition, they are in a residence, which makes that residence more costly to target for a potential criminal. Additionally, potential victims staying at home removes them from high crime places such as bars or night clubs. This idea of removing potential victims from high crime places also impacts potential offenders in the case of crimes against persons and likely explains the larger percentage of declines of incidents away from a residence. The lesser decline in crimes against persons in a residence may be motivated more by the replacement of potential conflict moments with focused watching, but the effect is less than the reduction in crimes away from a residence since less factors that

Table 1.3. The Effect of Binge-watchable Hours of Netflix Releases on Reported Crime Incidents by Location

	(1) Group A	(2) Person	(3) Property	(4) Society
A. *All locations*				
Hours released	-14.02^{***}	-4.25^{***}	-10.86^{***}	0.27
	(3.37)	(1.28)	(2.20)	(0.69)
$\overline{y}$	12,770.6	2,925.0	8,438.7	1,586.8
B. *Crimes away from a residence*				
Hours released	-5.94^{***}	-2.41^{***}	-5.40^{***}	1.04^{*}
	(2.16)	(0.85)	(1.39)	(0.56)
$\overline{y}$	7,199.7	1,174.7	4,922.1	1,248.2
C. *Crimes in a residence*				
Hours released	-8.07^{***}	-1.84^{***}	-5.46^{***}	-0.77^{***}
	(1.42)	(0.54)	(0.92)	(0.20)
$\overline{y}$	5,571.0	1,750.2	3,516.6	338.6

Notes: Each column in each panel is a separate regression with the column title describing the type of reported crime used as the dependent variable. Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** p $<$ 0.01, ** p $<$ 0.05, * p $<$ 0.1

lead to a crime against a person are mitigated. The reduction in crimes against a society in a residence likely follows similar reasoning that the incidents that are typically reported are reduced since some of the factors that led to the incidents are replaced by binge watching, either by the reporting party or the potential criminals.

Several papers have been motivated to investigate the potential link between violent conduct and violent media. Continuing this tradition, I investigate the possible link by separating runtime hours released into high violence content and low violence content. Table 1.4 shows results using (1.2) with releases separated by violence. All regressions include seasonal fixed effects, holiday fixed effects, and weather controls. Panel A presents the results across crime types with runtime hours separated by violence rating. Across columns 1–3, we see that the effect of low violence runtime hours and high violence runtime hours is similar in magnitude and the effects are not statistically different for Group A crimes, crimes against persons, and crimes against property, respectively. In column 4, we see that the effects of low violence runtime hours and high violence runtime hours on crimes against society have opposite signs, but neither coefficient is statistically significant on its own or statistically different from each other. Since no set of coefficients for low violence runtime hours and high violence runtime hours are statistically different from each other, we can conclude that violence has no effect on the reduction of crime that we observe in any crime type.

Table 1.4. The Effect of Binge-watchable Hours of Netflix Releases on Reported Crime Incidents by Violence Rating

	(1) Group A	(2) Person	(3) Property	(4) Society
Low violence	-14.19^{**}	-4.57^{**}	-11.50^{***}	1.21
	(5.81)	(1.90)	(3.54)	(1.11)
High violence	-14.16^{***}	-4.27^{***}	-10.61^{***}	-0.40
	(3.83)	(1.49)	(2.54)	(1.01)
$\overline{y}$	12,770.6	2,925.0	8,438.7	1,586.8
H_0: low = high violence	0.995	0.888	0.814	0.342

Notes: Each column in each panel is a separate regression with the column title describing the type of reported crime used as the dependent variable. Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. H_0: low = high violence is a p-value from a Wald test related to the coefficients in the table. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

Since crimes against persons are mostly assaults, column 2 provides a close comparison to the results presented in Dahl and DellaVigna (2009) and Cunningham, Engelstätter, and Ward (2016), where assaults are used as the outcome variable.[16] In each paper, they find that violent media decreases violent crime when violent crime is measured in assaults. I find that violent runtime hours reduce crime by about 4.6 incidents per run time hour. This

16. ↑Dahl and DellaVigna (2009) and Cunningham, Engelstätter, and Ward (2016) code assaults as aggregated assault, simple assault, and intimidation.

effect is statistically significant and has the same sign as the previous literature. Consistent with their results, I find that violent media does not increase violent crime.

Since viewership numbers are unavailable, perhaps the perceived quality of a series is an appropriate proxy. Further, higher rated series may have viewers that are more strongly influenced by the FOMO. Since we don't observe an objective measure of quality, I present results from both measures of quality that we do observe in Table 1.5. I separate the runtime hours into high quality and low quality for the two measures and estimate (1.2). All regressions use the full specification with weather controls, seasonal fixed effects, and holiday fixed effects. Panel A sorts runtime hours based on their series rating relative to a within-year median for series released that year. Panel B divides runtime hours into groups based on the CSM reviewer's overall rating. In both panels, we see that the coefficients are not statistically different from each other. Regardless of the imperfect measures of quality, we see that quality does not change the effect of runtime hours released on reported crime incidents regardless of the crime type. It should also be noted that violence could be a proxy for quality. Our results in Table 1.4 and Table 1.5 are consistent with this idea.

1.4.2 Intertemporal Effects on Crime

In order to investigate the pre-treatment and post-treatment effects, I extend (1.1) by adding lags and leads of Netflix_t. I estimate the following finite distributed lag model over 21 days

$$y_t = \sum_{k=-9}^{11} \beta_k \text{Netflix}_{t-k} + \psi \text{weather}_t + \alpha_s + \gamma_t + \varepsilon_t, \tag{1.3}$$

where there are 9 leads and 11 lags of Netflix_t along with the contemporaneous treatment. All other variables are as defined in (1.1). Eleven lags are used so the entire following weekend is included in the lags. Almost half the viewers that begin a season finish watching the season with seven days regardless of genre (Jurgensen 2013). Nine leads are used to include the entire prior weekend in the leads. Before the release of continuation seasons, viewers could spend time in the preceding days binge watching to catch up before the new season arrives.

Table 1.5. The Effect of Binge-watchable Hours of Netflix Releases on Reported Crime Incidents by Quality Ratings

	(1) Group A	(2) Person	(3) Property	(4) Society
A. *Hours released by quality (IMDb)*				
Low quality	-18.34^{***}	-5.55^{***}	-13.21^{***}	-0.34
	(4.95)	(1.72)	(3.33)	(1.02)
High quality	-10.39^{**}	-3.17^{**}	-8.92^{***}	0.80
	(4.28)	(1.52)	(2.78)	(1.03)
B. *Hours released by quality (CSM)*				
Low quality	-18.99^{***}	-5.80^{**}	-14.85^{***}	0.56
	(6.23)	(2.30)	(4.12)	(1.56)
High quality	-13.19^{***}	-4.15^{***}	-10.41^{***}	0.51
	(4.58)	(1.59)	(2.92)	(1.06)
$\overline{y}$	12,770.6	2,925.0	8,438.7	1,586.8
A. H_0: low = high quality	0.220	0.241	0.323	0.463
B. H_0: low = high quality	0.455	0.544	0.373	0.981

Notes: Each column in each panel is a separate regression with the column title describing the type of reported crime used as the dependent variable. Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. ***A.*** H_0: low = high quality is a p-value from a Wald test related to the coefficients in Panel A. ***B.*** H_0: low = high quality is a p-value from a Wald test related to the coefficients in Panel B. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

I estimate (1.3) for reported Group A offenses. I plot the coefficients with the 95-percent confidence bands based on HAC standard errors in Figure 1.3. Release day is represented as 0 on the horizontal axis.

Moving right from day zero, we see that the coefficients are negative and statistically significant for day zero and day one. Our results for the day of release and the day after release are consistent with the contemporaneous effects we previously discussed in Table 1.2. At days two through seven post release, coefficients are not statistically different from zero.

At day eight after a release, there is a statistically significant decrease in reported incidents. With Friday as the modal release day, eight days after the modal release would be the next Saturday. This eight day after a release may be the easiest day to finish binge watching

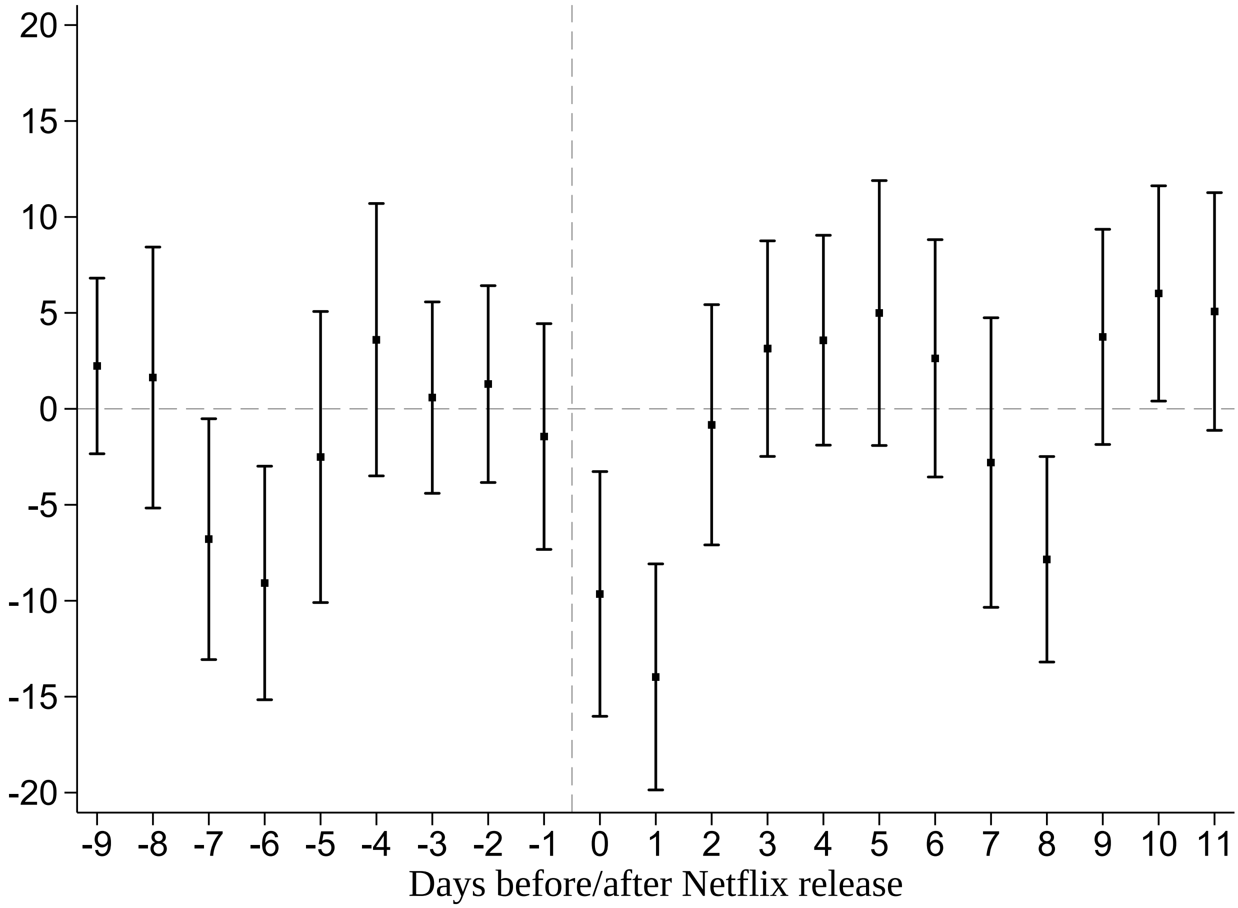


Figure 1.3. The Effect of Binge-watchable Hours of Netflix Releases on Reported Group A Crime Incidents, Finite Distributed Lag Model. The t-bars represent the 95% confidence interval using HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994).

a previous weekend's release or start watching that release after experiencing FOMO related to a quality release.

For days 9 through 11 post release, we see a statistically-significant increase on day 10. Otherwise, the day 10 and day 11 post release coefficients are not statistically different from zero. This statistically-significant increase after ten days is puzzling. For the modal release, ten days after a release would be a Monday. It may be possible that local agencies may be more willing to create incident reports at the start of the second work week after a release.

Moving left from day zero, we see that there are no coefficients statistically different from zero for the five days prior to a release. Six and seven days before a release, we observe a statistically-significant reduction in reported incidents. For the modal release date, six days before would be a Saturday. Like we previously discussed, Saturday may be the best day for most to dedicate to binge watching. Thus, consistent with FOMO, we may expect someone that intends to binge watch a new release to binge watch the prior content before the new release, with the most likely consumption day being Saturday.

Finally, I sum up all the coefficients after the release date (i.e., day 0 through day 11) and test to see if the sum of coefficients is different than zero. I fail to reject the hypothesis that the sum of all the coefficients is different than zero at even a ten percent significance level.[17] Therefore, I fail to find evidence that there is a net short-run effect of binge watching on reported crime incidents during an 12-day period after a release. If I conduct a similar test for the first three days after the release (i.e., day zero through day two), I find that the sum of coefficients is for all crime types is negative. Further, the sum of coefficients is statistically different than zero.[18] There is a net reduction in reported crime incidents in the first three days after a release, which coincides with the heavy watching period for newly released seasons.

Conlin, Billings, and Averset (2016) claim that FOMO may be increasing the rate of binge watching so viewers can stay current in cultural conversations. Conlin (2015) describes three types of binge watchers under the influence of FOMO: (1) *week-by-week*—watching as content airs to avoid missing any cultural moments, (2) *half-and-half*—binge watching content after it aired to join the cultural conversation, and (3) *accelerated*—binge watching without regard to the cultural conversation. If *half-and-half* binge watchers exist, there may be an observable decline in the pre-release period for continuation seasons that is not present in premiere seasons. Since there is no content to binge watch before a premiere season, there should be no response before the release. For the release of continuation seasons, viewers can watch content for the first time to be able to join the cultural conversation surrounding the release of the new season or viewers can watch content that they have previously seen to more deeply engage in the cultural conversation. If viewers are responding differently to

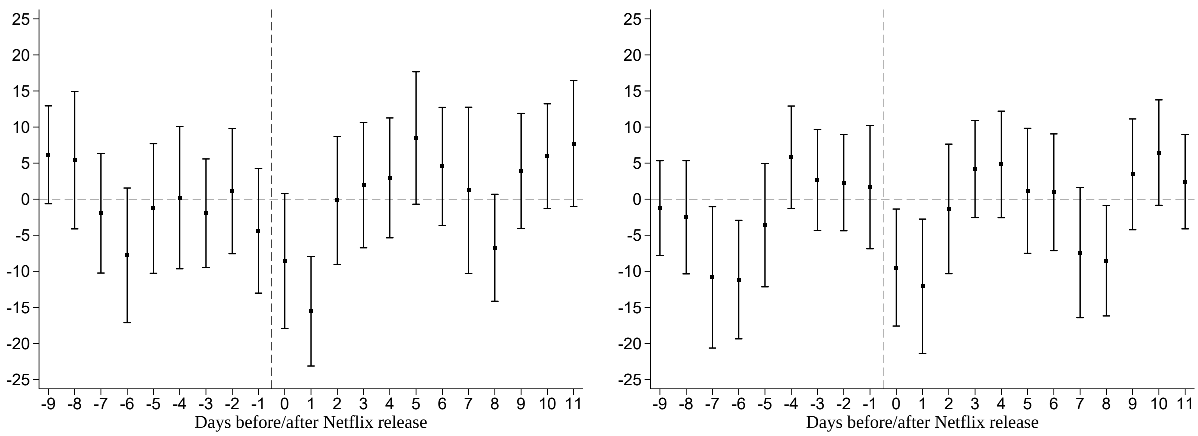
17. ↑In terms of (1.3), $\sum_{k=0}^{11} \beta_k = -5.91$ with a joint F-test of $H_0 : \sum_{k=0}^{11} \beta_k = 0$ with resulting p-value of 0.796.

18. ↑In terms of (1.3), $\sum_{k=0}^{2} \beta_k = -24.44$ with a joint F-test of $H_0 : \sum_{k=0}^{2} \beta_k = 0$ with resulting p-value of 0.001.

premiere and continuation seasons, we can further expand on the patterns we discussed in (1.3). Expanding (1.3), I estimate

$$y_t = \sum_{j \in J} \sum_{k=-9}^{11} \beta_{j,k} \text{Netflix}_{j,t-k} + \psi \text{weather}_t + \alpha_s + \gamma_t + \varepsilon_t, \tag{1.4}$$

where $J = \{\text{premiere, continuation}\}$ and the rest of the variables are as defined in (1.1). Figure 1.4 plots the estimated coefficients related to the changes in reported Group A offenses to runtime hours released for each type of season.



(a) Premiere seasons

(b) Continuation seasons

Figure 1.4. The Effect of Binge-watchable Hours of Netflix Releases on Reported Group A Offenses Incidents by Season Type, Finite Distributed Lag Model. The t-bars represent the 95% confidence interval using HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994).

Figure 1.4 panel (a) shows the response in reported crime incidents to release of premiere seasons. All the coefficients are not statistically different from zero except for a decrease in reported incidents one day after the premiere season is released. The coefficient for the first day of release is negative and consistent with our prior findings, however, we lack statistical power from the prior regression. We do see a statistically-significant decrease on the day after a release. This pattern of reduction is consistent with the idea that viewers may not be motivated to binge watch a series that they no prior attachment. However, viewers may experience FOMO if the series gains widespread acclaim in its first release day, providing incentive to binge watch the new series on the day after release. Further, we see that there is no change in reported crime incidents in anticipation of a premiere season. Consistent with our prior findings, we find that there is no evidence to support a net reduction in reported incidents in the 12 days after the release of a premiere season.[19] The net reduction for the first three days is also consistent with prior findings—a net negative reduction that is statistically different from zero.[20]

Effects on reported crime incidents for continuation seasons are plotted in Figure 1.4 panel (b). There are declines in reported crime incidents the day of release and the first and eighth day after a release. Compared to the premiere season response in panel (a), there is an immediate reduction in crime on the first day of release, which may be due to the FOMO being more influential for viewers when continuation seasons are released. FOMO may also provide the incentive for viewers to watch more intensely eight days after a release. This response after a release is consistent with our previous results. Reported crime incidents decline six and seven days before a continuation season release. Day six and seven before a modal release are Friday and Saturday respectively. Following on our previous discussion in FOMO and the likelihood that Saturday has the lowest opportunity cost to binge watch, this pre-release decline provides suggestive evidence that either explanation is possible by itself or has a combined effect. Lastly, I find no evidence that the release of continuation seasons has any net effect on crime over an 12-day period after a release.[21] Consistent with the other finite distributed lag models we've discussed, we do observe a negative net effect on crime in the first three days after the release of a continuation season.[22]

Decomposing Figure 1.3 into Figure 1.4 provides suggestive evidence for our previous discussions. We see that the reductions in reported incidents before a release are limited to continuation seasons—consistent with the idea that viewers are watching the previously

19. ↑In terms of (1.4), $\sum_{k=0}^{11} \beta_{premiere,k} = 5.69$ with a joint F-test of $H_0 : \sum_{k=0}^{11} \beta_{premiere,k} = 0$ with resulting p-value of 0.836.

20. ↑In terms of (1.4), $\sum_{k=0}^{2} \beta_{premiere,k} = -24.31$ with a joint F-test of $H_0 : \sum_{k=0}^{2} \beta_{premiere,k} = 0$ with resulting p-value of 0.009.

21. ↑In terms of (1.4), $\sum_{k=0}^{11} \beta_{continuation,k} = -15.44$ with a joint F-test of $H_0 : \sum_{k=0}^{11} \beta_{continuation,k} = 0$ with resulting p-value of 0.623.

22. ↑In terms of (1.4), $\sum_{k=0}^{2} \beta_{continuation,k} = -22.92$ with a joint F-test of $H_0 : \sum_{k=0}^{2} \beta_{continuation,k} = 0$ with resulting p-value of 0.032.

released content for the first time or an additional time in order to engage in the cultural conversation around a new season release. We also see a strong reduction on the day after a release, which is Saturday for the modal release day. It is likely the case for most viewers that Saturday has events that are the lowest opportunity cost for binge watching. Events on Friday may have higher opportunity cost, but the marginal difference can potentially be explained by the FOMO, which is less for a premiere season than a continuation season on average—consistent with our results.

Since it is clear that the FOMO and opportunity cost are key to our discussion, we explore the responses in reported incidents at a finer time level. I separate Group A incidents into four time bins per day: 00:00–05:59, 06:00–11:59, 12:00–17:59, and 18:00–23:59 Pacific time. On average, 12.5 percent, 26.6 percent, 32.5 percent, and 28.5 percent of daily reported incidents occur between 00:00–05:59, 06:00–11:59, 12:00–17:59, and 18:00–23:59 Pacific time, respectively.[23] I investigate the intertemporal effects the day before a release, the day of release, and the day after a release. I estimate the following

$$y_t = \sum_{k=-1}^{1} \beta_k \text{Netflix}_{t-k} + \psi \text{weather}_t + \alpha_s + \gamma_t + \varepsilon_t, \tag{1.5}$$

where all variables are defined as in (1.1), but the treatment is restricted to one lead, one lag, and the contemporaneous treatment.

Table 1.6 panel A shows the results from estimating (1.1) on reported Group A offenses where the column title reports the time range, in Pacific time, that restricts the incident reports.[24] Each regression has seasonal fixed effects, holiday fixed effects, and weather controls. The statistically-signficant coefficients are both negative in columns 3 and 4. I find a 0.9 percent and 2.8 percent decrease in reported Group A offenses on an average runtime release day between 12:00–17:59 and 18:00–23:59 Pacific time, respectively. We see a stronger effect in the later evening hours when people prefer to binge watch.

23. ↑These percents fall short of 100 percent due to rounding.

24. ↑I show additional results the contemporaneous effect separated by time bin for crimes against persons, crimes against property, crimes against society, Group A crimes away from a residence, and Group A crimes away from a residence in Appendix Tables 1.12–1.16.

Table 1.6. The Effect of Binge-watchable Hours Released on Reported Group A Crime Incidents by Time Bins

	(1) 00:00–05:59	(2) 06:00–11:59	(3) 12:00–17:59	(4) 18:00–23:59
A. *Contemporaneous effect*				
Hours released today (t)	-0.27	0.44	-3.89^{***}	-10.31^{***}
	(0.42)	(0.94)	(0.97)	(1.68)
B. *Intertemporal effect*				
Hours released tomorrow ($t+1$)	-0.14	0.63	0.40	-2.70^{***}
	(0.39)	(0.92)	(0.87)	(0.90)
Hours released today (t)	-0.92^{**}	-0.36	-4.61^{***}	-11.72^{***}
	(0.44)	(0.96)	(1.03)	(1.88)
Hours released yesterday ($t-1$)	-4.98^{***}	-6.89^{***}	-6.03^{***}	-8.35^{***}
	(0.47)	(1.19)	(0.95)	(1.68)
$\overline{y}$	1,590.3	3,399.0	4,145.2	3,636.1

Notes: Each column in each panel is a separate regression with the dependent variable restricted to the times listed as the column title (e.g., column 1's dependent variable is the total Group A crime incidents reported between 00:00 a.m. and 05:59 a.m. Pacific time). Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In Panel B, I estimate (1.5) with the seasonal fixed effects, holiday fixed effects, and weather controls on reported Group A offenses separated into time bins. The effect of tomorrow's releases is listed across the first row. Tomorrow's runtime hours do not effect reported incidents between 00:00–05:59, 06:00–11:59, or 12:00–17:59 Pacific time. In column 4, we see a small decline of 2.70 reported incidents per runtime hour released. This change is a modest 0.7 percent decline for an average runtime release day. Thus reported incidents decline in the hours immediately before runtime hours are made available at 00:00 Pacific time. Potential offenders or victims are changing their behavior immediately before a release. A plausible explanation for this behavior is that the binge watcher is preparing to binge by doing errands before the release time. The binge viewer may adjust their typical plans the night before an important release comes out. For example, instead of going to the bar with friends, a dedicated binge watcher would stay at home the night before the midnight Pacific time release time in order to be ready to start binge watching at exact midnight Pacific time.

Consistent with the idea that binge viewers want to start watching once the new content is available, I find that binge watching marginally reduces reported incidents during the six hours immediately after a release (i.e., 00:00–05:59 Pacific time). For the other time bins, the effect of runtime hours released the same day as the reported incidents follows the same pattern as we discussed in panel (a), where the effect is concentrated in the evening hours with the largest percent decline on an average runtime release day of 3.1 percent seen in the 18:00–23:59 Pacific time bin.

Finally, shown in the last row of panel B, reported crime incidents decline in every time bin the day after a release. This decline is consistent with viewers experiencing FOMO or viewers having the lowest opportunity cost to watch the day after a release. If we convert the declines in reported crime the day after a release into percentage changes for average release days, we have declines of 3.0 percent, 2.0 percent, 1.4 percent, and 2.2 percent for the 00:00–05:59, 06:00–11:59, 12:00–17:59, and 18:00–23:59 Pacific time bins, respectively.

The largest percentage declines in reported incidents are incidents from 18:00–23:59 Pacific time the day of release and incidents from 00:00–05:59 Pacific time the day after a release. Thus our strongest effects are contained to the 18–30 hours immediately after the content is made available, which is an extended and uninterrupted window that can accommodate the average release runtime. This combined time period matches the general preferences of binge watchers—an extended evening to watch as much as possible.

1.4.3 Robustness

Dahl and DellaVigna (2009) and Cunningham, Engelstätter, and Ward (2016) both estimate a log-linear version of (1.1). The results presented in this paper are robust to log-linear specification, except for testing the sum of coefficients in (1.3). I show a version of Table 1.3 with log dependent variables in Appendix Table 1.9.

I show the effect is robust to transforming the treatment into a binary variable for whether new content was released on the specific day in Appendix Table 1.7. However, the entire effect is not captured by the binary treatment variable.

Heterogeneity in reporting agencies may affect the results. Reporting agencies are spread unevenly across the country. Reporting agencies could have important differences in the kind of crimes that they face and how crimes are reported in their region. I create another data set where I aggregate to timezone-day level. I estimate variations of (1.1) and (1.2) where time zone fixed effects are interacted with everything on the left hand side of the regression equation except runtime hours and the weather controls. I report spatial correlation consistent (SCC) standard errors (Driscoll and Kraay 1998) with 41 lags (Newey and West 1994) that are robust to heteroskedasticity, cross-sectional correlation, and autocorrelation. I present the panel data version of Table 1.1 in Appendix Table 1.8. Appendix Table 1.8 shows that the baseline results are robust to changing the data structure to a panel from a time series. Since the coefficients are the effect per time zone, we can multiply each coefficient by four to estimate the effect on the national-level. After this adjustment, we can see that the baseline findings are the same sign and similar magnitude.

