"No Hatred or Malice, Fear or Affection": Media and Sentencing

Arnaud Philippe

University of Bristol

Aurélie Ouss

University of Pennsylvania

We explore how television broadcasting of unrelated criminal justice events affects sentencing. Exploiting as-good-as-random variation in news content before a verdict, we find that sentences are 3 months longer when the verdict is reached after coverage of crime. Sentences increase with media exposure to crime, not crime itself, and the effect tapers off quickly. Our results suggest that professional experience and expertise mitigate the effect of irrelevant external information. This paper highlights the influence of noise in the news cycle: media can temporarily influence decisions by changing what is top of the mind rather than signaling deeper changes in offending or societal concerns.

> You swear and promise not to listen to hatred or malice or fear or affection . . . and to decide according to your conscience and intimate conviction, with the impartiality and firmness that befits an honest and free person. (French juror pledge)

I. Introduction

The right to a fair trial, one of the cornerstones of the criminal justice system, implies that defendants should be tried only on the basis of the facts

at hand and not on extraneous factors. However, a growing literature shows that decisions of both citizens (mock juries) and judges are vulnerable to extraneous influences such as physiological fatigue.[1] We show that in an important field setting—criminal courts—actual jurors respond to irrelevant information. Specifically, we show that sentence length varies with media coverage of unrelated crimes.

Our study exploits as-good-as-random variation in television news content at time of trial to estimate the causal effects of media on sentencing decisions made within the French criminal justice system. We combine original administrative data on all criminal records for jury trials in France between 2004 and 2010 (a total of 16,342 criminal cases) and data on the content of the main 8:00 p.m. national television newscast, which is followed by 20 percent of households in France.[2] Because trial start and end dates are set months in advance, cases examined are plausibly independent from what is on the news at the time of a particular trial. Empirically, cases examined are similar regardless of what kind of news stories run immediately before verdicts are handed down. We compare outcomes of trials whose verdicts happen to be rendered just after relatively more coverage of crimes unrelated to the case at hand versus those preceded by relatively less coverage. We examine jury decisions for both convictions and sentences. We rule out reverse causality—the possibility that TV could reflect ongoing trials—by focusing on stories that do not mention trials or legislation and by demonstrating that coverage of crimes is not influenced by ongoing trials.

We find that news stories do not affect convictions. In France, an investigating judge decides which cases to bring to criminal court, so jurors examine only relatively strong cases that have cleared that hurdle. Only 7 percent of cases in criminal court end in an acquittal.

[1] See Danziger, Levav, and Avnaim-Pesso (2011). Other papers have looked at the degree to which common psychological biases like the gambler's fallacy apply to judges (Chen, Moskowitz, and Shue 2016). Compared to research on judges, there is a much larger literature using mock juries (see Devine et al. [2001] for a review).

[2] Authors' calculations using data from Médiamétrie, a French audience measurement company. Daily audiences are included in the main media data set that we obtained from the French National Audiovisual Institute.

Jurors in France also vote on the sentence, and we find that news content on the day before the verdict affects sentence length in jury trials. In criminal courts with lay jurors, each additional news story about felonies increases sentences the following day by an average of 26 days. Being tried after media coverage of crime (which happens for 45 percent of cases) versus no stories on crime increases sentences by 86 days, or close to 3 months.

In criminal courts in France, sentences for crimes are full years of prison, so in terms of magnitude, the average effect of media coverage of crimes can be interpreted as an increased likelihood that a person will receive a sentence of at least 1 extra year in prison. The average increase of prison sentences by 3 months after coverage of crime translates into one in four defendants receiving an additional year in prison. As one point of comparison, Abrams and Yoon (2007) find that, in the United States, an attorney with 11 years of experience will, on average, obtain sentences that are 1.2 months shorter than someone with only 1 year of experience.

We apply the same methodology to look at the effect of judicial errors on sentencing. Coverage of judicial errors is less frequent and is concentrated around the coverage of one particular incident of miscarriage of justice, raising questions of external validity. With these caveats in mind, we find that sentences are 40 days shorter after coverage of judicial errors, suggesting that jurors adjust sentences both upward and downward depending on media coverage of criminal justice issues. These first-order results on judicial errors are robust to changing the specification, but results on mechanisms are not as robust.