I investigate the robustness of the reductions in reported crime by content type by splitting the content into four groupings: high violence, high quality; high violence, low quality; low, violence, high quality; low violence, and low quality. I run a regression based on a modification of (1.2) and present the results in Appendix Table 1.10 with panel A using the IMDb ratings and panel B using the CSM ratings. In this table, each set of four groupings displays the same pattern in magnitudes and statistical significance across all outcome variables. I fail to reject the hypothesis that the aggregate effect of high-violence content is different than the aggregate effect of low-violence content on reported incidents, which supports our earlier finding that violent content does not increased reported crime.

I explore the robustness of the finite distributed lag models in two ways. First, I estimate (1.3) on crimes against persons, crimes against persons, and crimes against society. These results are plotted in Appendix Figure 1.5. These results follow the patterns seen in Figure 1.3. Second, I conduct a robustness exercise on the number of lags included in the finite distributed lag model. I estimate (1.3) with 18 lags to cover a second weekend in the future. I plot the resulting coefficients with 95% confidence interval t-bars in Appendix Figure 1.6. The trend of the coefficients is generally the same across Figure 1.3, however, the t-bars are wider due to the additional coefficients being estimated.

1.5 Discussion

Binge watching reduces reported crime incidents but the channels that produce the effect are hard to isolate. The likely mechanism is incapacitation—offenders or victims are captivated by binge watching new content and may not be as exposed to situations that are correlated with crime.

For crimes against persons, it is impossible to separate an offender incapacitation from a victim incapacitation since crimes against persons must be reported with at least one offender and at least one victim. The modal crime against person incident report contains one of each.

Crimes against property, on the other hand, often do not have a listed victim, much less a victim that is a person. However, crimes against property must at some point be observed by a reporting party and then have a report written by an officer. In either case, reported property crimes could decrease because it is too costly for non-police observers to report the crime to police or it is too costly for police to write the report. If there is no effect on offenders that commit property crimes, then we could observe an increase in reported property crimes on days in which officers or non-police observers are less likely to binge watch. We don't observe any statistically significant increases in reported property crimes when we estimate the effect of binge watching distributed across multiple days. This suggests that the decrease in reported crime incidents is likely though incapacitation of potential offenders.

It is possible that reporting officers are also incapacitated by binge watching. There is anecdotal evidence to support this idea (see Chavez 2019). The conventional test for police reporting effects on reported incidents is to check the effect on reported homicides. The fundamental idea is that the incentives for homicides to be reported timely are so high that it would be extremely unusual for a homicide to go unreported. I find binge watching has no effect on reported homicides (see Appendix Table 1.11 column 1), which suggests that police reporting is not affected.

As suggested in Dahl and DellaVigna (2009), the type of content may drive reductions in crime through a satiation effect.[25] While we do observe reductions in crime from violent content, the reductions in crime in response to high-violence content are not statistically different to the reductions in crime in response to low-violence content. Indeed, Table 1.4 shows for crimes against persons and crimes against property the magnitude of the reduction is almost identical.

Another potential mechanism is that binge watching changes where people are. For example, instead of going to a concert and being exposed to pickpockets or aggressive intoxicated people, a binge watcher would be sitting on their couch at home. The first part of that is the binge watcher removes themselves from a situation with a relatively high probability of a crime. The second part is that the binge watcher stays home. By staying home, the binge watcher protects their in-residence property and the property that they would have exposed by leaving their residence (e.g., wallet, cell phone, jewelry). I find that there is a larger percentage reduction in reported property crimes in a residence than reported property crimes away from a residence. This difference suggests that the reduction in reported property crimes is likely due to a change in behavior of potential offenders.

A potential byproduct of binge watching is voluntary cohabitation. This separates this paper from the growing number of papers addressing the effect of Covid-19 lockdown-induced

25. ↑Cunningham, Engelstätter, and Ward (2016) labeled this effect catharsis.

forced cohabitation, which have mixed results.[26] I find that there is no increase in crimes against persons reported in a residence in response to a new content release, where voluntary cohabitation may be increased.

Binge watching reduces reported crime incidents, primarily through reductions in crimes against persons and crimes against property. There is a reduction in reported crime in the hours immediately preceding a new release in anticipation of that release. The largest percentage decreases in reported crime coincide with the reported preferred binge watching times. There are further reductions in crime eight days after a release as people finish watching the content released the previous week. Reductions in reported crime are likely due to changes in the behavior of potential offenders. Potential offenders could either be incapaciated by watching the new release or their cost to commit an offense could be increased due to the changes in the set of crimes of opportunity available.

26. ↑See Campedelli, Aziani, and Favarin (2020) and Mohler et al. (2020) for papers using U.S. crime data.

1.A Robustness

Table 1.7. The Effect of Newly Available Content on Reported Group A Crime Incidents

	(1)	(2)	(3)	(4)
A. *All seasons*				
Content released	2076.97***	−204.91***	−197.83***	−180.35***
	(143.53)	(45.76)	(41.06)	(40.15)
B. *First season and continuations separated*				
Content released, premiere	1254.78***	−231.38***	−219.67***	−159.40***
	(150.52)	(58.12)	(53.77)	(45.12)
Content released, continuation	1741.43***	−58.40	−68.65	−110.25***
	(133.65)	(48.12)	(49.55)	(42.58)
Seasonal FE		✓	✓	✓
Holiday FE			✓	✓
Weather controls				✓
$\overline{y}$	12,770.6	12,770.6	12,770.6	12,770.6
H_0: premiere = continuation	0.028	0.038	0.067	0.431

Notes: Each column in each panel is a separate regression. Seasonal FE include fixed effects for day-of-week, daylight savings time, month × day-of-month, and year × month. Weather controls include variables for hot, cold, and rain. Holiday FE include fixed effects for each of the following days: Super Bowl Sunday, Martin Luther King Jr. Day, Presidents' Day, Memorial Day, Labor Day, Columbus Day, Easter, Thanksgiving, Mother's Day, and the extended weekends associated with Martin Luther King Jr. Day, Presidents' Day, Memorial Day, Labor Day, Columbus Day, and Thanksgiving. $\overline{y}$ is the mean of the dependent variable. H_0: premiere = continuation is a p-value from a Wald test related to the coefficients in Panel B. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 1.8. The Effect of Binge-watchable Hours Released on Reported Group A Crime Incidents, Time Zone Panel

	(1)	(2)	(3)	(4)
A. *All seasons*				
Hours released	41.72***	−3.55***	−3.57***	−3.50***
	(3.16)	(0.81)	(0.83)	(0.83)
B. *First season and continuations separated*				
Hours released, premiere	33.96***	−4.58***	−4.46***	−3.58***
	(3.96)	(1.30)	(1.21)	(1.07)
Hours released, continuation	48.44***	−2.64**	−2.78**	−3.43***
	(3.73)	(1.10)	(1.11)	(1.03)
Seasonal FE		✓	✓	✓
Holiday FE			✓	✓
Weather controls				✓
$\overline{y}$	3,192.7	3,192.7	3,192.7	3,192.7
H_0: premiere = continuation	0.002	0.269	0.289	0.904

Notes: Each column in each panel is a separate regression. Seasonal FE include fixed effects for day-of-week, daylight savings time, month × day-of-month, and year × month. Weather controls include variables for hot, cold, and rain. Holiday FE include fixed effects for each of the following days: Super Bowl Sunday, Martin Luther King Jr. Day, Presidents' Day, Memorial Day, Labor Day, Columbus Day, Easter, Thanksgiving, Mother's Day, and the extended weekends associated with Martin Luther King Jr. Day, Presidents' Day, Memorial Day, Labor Day, Columbus Day, and Thanksgiving. $\overline{y}$ is the mean of the dependent variable. H_0: premiere = continuation is a p-value from a Wald test related to the coefficients in Panel B. Spatial correlation consistent (SCC) standard errors (Driscoll and Kraay 1998) with 41 lags (Newey and West 1994) in parentheses. $N = 17,484$.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.9. The Effect of Binge-watchable Hours Released on log Reported Crime Incidents by Crime Categories

	(1) Group A	(2) Person	(3) Property	(4) Society
A. *All seasons*				
Hours released	-0.0018^{***}	-0.0018^{***}	-0.0016^{***}	-0.0046^{***}
	(0.0003)	(0.0004)	(0.0003)	(0.0004)
B. *First season and continuations separated*				
Hours released, premiere	-0.0017^{***}	-0.0011^{**}	-0.0016^{***}	-0.0049^{***}
	(0.0004)	(0.0005)	(0.0003)	(0.0006)
Hours released, continuation	-0.0019^{***}	-0.0025^{***}	-0.0017^{***}	-0.0043^{***}
	(0.0004)	(0.0005)	(0.0004)	(0.0006)
$\overline{y}$	12,770.6	2,925.0	8,438.7	1,586.8
H_0: premiere = continuation	0.512	0.003	0.800	0.456

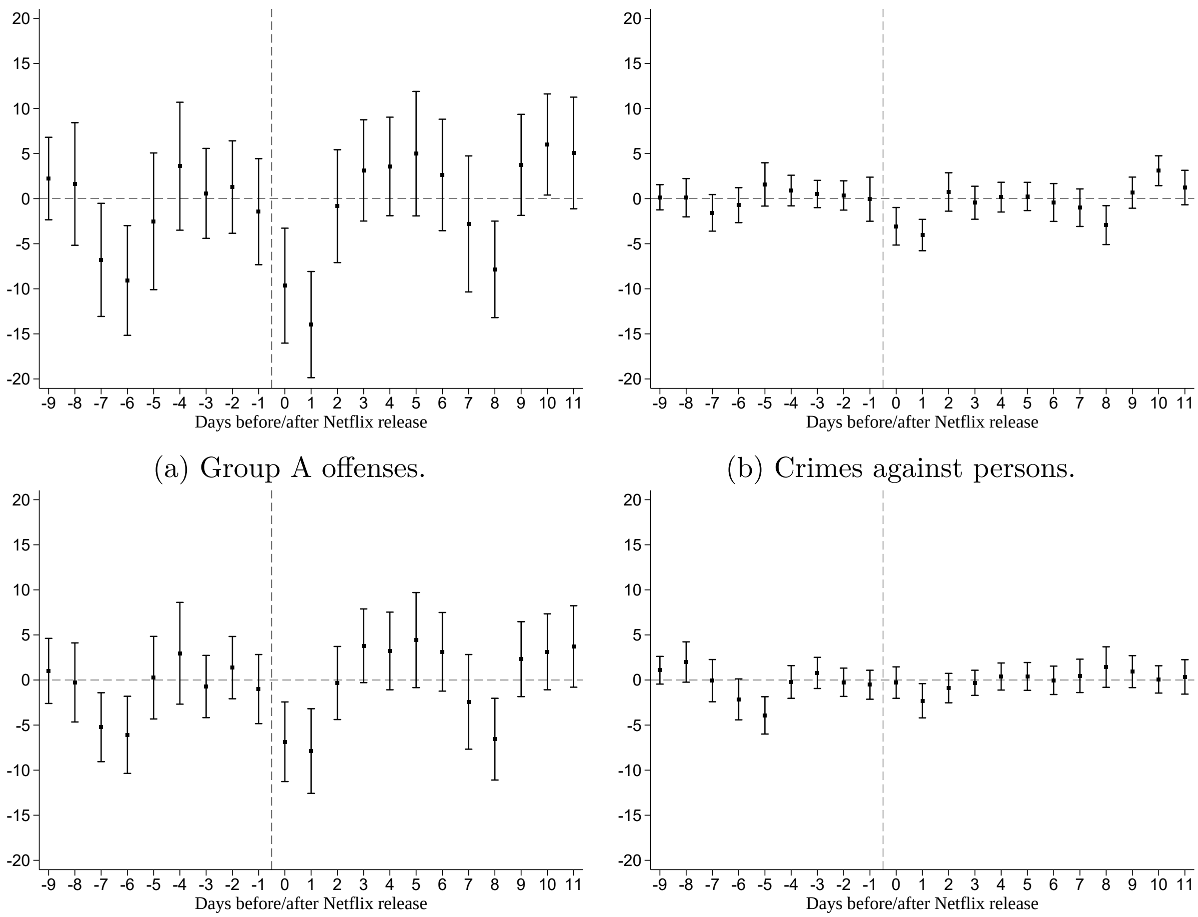
Notes: Each column in each panel is a separate regression with the column title describing the type of reported crime used as the dependent variable transformed with the natural log. Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.8 notes for details). $\overline{y}$ is the mean of the dependent variable. H_0: premiere = continuation is a p-value from a Wald test related to the coefficients in Panel B. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 1.10. The Effect of Binge-watchable Hours of Netflix Releases on Reported Crime Incidents by Quality and Violence Ratings

	(1) Group A	(2) Person	(3) Property	(4) Society
A. *Hours released by violence and quality (IMDb rating)*				
Low quality, low violence	-20.57^{**}	-7.45^{***}	-14.91^{***}	0.93
	(8.09)	(2.65)	(5.25)	(1.50)
Low quality, high violence	-9.97	-1.59	-7.44	-1.84
	(8.31)	(2.96)	(5.93)	(1.85)
High quality, low violence	-6.92	-1.40	-7.74	1.76
	(8.08)	(2.17)	(5.25)	(1.88)
High quality, high violence	-14.39^{***}	-4.55^{***}	-11.03^{***}	0.04
	(4.14)	(1.72)	(2.61)	(1.28)
B. *Hours released by violence and quality (CSM rating)*				
Low quality, low violence	-25.94^{***}	-8.66^{***}	-17.59^{***}	-0.54
	(9.37)	(3.13)	(5.63)	(2.17)
Low quality, high violence	-5.17	-0.87	-6.16	0.76
	(6.73)	(2.55)	(4.93)	(2.22)
High quality, low violence	-7.40	-2.22	-7.97^{*}	2.24
	(7.33)	(2.16)	(4.62)	(1.65)
High quality, high violence	-17.17^{***}	-5.44^{***}	-12.08^{***}	-0.77
	(5.00)	(1.84)	(3.10)	(1.37)
$\overline{y}$	12,770.6	2,925.0	8,438.7	1,586.8
A. H_0: low = high violence	0.840	0.599	0.657	0.261
B. H_0: low = high violence	0.423	0.319	0.356	0.695

Notes: Each column in each panel is a separate regression with the column title describing the type of reported crime used as the dependent variable. Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.8 notes for details). $\overline{y}$ is the mean of the dependent variable. ***A.*** H_0: low = high violence is a p-value from a Wald test related to the coefficients in Panel A where the null hypothesis is low quality, low violence + high quality, low violence = low quality, high violence + high quality, high violence. ***B.*** H_0: low = high violence is a p-value from a Wald test related to the coefficients in Panel B where the null hypothesis is low quality, low violence + high quality, low violence = low quality, high violence + high quality, high violence. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.



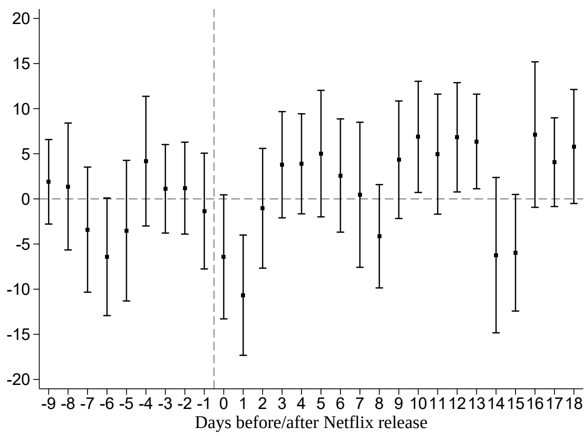
(a) Group A offenses.

(b) Crimes against persons.

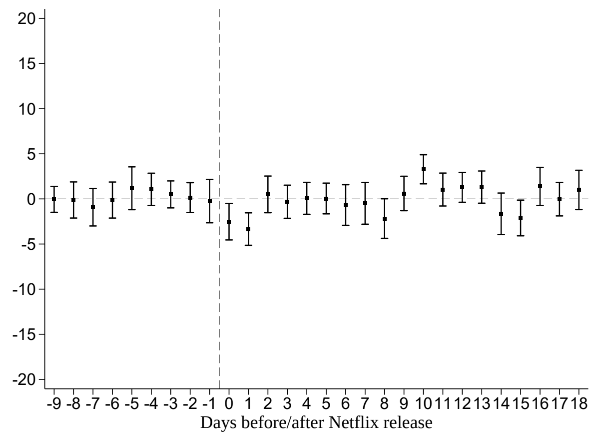
(c) Crimes against property.

(d) Crimes against society.

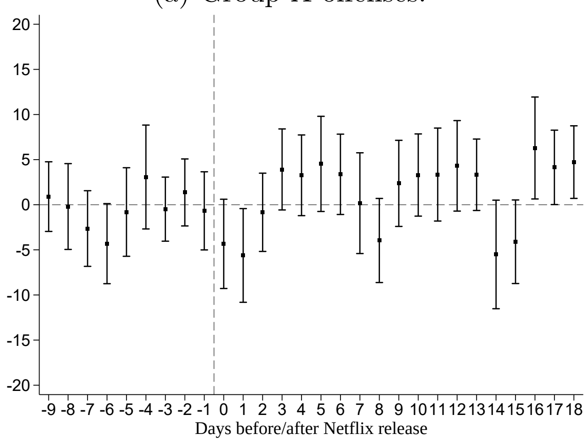
Figure 1.5. The Effect of Binge-watchable Hours of Netflix Releases on Reported Crime Incidents, Finite Distributed Lag Models. Each panel is a separate regression using the full specification. The t-bars represent the 95% confidence interval using HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994).



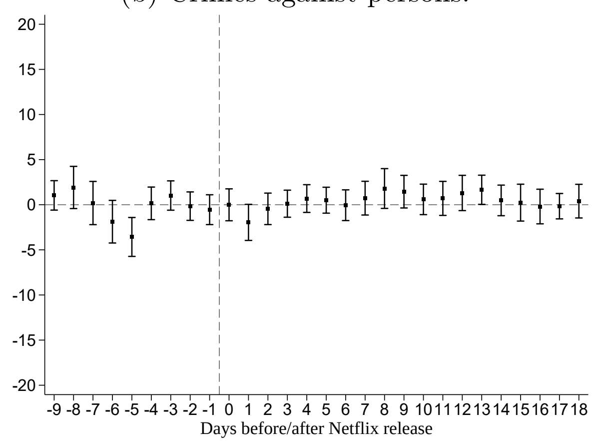
(a) Group A offenses.



(b) Crimes against persons.



(c) Crimes against property.



(d) Crimes against society.

Figure 1.6. The Effect of Binge-watchable Hours Released on Reported Crime Incidents, Finite Distributed Lag Models. Each panel is a separate regression using the full specification. The t-bars represent the 95% confidence interval using HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994).

Table 1.11. The Effect of Binge-watchable Hours Released on Reported Group A Crime Incidents, Specific Crimes

	(1) Homicide	(2) Assault
A. *All seasons*		
Hours released	0.02	-4.29^{***}
	(0.02)	(1.26)
B. *First season and continuations separated*		
Hours released, premiere	0.00	-1.98
	(0.03)	(1.41)
Hours released, continuation	0.03	-6.35^{***}
	(0.04)	(1.41)
$\overline{y}$	8.7	2,876.7
H_0: premiere = continuation	0.652	0.004

Notes: Each column in each panel is a separate regression with the column title describing the type of reported crime used as the dependent variable. Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.8 notes for details). $\overline{y}$ is the mean of the dependent variable. H_0: premiere = continuation is a p-value from a Wald test related to the coefficients in Panel B. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.12. The Effect of Binge-watchable Hours Released on Crimes Against Persons by Time Bins

	(1) 00:00–05:59	(2) 06:00–11:59	(3) 12:00–17:59	(4) 18:00–23:59
A. *Contemporaneous effect*				
Hours released today (t)	0.02	−0.09	−0.52	−3.66***
	(0.17)	(0.37)	(0.41)	(0.68)
B. *Intertemporal effect*				
Hours released tomorrow ($t+1$)	0.30	−0.13	−0.18	−0.99**
	(0.21)	(0.53)	(0.36)	(0.39)
Hours released today (t)	−0.27	−0.24	−0.63	−4.12***
	(0.18)	(0.31)	(0.42)	(0.73)
Hours released yesterday ($t-1$)	−2.63***	−1.06***	−0.64*	−2.65***
	(0.33)	(0.34)	(0.36)	(0.62)
$\overline{y}$	367.6	665.9	945.6	945.9

Notes: Each column in each panel is a separate regression with the dependent variable restricted to the times listed as the column title (e.g., column 1's dependent variable is the total crimes against persons reported between 00:00 a.m. and 05:59 a.m. Pacific time). Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 1.13. The Effect of Binge-watchable Hours Released on Reported Crimes Against Property by Time Bins

	(1) 00:00–05:59	(2) 06:00–11:59	(3) 12:00–17:59	(4) 18:00–23:59
A. *Contemporaneous effect*				
Hours released today (t)	−0.38	0.12	−2.83***	−7.77***
	(0.35)	(0.54)	(0.68)	(1.25)
B. *Intertemporal effect*				
Hours released tomorrow ($t+1$)	−0.53*	−0.27	0.39	−1.22**
	(0.29)	(0.56)	(0.57)	(0.61)
Hours released today (t)	−0.78**	−0.37	−3.39***	−8.75***
	(0.36)	(0.56)	(0.74)	(1.41)
Hours released yesterday ($t-1$)	−2.61***	−3.58***	−4.72***	−6.39***
	(0.32)	(0.70)	(0.84)	(1.36)
$\overline{y}$	1,071.2	2,431.6	2,757.0	2,178.9

Notes: Each column in each panel is a separate regression with the dependent variable restricted to the times listed as the column title (e.g., column 1's dependent variable is the total crimes against property reported between 00:00 a.m. and 05:59 a.m. Pacific time). Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.14. The Effect of Binge-watchable Hours Released on Reported Crimes Against Society by Time Bins

	(1) 00:00–05:59	(2) 06:00–11:59	(3) 12:00–17:59	(4) 18:00–23:59
A. *Contemporaneous effect*				
Hours released today (t)	0.04	0.32	-0.72^{***}	0.62
	(0.14)	(0.27)	(0.26)	(0.39)
B. *Intertemporal effect*				
Hours released tomorrow ($t+1$)	0.08	0.94^{***}	0.11	-0.55
	(0.13)	(0.33)	(0.29)	(0.35)
Hours released today (t)	0.04	0.17	-0.79^{***}	0.60
	(0.15)	(0.28)	(0.29)	(0.41)
Hours released yesterday ($t-1$)	-0.03	-2.16^{***}	-0.67^{**}	0.34
	(0.14)	(0.40)	(0.28)	(0.40)
$\overline{y}$	176.3	331.8	495.7	583.0

Notes: Each column in each panel is a separate regression with the dependent variable restricted to the times listed as the column title (e.g., column 1's dependent variable is the total crimes against society reported between 00:00 a.m. and 05:59 a.m. Pacific time). Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.15. The Effect of Binge-watchable Hours Released on Reported Group A Offenses Away From a Residence by Time Bins

	(1) 00:00–05:59	(2) 06:00–11:59	(3) 12:00–17:59	(4) 18:00–23:59
A. *Contemporaneous effect*				
Hours released today (t)	0.10	0.18	-1.90^{***}	-4.32^{***}
	(0.31)	(0.73)	(0.71)	(0.87)
B. *Intertemporal effect*				
Hours released tomorrow ($t+1$)	0.19	0.81	0.52	-1.44^{**}
	(0.28)	(0.90)	(0.64)	(0.57)
Hours released today (t)	-0.14	-0.32	-2.21^{***}	-5.03^{***}
	(0.31)	(0.75)	(0.73)	(0.99)
Hours released yesterday ($t-1$)	-2.08^{***}	-4.72^{***}	-2.91^{***}	-4.07^{***}
	(0.36)	(0.95)	(0.51)	(1.02)
$\overline{y}$	823.7	1,903.4	2,466.2	2,006.3

Notes: Each column in each panel is a separate regression with the dependent variable restricted to the times listed as the column title (e.g., column 1's dependent variable is the total Group A offenses away from a residence reported between 00:00 a.m. and 05:59 a.m. Pacific time). Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.16. The Effect of Binge-watchable Hours Released on Reported Group A Offenses in a Residence by Time Bins

	(1) 00:00–05:59	(2) 06:00–11:59	(3) 12:00–17:59	(4) 18:00–23:59
***A.** Contemporaneous effect*				
Hours released today (t)	−0.37	0.26	−1.98***	−5.98***
	(0.30)	(0.39)	(0.37)	(0.91)
***B.** Intertemporal effect*				
Hours released tomorrow ($t+1$)	−0.32	−0.18	−0.12	−1.26**
	(0.26)	(0.40)	(0.42)	(0.52)
Hours released today (t)	−0.78**	−0.04	−2.40***	−6.69***
	(0.31)	(0.40)	(0.41)	(1.00)
Hours released yesterday ($t-1$)	−2.90***	−2.17***	−3.11***	−4.28***
	(0.33)	(0.45)	(0.60)	(0.81)
$\overline{y}$	766.5	1,495.6	1,679.0	1,629.8

Notes: Each column in each panel is a separate regression with the dependent variable restricted to the times listed as the column title (e.g., column 1's dependent variable is the total Group A offenses in a residence reported between 00:00 a.m. and 05:59 a.m. Pacific time). Each regression includes seasonal fixed effects, weather controls, and holiday fixed effects (see Table 1.1 notes for details). $\overline{y}$ is the mean of the dependent variable. HAC standard errors (Newey and West 1987) with automatic lag selection (Newey and West 1994) in parentheses. $N = 4,371$.

*** p < 0.01, ** p < 0.05, * p < 0.1

1.B Data

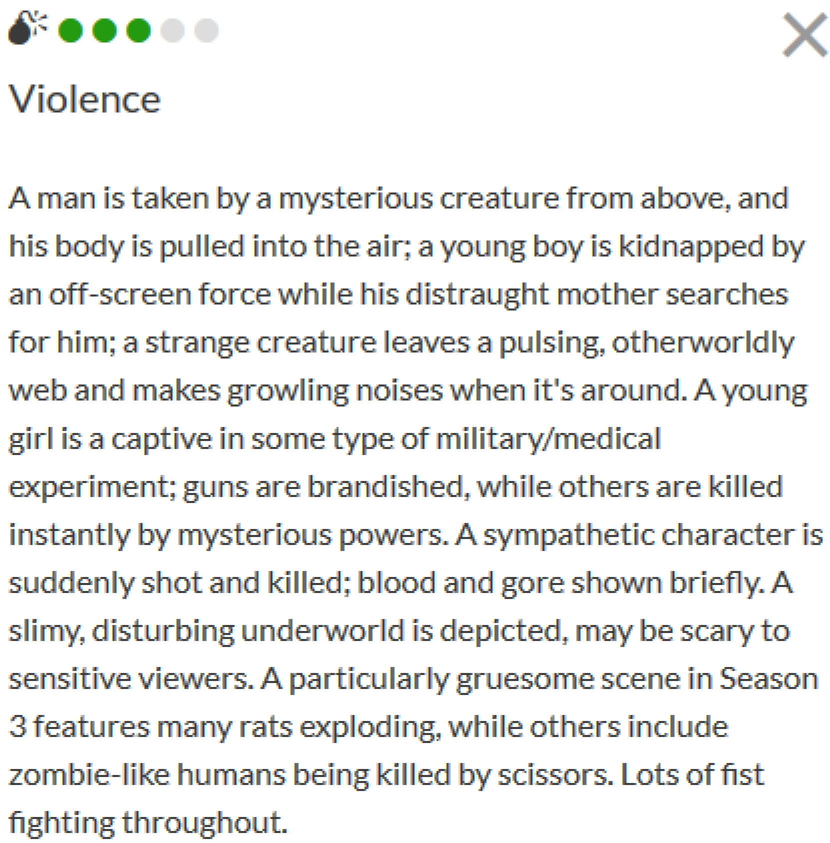
1.B.1 Holiday Controls

I control for New Year's Day, Easter, Memorial Day, Independence Day, Labor Day, Thanksgiving Day, Christmas Day, Martin Luther King Jr. Day, Presidents' Day, Columbus Day, Cinco de Mayo, Saint Patrick's Day, Mother's Day, Valentine's Day, Halloween, and Veterans Day with separate indicator variables.

Holiday observance periods vary with the holiday. I make separate indicators for the three days before and the day after for the following holidays: Martin Luther King Jr. Day, Presidents' Day, Memorial Day, Labor Day, and Columbus Day. I create indicators for the two days before and the day after New Year's Day. I create separate indicators for the five days before and the five days after Christmas Day. I create separate indicators for the day before and the four days following Thanksgiving Day.

Finally I create an indicator variable for holiday weekends. The indicator turns on for both Saturday and Sunday if any of the following holidays land on either Saturday or Sunday: Independence Day, Veterans Day, Christmas Day, New Year's Day, and Valentine's Day.

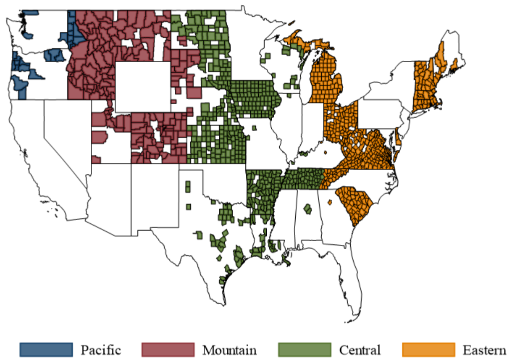
1.B.2 Supplemental Figures and Tables



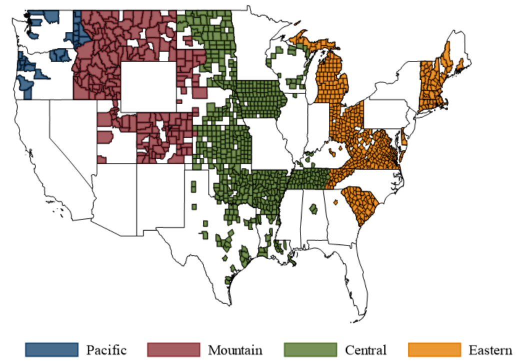
Violence

A man is taken by a mysterious creature from above, and his body is pulled into the air; a young boy is kidnapped by an off-screen force while his distraught mother searches for him; a strange creature leaves a pulsing, otherworldly web and makes growling noises when it's around. A young girl is a captive in some type of military/medical experiment; guns are brandished, while others are killed instantly by mysterious powers. A sympathetic character is suddenly shot and killed; blood and gore shown briefly. A slimy, disturbing underworld is depicted, may be scary to sensitive viewers. A particularly gruesome scene in Season 3 features many rats exploding, while others include zombie-like humans being killed by scissors. Lots of fist fighting throughout.

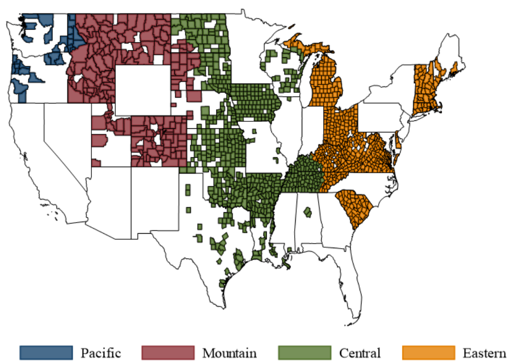
Figure 1.7. *Stranger Things* TV Review Violence Rating Description. *Source:* Common Sense Media (Slaton 2020).



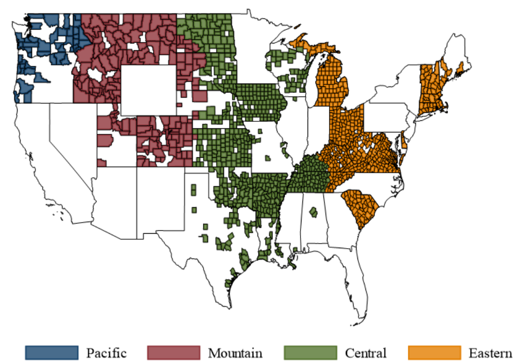
(a) 2007. 3,645 agencies cover 69.49 million in 33 states.



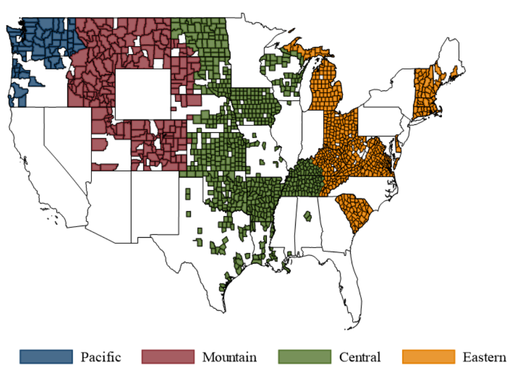
(b) 2008. 3,859 agencies cover 72.89 million in 33 states.



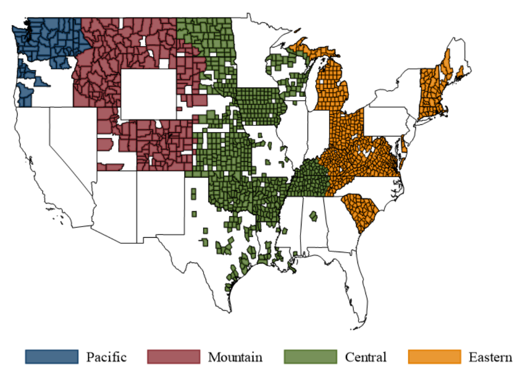
(c) 2009. 4,244 agencies cover 77.87 million in 34 states.



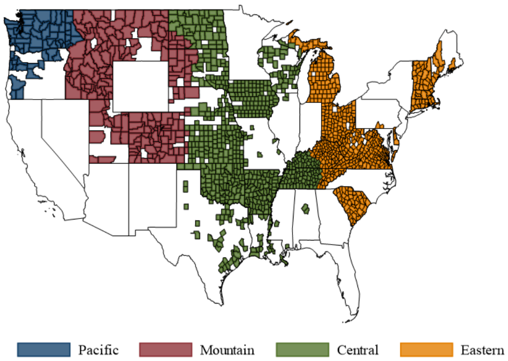
(d) 2010. 4,189 agencies cover 79.71 million people in 34 states.



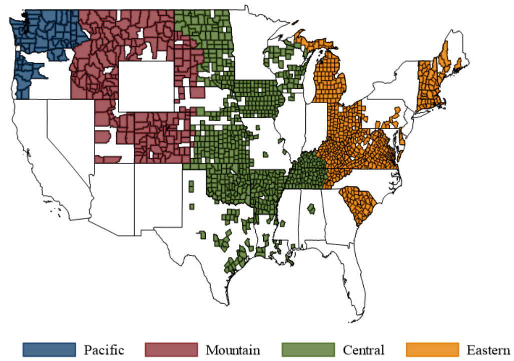
(e) 2011. 4,406 agencies cover 82.83 million people in 34 states.



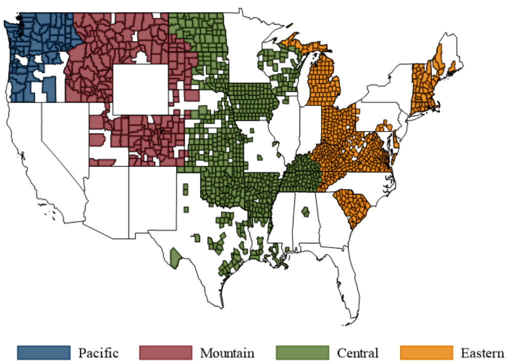
(f) 2012. 4,659 agencies cover 86.69 million people in 34 states.



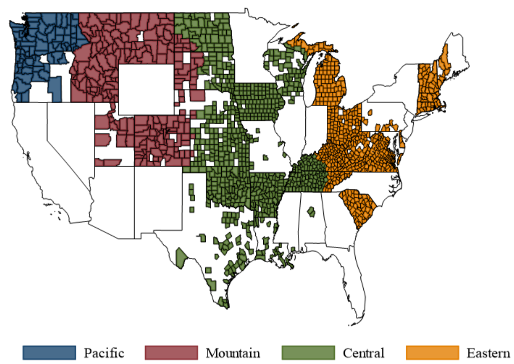
(g) 2013. 4,707 agencies cover 88.42 million people in 35 states.



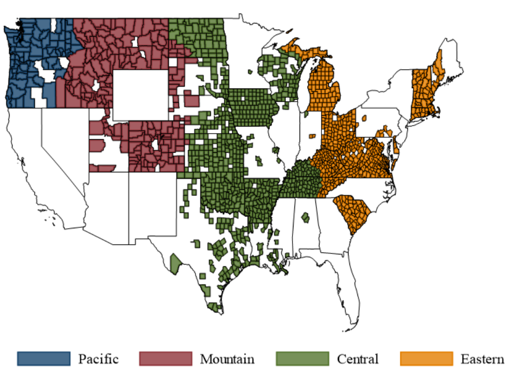
(h) 2014. 4,733 agencies cover 90.05 million people in 35 states.



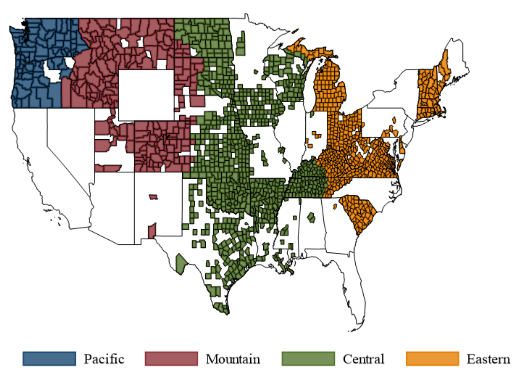
(i) 2015. 4,842 agencies cover 91.66 million people in 35 states.



(j) 2016. 5,101 agencies cover 98.17 million people in 36 states.

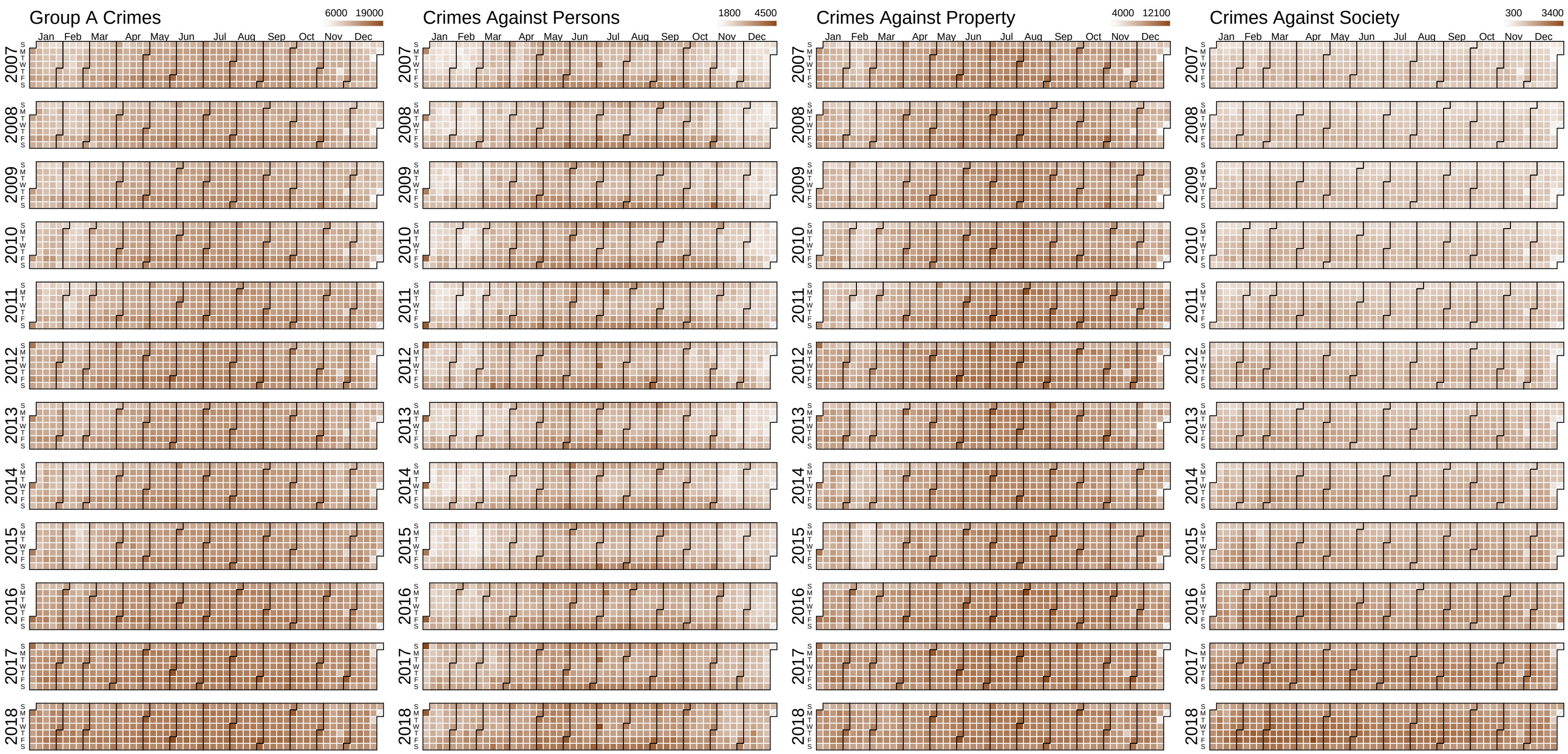


(k) 2017. 5,088 agencies cover 101.23 million people in 38 states.



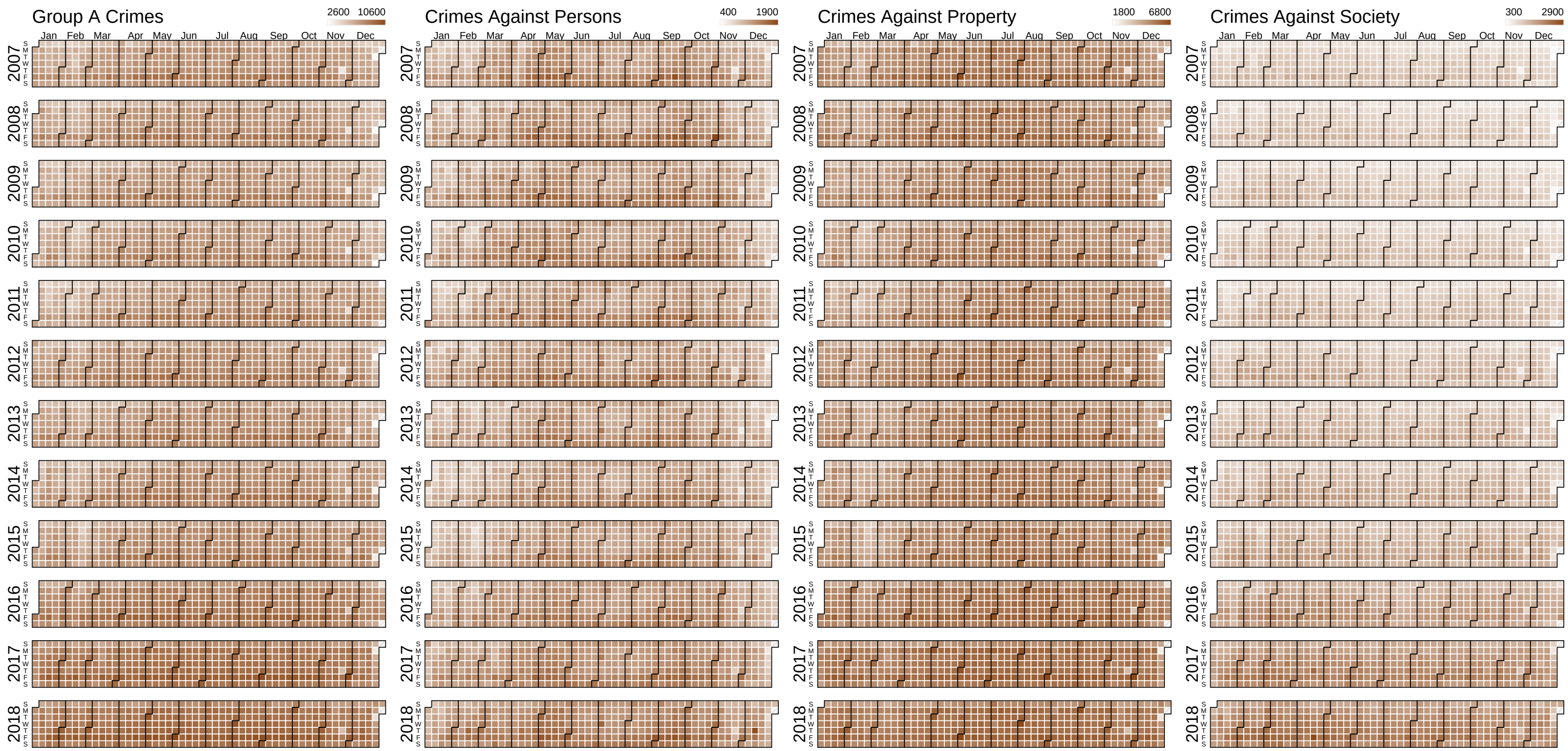
(l) 2018. 5,191 agencies cover 105.68 million people in 40 states.

Figure 1.8. NIBRS Coverage by County, 2007–2018. Each shaded county has at least one reporting agency that reports crime incidents for each month in the specified year.



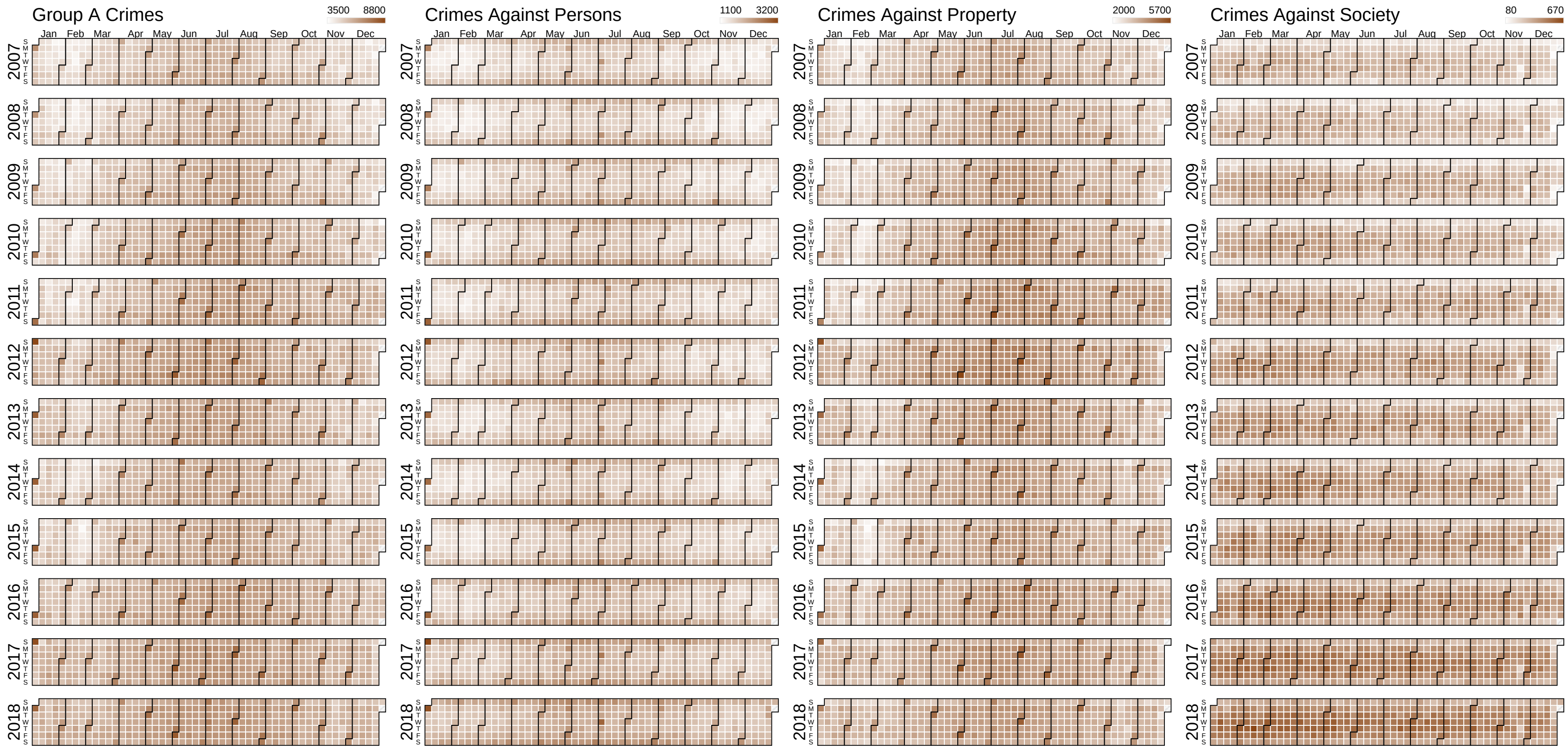
(a) Group A offenses. (b) Crimes against persons. (c) Crimes against property. (d) Crimes against society.

Figure 1.9. Reported Incidents by Category, 2007–2018. Each cell represents a day. Darker shades indicate more reported incidents within the specified time bin.



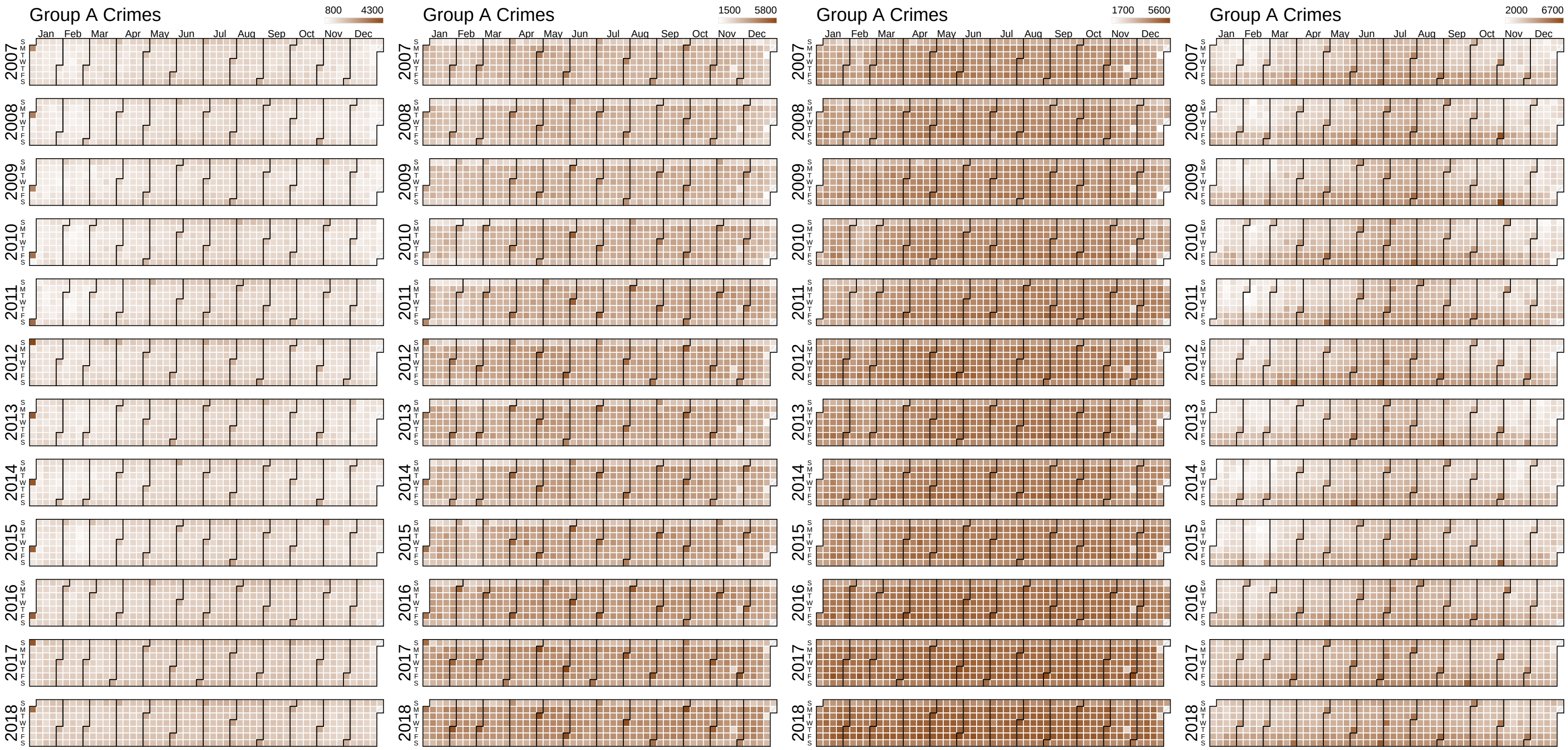
(a) Group A offenses. (b) Crimes against persons. (c) Crimes against property. (d) Crimes against society.