We hypothesize that news on crime temporarily makes crime more salient in jurors' minds. Several facts are consistent with this interpretation. We find that sentences do not respond to local variations in the number of crimes, but only to differences in media coverage of crime. We then show that while news on the day before a verdict is released has a significant effect on sentencing, news further back in time does not, even when the trial lasted several days. Looking at a national survey about public safety concerns and exploiting variations in the exact timing of the survey date, we find that people are more likely to report "feeling unsafe" on the day after more media coverage of crime; but in that case also, the effect tapers off quickly. Furthermore, our effects are driven by the presence of crime news rather than the amount of news on crime. Finally, we find that sentence lengths vary only with news about criminal justice, not with news on other upsetting topics such as natural catastrophes or unemployment.

We explore whether experience and expertise can help mitigate media biases, by asking whether judges are less prone to respond to media content. We show in different settings that news stories do not affect decisions made by judges. There is no effect of media on decisions in corrections courts, where professional judges examine misdemeanors. Our analyses focus on the most serious misdemeanors, but we cannot reject that this

is due to the difference in types of cases being tried. We then exploit a discontinuity at age 16, leading felonies to be judged by laypeople instead of only by judges. We find that news stories have no effect on decisions for defendants younger than 16 (judged only by professional judges), while they affect sentences in cases with defendants older than 16. This suggests that professionalism mitigates the effect of domain-pertinent but irrelevant external information, though we note that the difference in effect sizes between the two age groups is not statistically significant. Finally, we also find no evidence of an effect of media in appellate courts, where judges and attorneys are more experienced. These results in different contexts suggest that professional experience and expertise reduce the effect of domain-pertinent but irrelevant external information.

Our paper provides some of the first quasi-experimental field evidence of the effects of irrelevant information on criminal justice outcomes. While several studies look at the effect of media on sentencing, they use surveys of jurors (Dowler 2003; Surette 2014) or mock juries (see Greene [1990] for a review). Some recent papers have used data on real convictions but focus on sentencing variations due to intrinsic characteristics of jurors and defendants, including age (Anwar, Bayer, and Hjalmarsson 2014), race (Gazal-Ayal and Sulitzeanu-Kenan 2010; Shayo and Zussman 2011; Anwar, Bayer, and Hjalmarsson 2012), or political opinions (Anwar, Bayer, and Hjalmarsson 2015). Other work looks at how consistent media coverage of courts can influence judges' decisions by increasing accountability (Lim 2015; Lim, Snyder, and Strömberg 2015).

Our paper also contributes to a growing literature showing how news provision and its biases influence policy-relevant behaviors (reviewed in DellaVigna and La Ferrara [2015]), including voter turnout (Gentzkow 2006; Cagé 2013), corruption (Ferraz and Finan 2008), political accountability (Snyder and Strömberg 2010), election results (DellaVigna and Kaplan 2007; Gerber, Karlan, and Bergan 2009; Enikolopov, Petrova, and Zhuravskaya 2011), or conflicts (Yanagizawa-Drott 2014). More broadly, audiovisual content can also affect offending (Dahl and DellaVigna 2009), adolescent attitudes (Kearney and Levine 2015), and fertility (La Ferrara, Chong, and Duryea 2012). We show that there can be large impacts from small variations in what media coverage puts top of the mind before a decision. This factor is somewhat accounted for by modern democracies: for example, in France, the media may not interview candidates or publish polls the day before an election; in the United States, juries may be isolated during a trial so that they will not be influenced by media coverage of the case they are considering.

The rest of the paper is organized as follows. In Section II, we describe French institutions and the data we use. Section III discusses our identification strategy. Section IV presents the effect of media on juror decisions, and Section V explores mechanisms. Section VI turns to the effects of media content on judges' decisions, and Section VII presents conclusions.