Figure 1.10. Reported Incidents Committed Away From a Residence by Category, 2007–2018. Each cell represents a day. Darker shades indicate more reported incidents within the specified time bin.



(a) Group A offenses. (b) Crimes against persons. (c) Crimes against property. (d) Crimes against society.

Figure 1.11. Reported Incidents Committed in a Residence by Category, 2007–2018. Each cell represents a day. Darker shades indicate more reported incidents within the specified time bin.



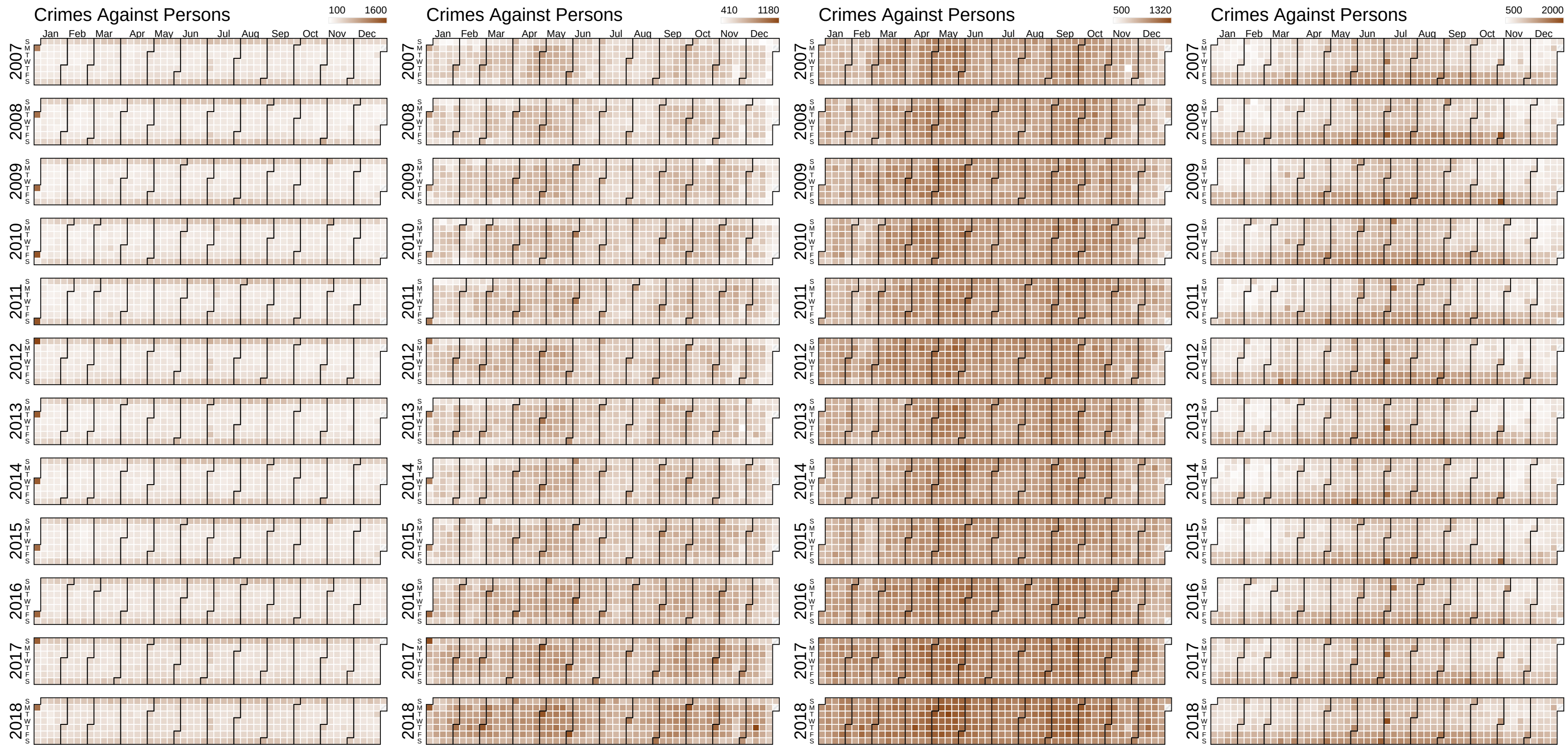
(a) 00:00–05:59 Pacific time.

(b) 06:00–11:59 Pacific time.

(c) 12:00–17:59 Pacific time.

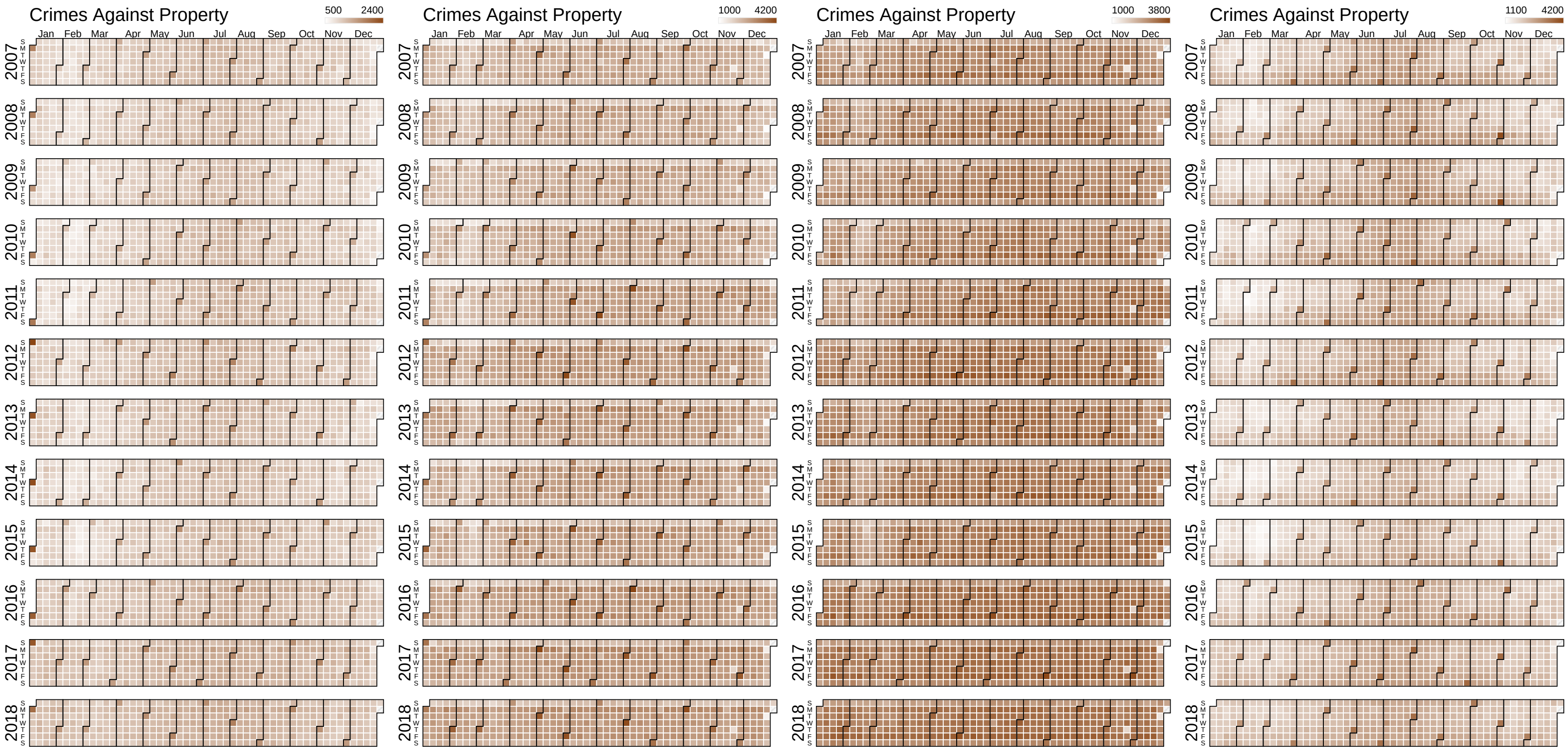
(d) 18:00–23:59 Pacific time.

Figure 1.12. Reported Group A Offenses by Time Bin, 2007–2018. Each cell represents a day. Darker shades indicate more reported incidents within the specified time bin.



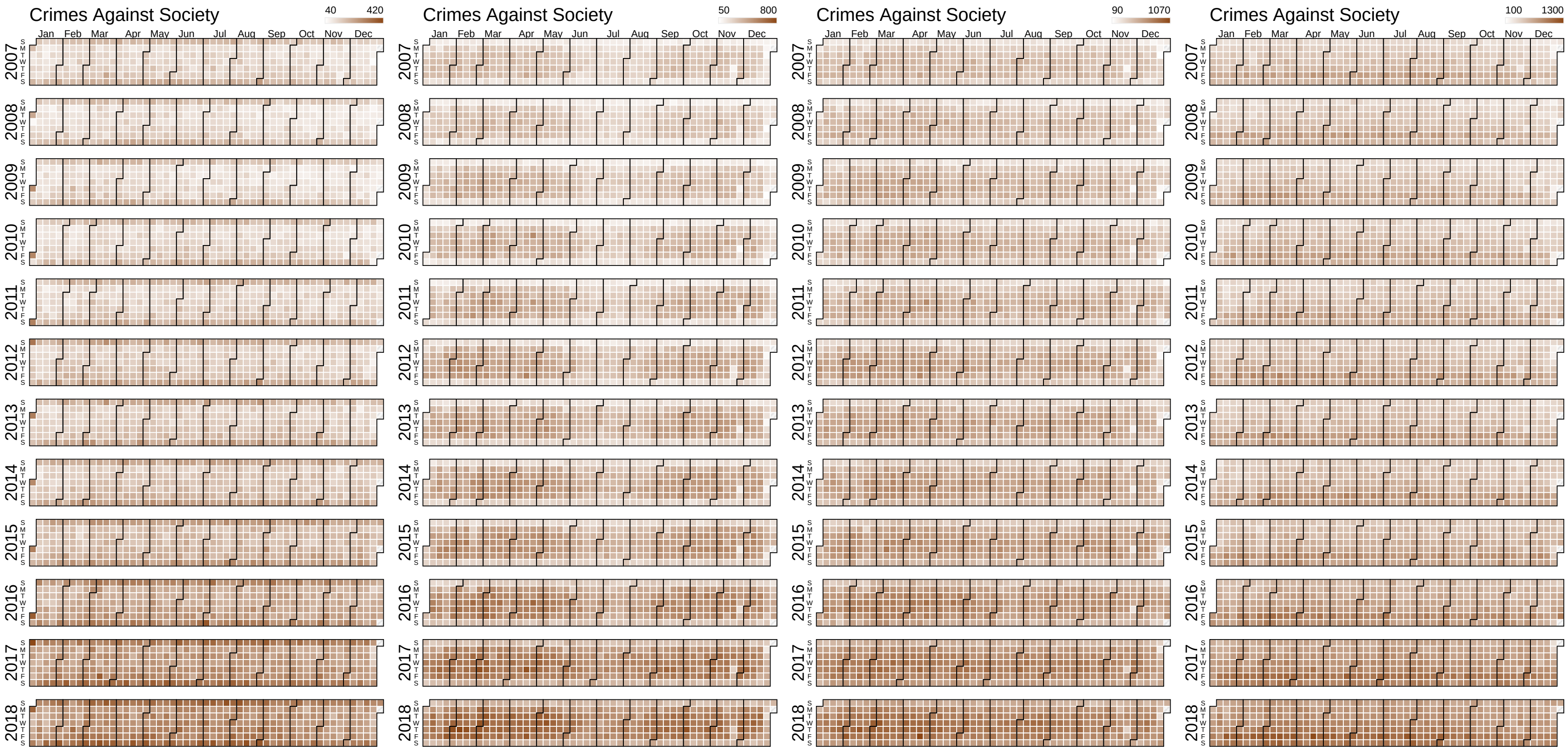
(a) 00:00–05:59 Pacific time. (b) 06:00–11:59 Pacific time. (c) 12:00–17:59 Pacific time. (d) 18:00–23:59 Pacific time.

Figure 1.13. Reported Crimes Against Persons by Time Bin, 2007–2018. Each cell represents a day. Darker shades indicate more reported incidents within the specified time bin.



(a) 00:00–05:59 Pacific time. (b) 06:00–11:59 Pacific time. (c) 12:00–17:59 Pacific time. (d) 18:00–23:59 Pacific time.

Figure 1.14. Reported Crimes Against Property by Time Bin, 2007–2018. Each cell represents a day. Darker shades indicate more reported incidents within the specified time bin.



(a) 00:00–05:59 Pacific time.
(b) 06:00–11:59 Pacific time.
(c) 12:00–17:59 Pacific time.
(d) 18:00–23:59 Pacific time.

Figure 1.15. Reported Crimes Against Society by Time Bin, 2007–2018. Each cell represents a day. Darker shades indicate more reported incidents within the specified time bin.

Table 1.17. FBI Group A Offenses

		Crimes against		
Code	Description	Person	Property	Society
720	Animal cruelty			✓
200	Arson		✓	
13A	Aggravated assault	✓		
13B	Simple assault	✓		
13C	Intimidation	✓		
510	Bribery		✓	
220	Burglary/breaking & entering		✓	
250	Counterfeiting/forgery		✓	
290	Destruction/damage/vandalism of property		✓	
35A	Drug/narcotic violations			✓
35B	Drug equipment violations			✓
270	Embezzlement		✓	
210	Extortion/blackmail		✓	
26A	False pretenses/swindle/confidence game		✓	
26B	Credit card/automated teller machine fraud		✓	
26C	Impersonation		✓	
26D	Welfare fraud		✓	
26E	Wire fraud		✓	
26F	Identity theft		✓	
26G	Hacking/computer invasion		✓	
39A	Betting/wagering			✓
39B	Operating/promoting/assisting gambling			✓
39C	Gambling equipment violations			✓
39D	Sports tampering			✓
09A	Murder & non-negligent manslaughter	✓		
09B	Negligent manslaughter	✓		
09C	Justifiable homicide[1]			
64A	Human trafficking, commercial sex acts	✓		
64B	Human trafficking, involuntary servitude	✓		
100	Kidnapping/abduction	✓		
23A	Pocket-picking		✓	
23B	Purse-snatching		✓	
23C	Shoplifting		✓	
23D	Theft from building		✓	
23E	Theft from coin-operated machine or device		✓	
23F	Theft from motor vehicle		✓	
23G	Theft of motor vehicle parts or accessories		✓	
23H	All other larceny		✓	
240	Motor vehicle theft		✓	
370	Pornography/obscene material			✓
40A	Prostitution			✓
40B	Assisting or promoting prostitution			✓
40C	Purchasing prostitution			✓
120	Robbery[2]	✓	✓	
11A	Rape	✓		
11B	Sodomy	✓		
11C	Sexual assault with an object	✓		
11D	Fondling	✓		
36A	Incest	✓		
36B	Statutory rape	✓		
280	Stolen property offenses		✓	
520	Weapon law violations			✓

Source: FBI NIBRS Technical Manual, Appendix Table A-1.
[1] Justifiable homicide is not a crime.
[2] Robbery is categorized as a crime against property. However, assault is a lesser included offense of robbery. Thus robbery is coded as both a crime against a person and a crime against property.

Table 1.18. Netflix Original Series Ratings, Part 1

				Common Sense Media		IMDb
Title	Year	Continuation	TV Rating	Violence	Star Rating	Rating Median
13 Reasons Why	2017	✓		4	4	2
3Below: Tales of Arcadia	2018	✓		3	4	2
7 Days Out	2018					1
72 Dangerous Animals: Asia	2018	✓				1
72 Dangerous Animals: Latin America	2017					1
A Little Help with Carol Burnett	2018		TV-G	0	4	1
A Series of Unfortunate Events	2017	✓	TV-PG	0	4	2
Abstract: The Art of Design	2017		TV-14	1	4	2
Afflicted	2018					1
Alexa & Katie	2018	✓	TV-Y7	0	4	1
All About the Washingtons	2018			0	3	1
All Hail King Julien	2014	✓	TV-Y7-FV	2	3	1
All Hail King Julien: Exiled	2017	✓	TV-Y7-FV	2	3	1
Altered Carbon	2018			5	2	2
Amazing Interiors	2018					1
American Vandal	2017	✓		1	4	2
Arrested Development	2013	✓	TV-MA	1	5	2
Ask the Storybots	2016	✓	TV-G	0	4	2
Atypical	2017	✓		1	4	2
Bad Samaritans	2013					1
Battlefish	2018					1
Best.Worst.Weekend.Ever.	2018		TV-PG	2	4	1
Beyond Stranger Things	2017	✓				1
Big Mouth	2017	✓		1	4	2
Bill Nye Saves the World	2017	✓	TV-14	0	3	1
Black Mirror	2016	✓	TV-MA	4	5	2
Bloodline	2015	✓	TV-MA	4	4	2
BoJack Horseman	2014	✓	TV-MA	2	3	2
Bobby Kennedy for President	2018		TV-MA	3	4	2
Brainchild	2018		TV-Y7	0	5	2

Notes: Restricted to English-language productions with a United States market worldwide premiere on Netflix Streaming. Year is the first year the title had a release on Netflix. Continuation identifies series that are a continuation of another series or have additional seasons in the sample period. TV Rating is the United States TV parental guideline. Violence and star ratings are the 0–5 ratings, with 5 high, assigned to the title by the Common Sense Media reviewer. Rating Median the title's IMDb rating grouping, above median (= 2) or below median (= 1), in its year of release.

Table 1.19. Netflix Original Series Ratings, Part 2

				Common Sense Media		IMDb
Title	Year	Continuation	TV Rating	Violence	Star Rating	Rating Median
Buddy Thunderstruck	2017			0	3	2
Captive	2016			3	4	1
Car Masters: Rust to Riches	2018					2
Care Bears & Cousins	2015	✓	TV-Y	0	4	1
Castlevania	2017	✓		4	3	2
Chasing Cameron	2016			0	1	1
Chef's Table	2015	✓	TV-MA	1	4	2
Chef's Table: France	2016	✓	TV-MA	1	4	2
Chelsea Does	2016		TV-MA	1	3	1
Chilling Adventures of Sabrina	2018		TV-14	4	4	2
Cirque du Soleil: Luna Petunia	2016	✓	TV-G	0	4	1
Coach Snoop	2018	✓				1
Comedians in Cars Getting Coffee	2018	✓		0	4	2
Cooked	2016		TV-PG	1	4	2
Cooking on High	2018		TV-MA	0	3	1
Cupcake & Dino: General Services	2018		TV-Y7	0	4	1
Dancing Queen	2018		TV-14	0	3	1
Daredevil	2015	✓	TV-14	4	4	2
Dark Tourist	2018					1
Daughters of Destiny	2017			2	4	2
Dawn of the Croods	2015	✓	TV-Y7	2	3	1
Dear White People	2017	✓	TV-MA	1	4	1
Death By Magic	2018		TV-14	3	4	1
Dinotrux	2015	✓		2	4	1
Dinotrux Supercharged	2017	✓		2	4	1
Dirty Money	2018		TV-14	1	3	2
Disenchantment	2018	✓	TV-14	3	3	1
Disjointed	2017	✓		0	2	1
Dogs	2018		TV-PG	2	5	2
Dope	2017	✓	TV-MA	4	3	1

Notes: See Table 1.18 notes.

Table 1.20. Netflix Original Series Ratings, Part 3

Title	Year	Continuation	TV Rating	Common Sense Media		IMDb
				Violence	Star Rating	Rating Median
Dragons: Race to the Edge	2015	✓	TV-Y7-FV	3	4	2
Drug Lords	2018	✓				1
Easy	2016	✓	TV-MA	0	3	1
Edgar Rice Burroughs' Tarzan and Jane	2017	✓		0	3	1
Everything Sucks!	2018			1	4	1
Evil Genius: The True Story of America's Most Diabolical Bank Heist	2018					2
F Is for Family	2015	✓	TV-MA	2	3	2
Fastest Car	2018					1
Fearless	2016					1
FightWorld	2018		TV-MA	3	3	2
Fire Chasers	2017			2	5	1
First Team: Juventus	2018	✓				1
First and Last	2018					1
Five Came Back	2017		TV-MA	4	4	2
Flaked	2016	✓	TV-MA	2	3	1
Flint Town	2018		TV-MA	3	4	2
Follow This	2018	✓	TV-MA	2	4	1
Free Rein	2017	✓	TV-G	0	4	1
Friends from College	2017			0	4	1
Fuller House	2016	✓	TV-Y7	0	3	1
GLOW	2017	✓		2	4	2
Gilmore Girls: A Year in the Life	2016	✓		0	3	2
Girlboss	2017			1	4	1
Girls Incarcerated	2018		TV-MA	3	4	1
Godless	2017		TV-MA	5	3	2
Grace and Frankie	2015	✓		1	4	2
Greenhouse Academy	2017	✓	TV-PG	0	3	1
Gypsy	2017		TV-MA	1	2	1
Harvey Girls Forever!	2018					1
Haters Back Off	2016	✓	TV-14	0	2	1

Notes: See Table 1.18 notes.

Table 1.21. Netflix Original Series Ratings, Part 4

				Common Sense Media		IMDb
Title	Year	Continuation	TV Rating	Violence	Star Rating	Rating Median
Haunted	2018		TV-MA	4	2	1
Hemlock Grove	2013	✓	TV-MA	5	3	1
Hilda	2018			1	4	2
Hip-Hop Evolution	2018	✓	TV-MA	4	4	
Home: Adventures with Tip and Oh	2016	✓	TV-Y7	1	3	1
Hot Girls Wanted: Turned On	2017		TV-MA	3	2	1
House of Cards	2013	✓	TV-14	2	4	2
Insatiable	2018			4	2	1
Inside the Real Narcos	2018					2
Inside the World's Toughest Prisons	2018	✓				1
Iron Fist	2017	✓		3	3	1
Jack Whitehall: Travels with My Father	2017	✓	TV-MA	2	3	2
Jessica Jones	2015	✓	TV-MA	4	4	2
Julie's Greenroom	2017			0	5	2
Justin Time GO!	2016	✓	TV-G	0	4	1
Kong: King of the Apes	2016	✓	TV-Y7-FV	3	3	1
Kulipari: An Army of Frogs	2016	✓	TV-Y7-FV	3	4	1
Kulipari: Dream Walker	2018	✓	TV-Y7-FV	3	4	1
Lady Dynamite	2016	✓		2	4	1
Larva Island	2018		TV-Y7	3	2	1
Last Chance U	2016	✓	TV-MA	3	4	2
Legend Quest	2017		TV-Y7	0	4	1
Lego Bionicle: The Journey to One	2016	✓	TV-Y7-FV	2	3	1
Lego Elves: Secrets of Elvendale	2017			1	3	1
Lego Friends: The Power of Friendship	2016	✓	TV-Y7	0	3	1
Llama Llama	2018		TV-Y	0	5	1
Longmire	2015	✓	TV-14	4	4	2
Lost in Space	2018			3	3	1
Love	2016	✓		0	4	2
Lovesick	2016	✓	TV-MA	2	4	2

Notes: See Table 1.18 notes.

Table 1.22. Netflix Original Series Ratings, Part 5

				Common Sense Media		IMDb
Title	Year	Continuation	TV Rating	Violence	Star Rating	Rating Median
Luke Cage	2016	✓	TV-MA	4	4	1
Luna Petunia: Return to Amazia	2018	✓	TV-G	0	4	1
Magic for Humans	2018		TV-14	0	4	1
Making a Murderer	2015	✓	TV-14	3	4	2
Maniac	2018		TV-MA	3	4	2
Marching Orders	2018		TV-14	0	3	1
Marco Polo	2014	✓	TV-MA	3	3	2
Master of None	2015	✓		0	4	2
MeatEater	2018	✓				2
Medal of Honor	2018		TV-MA	3	4	2
Mindhunter	2017		TV-MA	4	4	2
Motown Magic	2018		TV-Y	0	5	2
Murder Mountain	2018					1
Mystery Science Theater 3000: The Return	2017	✓		2	4	2
Nailed It!	2018	✓		0	4	1
Nailed it! Holiday	2018	✓		0	4	2
Narcos	2015	✓		5	4	2
Narcos: Mexico	2018	✓		5	4	2
Neo Yokio	2017		TV-MA	2	2	1
Norm Macdonald Has a Show	2018		TV-MA	0	2	2
On My Block	2018		TV-14	2	4	2
One Day at a Time	2017	✓	TV-PG	0	4	2
Orange Is the New Black	2013	✓	TV-MA	3	4	2
Ozark	2017	✓		4	3	2
Paradise PD	2018		TV-MA	4	2	1
Popples	2015	✓	TV-Y	0	3	1
Prince of Peoria	2018		TV-Y7	1	3	1
Project Mc2	2015	✓	TV-Y7	0	4	1
Queer Eye	2018	✓	TV-14	0	4	2
Rapture	2018			3	3	1

Notes: See Table 1.18 notes.

Table 1.23. Netflix Original Series Ratings, Part 6

				Common Sense Media		IMDb
Title	Year	Continuation	TV Rating	Violence	Star Rating	Rating Median
Real Rob	2015	✓	TV-MA	2	1	1
Richie Rich	2015	✓	TV-G	0	1	1
Roman Empire	2016	✓				1
Rotten	2018					1
Russell Peters Vs. the World	2013					1
Salt, Fat, Acid, Heat	2018		TV-PG	0	4	2
Santa Clarita Diet	2017	✓		4	4	2
Sense8	2015	✓		3	3	2
Seven Seconds	2018		TV-MA	4	4	2
She's Gotta Have It	2017			2	4	1
She-Ra and the Princesses of Power	2018		TV-Y7-FV	3	4	2
Shot in the Dark	2017					2
Skylanders Academy	2016	✓	TV-Y7	2	4	1
Somebody Feed Phil	2018	✓	TV-14	1	3	2
Spirit Riding Free	2017	✓	TV-Y7	2	4	1
Spy Kids: Mission Critical	2018	✓	TV-Y7-FV	2	3	1
Star Wars: The Clone Wars	2014	✓	TV-PG	3	3	2
Stay Here	2018					1
StoryBots Super Songs	2016	✓	TV-G	0	4	2
Stranger Things	2016	✓	TV-14	3	4	2
Stretch Armstrong and the Flex Fighters	2017	✓	TV-Y7-FV	2	3	1
Sugar Rush	2018		TV-PG	0	4	1
Sunderland 'Til I Die	2018		TV-MA	3	4	2
Super Monsters	2017	✓	TV-Y	0	5	1
Terrorism Close Calls	2018					1
The Adventures of Puss in Boots	2015	✓	TV-Y7-FV	2	4	1
The Boss Baby: Back in Business	2018	✓	TV-Y7	0	4	1
The Characters	2016			1	3	1
The Confession Tapes	2017		TV-14	4	3	1
The Crown	2016	✓	TV-MA	3	5	2

Notes: See Table 1.18 notes.

Table 1.24. Netflix Original Series Ratings, Part 7

				Common Sense Media		IMDb
Title	Year	Continuation	TV Rating	Violence	Star Rating	Rating Median
The Curious Creations of Christine McConnell	2018		TV-PG	2	4	2
The Defenders	2017	✓	TV-14	2	3	1
The Dragon Prince	2018			3	4	2
The Epic Tales of Captain Underpants	2018			3	3	1
The Final Table	2018		TV-PG	0	3	2
The Fix	2018		TV-PG	3	2	1
The Get Down	2016	✓		3	4	2
The Good Cop	2018		TV-PG	2	4	1
The Haunting of Hill House	2018		TV-MA	4	4	2
The Hollow	2018			3	4	1
The Innocent Man	2018		TV-MA	3	3	1
The Innocents	2018		TV-MA	4	4	1
The Joel McHale Show with Joel McHale	2018	✓	TV-MA	0	3	1
The Keepers	2017			2	4	2
The Killing	2014	✓	TV-14	4	4	2
The Kominsky Method	2018		TV-MA	0	3	2
The Last Kingdom	2018	✓	TV-14	4	4	2
The Magic School Bus Rides Again	2017	✓		0	4	1
The Mr. Peabody & Sherman Show	2015	✓	TV-Y7	2	3	1
The OA	2016		TV-MA	4	3	2
The Ponysitters Club	2018	✓	TV-Y7	0	3	1
The Punisher	2017	✓		5	3	2
The Ranch	2016	✓		2	2	1
The Staircase	2018	✓	TV-14	4	5	2
The Toys That Made Us	2017	✓	TV-14	0	4	2
The Who Was? Show	2018		TV-Y7	0	4	1
Tidelands	2018		TV-MA	3	2	1
Trailer Park Boys	2014	✓	TV-MA	3	3	2
Trailer Park Boys: Out of the Park: Europe	2016	✓	TV-MA	3	3	1
Trailer Park Boys: Out of the Park: USA	2017	✓	TV-MA	3	3	1

Notes: See Table 1.18 notes.

Table 1.25. Netflix Original Series Ratings, Part 8

Title	Year	Continuation	TV Rating	Common Sense Media		IMDb
				Violence	Star Rating	Rating Median
Trollhunters: Tales of Arcadia	2016	✓		3	4	2
Trolls: The Beat Goes On!	2018	✓	TV-G	2	4	1
True and the Rainbow Kingdom	2017	✓	TV-Y	0	5	1
True: Magical Friends	2018	✓	TV-Y	0	5	1
True: Wonderful Wishes	2018	✓	TV-Y	0	5	1
Turbo FAST	2013	✓	TV-Y7-FV	2	3	1
Ugly Delicious	2018			0	4	2
Ultimate Beastmaster	2017	✓	TV-14	0	3	1
Unbreakable Kimmy Schmidt	2015	✓	TV-14	0	4	2
VeggieTales in the City	2017	✓		1	4	1
VeggieTales in the House	2014	✓	TV-Y	0	3	1
Voltron: Legendary Defender	2016	✓		3	4	2
W/Bob & David	2015		TV-MA	2	4	1
We're Lalaloopsy	2017			0	4	1
Westside	2018		TV-MA	1	2	1
Wet Hot American Summer: First Day of Camp	2015	✓	TV-MA	2	3	1
Wet Hot American Summer: Ten Years Later	2017	✓	TV-MA	1	2	1
White Rabbit Project	2016			2	4	1
Wild Wild Country	2018		TV-MA	3	5	2
Word Party	2016	✓		0	4	1
World of Winx	2016	✓	TV-Y7	1	2	1
Wormwood	2017		TV-14	3	3	1

Notes: See Table 1.18 notes.

2. FAMILY VIOLENCE AND FOOTBALL: THE EFFECT OF UNEXPECTED EMOTIONAL CUES ON VIOLENT BEHAVIOR: COMMENT

2.1 Introduction

Card and Dahl (2011), hereafter CD, present a seminal paper that lays out a theoretical model examining the relationship between unexpected emotional cues and violent behavior. They proceed into an empirical application that shows that upset losses in professional American football games cause an increase in the probability of reported male-to-female intimate partner violence in a residence.

CD has been highly influential. According to Google Scholar, CD has been cited over 600 times in the 10 years since its publication. CD has been cited in articles published by top economic journals in topics such as the economics of crime (e.g., Miller and Segal (2018)), labor economics (e.g., DellaVigna et al. (2017)), and experimental economics (e.g., Gill and Prowse (2012)). From 2005 to 2015 intimate partner violence against women has had a level trend. In CD, intimate partner violence against women was declining over the study period before reaching the trough in 2005 where the rate of violence has remained stable (Office for Victims of Crime 2018).

CD present a general theoretical model and a limited empirical exercise. However, when CD is cited, their work is often summarized as an general empirical finding with broad external validity. Bhuller et al. (2013) write that CD "show that emotional cues provided by local NFL football games cause a spike in family violence." Miller and Segal (2018) write "[domestic violence] rates are also found to be elevated by... unexpected football losses...", where CD's results are summarized by three words in a four-part list. These general interpretations are despite the fact that CD carefully discuss the limitations of their model and their data.

CD's results are limited due the data available at the time. Their emotional cues are state-based shocks, limiting their sample to states that have a lone NFL team. In addition, all other variables are assigned at the state-level. Further advances in data allow for the assignment of county-level characteristics and designated market area (DMA) based emotional cues. Expanding the analysis to DMA-based emotional cues allows for the sample to extend beyond states that have one NFL team to all states, while also allowing for teams with strong regional influence near a state boundary to properly influence a market that is across a state boundary. DMA-based emotional cues allow for standard errors clustered at the DMA-level. As CD note, their replication files do not quantitatively replicate the results in their paper. They suggest that these differences are due to changes in a statistical program. This inconsistency adds further value to independently replicate their baseline results.

This paper has two contributions. First, I replicate CD's baseline results using an alternate approach to NIBRS data while using CD's independent variables and variables from new sources. I show that CD's baseline results hold as long as their original design is used. Second, I expand the empirical exercise using recently available data and DMA-based emotional cues. CD's baseline result is not robust to changing the study period. I show that the effects of state-based emotional cues and the effects of DMA-based emotional cues are not statistically different. Thus, the likely difference in the effect of upset losses is a change in marginal potential offender due to the differences in the rates of intimate partner violence against women between the two study periods.

2.2 Data

CD provide detailed replication files for their paper online. They acknowledge in their replication file that the file itself does not quantitatively reproduce the results published in their paper. CD do not provide data sources for their NFL game data or the gambling spreads utilized in the paper. They also use propriety Nielsen local television ratings data that is not available for this paper.

CD perform their analysis on all Sundays in NFL regular seasons in which games are played. They aggregate to the reporting agency-day level, with each day covering noon to 11:59 PM Eastern time. The primary variable of interest is the count of male-to-female intimate partner violence in a residence that occurs in a reporting agency-day.

2.2.1 Crime Data

CD use data from the Federal Bureau of Investigation's (2020a) NIBRS, which part of the Uniform Crime Reporting (UCR) System. NIBRS is the premier source of crime reporting in the United States due to the details provided on each incident. I process NIBRS data from 1995–2019. Since CD's original analysis, NIBRS still not nationally representative due to voluntary crime reporting. In 2016, NIBRS covered 33 percent of the population and accounted for 28 percent of all crimes reported to the UCR Program (Federal Bureau of Investigation 2017).

NIBRS is a multi-table data set that has detailed data on incidents, offenses, offenders, victims, property, and the reporting agencies. Details per offense include incident date and incident hour so we can closely match incident reports with times of day that have NFL games. In this paper, I use incidents that are reported on the same day that they occur. NIBRS requires that detailed victim and offender data be included for every incident that involves a crime against a person. These details include demographic information on victims and offenders as well as each victim's relationship to each offender. With the detail involved, NIBRS can be processed in different ways and aggregated in ways that result in distinctly different observations (Akiyama and Nolan 1999). CD present their theoretical model as a potential offender reacting to an emotional cue. Therefore, I aggregate offender-level data to the reporting agency-day level.

CD restrict their sample to local reporting agencies, which they categorize as local or county police. Agency type is reported annually in NIBRS but is self-reported and inconsistent over time. The UCR separately maintains a list of reporting agencies that is consistent over time. In this paper, I use the UCR's list over the variable in NIBRS to consistently assign agency type over time.

CD restrict agencies in the sample based on the frequency of incidents reported. They require that at least one incident must be reported every week to count as an agency reporting in that seven-day period. CD require that each agency has at least 13 weeks where any incident is reported out of a possible 17 weeks for that agency-year to remain in the sample. This restriction may exclude reporting agencies in low-crime areas since NIBRS only requires agencies to report crimes that fit into 51 crime types. Since low-crime areas may have no reportable crime in the monthly filing period, NIBRS is designed with reporting flags to indicate the reporting status of each reporting agency each month. In this paper, agencies must report at least three months of incidents from a typical NFL season (i.e., at least three months from September, October, November, or December) to stay in the sample. This requirement is effectively the same as CD's requirement that agencies report at least 13 weeks out of 17 weeks. By using the reporting flags, I am also able to verify that legitimate zero-incident days remain in the sample.

CD define an assault as an offense of aggravated assault, simple assault, or intimidation and exclude higher level charges such as homicide. NIBRS describes the charges of aggravated assault, simple assault, or intimidation as lesser included offenses that should not be reported in incidents that already include a higher-level assault such as murder. In this paper, I define assault as the class of lesser assaults plus all greater offenses that include lesser assaults.[1]

2.2.2 NFL Data and Gambling Spreads

CD do not report their NFL game data or gambling spread data sources. I obtain both from SportsDatabase.com (2021) for the 1990–2020 NFL seasons. I supplement that data with game day data from Pro-Football-Reference.com for game start times (Sports Reference LLC 2021).

Data for NFL games is consistent between sources. However, the gambling spreads are not consistent between sources. There are games that have different gambling lines from CD's replication data, SportsDatabase.com and Pro-Football-Reference.com even though the latter two sources both claim to provide the consensus gambling spread.

For the empirical exercises in this paper, I use NFL data from 1995–2019 seasons, where each season consists of 17 weekends with regular season games.[2]

1. ↑This expands the definition of assault to cover the offenses: murder & nonnegligent manslaughter, rape, sodomy, sexual assault with an object, fondling, robbery, aggravated assault, simple assault, and intimidation. Practically, for offenses that occur on Sundays from 12:00–23:59 Eastern time between male-to-female intimate partners in a residence, this is a small change in the number of reported assaults and has no qualitative effect.

2. ↑The 2001 NFL season canceled all week 2 games in observance of the September 11 attacks. Those games were postponed creating an 18-week season with 17 weekends with games.

CD define a predicted win for a team when the team has a spread of -4 or less, which means that the gambling line considers that team at least a four-point favorite. A predicted loss is when a team has a spread greater than or equal to $+4$. All other spreads (i.e., $-4 <$ spread $< +4$) are called predicted close. CD define an upset loss as when a team loses a game that they are favored to win (i.e., spread < -4). An upset win is when a team wins a game that they are favored to lose (i.e., spread $> +4$). A close loss is win a team loses a game that is predicted close (i.e., $-4 <$ spread $< +4$).

2.2.3 DMA-based Emotional Cues with Google Trends

A key limitation to CD's study is the need to assign team-based wins and losses as a state-based emotional cue. Combined with the lack of NIBRS coverage in the early years of their analysis, this restriction results in the original CD sample to be limited to 6 out of the 32 NFL teams and 8 of 50 states.

Technology companies specializing in social media and search provide a way to see interest in a term or topic over regions in a specified time frame based on likes or search volume. Facebook and Twitter have used likes and follows, respectively, to make county-level maps detaining interest in NFL teams (Meyer 2014; Twitter 2014). Google News Lab (2015, 2018) has released county-level NFL team interest heat maps based on Google Trends search volume twice. While these maps have been made available, the underlying data is restricted.

Google Trends does allow anyone to browse search trends. However, the product is limited and the limitations pose an issue for replication. Queries on Google Trends analyze a small subsample of the population of all Google searches. This subsample changes over time. Google Trends allows the user to compare the five search terms relative to each other but does not provide actual search volume. If the subsample contains fewer than a minimum number of any of the requested terms in a reporting area, the results in that area will be censored.

Google Trends allows users to search based on specific terms or based on topics. Searching for terms provides results that have an exact match, regardless of potential relevance. When searching a topic though, Google uses machine learning algorithms categorize searches into the topic and reports the interest in the topic overall.[3] This topic search allows for abbreviations or misspellings and exact matches to be part of the results while also excluding searches that are unlikely to be of interest. For example, a search of the term "Giants" would show the search interest for a football team, a baseball team, and numerous other products whereas a search for the topic "New York Giants" would be restricted to searches where users showed interest in the football team itself.

In order to allow for a large enough search volume at the DMA-level, I use search results from Google LLC (2021) covering January 1, 2004–the beginning of Google Trends data–to December 31, 2019–the end of NIBRS data. Since searches are limited to five terms and provide a relative ranking, I use a term that will dominate all NFL-related search results to establish a common reference point and fill the other four slots with teams in the same NFL division.[4] I conducted searches for all eight NFL divisions, combined them, and used the results relative to the reference topic to rank each team in each DMA. In the presence of ties, I conducted additional searches containing the tied teams and assigned the highest ranked team in the relative search as the most popular team for that DMA. The result is a DMA coverage map for the most popular team based on aggregate search results from 2004–2019. In order to link the DMA-level data to county-level data, I use a crosswalk created by Schneider (2020).

2.2.4 County-level Data

NIBRS allows for each reporting agency to report its respective coverage area as up to 5 separate counties. [5] Along with the counties covered, NIBRS calculates the population covered in the reported counties by that respective agency. NIBRS takes care to not allow for any double counting of the population in their coverage areas and assigns population to local agencies, such as city-level police agencies, before assigning population to agencies

3. ↑Posing a further issue for replication, it is likely that Google continuously improves this machine learning algorithm over time without documentation.

4. ↑For example, the AFC South contains the Houston Texans, the Indianapolis Colts, the Jacksonville Jaguars, and the Tennessee Titans. A Google Trends search related to the AFC South would be the topics: "School", "Houston Texans", "Indianapolis Colts", "Jacksonville Jaguars", and "Tennessee Texans". "School" would be the relative reference term used to benchmark the results against other search results.

5. ↑In practice, few reporting agencies report more than 2 counties in their coverage area.

with a broader coverage area, such as county-level police agencies. I match county-level characteristics to each reporting agency through the most populated county in their respective coverage area, updating the most populated county each year.

I assign county-level time zones from the National Weather Service (2019). I assign the first time zone listed as the representative time zone for counties that cover multiple time zones. I keep counties in the sample that observe daylight savings time in the Eastern, Central, Mountain, and Pacific time zones. In CD, time zones are assigned as a state-wide characteristic despite three of the eight states in their sample have two time zones (i.e., Kansas, Michigan, and Tennessee).

County-level daily weather data on maximum temperature, minimum temperature, and precipitation is from Schlenker and Roberts (2009). Schlenker and Roberts (2009) extend a model that from Schlenker and Roberts (2006) to model daily weather patterns by using nonlinear estimation. They combine monthly weather data from PRISM Climate Group and daily weather readings from local weather reporting stations to create weather estimates that are consistently reported and account for spatial differences. These estimates remove the concerns about weather data that is not reported due to issues such as sparsely positioned stations or equipment failure. For each agency day, I create seven indicator variables for daily weather. Following Dahl and DellaVigna (2009), I make three indicators for hot days: maximum temperature between 80 and 90 degrees Fahrenheit, maximum temperature between 90 and 100 degrees Fahrenheit, and maximum temperature more than 100 degrees Fahrenheit. Similarly, I generate three indicators for cold days: minimum temperature between 20 and 32 degrees Fahrenheit, minimum temperature between 10 and 20 degrees Fahrenheit, and minimum temperature less than 10 degrees Fahrenheit. Lastly, I create a single indicator for agency days with at least one tenth of an inch of precipitation.

CD assign state-level weather variables based on the daily weather reported in each state capital. They construct the following dummy variables: *hot* for maximum temperatures greater than 80 degrees Fahrenheit; *cold* for minimum temperatures less than or equal to 32 degrees Fahrenheit; *hiheatindex* for heat indexes over 100; *windy* for maximum daily wind speeds greater than 17 knots; *anyrain* for any reported rainfall; and *anysnow* for any reported snowfall. State-wide weather is problematic in areas that have regional characteristics such as high variance in elevation or humidity, which is present in several states in CD's sample (e.g, Colorado and Tennessee).

In addition, CD control for the fixed-date holidays: Halloween, Christmas Eve, Christmas Day, New Year's Eve, and New Year's Day. CD also create identifiers for the following weekends: Thanksgiving, Labor Day, Columbus, and Veterans Day. I create matching identifiers throughout the sample.[6]

2.3 Results

I examine CD's results using eight different samples. All samples use the crime data described above. Four samples use state-based emotional cues and are restricted to reporting agencies located in states used in CD—Colorado, Kansas, Massachusetts, Michigan, New Hampshire, South Carolina, Tennessee, and Vermont. The first state-based emotional cue sample uses CD's independent variables and covers the 1995–2006 NFL seasons. The latter three state-based emotional cue samples use county-level weather data and gambling spreads from SportsDatabase.com (2021) while covering the 1995–2006, 2007–2019, and 1995–2019 NFL season ranges respectively. All state-based emotional cue samples cluster standard errors by team times season.

I construct four samples using DMA-based emotional cues. All DMA-based emotional cue samples use county-level weather and gambling spreads from SportsDatabase.com (2021). Three of these samples cover the 2007–2019 NFL seasons—seasons outside of CD—while the last sample covers the 2004–2019 NFL seasons—the full range of seasons covered by Google Trends. The 2007–2019 season samples are separated into three groups: all states, eight states used in CD, and the states not used in CD. The 2004–2019 sample uses all states. All DMA-based emotional cue samples cluster standard errors by DMA.

I use CD's baseline Poisson model

$$\log\left(\mu_{jt}\right) = \theta_j + X_{jt} + g\left(S_{jt}, y_{jt}; \lambda\right), \tag{2.1}$$

6. ↑The Veterans Day weekend identifier is created for Veterans Days that fall on Sunday or Monday.

where μ_{jt} is the expected number of reported intimate partner offenders by agency j at time t, θ_j is a reporting agency fixed effect, X_{jt} is a matrix of time-varying controls (i.e., season, season of week, holiday and weather controls), and

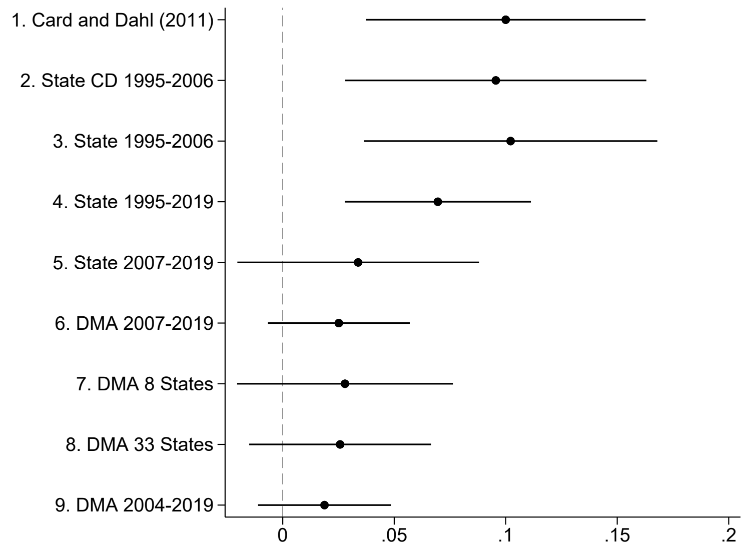
$$\begin{aligned} g\left(S_{jt}, y_{jt}; \lambda\right) = \lambda_1 \cdot 1\left(S_{jt} \leq -4\right) + \lambda_2 \cdot 1\left(S_{jt} \leq -4\right) 1\left(y_{jt} = 0\right) \\ + \lambda_3 \cdot 1\left(-4 < S_{jt} < 4\right) + \lambda_4 \cdot 1\left(-4 < S_{jt} < 4\right) 1\left(y_{jt} = 0\right) \\ + \lambda_5 \cdot 1\left(S_{jt} \geq 4\right) + \lambda_6 \cdot 1\left(S_{jt} \geq 4\right) 1\left(y_{jt} = 1\right), \quad (2.2) \end{aligned}$$

where λ_2, λ_4, and λ_6 are the coefficients related to upset loss, close loss, and upset win, respectively. $1\left(S_{jt} \leq -4\right), \left(-4 < S_{jt} < 4\right)$, and $\left(S_{jt} \geq 4\right)$ are indicator functions for predicted win, predicted close, and predicted loss, respectively. $1\left(y_{jt} = 0\right)$ and $1\left(y_{jt} = 1\right)$ are indicator functions for game day loss and game day win, respectively. Thus, $1\left(S_{jt} \leq -4\right) 1\left(y_{jt} = 0\right)$, $1\left(-4 < S_{jt} < 4\right) 1\left(y_{jt} = 0\right)$, and $1\left(S_{jt} \geq 4\right) 1\left(y_{jt} = 1\right)$ are the indicators for upset loss, close loss, and upset win, respectively. CD's hypothesis is that upsets will generate emotional responses and that those responses will be asymmetrical following loss aversion.

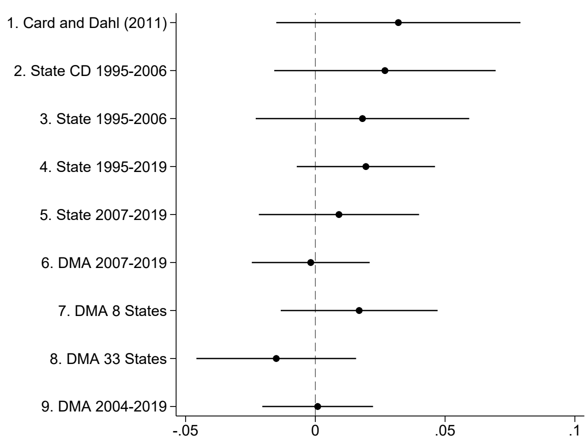
I estimate (2.1) using the eight different samples.[7] Each of the following tables corresponds to CD's Table IV columns (1)–(3) respectively, while omitting columns (4)–(5) due to the missing Nielsen ratings data. Column (1)s estimates the model with the game outcome variables and agency fixed effects. Column (2)s add NFL season, week of season, and holiday fixed effects. Column (3)s include the sample's weather variables. The table row labeled loss aversion shows the respective test using the coefficients for upset win and upset loss. I present the results tables for each sample described above in section 2.A. However, we are interested in comparing the baseline results presented in column (3)s, covering the coefficients for upset losses, close losses, and upset wins. Therefore, I present a comparison of these coefficients and their respective 95% confidence intervals in Figure 2.1 with the coefficient represented by the marker and the confidence interval represented by the line with the vertical axis labeling the regression sample. In addition to the results estimated here, I plot CD's published estimates at the top of the panels as the reference point. Panel (a)

7. ↑Models are estimated in Stata 16.1 using the command ppmlhdfe (Correia, Guimarães, and Zylkin 2019a, 2019b).

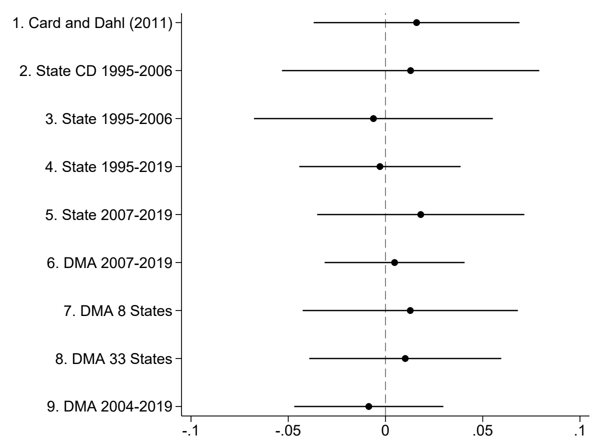
plots the coefficient for upset losses. Coefficients for close losses are shown in panel (b). Coefficients for upset wins are plotted in panel (c). I name and summarize the details of each regression sample in Table 2.1, again entering information from CD's published results at the top of the table. Each sample name is numbered, with the number corresponding to the same regression in Figure 2.1 and Table 2.1.



(a) Loss × predicted win (*upset loss*).



(b) Loss × predicted close (*close loss*).



(c) Win × predicted loss (*upset win*).

Figure 2.1. Baseline Results Coefficient Comparison Plot. Each plot contains the coefficients, represented by markers, and 95% confidence intervals, represented by lines, from nine Poisson regressions following (2.1). Labels on the vertical axis indicate the regression sample. Each regression sample is described in Table 2.1. Each panel label corresponds to the coefficient in the sample's respective table.

Table 2.1. Baseline Results Sample Comparison Summary

Sample	NFL Seasons	Loss Aversion Test	Agencies	Observations
1. Card and Dahl (2011)	1995–2006	0.00	764	79,386
2. State CD 1995–2006	1995–2006	0.07	1,371	198,547
3. State 1995–2006	1995–2006	0.01	1,372	198,977
4. State 1995–2019	1995–2019	0.01	1,799	599,821
5. State 2007–2019	2007–2019	0.68	1,625	335,240
6. DMA 2007–2019	2007–2019	0.41	4,564	785,335
7. DMA 8 States	2007–2019	0.69	1,625	335,240
8. DMA 33 States	2007–2019	0.61	2,939	450,095
9. DMA 2004–2019	2004–2019	0.28	4,719	953,536

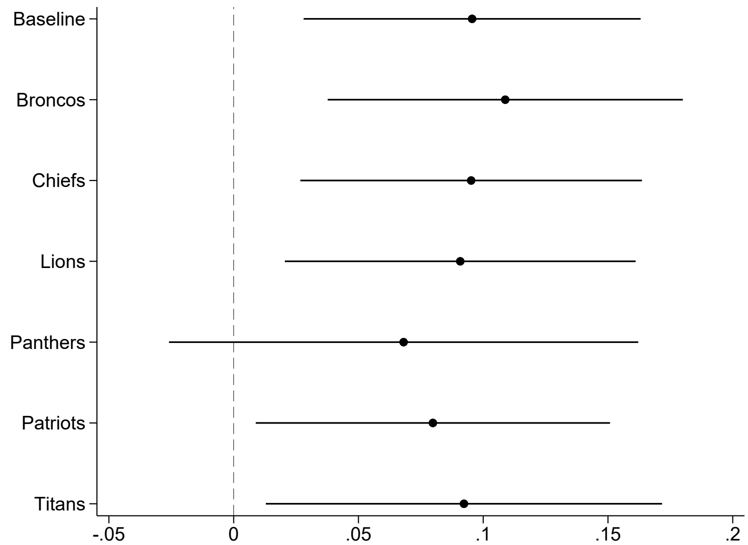
Notes: Each row contains sample details for the Poisson regression related Card and Dahl's (2011) Table IV column (3), but using the named sample. Card and Dahl (2011) is the baseline sample from CD's Table IV column (3). All other samples utilize National Incident-Based Reporting System (NIBRS) data as described in section 2.2. All samples beginning with "State" use state-based emotional cues and are restricted to reporting agencies in the eight states used in CD—Colorado, Kansas, Massachusetts, Michigan, New Hampshire, South Carolina, Tennessee, and Vermont. Samples beginning with "DMA" use DMA-based emotional cues. "State CD" use CD's independent variables. All other samples use county-level weather and spreads from SportsDatabase.com (2021). DMA 8 States is restricted to reporting agencies located in states used in CD. DMA 33 States is restricted to reporting agencies located in states not used in CD—Alabama, Arkansas, Connecticut, Georgia, Idaho, Illinois, Indiana, Iowa, Kentucky, Louisiana, Maine, Maryland, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Dakota, Texas, Utah, Virginia, Washington, West Virginia, Wisconsin, and Wyoming. Loss aversion test is the p-value for the test that the coefficients for upset loss and upset win are equal. Agencies is the number of reporting agencies in the sample. Observations are the number of reporting agency-days in the sample.

The first three lines in each panel in Figure 2.1 show that we qualitatively replicate CD's baseline results across all the coefficients of interest. The first three lines of Table 2.1 show that the State CD 1995–2006 and State 1995–2006 samples both increase the agencies and observations significantly from CD. The other major difference is there is only marginal evidence for the asymmetry in loss aversion in the State CD sample. These results show that as long as the original sample design is maintained the results are replicable and robust to changes in cleaning the crime data, weather data source, and gambling spread data source. Adding the 2007–2019 seasons to the sample, we see that CD's baseline result is robust to adding additional seasons in the State 1995–2019 sample. However, we notice that the magnitude of the baseline effect has decreased. In order to investigate this decrease in effect, I estimate (2.1) on state-based emotional cues for the 2007–2019 seasons in the State 2007–2019 sample. We can see that there is no effect of upset losses on male-on-female intimate partner violence in a residence during the 2007–2019 seasons. Thus CD's results are no longer robust when changing the study period to the seasons out of the original sample.

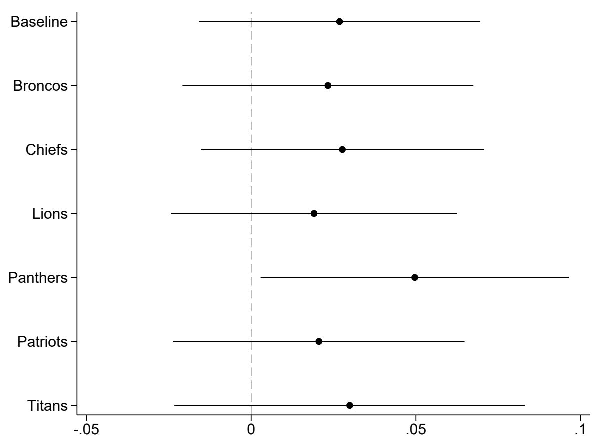
Moving to DMA-based emotional cues, we notice that there is no effect of upset losses on male-to-female intimate partner violence in a residence regardless of season coverage range or states included in the sample. The sample limited to eight states (DMA 8 States) does show some potential bias in the coefficient for close losses and generally larger confidence intervals. The loss of precision is a likely consequence of the reduction in statistical power in the eight state sample. Table 2.1 shows that the samples using DMA-based emotional cues cover have greater statistical power with greatly increased sample sizes. Since we find no statistically significant effects of upset losses or upset wins, it is not surprising that we find no evidence that there is loss aversion asymmetry.

Since CD's baseline results are robust as long as their original design is maintained, I investigate the particular effects of teams and seasons. For the State CD 1995–2006, State 1995–2006, and DMA 2004–2019 samples, I estimate (2.1) multiple times while excluding either a team or a season from the sample. I present the results in coefficient plots similar to Figure 2.1. Each figure has three panels with markers representing the coefficients and lines representing the 95% confidence interval. Each panel has the baseline model with all independent variables shown at the top. After that, the label on the vertical axis describes the team or NFL season that is excluded from the sample used in the regression.

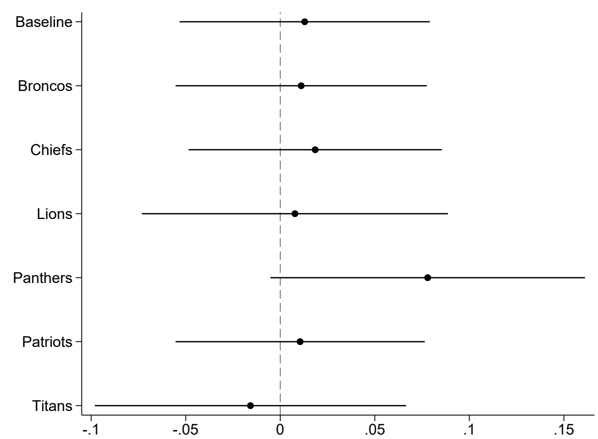
Figure 2.2 shows the coefficients for state-based emotional cues using CD's independent variables sample (State CD 1995–2006). We see that the coefficients are relatively stable when omitting teams from the sample except for when we leave out the Panthers. Panel (a) shows that leaving out the Panthers introduces bias and imprecision when estimating the effect of an upset loss. Panel (b) and (c) show that leaving out the Panthers creates bias in estimating the effects, but not imprecision. Thus, the State CD 1995–2006 sample design without the Panthers loses the effect of upset wins on intimate partner violence but gains an effect from close losses. Since emotional cues are state-based in this sample, these biases from leaving out the Panthers can be interpreted as the residents of South Carolina are more sensitive to upset loss, close losses, and close wins of the Panthers than other state residents in the sample are of the results to their respective teams.



(a) Loss × predicted win (*upset loss*).



(b) Loss × predicted close (*close loss*).



(c) Win × predicted loss (*upset win*).

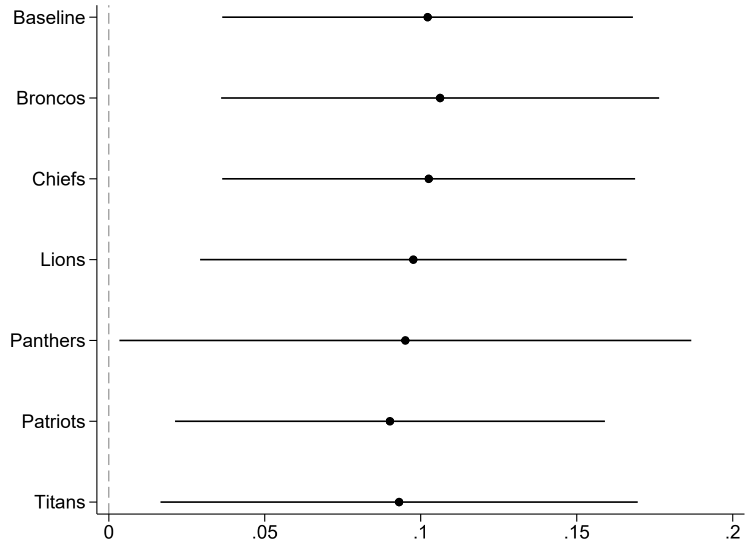
Figure 2.2. Coefficient Comparison Plot of Leaving Out Particular NFL Teams from State CD 1995–2006. Each plot contains the coefficients, represented by markers, and 95% confidence intervals, represented by lines, from seven Poisson regressions following (2.1). Each panel label corresponds to the coefficient in the sample's respective table. Labels on the vertical axis indicate the team that was excluded from the regression sample.

Figure 2.3 repeats the exercise with state-based emotional cues, county-level weather, and SportsDatabase.com's (2021) spreads from the 1995–2006 seasons (State 1995–2006 sample).

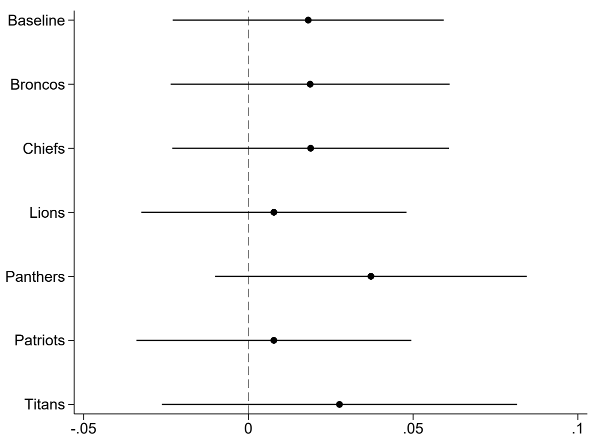
With the regional varying weather variables and alternate gambling spreads, we see that leaving out the Panthers has less of an effect on the variables of interest. Specifically, in panel (a), we see that leaving out the Panthers still introduces imprecision but does not bias the estimate. Further, in panel (b) and (c), we see that it does not appear that the imprecision is different but there is bias in the estimates. These effects are the same as we see from Figure 2.2, but there is no longer a bias in the estimate for the effect of upset losses when the Panthers are left out of the sample. Further, Figure 2.3 shows that leaving any team out of the State 1995–2006 sample does not qualitatively affect the results of the baseline model after we allow for regional variation in weather variables and use the longer time series of gambling spreads.

I repeat the leave-one-team-out exercise for the sample with DMA-based emotional cues, county-level weather, and spreads from SportsDatabase.com (2021) covering the 2004–2019 seasons (DMA 2004–2019 sample) and present the results in Figure 2.4. Across all three panels, we notice that there does not appear to be any affect of leaving a particular team out on precision, which is not surprising due to the statistical power in this sample. Leaving out certain teams seems to introduce bias in the estimates, which we can interpret as the effect driven by the respective team's fans since we have DMA-level emotion cues. Leaving out the Cowboys and Steelers appears to bias the effect of an upset loss upward, indicating that Cowboys and Steelers fans are not as bothered by upset losses. Conversely, leaving out the Patriots appears to introduce negative bias to the effect of upset losses, indicating that Patriots fans are the most bothered by upset losses. The estimates for upset wins appear to have a noticeable negative bias when the Browns and Panthers are left out of the sample, which indicates that Browns and Panthers fans have the most emotional response to an upset win. These effects are based on changes in the magnitude of the coefficents, however, leaving out any particular team does not affect statistical significance of upset losses, close losses, or upset wins.

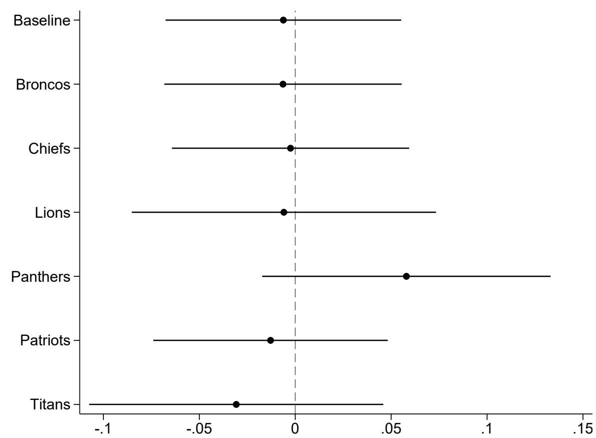
I repeat the leave out exercises for seasons. I present those results in Figure 2.5, Figure 2.6, and Figure 2.7 for the State CD 1995–2006, State 1995–2006, and DMA 2004–2019 samples, respectively. These figures show that the compositions of the seasons in the sample do not



(a) Loss × predicted win (*upset loss*).



(b) Loss × predicted close (*close loss*).

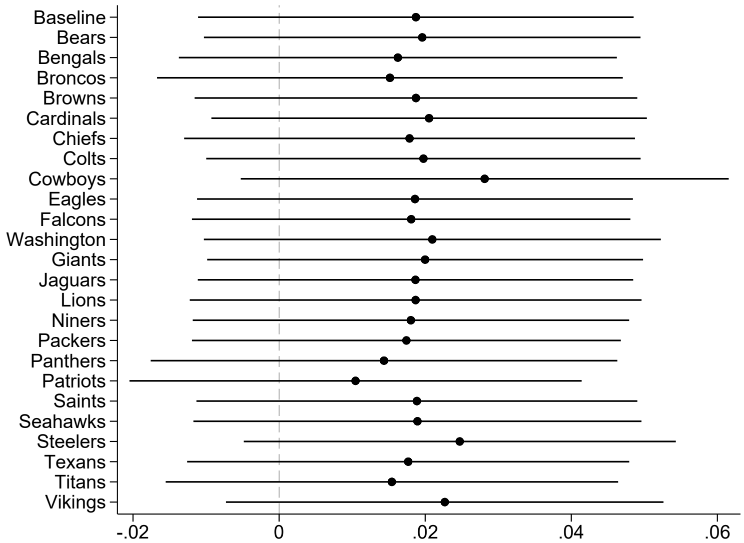


(c) Win × predicted loss (*upset win*).

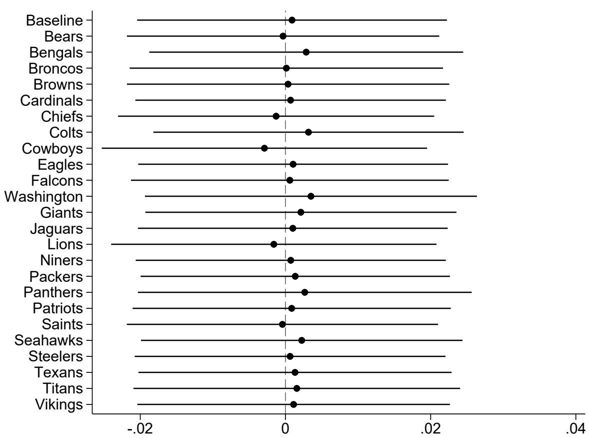
Figure 2.3. Coefficient Comparison Plot of Leaving Out Particular NFL Teams from State 1995–2006. Each plot contains the coefficients, represented by markers, and 95% confidence intervals, represented by lines, from seven Poisson regressions following (2.1). Each panel label corresponds to the coefficient in the sample's respective table. Labels on the vertical axis indicate the team that was excluded from the regression sample.

seem to have any affect on the estimated effect of upset losses, close losses, or upset wins on intimate partner violence.

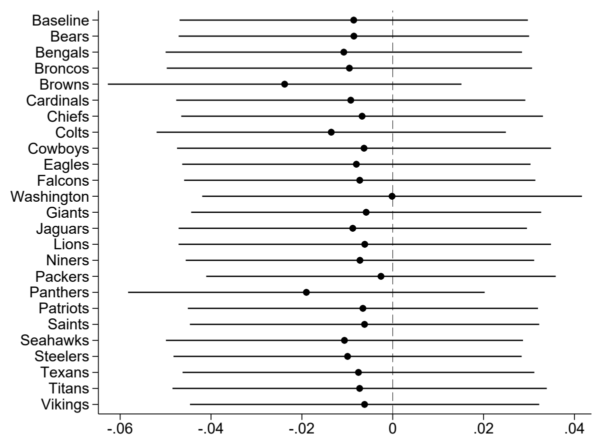
No particular team or season impacts the results once we account for regional weather variation and use the longer time series of gambling spreads. Using seasons outside of CD's



(a) Loss × predicted win (*upset loss*).



(b) Loss × predicted close (*close loss*).



(c) Win × predicted loss (*upset win*).

Figure 2.4. Coefficient Comparison Plot of Leaving Out Particular NFL Teams from DMA 2004–2019. Each plot contains the coefficients, represented by markers, and 95% confidence intervals, represented by lines, from 25 Poisson regressions following (2.1). Each panel label corresponds to the coefficient in the sample's respective table. Labels on the vertical axis indicate the team that was excluded from the regression sample.

study period, we see the same effects of upset losses on intimate partner violence if we use the sample covering every state available to the samples covering the 8 states in CD or the 33 states not covered in CD. DMA-based emotional cues better align the shocks to fans. However, as we see when we compare the results from State 2007–2019 and DMA 2007–2019, changing to DMA-based emotional cues from state-based emotional cues does not affect the results. The likely difference is the change in study period. A major difference in the two study periods is the rate of intimate partner violence against women. Estimated rates of intimate partner violence against women decreased steadily from 15.5 per 1000 women over age 12 in 1995 to a low of 4.9 per 100 women over age 12 in 2005—the period that aligns with CD. Since 2005, the rate of intimate partner violence against women remained relatively flat, reaching 5.4 per 1000 women over age 12 in 2015 (Office for Victims of Crime 2018). That is, in the later study period, there is a level decrease in intimate partner violence when compared to the level of intimate partner violence seen in the earlier study period. Further, it is likely the aggregate difference in taste over many years is the difference in the effect of upset losses, as we show in the leave-out-a-season exercise that no particular season by itself significantly impacts results in any of the samples.

2.4 Conclusion

Card and Dahl (2011) is a seminal paper in economics that combines a general theoretical model with a complex empirical exercise. Their baseline results are robust to changes in sample assembly as long as their original empirical design is maintained. Their results, however, were not robust changing the study period. It is possible that the reduced effect of upset losses on intimate partner violence in a residence is from a reduction in the preference for domestic violence in the new study period that changed the marginal potential offender. This paper shows the need to continue to examine important effects when tastes change, data becomes available, or additional statistical power can be utilized to expand the researcher's understanding of the effects.

2.A Baseline Result Tables

Table 2.2. State-based Emotional Cues from NFL Games and Male-on-Female Intimate Partner Violence in a Residence, 1995–2006

	(1)	(2)	(3)
(a) Loss × predicted win (*upset loss*)	0.136***	0.098***	0.096***
	(0.038)	(0.035)	(0.034)
Loss × predicted close (*close loss*)	0.032	0.027	0.027
	(0.028)	(0.022)	(0.022)
(b) Win × predicted loss (*upset win*)	−0.025	0.002	0.013
	(0.047)	(0.034)	(0.034)
Predicted win	−0.018	−0.020	−0.017
	(0.029)	(0.025)	(0.026)
Predicted close	−0.029	−0.025	−0.025
	(0.026)	(0.028)	(0.027)
Predicted loss	−0.035	−0.030	−0.038
	(0.046)	(0.028)	(0.029)
Agency fixed effects	✓	✓	✓
Season, week of season, and holiday FE		✓	✓
Weather variables			✓
Loss aversion: H_0: (a) = − (b)	0.007	0.042	0.070
Number of agencies	1,371	1,371	1,371
Observations	198,547	198,547	198,547

Notes: Each column is a separate Poisson regression that corresponds to the same column number in Card and Dahl's (2011) Table IV. Sample uses NIBRS crime data and Card and Dahl's (2011) independent variables with state-based emotional cues from 1995–2006. Predicted win indicates a point spread ≤ -4. Predicted loss indicates a point spread $\geq +4$. Predicted close indicates a point spread where $-4 <$ spread < 4. Agencies are city or county police agencies that report to NIBRS. Observations are at the agency-day level, where a day is noon to 11:59 PM Eastern time. Standard errors in parentheses, clustered by team × season (62 groups).

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 2.3. State-based Emotional Cues from NFL Games and Male-on-Female Intimate Partner Violence in a Residence, 1995–2006

	(1)	(2)	(3)
(a) Loss × predicted win (*upset loss*)	0.132***	0.099***	0.102***
	(0.037)	(0.034)	(0.034)
Loss × predicted close (*close loss*)	0.028	0.018	0.018
	(0.028)	(0.021)	(0.021)
(b) Win × predicted loss (*upset win*)	−0.033	−0.009	−0.006
	(0.045)	(0.032)	(0.031)
Predicted win	−0.014	−0.017	−0.015
	(0.030)	(0.026)	(0.026)
Predicted close	−0.030	−0.026	−0.024
	(0.025)	(0.028)	(0.026)
Predicted loss	−0.032	−0.023	−0.025
	(0.045)	(0.028)	(0.027)
Agency fixed effects	✓	✓	✓
Season, week of season, and holiday FE		✓	✓
Weather variables			✓
Loss aversion: H_0: (a) = − (b)	0.003	0.014	0.014
Number of agencies	1,372	1,372	1,372
Observations	198,977	198,977	198,977

Notes: Each column is a separate Poisson regression that corresponds to the same column number in Card and Dahl's (2011) Table IV. Sample uses NIBRS crime data, county-level weather data, Sports-Database.com's (2021) point spreads using state-based emotional cues from 1995–2006. Predicted win indicates a point spread ≤ -4. Predicted loss indicates a point spread $\geq +4$. Predicted close indicates a point spread where $-4 <$ spread < 4. Agencies are city or county police agencies that report to NIBRS. Observations are at the agency-day level, where a day is noon to 11:59 PM Eastern time. Standard errors in parentheses, clustered by team × season (62 groups).

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.4. State-based Emotional Cues from NFL Games and Male-on-Female Intimate Partner Violence in a Residence, 1995–2019

	(1)	(2)	(3)
(a) Loss × predicted win (*upset loss*)	0.096***	0.072***	0.070***
	(0.024)	(0.021)	(0.021)
Loss × predicted close (*close loss*)	0.014	0.019	0.019
	(0.015)	(0.014)	(0.014)
(b) Win × predicted loss (*upset win*)	−0.003	−0.002	−0.003
	(0.028)	(0.021)	(0.021)
Predicted win	0.004	−0.007	−0.004
	(0.018)	(0.015)	(0.015)
Predicted close	−0.005	−0.020	−0.019
	(0.015)	(0.015)	(0.014)
Predicted loss	−0.007	−0.007	−0.005
	(0.023)	(0.016)	(0.016)
Agency fixed effects	✓	✓	✓
Season, week of season, and holiday FE		✓	✓
Weather variables			✓
Loss aversion: H_0: (a) = − (b)	0.006	0.010	0.012
Number of agencies	1,799	1,799	1,799
Observations	599,821	599,821	599,821

Notes: Each column is a separate Poisson regression that corresponds to the same column number in Card and Dahl's (2011) Table IV. Sample uses NIBRS crime data, county-level weather data, Sports-Database.com's (2021) point spreads using state-based emotional cues from 1995–2019. Predicted win indicates a point spread ≤ -4. Predicted loss indicates a point spread $\geq +4$. Predicted close indicates a point spread where $-4 <$ spread < 4. Agencies are city or county police agencies that report to NIBRS. Observations are at the agency-day level, where a day is noon to 11:59 PM Eastern time. Standard errors in parentheses, clustered by team × season.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.5. State-based Emotional Cues from NFL Games and Male-on-Female Intimate Partner Violence in a Residence, 2007–2019

	(1)	(2)	(3)
(a) Loss $\times$ predicted win (*upset loss*)	0.040	0.037	0.034
	(0.031)	(0.028)	(0.028)
Loss $\times$ predicted close (*close loss*)	−0.000	0.009	0.009
	(0.016)	(0.016)	(0.016)
(b) Win $\times$ predicted loss (*upset win*)	0.021	0.026	0.018
	(0.030)	(0.026)	(0.027)
Predicted win	0.006	−0.003	0.001
	(0.020)	(0.019)	(0.019)
Predicted close	0.011	−0.008	−0.005
	(0.018)	(0.017)	(0.017)
Predicted loss	−0.003	−0.009	−0.004
	(0.023)	(0.020)	(0.020)
Agency fixed effects	✓	✓	✓
Season, week of season, and holiday FE		✓	✓
Weather variables			✓
Loss aversion: H_0: (a) = − (b)	0.652	0.767	0.676
Number of agencies	1,625	1,625	1,625
Observations	335,240	335,240	335,240

Notes: Each column is a separate Poisson regression that corresponds to the same column number in Card and Dahl's (2011) Table IV. Sample uses NIBRS crime data, county-level weather data, Sports-Database.com's (2021) point spreads using state-based emotional cues from 2007–2019. Predicted win indicates a point spread ≤ -4. Predicted loss indicates a point spread $\geq +4$. Predicted close indicates a point spread where $-4 <$ spread < 4. Agencies are city or county police agencies that report to NIBRS. Observations are at the agency-day level, where a day is noon to 11:59 PM Eastern time. Standard errors in parentheses, clustered by team $\times$ season.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.6. DMA-based Emotional Cues from NFL Games and Male-on-Female Intimate Partner Violence in a Residence, 2007–2019.

	(1)	(2)	(3)
(a) Loss × predicted win (*upset loss*)	0.032*	0.025	0.025
	(0.017)	(0.016)	(0.016)
Loss × predicted close (*close loss*)	−0.006	−0.001	−0.002
	(0.011)	(0.011)	(0.011)
(b) Win × predicted loss (*upset win*)	0.010	0.008	0.005
	(0.019)	(0.018)	(0.018)
Predicted win	0.008	0.003	0.003
	(0.013)	(0.013)	(0.013)
Predicted close	0.019	0.007	0.008
	(0.012)	(0.012)	(0.012)
Predicted loss	0.008	0.002	0.005
	(0.012)	(0.013)	(0.013)
Agency fixed effects	✓	✓	✓
Season, week of season, and holiday FE		✓	✓
Weather variables			✓
Loss aversion: H_0: (a) = − (b)	0.402	0.491	0.411
Number of agencies	4,564	4,564	4,564
Observations	785,335	785,335	785,335

Notes: Each column is a separate Poisson regression that corresponds to the same column number in Card and Dahl's (2011) Table IV. Sample uses NIBRS crime data, county-level weather data, Sports-Database.com's (2021) point spreads using DMA-based emotional cues from 2007–2019. Predicted win indicates a point spread ≤ -4. Predicted loss indicates a point spread $\geq +4$. Predicted close indicates a point spread where $-4 <$ spread < 4. Agencies are city or county police agencies that report to NIBRS. Observations are at the agency-day level, where a day is noon to 11:59 PM Eastern time. Standard errors in parentheses, clustered by designated market area.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.7. DMA-based Emotional Cues from NFL Games and Male-on-Female Intimate Partner Violence in a Residence, States in Card and Dahl (2011), 2007–2019.

	(1)	(2)	(3)
(a) Loss × predicted win (*upset loss*)	0.037	0.030	0.028
	(0.028)	(0.025)	(0.024)
Loss × predicted close (*close loss*)	0.003	0.017	0.017
	(0.013)	(0.016)	(0.015)
(b) Win × predicted loss (*upset win*)	0.009	0.020	0.013
	(0.029)	(0.028)	(0.027)
Predicted win	0.009	0.003	0.005
	(0.020)	(0.020)	(0.021)
Predicted close	0.011	−0.007	−0.003
	(0.022)	(0.022)	(0.023)
Predicted loss	0.015	0.007	0.013
	(0.017)	(0.020)	(0.021)
Agency fixed effects	✓	✓	✓
Season, week of season, and holiday FE		✓	✓
Weather variables			✓
Loss aversion: H_0: (a) = − (b)	0.507	0.804	0.689
Number of agencies	1,625	1,625	1,625
Observations	335,240	335,240	335,240

Notes: Each column is a separate Poisson regression that corresponds to the same column number in Card and Dahl's (2011) Table IV. Sample uses NIBRS crime data, county-level weather data, Sports-Database.com's (2021) point spreads using DMA-based emotional cues from 2007–2019. Sample is restricted to reporting agencies located in states used in Card and Dahl (2011)—Colorado, Kansas, Massachusetts, Michigan, New Hampshire, South Carolina, Tennessee, and Vermont. Predicted win indicates a point spread ≤ -4. Predicted loss indicates a point spread $\geq +4$. Predicted close indicates a point spread where $-4 <$ spread < 4. Agencies are city or county police agencies that report to NIBRS. Observations are at the agency-day level, where a day is noon to 11:59 PM Eastern time. Standard errors in parentheses, clustered by designated market area.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.8. DMA-based Emotional Cues from NFL Games and Male-on-Female Intimate Partner Violence in a Residence, States Excluded from Card and Dahl (2011), 2007–2019.

	(1)	(2)	(3)
(a) Loss × predicted win (*upset loss*)	0.029	0.024	0.026
	(0.021)	(0.020)	(0.020)
Loss × predicted close (*close loss*)	−0.014	−0.014	−0.015
	(0.015)	(0.015)	(0.015)
(b) Win × predicted loss (*upset win*)	0.011	0.009	0.010
	(0.025)	(0.025)	(0.024)
Predicted win	0.007	0.004	0.003
	(0.018)	(0.017)	(0.017)
Predicted close	0.026*	0.019	0.019
	(0.013)	(0.014)	(0.014)
Predicted loss	0.002	−0.003	−0.003
	(0.018)	(0.017)	(0.017)
Agency fixed effects	✓	✓	✓
Season, week of season, and holiday FE		✓	✓
Weather variables			✓
Loss aversion: H_0: (a) = − (b)	0.578	0.630	0.609
Number of agencies	2,939	2,939	2,939
Observations	450,095	450,095	450,095

Notes: Each column is a separate Poisson regression that corresponds to the same column number in Card and Dahl's (2011) Table IV. Sample uses NIBRS crime data, county-level weather data, Sports-Database.com's (2021) point spreads using DMA-based emotional cues from 2007–2019. Sample is restricted to reporting agencies located in states not used in Card and Dahl (2011)—Alabama, Arkansas, Connecticut, Georgia, Idaho, Illinois, Indiana, Iowa, Kentucky, Louisiana, Maine, Maryland, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Dakota, Texas, Utah, Virginia, Washington, West Virginia, Wisconsin, and Wyoming. Predicted win indicates a point spread ≤ -4. Predicted loss indicates a point spread $\geq +4$. Predicted close indicates a point spread where $-4 <$ spread < 4. Agencies are city or county police agencies that report to NIBRS. Observations are at the agency-day level, where a day is noon to 11:59 PM Eastern time. Standard errors in parentheses, clustered by designated market area.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

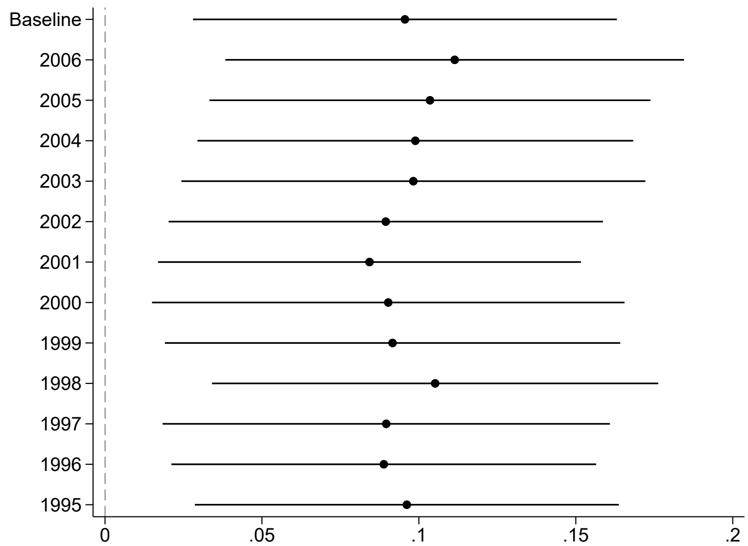
Table 2.9. DMA-based Emotional Cues from NFL Games and Male-on-Female Intimate Partner Violence in a Residence, 2004–2019.

	(1)	(2)	(3)
(a) Loss × predicted win (*upset loss*)	0.038**	0.019	0.019
	(0.016)	(0.015)	(0.015)
Loss × predicted close (*close loss*)	−0.003	0.001	0.001
	(0.010)	(0.010)	(0.010)
(b) Win × predicted loss (*upset win*)	−0.004	−0.006	−0.009
	(0.018)	(0.017)	(0.017)
Predicted win	0.006	0.004	0.004
	(0.011)	(0.012)	(0.011)
Predicted close	0.016	0.004	0.006
	(0.011)	(0.011)	(0.011)
Predicted loss	0.015	0.010	0.013
	(0.012)	(0.012)	(0.012)
Agency fixed effects	✓	✓	✓
Season, week of season, and holiday FE		✓	✓
Weather variables			✓
Loss aversion: H_0: (a) = − (b)	0.108	0.292	0.248
Number of agencies	4,719	4,719	4,719
Observations	953,536	953,536	953,536

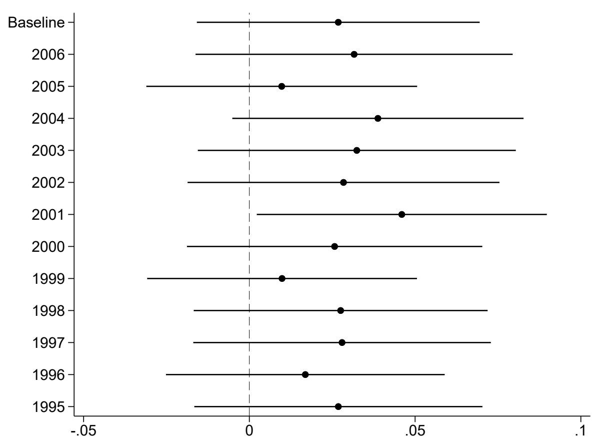
Notes: Each column is a separate Poisson regression that corresponds to the same column number in Card and Dahl's (2011) Table IV. Sample uses NIBRS crime data, county-level weather data, Sports-Database.com's (2021) point spreads using DMA-based emotional cues from 2004–2019. Predicted win indicates a point spread ≤ -4. Predicted loss indicates a point spread $\geq +4$. Predicted close indicates a point spread where $-4 <$ spread < 4. Agencies are city or county police agencies that report to NIBRS. Observations are at the agency-day level, where a day is noon to 11:59 PM Eastern time. Standard errors in parentheses, clustered by designated market area (162 groups).

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

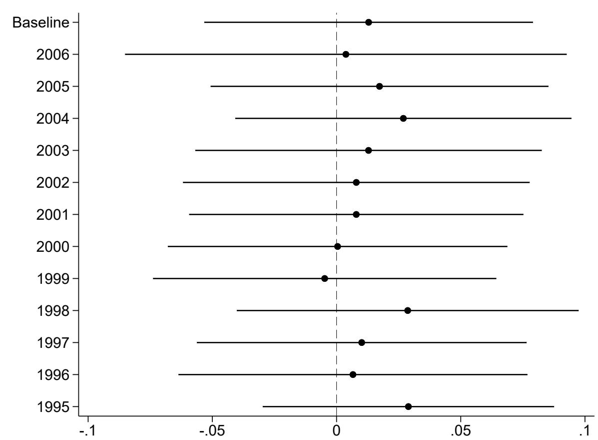
2.B Figures



(a) Loss × predicted win (*upset loss*).

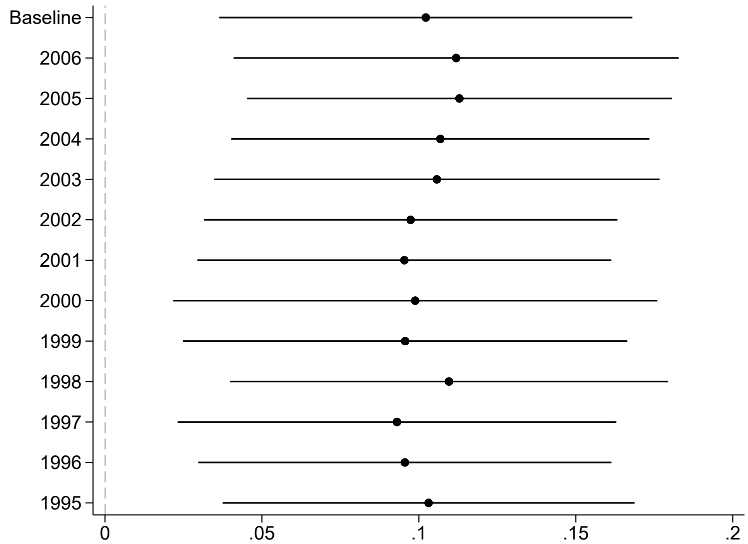


(b) Loss × predicted close (*close loss*).

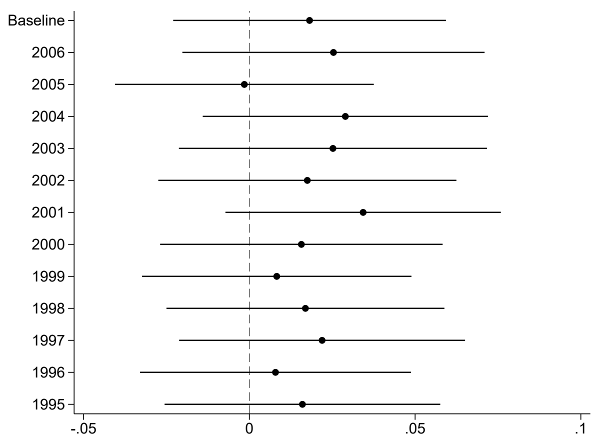


(c) Win × predicted loss (*upset win*).

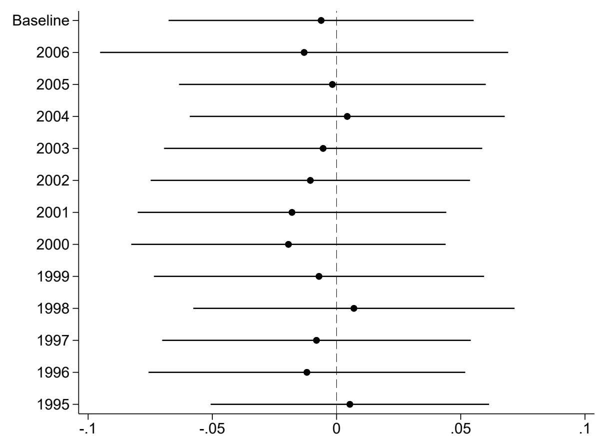
Figure 2.5. Coefficient Comparison Plot of Leaving Out Particular NFL Seasons from State CD 1995–2006. Each plot contains the coefficients, represented by markers, and 95% confidence intervals, represented by lines, from 13 Poisson regressions following (2.1). Each panel label corresponds to the coefficient in the sample's respective table. Labels on the vertical axis indicate the season that was excluded from the regression sample.



(a) Loss × predicted win (*upset loss*).

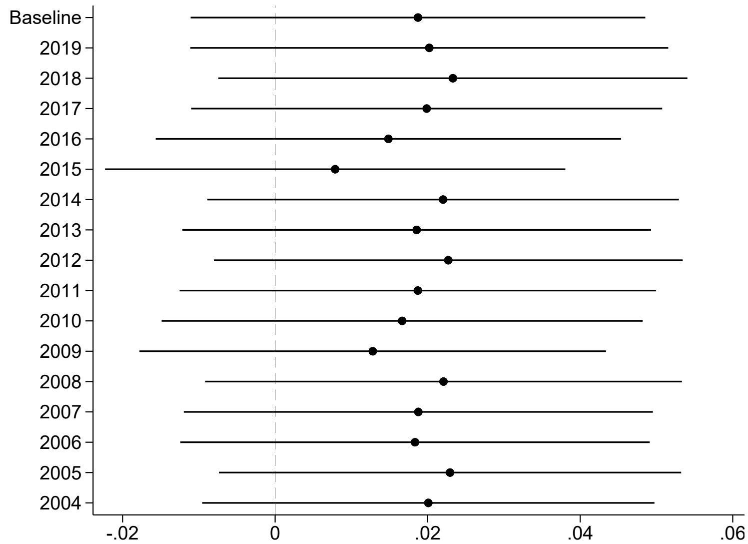


(b) Loss × predicted close (*close loss*).

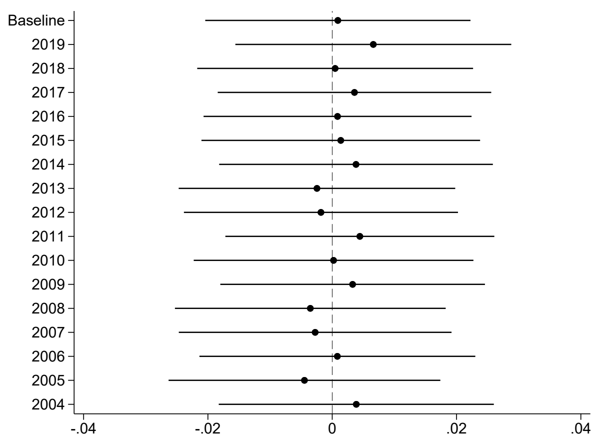


(c) Win × predicted loss (*upset win*).

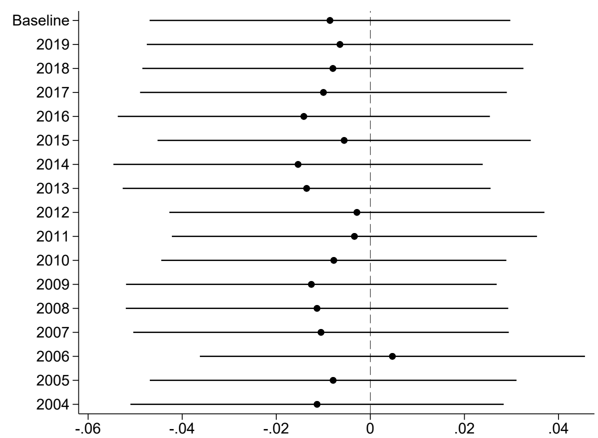
Figure 2.6. Coefficient Comparison Plot of Leaving Out Particular NFL Seasons from State 1995–2006. Each plot contains the coefficients, represented by markers, and 95% confidence intervals, represented by lines, from 13 Poisson regressions following (2.1). Each panel label corresponds to the coefficient in the sample's respective table. Labels on the vertical axis indicate the season that was excluded from the regression sample.



(a) Loss × predicted win (*upset loss*).



(b) Loss × predicted close (*close loss*).



(c) Win × predicted loss (*upset win*).

Figure 2.7. Coefficient Comparison Plot of Leaving Out Particular NFL Seasons from DMA 2004–2019. Each plot contains the coefficients, represented by markers, and 95% confidence intervals, represented by lines, from 17 Poisson regressions following (2.1). Each panel label corresponds to the coefficient in the sample's respective table. Labels on the vertical axis indicate the season that was excluded from the regression sample.

3. LIFELINE'S FATALITIES: THE EFFECT OF CELL PHONES ON TRAFFIC FATALITIES

3.1 Introduction

Distraction-affected crashes increased 9.9-percent in 2019 from 2018. Distraction-affected crashes resulting in fatalities is one of the few categories of fatalities that has increased in recent years (National Center for Statistics and Analysis 2020). This increase in fatalities have made distracted drivers a key area of interest for policymakers and researchers. Policymakers are especially concerned with cell phone-related distractions while driving (National Highway Traffic Safety Administration 2016).

Since their invention, phones have enabled lower-cost communication that has reduced frictions in the job market and has provided easier access to essential services. These benefits were contrasted with few costs in the wireline-phone era since phones were tethered to a location. Now with the prevalence of wireless phones, society is confronted with the externalities associated with the constant access to communication devices.

Wireless phones introduce numerous externalities. There are numerous benefits to lowering the cost of communication. Users can contact emergency services immediately when they are needed. Patients can maintain more direct lines of communication with health care providers. Potential workers can respond to calls from potential employers more rapidly. Potential drunk drivers have fewer frictions in finding a designated driver.

Wireless phones also impose costs on society. Wireless phones allow for conversations to happen anywhere, even where it may be frowned upon, such as a library or while driving a car. Wireless phones also allow communication through text messages. Internet-enabled wireless phones also allow for access to applications that are often designed to engage the user. Having a verbal conversation, writing text messages, or engaging with an application can often be a distraction if the user is attempting to multitask. These distractions can have significant costs if the user is engaged in a potentially dangerous activity such as driving. Bhargava and Pathania (2013) investigate the potential link of increased call rate and crash rates but find no evidence that relative crash rate has changed.

These distractions have attracted the attention of policymakers, who have created laws to prohibit texting while driving and to require hands-free wireless phone use while driving. As of 2017, texting while driving is such a concern that more states have passed laws prohibiting texting while driving than there are states that require the use of seat belts.[1] Through 2018, only 16 require that wireless phone use in a car must be hands free.[2]

In addition to policymakers, researches have become interested in the potential for wireless phone-related distraction on traffic fatalities. Researchers have generally focused on two identification strategies to examine the effect of cell phones. First, researchers have focused on the implementation of laws prohibiting texting while driving and laws requiring the hands-free use of cellphones (e.g., Cheng (2015) and Rocco and Sampaio (2016)). Second, researchers have used the expansion of cell phone towers (e.g., Edlund and Machado (2019) and Hersh, Lang, and Lang (2019)).

In this paper, I use the expansion of Lifeline to wireless phones as a shock to the stock of wireless phones in a market. This shock coincides with a clear-cut policy implementation, setting up the conventional difference-in-differences framework. Further, I use the existence of laws restricting the use of cellphones while driving to infer the likely behavior exhibited by drivers as the shock arrives. I find that the expansion of Lifeline to pre-paid phones causally increases the number of traffic fatalities in states that have no restrictions on cellphone use while driving and states that prohibit texting and require hands-free calling. The expansion of Lifeline in states that prohibit either texting or require hands-free use has no effect on traffic fatalities.

3.2 Background

For most of their early history, cellular telephones were luxury items. Few could afford them at their introduction. As time has progressed, device functionality increased,

1. ↑As of 2017, 42 states prohibit texting while driving and 35 states require seat belts while driving. See Table 3.7 for a summary of primary enforcement texting while driving bans and Table 3.9 for a summary of primary enforcement seat belt laws.
2. ↑See Table 3.8 for a summary of states with primary enforcement hands-free wireless phone use while driving laws.

costs decreased, and market penetration grew. The cost, however, was still high enough to potentially keep low-income households out of the market.

Beginning in 2008, approved carriers began to offer cellular telephones subsidized by the Lifeline Assistance Program (Lifeline). These carriers offered free handsets that were pre-loaded with a set amount of minutes. The cost of each month's pre-loaded minutes were covered by the Lifeline subsidy payment. Eligible low-income households could now obtain a cell phone with a limited amount of minutes with no out-of-pocket costs. For the remainder of this paper, I refer to this change as the pre-paid Lifeline program.

As far back as 1934, Congress has codified the desire for all Americans to have affordable access to wire or radio communications.[3] In the shadow of AT&T's 1984 divestiture, Congress was concerned that local market changes could result in rate increases that could result in low-income households disconnecting from voice service. Congress was specifically concerned with subscriber line charges (SLC). Thus, in 1985, Congress established Lifeline to reimburse service providers for waiving the SLC for low-income households.

In order to further support universal service, The Telecommunications Act of 1996 established the Universal Service Fund (USF). The USF is funded through monthly services fees added to every telephone bill. The Universal Service Administrative Company (USAC), created in 1996 and regulated by the Federal Communications Commission (FCC), administers the USF through four programs: High Cost Program, Low Income Program, School and Libraries Program, and Rural Healthcare Program. Lifeline is a part of the Low Income Program. In order for a carrier to be an eligible telecommunications carrier (ETC) and receive support from the USF, they must be approved by the FCC. This approval process includes many facets but of particular interest is the facilities requirement. In practice, this requires that an ETC must provide at least one supported service through its own facilities. Additionally, the FCC ruled that a carrier could not receive universal service support for a product that was provided entirely through resale of wholesale service. This limited the market to major carriers that maintained telecommunications infrastructure (e.g., AT&T, Verizon, and CenturyLink). In 2005, Tracfone obtained conditional approval to waive the

3. ↑See Communications Act of 1934.

facilities requirement and participate in the federal Lifeline program.[4] Beginning in 2008, Tracfone, through its subsidiary SafeLink Wireless, entered Lifeline markets by offering free handsets with a monthly subscription for a block pre-paid minutes at a price point equal to the Lifeline monthly subsidy. This market innovation led to the rapid expansion of the Lifeline program. From 2005-2008, Lifeline expenditures were around $800 million annually. Expenditures rose rapidly after wireless Lifeline was introduced: 1 billion in 2009, 1.3 billion in 2010, nearly 1.8 billion in 2011, and peaking at nearly 2.2 billion in 2012. FCC adopted reforms in 2012 to reduce costs. In the three years following this reform, the FCC saved an estimated $2 billion with claims decreasing to 1.5 billion in 2015.

The existing literature on Lifeline is primary focused on the program's effectiveness at establishing its stated goal: providing telephone access. Most of this analysis is limited to landline phones (e.g., Garbacz and Thompson (1997) among others). There are two papers that discuss the effectiveness of wireless Lifeline program. First, Ukhaneva (2015) estimates that only 1 in 20 households is a marginal subscriber in the first two years that the wireless Lifeline program was available. Second, Conkling (2018) shows that most phones distributed under the wireless Lifeline program were supplied to customers that already had a wireless phone and used the pre-paid phone as a source of additional voice minutes.

3.3 Data

This paper utilizes a panel data set combined from several sources spanning January 2003 to December 2015. First, administrative data from the Universal Service Administrative Company (2020) (USAC) establishes when pre-paid Lifeline phones were made available in a state market. Every quarter USAC files reports on all reimbursement requests related to the USF to the FCC. Each quarterly report contains each monthly reimbursement request per Study Area Code (SAC). SAC is a unique number assigned to an ETC for each service area in which it operates. An ETC has a different SAC for each state where it operates and may have multiple SACs per state. Additionally, USAC maintains records on the type of service provided. To identify pre-paid Lifeline service providers, I manually matched known pre-paid

4. ↑Conditions included methods for approving eligible users and 911 technology requirements.

Lifeline providers to their respective Service Provider Identification Number (SPIN). SPIN is a unique number that identifies an ETC to USAC. Most ETCs operate under one national-level SPIN. Table 3.6 summarizes the lists the dates in which pre-paid Lifeline phones were made widely available in each state.

I use traffic fatality data from the National Highway Traffic Safety Administration's (2019) Fatality Analysis Reporting System (FARS). FARS provides detailed information on every traffic fatality that occurs in the United States. I define a traffic fatality as alcohol-related if there is a driver involved with a blood alcohol concentration (BAC) greater than or equal to 0.02. At a BAC of 0.02, the ability to track moving targets declines and the ability to perform two tasks at once declines (National Highway Traffic Safety Administration 2016). Therefore at a BAC of 0.02%, drivers are more easily distracted. I combine the traffic fatalities with vehicles miles traveled (VMT) from the Office of Highway Policy Information (2021) to find the rate of fatalities per 100 million VMT. I use Kittleson & Associates, Inc.'s (2021) Roundabouts Database to count the number of roundabouts that exist per year in each state.

I gather annual state poverty rate, annual state median household income, and monthly state Supplemental Nutrition Assistance Program (SNAP) recipients from the United States Census Bureau's (2021) Small Area Income and Poverty Estimates (SAIPE) program. I use the police enforcement employee data from the Uniform Crime Reporting System (2020) to calculate the number of full-time officers per 1,000 people in a state. I get monthly state unemployment from the United States Bureau of Labor Statistics (2021). I take monthly precipitation data from the National Oceanic and Atmospheric Administration's Monthly U.S. Climate Divisional Database (NClimDiv) (Vose et al. 2014).

Lastly, I combine policy information on states with primary enforcement seat belt laws (Harper and Strumpf 2017), primary enforcement text while driving bans, primary enforcement hands-free phone use while driving laws (McCartt, Kidd, and Teoh 2014), and Medicaid expansion under the Affordable Care Act.[5] Table 3.1 shows the summary statistics for the sample from 2003–2015. Observations in the sample are state-months. The sample is a bal-

5. ↑Primary enforcement laws allow law enforcement to enforce the law without any other concurrent infraction. Table 3.10 shows the effective dates of Medicaid expansion under the Affordable Care Act.

anced panel from 49 states covering 156 months.[6] Conditional on the program being active, pre-paid Lifeline provides an average of 184,000 wireless phones per state-month.

Table 3.1. Summary Statistics, 2003–2015

	mean	sd	min	max
Traffic fatalities	63.092	64.573	0	462
Alcohol-related traffic fatalities	16.132	17.132	0	137
Vehicle miles traveled (100 millions)	50.408	50.984	2.94	328
Traffic fatalities per 100 million VMT	1.308	0.512	0	8.5
Alcohol-related traffic fatalities per 100 million VMT	0.354	0.222	0	3.83
Roundabouts	45.675	60.868	0	316
Officers per 1000	2.287	0.553	1.45	5.85
Precipitation (inches)	3.172	2.057	.01	15.9
Poverty rate	13.780	3.231	6.4	23.9
Median household income (thousands)	49.612	8.607	32.4	75.8
Unemployment rate	6.181	2.126	2	15.4
SNAP recipients (millions)	0.720	0.787	.0102	4.44
Lifeline with no bans	0.132	0.339	0	1
Lifeline with one ban	0.187	0.390	0	1
Lifeline with both bans	0.074	0.262	0	1
One ban (text ban or hands-free law)	0.241	0.428	0	1
Both bans (text ban and hands-free law)	0.092	0.289	0	1
Seat belt law	0.541	0.498	0	1
ACA Medicaid expansion	0.081	0.272	0	1
Observations	7644			

Notes: Alcohol-related traffic fatalities involve at least one driver with a blood alcohol concentration greater than or equal to 0.02. Roundabouts, officers per 1000, poverty rate, and median household income all vary by year. All other variables vary by month. Observations are state-months. Conditional on the program being active, pre-paid Lifeline provides an average of 184,000 wireless phones per state-month.

My baseline model that follows employs indicator policy variables, but we can estimate the following model with continuous variables for the estimated number of pre-paid cell phones reimbursed through Lifeline while under the various cell phone use while driving restrictions by state. All other variables remain the same. Lifeline's reimbursement per phone was steady throughout the study period at $9.25 per line claimed. The estimated number of cell phones reimbursed, however, is complicated by a few additional factors. First, each

6. ↑Washington D.C. and Hawaii are not in the estimation sample.

SPIN can submit their reimbursement requests quarterly by SAC.[7] Second, USAC estimated monthly reimbursement payments, paid the estimated amount, and then reconciled claims after the SAC's claims forms were submitted. After the 2012 Lifeline reforms, USAC made payments after the reimbursement claim forms were formally submitted. Lastly, USAC periodically audits SPINs by SAC for claims accuracy. SPINs with large annual claims are audited every year. In the event that any SAC has been overcompensated, USAC deducts the overpayment from the month's payment in which the SAC's audit is completed. This audit policy has led to monthly claims going negative. These factors lead to a problematic time series with variation that is introduced mechanically by the institutional rules around the claims process. In order to smooth the series, I use a nonparametric local-linear regression on the reimbursement amounts by SAC. After obtaining the smoothed series, I aggregate the reimbursement amounts by state-month. Then I divide the aggregated series by the constant reimbursement amount (i.e., \$9.25) to estimate the number of pre-paid cell phones supplied by Lifeline in a state-month.

3.4 Difference-in-differences

In order to identify the causal effect of Lifeline's expansion to cover pre-paid cell phones, I employ a difference-in-differences estimation strategy. I use the fixed effect model

$$y_{st} = \alpha_s + \psi_t + \sum_{k=0}^{2} \gamma_k Lifeline_{st}^{ban=k} + X'_{st}\beta + \epsilon_{st}, \tag{3.1}$$

where y_{st} is the rate of traffic fatalities per 100 million vehicle miles traveled in state s at time t, α_s are state fixed effects, ψ_t are year-month fixed effects, X'_{st} is a matrix of time-varying state-level control variables, and ϵ_{st} is an idiosyncratic error term. $Lifeline_{st}^{ban=k}$ are the three indicator variables of interest and γ_k are the related coefficients of interest. Each Lifeline variable is mutually exclusive and is matched to the number of laws restricting cell phone use while driving in the state (e.g., $Lifeline_{st}^{ban=1} = 1$ in state s at time t when

7. ↑Through 2018, these reimbursement requests were made by filing FCC Form 497. As a result of the 2016 Lifeline Modernization Order, USAC transitioned reimbursement claims to the online-based Lifeline Claims System in 2018.

wireless Lifeline pre-paid phones are available and the state has a text ban or a hands-free law; these states are called “Lifeline with one ban” in the results tables that follow.) The sample is a balanced panel covering state-months. Therefore, I cluster standard errors by state for inference (Bertrand, Duflo, and Mullainathan 2004). By separating the wireless expansion of Lifeline into three mutually exclusive cases, we obtain casual estimates for the addition of cell phones under various state-level cell phone use while driving restrictions.

Identification in difference-in-differences relies on the assumption that the treated and control groups have outcomes that trend similarly. There is no empirical test for this assumption, but it has become standard to provide plots of the outcome variables or create so-called event studies. Both of these methods require treatment that becomes active and stays active through the rest of the study period. The program indicators in this estimation are not conventional policy variables that become active and stay active during the entire study duration. Two of the three Lifeline indicator variables can become active and then become inactive as laws relating to cell phone use while driving change. Only Lifeline states with two bans active remain active for the duration of the study. States can, and do, move between the three treatment groups as the study period moves forward. In light of these issues, I add state-specific linear time trends or state-specific quadratic time trends as robustness checks.

Another threat to identification in the difference-in-differences design are other policies that may be changing during the study period. Since Lifeline is means-tested program, we are especially concerned with any policy changes that affect low-income households and health outcomes. In order to control for this potential bias, I add an indicator for the expansion of Medicaid under the Affordable Care Act. I add additional controls for continuous variables that may also be correlated with means-tested programs and health outcomes, specifically controls for the annual poverty rate, median household annual income, monthly unemployment rate, and the monthly number of SNAP recipients. Finally, I control for other factors that may affect traffic fatalities: annual number of existing roundabouts, annual number of officers per 1000, monthly precipitation, and indicators for one ban on cell phone use while driving (i.e., a texting ban or a hands-free law), two bans on cell phone use while driving, and a seat belt law.

In this model, we identify casual estimates through the quasi-random state-based expansion of the wireless Lifeline program and the variation in the Lifeline policy variables with respect to state-specific cell phone use restrictions while driving over time. Our model identifies the effects based on the addition and removal of the wireless Lifeline policy state variables in the cases of no bans and one ban. In the case of the Lifeline state with two bans, the effect is identified through the addition of the wireless Lifeline policy. By moving the Lifeline variables from indicators to continuous variables, the model identifies the effect of additional cell phones provided by Lifeline under particular cell phone use restrictions while driving by using the variation in the estimated amount of pre-paid cell phones claimed through the program by state over time.

3.5 Results

I estimate (3.1) on all traffic fatalities per 100 million vehicle miles traveled from 2003–2015 and present the results in Table 3.2.[8] Column (1) shows a baseline model with Lifeline policy variables and controlling for cell phone use restrictions along with state and year-month fixed effects. We see a significant positive increase in traffic fatalities in states that prohibit texting and require hands-free use while driving. Moving to column (2), we add all the state-level controls to the model. The earlier increase from Lifeline states with two bans remains while the increase in traffic fatalities from Lifeline states with no bans becomes statistically significant. As a robustness check, I estimate the model with state-specific linear time trends in column (3) and state-specific quadratic time trends in column (4). The effect of Lifeline states with no bans on traffic fatalities remains robust regardless of the functional form of the state-specific trends added to the model. The effect of Lifeline states with two bans, however, decreases in magnitude by over half and becomes marginally statistically significant under linear and quadratic state-specific time trends.

8. ↑Models are estimated in Stata 16.1 using the command reghdfe (Correia 2017).

Table 3.2. The Effect of Wireless Lifeline Expansion on Traffic Fatalities per 100 Million Vehicle Miles Traveled, 2003–2015

	(1)	(2)	(3)	(4)
Lifeline with no bans	0.059	0.089**	0.073***	0.082***
	(0.039)	(0.035)	(0.025)	(0.026)
Lifeline with one ban	−0.008	0.025	−0.044	−0.029
	(0.063)	(0.049)	(0.042)	(0.046)
Lifeline with two bans	0.133***	0.164***	0.053*	0.057*
	(0.042)	(0.061)	(0.031)	(0.033)
One ban (text ban or hands-free law)	0.102	0.106*	0.092*	0.104**
	(0.071)	(0.061)	(0.048)	(0.050)
Two bans (text ban and hands-free law)	−0.002	0.010	−0.011	0.012
	(0.034)	(0.041)	(0.031)	(0.036)
State and year-month fixed effects	✓	✓	✓	✓
State-level controls		✓	✓	✓
State linear time trends			✓	
State quadratic time trends				✓
R^2	0.573	0.584	0.600	0.597
Observations	7,644	7,644	7,644	7,644

Notes: Each column is a separate regression. The dependent variable is all traffic fatalities per 100 million vehicle miles traveled (VMT). Each variable listed in the table is an indicator. "Lifeline with no bans" indicates a state that has wireless Lifeline and neither a text ban nor a hands-free law. "Lifeline with one ban" indicates a state that has wireless Lifeline with a text ban or a hands-free law. "Lifeline with two bans" indicates a state that has wireless Lifeline with a text ban and a hands-free law. "One ban" indicates a state with a text ban or a hands-free law. "Two bans" indicates a state with a text ban and a hands-free law. State-level controls include indicators (seat belt law and Medicaid expansion under the Affordable Care Act) and continuous variables (poverty rate, median household income, unemployment rate, SNAP recipients, officers per 1000, precipitation, and the number of roundabouts). Observations are state-months. Standard errors in parentheses, clustered by state.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Drivers begin to be impaired at a BAC of 0.02. At a BAC of 0.02, drivers have less a slower reaction time and are less able to manage several tasks at once (National Highway Traffic Safety Administration 2016).[9] With a reduced ability to multitask and a comprised react time, distractions from using a cell phone while driving may have greater consequences for drivers that have consumed alcohol before driving. I restrict the fatalities to fatalities where at least one driver involved had a blood alcohol test that returned a BAC of at least 0.02. I estimate model (3.1) with these alcohol-related traffic fatalities and present the results in Table 3.3. We see that the results largely mirror the results from Table 3.2 in the pattern of statistical significance but have reduced magnitudes. In column (1), we see that Lifeline states with two bans have a statistically significant increase in fatalities. After adding the state-level controls in column (2), the effect from Lifeline states without any bans becomes statistically significant while the effect from Lifeline states with two bans remains so. We see that the effect of Lifeline states without any bans is robust to adding state-specific linear time trends in column (3) and state-specific quadratic time trends in column (4). Lifeline states with two bans, however, are only robust to adding state-specific quadratic time trends. When we add state-specific linear time trends, the effect of Lifeline states with two bans becomes marginally statistically significant.

9. ↑Guidelines on impaired driving suggest that a BAC of 0.02 would result from consuming about two standard-size alcoholic beverages (Centers for Disease Control and Prevention 2020).

Table 3.3. The Effect of Wireless Lifeline Expansion on Alcohol-related Traffic Fatalities per 100 Million Vehicle Miles Traveled, 2003–2015

	(1)	(2)	(3)	(4)
Lifeline with no bans	0.033*	0.041**	0.033**	0.037***
	(0.019)	(0.018)	(0.012)	(0.012)
Lifeline with one ban	0.007	0.019	−0.012	−0.012
	(0.034)	(0.027)	(0.029)	(0.029)
Lifeline with two bans	0.063***	0.070**	0.050*	0.061***
	(0.022)	(0.030)	(0.026)	(0.022)
One ban (text ban or hands-free law)	0.045	0.050	0.040	0.048
	(0.038)	(0.034)	(0.032)	(0.032)
Two bans (text ban and hands-free law)	−0.027	−0.022	−0.019	−0.021
	(0.019)	(0.023)	(0.022)	(0.021)
State and year-month fixed effects	✓	✓	✓	✓
State-level controls		✓	✓	✓
State linear time trends			✓	
State quadratic time trends				✓
R^2	0.377	0.384	0.403	0.400
Observations	7,644	7,644	7,644	7,644

Notes: Each column is a separate regression. The dependent variable is all traffic fatalities per 100 million vehicle miles traveled (VMT) with a blood alcohol concentration greater than or equal to 0.02. See Table 3.2 for a description of variables and terms. Observations are state-months. Standard errors in parentheses, clustered by state.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The percentage increase in the effect from Lifeline states with no bans on alcohol-related fatalities is larger than the percentage increase in the effect from Lifeline states with no bans on all traffic fatalities. This is consistent with drivers beginning to be impaired at a BAC of 0.02 and are more susceptible to being distracted.

In each set of results, we see positive and statistically significant effects in Lifeline states without any restrictions on cell phone use. In this case, it seems likely that cell phone users are eager to consume more cell phone services as they become available and that increased consumption likely leads to increased distractions that cause more accidents, which also creates more opportunities for fatalities.

The effect from Lifeline states that prohibit texting while driving and have a hands-free law is also positive and statistically significant. Cell phone users in these states are likely just as eager to consume more services. However, the restrictions on cell phone use while driving likely force these consumers into riskier behavior to hide their use. This evasion from enforcement likely explains the larger magnitude effects that we see from Lifeline states with two bans opposed to Lifeline states with no bans.

Neither set of regressions have significant effects from Lifeline states that either prohibit texting or require hands-free cell phone use. If we assume that the taste for cell phone services should be consistent across states regardless of use restrictions, it may be possible that cell phone users in states that have one restriction become specialized in using that method. With their increased skill, they're less likely to become distracted by using that method more when more cell phones become available. In the case of states that have no bans or two bans on cell phone use, consumers have no incentive to become as specialized in one method of cell phone use.

As a final exercise, I estimate (3.1) with continuous versions of the Lifeline variables that use the estimated number of pre-paid cell phones claimed through Lifeline (in millions) under the various cell phone use restrictions while driving. I present the results for all traffic fatalities per VMT in Table 3.4. In columns (1) and (2), we see that Lifeline phones in states without any restrictions reduce traffic fatalities. In columns (3) and (4), however, we see that these results are not robust to adding state-specific trends. There is no evidence that additional cell phones provided by Lifeline reduce traffic fatalities under any cell phone use restrictions. I show the results for alcohol-related traffic fatalities per VMT in Table 3.5. Pre-paid Lifeline cell phones have no effect on alcohol-related traffic fatalities that are robust across specifications. Further, the magnitudes of all the coefficients for the effect of Lifeline pre-paid cell phones on alcohol-related traffic fatalities are sensitive to specification changes.

Table 3.4. The Effect of Wireless Lifeline Pre-paid Phones on Traffic Fatalities per 100 Million Vehicle Miles Traveled, 2003–2015

	(1)	(2)	(3)	(4)
Lifeline connections with no bans	0.125**	0.108**	0.003	0.049
	(0.058)	(0.046)	(0.046)	(0.048)
Lifeline connections with one ban	−0.048	−0.035	−0.169**	−0.153**
	(0.121)	(0.101)	(0.065)	(0.065)
Lifeline connections with two bans	0.115	−0.038	−0.060	−0.056
	(0.114)	(0.102)	(0.074)	(0.074)
One ban (text ban or hands-free law)	0.075*	0.073*	0.034	0.056*
	(0.042)	(0.039)	(0.030)	(0.031)
Two bans (text ban and hands-free law)	0.065	0.085**	−0.023	−0.004
	(0.048)	(0.033)	(0.031)	(0.040)
State and year-month fixed effects	✓	✓	✓	✓
State-level controls		✓	✓	✓
State linear time trends			✓	
State quadratic time trends				✓
R^2	0.573	0.583	0.599	0.596
Observations	7,644	7,644	7,644	7,644

Notes: Each column is a separate regression. The dependent variable is all traffic fatalities per 100 million vehicle miles traveled (VMT). "Lifeline connections with no bans" is the estimated number of pre-paid phones (in millions) reimbursed through the Lifeline program in a state with neither a text ban nor a hands-free law. "Lifeline connections with one ban" is the estimated number of pre-paid phones (in millions) reimbursed through the Lifeline program in a state that has wireless Lifeline with a text ban or a hands-free law. "Lifeline connections with two bans" is the estimated number of pre-paid phones (in millions) reimbursed through the Lifeline program in a state that has wireless Lifeline with a text ban and a hands-free law. "One ban" indicates a state with a text ban or a hands-free law. "Two bans" indicates a state with a text ban and a hands-free law. The former three variables are continuous and the latter two variables are indicators. State-level controls include indicators (seat belt law and Medicaid expansion under the Affordable Care Act) and continuous variables (poverty rate, median household income, unemployment rate, SNAP recipients, officers per 1000, precipitation, and the number of roundabouts). Observations are state-months. Standard errors in parentheses, clustered by state.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3.5. The Effect of Wireless Lifeline Pre-paid Phones on Alcohol-related Traffic Fatalities per 100 Million Vehicle Miles Traveled, 2003–2015

	(1)	(2)	(3)	(4)
Lifeline connections with no bans	0.020	0.005	-0.030^{**}	-0.002
	(0.026)	(0.022)	(0.012)	(0.015)
Lifeline connections with one ban	-0.012	-0.031	-0.100^{**}	-0.078^{**}
	(0.045)	(0.039)	(0.040)	(0.036)
Lifeline connections with two bans	0.025	-0.048	-0.081^{**}	-0.028
	(0.081)	(0.075)	(0.035)	(0.040)
One ban (text ban or hands-free law)	0.029	0.036^{*}	0.016	0.024
	(0.021)	(0.021)	(0.017)	(0.017)
Two bans (text ban and hands-free law)	-0.002	0.004	-0.011	-0.011
	(0.019)	(0.016)	(0.012)	(0.016)
State and year-month fixed effects	✓	✓	✓	✓
State-level controls		✓	✓	✓
State linear time trends			✓	
State quadratic time trends				✓
R^2	0.375	0.383	0.403	0.399
Observations	7,644	7,644	7,644	7,644

Notes: Each column is a separate regression. The dependent variable is all traffic fatalities per 100 million vehicle miles traveled (VMT) with a blood alcohol concentration greater than or equal to 0.02. See Table 3.4 for a description of variables and terms. Observations are state-months. Standard errors in parentheses, clustered by state.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Our results are not robust to switching the Lifeline variables from indicator variables to continuous variables. This erodes our confidence in the effect that the wireless expansion of Lifeline has on reducing traffic fatalities. However, the continuous Lifeline variables suffer from mechanical variation that may affect the estimates we obtained. I estimated (3.1) with the estimated number of pre-paid phones resulting from the raw claims data and the smoothed claims data. There is no qualitative difference between the sets of results. This indicates that there may be further bias from the 2012 Lifeline reforms that we were unable to control when we use continuous Lifeline variables.

3.6 Conclusion

Distracted driving concerns policymakers due to the increasing amount of distraction-affected traffic fatalities. The principle area of concern in distracted driving is texting while driving (National Highway Traffic Safety Administration 2021). Texting while driving becomes more available and provides more utility when the cell phone market is saturated and the network effects are maximized.

When the stock of cell phones is suddenly increased, traffic fatalities increase in response. These increases are found in states that either have no restrictions on cell phone use, therefore allowing people to choose how to use their cell phones. Increases are also found in states that prohibit most interactions with a cell phone while driving where users are incentivized to conceal their use. This evasion from enforcement appears to increase the distraction caused by cell phone use. Curiously, states that ban one of the two primary methods of cell phone use see no increases in traffic fatalities when the stock of cell phones is increased. States that have one ban and not the other mainly prohibit texting while driving. This prohibition moves cell phone users into voice-driven interactions with their cell phone while driving, which may be less distracting.

The distinction in the effect of cell phones between policy implementations suggests that the optimal policy to reduced distracted driving is to prohibit texting while driving only. Further, we see that the second-best policy to reduce distracted driving is to have no restrictions on cell phone use while driving. Prohibiting texting while driving and requiring hands-free use while driving likely does not decrease cell phone use and adds incentive to conceal cell phone use, which only increases the amount of distraction that cell phones have on driving.

3.A Tables

Table 3.6. Lifeline Wireless Pre-paid Phones First Availability by State

State	Effective Date	State	Effective Date
Alabama	July 2009	Montana	
Alaska		Nebraska	September 2013
Arizona	July 2011	Nevada	June 2010
Arkansas	October 2009	New Hampshire	July 2009
California	September 2011	New Jersey	July 2009
Colorado	November 2012	New Mexico	June 2012
Connecticut	July 2009	New York	March 2009
Delaware	April 2009	North Carolina	April 2009
District of Columbia	July 2009	North Dakota	June 2013
Florida	October 2008	Ohio	September 2009
Georgia	February 2009	Oklahoma	December 2010
Hawaii	September 2013	Oregon	April 2013
Idaho	September 2013	Pennsylvania	April 2009
Illinois	September 2009	Rhode Island	November 2010
Indiana	November 2011	South Carolina	December 2010
Iowa	September 2011	South Dakota	January 2013
Kansas	December 2009	Tennessee	September 2008
Kentucky	March 2011	Texas	February 2010
Louisiana	July 2009	Utah	March 2011
Maine	April 2010	Vermont	November 2013
Maryland	December 2009	Virginia	November 2008
Massachusetts	February 2009	Washington	October 2010
Michigan	April 2009	West Virginia	July 2009
Minnesota	February 2012	Wisconsin	July 2009
Mississippi	October 2010	Wyoming	June 2013
Missouri	December 2009		

Source(s): Universal Service Administrative Company (2020) and author's calculations.

Table 3.7. Effective Date of Primary Enforcement Texting While Driving Bans by State

State	Effective Date	State	Effective Date
Alabama	August 1, 2012	Mississippi	July 1, 2015
Alaska	September 1, 2008	Nevada	January 1, 2012
Arkansas	October 1, 2009	New Hampshire	January 1, 2010
California	January 1, 2009	New Jersey	March 1, 2008
Colorado	December 1, 2009	New Mexico	July 1, 2014
Connecticut	October 1, 2005	New York	July 12, 2011
Delaware	January 2, 2011	North Carolina	December 1, 2009
District of Columbia	July 1, 2004	North Dakota	August 1, 2011
Georgia	July 1, 2010	Oklahoma	November 1, 2015
Hawaii	July 1, 2013	Oregon	January 1, 2010
Idaho	July 1, 2012	Pennsylvania	March 8, 2012
Illinois	January 1, 2010	Rhode Island	November 9, 2009
Indiana	July 1, 2011	South Carolina	June 9, 2014
Iowa	July 1, 2017	Tennessee	July 1, 2009
Kansas	July 1, 2010	Texas	September 1, 2017
Kentucky	July 13, 2010	Utah	May 12, 2009
Louisiana	August 15, 2010	Vermont	June 1, 2009
Maine	September 28, 2011	Virginia	July 1, 2013
Maryland	October 1, 2009	Washington	June 10, 2010
Massachusetts	September 30, 2010	West Virginia	July 1, 2013
Michigan	July 1, 2010	Wisconsin	December 1, 2010
Minnesota	August 1, 2008	Wyoming	July 1, 2010

Source(s): McCartt, Kidd, and Teoh (2014) and author.

Table 3.8. Effective Date of Primary Enforcement Hands-free Driving Laws by State

State	Effective Date
California	July 1, 2008
Connecticut	October 1, 2005
Delaware	January 2, 2011
District of Columbia	July 1, 2004
Hawaii	July 1, 2013
Illinois	January 1, 2014
Maryland	October 1, 2013
Nevada	January 1, 2012
New Hampshire	July 1, 2015
New Jersey	March 1, 2008
New York	November 1, 2001
Oregon	January 1, 2010
Rhode Island	June 1, 2018
Vermont	October 1, 2014
Washington	June 10, 2010
West Virginia	July 1, 2013

Source(s): McCartt, Kidd, and Teoh (2014) and author.

Table 3.9. Effective Date of Primary Enforcement Seat Belt Laws by State

State	Effective Date	State	Effective Date
Alabama	December 9, 1999	Michigan	April 1, 2000
Alaska	May 1, 2006	Minnesota	June 9, 2009
Arkansas	June 30, 2009	Mississippi	May 27, 2006
California	January 1, 1993	New Jersey	May 1, 2000
Connecticut	January 1, 1986	New Mexico	January 1, 1986
Delaware	June 30, 2003	New York	December 1, 1984
District of Columbia	October 1, 1997	North Carolina	October 1, 1985
Florida	June 30, 2009	Oklahoma	November 1, 1997
Georgia	July 1, 1996	Oregon	December 7, 1990
Hawaii	February 16, 1985	Rhode Island	June 30, 2011
Illinois	July 3, 2003	South Carolina	December 9, 2005
Indiana	July 1, 1998	Tennessee	January 1, 2004
Iowa	July 1, 1986	Texas	September 1, 1985
Kansas	June 10, 2010	Utah	May 12, 2015
Kentucky	July 20, 2006	Washington	July 1, 2002
Louisiana	September 1, 1995	West Virginia	July 1, 2013
Maine	September 20, 2007	Wisconsin	June 30, 2009
Maryland	October 1, 1997		

Source(s): Harper and Strumpf (2017) and author.

Table 3.10. Medicaid Expansion under the Affordable Care Act Effective Dates by State

State	Effective Date	State	Effective Date
Alaska	September 1, 2015	Michigan*	April 1, 2014
Arizona*	January 1, 2014	Minnesota	January 1, 2014
Arkansas*	January 1, 2014	Montana*	January 1, 2016
California	January 1, 2014	Nevada	January 1, 2014
Colorado	January 1, 2014	New Hampshire*	August 15, 2014
Connecticut	January 1, 2014	New Jersey	January 1, 2014
Delaware	January 1, 2014	New Mexico	January 1, 2014
District of Columbia	January 1, 2014	New York	January 1, 2014
Hawaii	January 1, 2014	North Dakota	January 1, 2014
Illinois	January 1, 2014	Ohio	January 1, 2014
Indiana*	February 1, 2015	Oregon	January 1, 2014
Iowa*	January 1, 2014	Pennsylvania	January 1, 2015
Kentucky*	January 1, 2014	Rhode Island	January 1, 2014
Louisiana	July 1, 2016	Vermont	January 1, 2014
Maryland	January 1, 2014	Washington	January 1, 2014
Massachusetts	January 1, 2014	West Virginia	January 1, 2014

Notes: * indicates a state that has a Section 1115 waiver that allows the state to operate their Medicaid expansion without meeting some requirements contained in the Affordable Care Act.
Source(s): Author.

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