II.   Institutions and Data

A.   French Courts

There are three types of criminal courts in France: corrections courts (*tribunal correctionnel*), criminal courts (*cour d'assises*), and juvenile courts (*tribunal pour enfants*). If the maximum sentence for an offense is less than 10 years, it is tried in corrections court; if the maximum sentence is more than 10 years, the offense is tried in criminal court. For simplicity, we refer to cases judged in criminal court as "felonies," even though they represent only a small subset of all felonies—the most severe ones.[3] Defendants younger than 18 at the time of misdemeanors, or 16 at the time of felonies, are judged in juvenile court. While offenders may admit guilt in court, there is no plea-bargaining procedure for felonies: all felony defendants have to go to trial. Importantly for our purposes, this means that the selection of felony cases that go to trial cannot depend on what is in the news.

Guilt and sentencing are both determined on the day of the verdict in criminal court. Criminal courts examine about 3,000 cases per year. Judicial decisions are made by a jury of nine (first instance) or 12 (appeals) civilian jurors and three judges.[4] Defendants are tried in the county (*département*) where the offense occurred in first-instance trials and in a different county within the same judicial region for appellate trials.

Jurors are randomly drawn from among French citizens older than 23 who are registered to vote. A pool of 40 potential jurors are selected for a court session, which typically lasts about 2 weeks, during which one to 10 cases are heard, with each trial generally lasting 2 or 3 days. All 40 jurors show up to court at the beginning of each case, and members of the jury are selected at random. Attendance is mandatory, with a compensation of roughly US$100 per day in court. The defense attorneys and prosecutor are allowed to exclude five and four jurors, respectively. However, in contrast to the United States, there is no questioning of potential jurors and selection relies on very little information: name, age, gender, occupation, and a person's demeanor as he or she walks to the jury box. Importantly for our work, this means that jurors cannot be excluded on the basis of their consumption of media or their perception of current events.

The jury first votes on culpability. There is a single vote on guilt, and a conviction needs a two-thirds majority. Only 6–7 percent of defendants

---

[3] Using the criminal record data described below, we find that only about 0.5 percent of criminal cases are tried in criminal court and that these are mainly cases of murder, forcible rape, armed robbery, and aggravated assaults leading to permanent disability. The vast majority of cases, including many felonies, are tried in corrections court.

[4] In this paper, we examine pre-2012 institutions. Since 2012, these numbers went down to six and nine jurors, respectively. One of the three judges, known as the president, leads the trial. Very high-profile cases, in particular linked to terrorism, are judged solely by judges.

are acquitted in criminal court.[5] This number is low likely because investigating judges have to decide that there is enough evidence against the defendant for the case to be pursued in criminal court. Though these numbers are not directly comparable, in the United States, acquittals represent only 1 percent of felony case outcomes (Reaves 2013).[6] In England and Wales, 11.9 percent of offenders judged by Crown courts were found not guilty (Ministry of Justice 2013, 31).

If the defendant is found guilty, the jurors vote on sentence length immediately after. Each juror writes down a sentence length. There are as many iterations of this vote as needed for one sentence length to obtain a strict majority (according to articles 355–365 of the French penal code). There are no strict sentencing guidelines in France: the minimum possible sentence in criminal court is generally 1 year, or 2 years if the maximum penalty is life imprisonment. Sentences in criminal courts are typically given in increments of full years.

For criminal cases, investigations typically take a long time: the median length of investigation is 3.5 years.[7] Court dates are determined months in advance, and the jury pool receives a notification at least 1 month before the trial, reinforcing the idea that the precise date of a trial is not correlated with events that take place at that time.

Felonies committed by 16- and 17-year-olds are judged in criminal court, but juvenile laws still apply. While possible penalties are less severe than for adults, the structure of this criminal court is similar in many ways to adult criminal court and, in particular, also includes civilian jurors. By contrast, juvenile courts have one judge and two assessors, who are appointed for 4 years. About 350 felonies committed by offenders under the age of 16 are tried each year, and about 300 juveniles aged 16–17 years old are tried for felonies in criminal court.[8]

---

[5] Calculated by the authors using official statistics on overall outcomes of trials from the Ministry of Justice (Ministère de la Justice 2012, 127). Chaussebourg and Lumbroso (2008) look at appellate cases and find a rate of acquittal of 7 percent for this subset of cases—which is low, given the selection. For the counties for which we have data on acquittals, we find that 7.5 percent of cases ended in an acquittal.

[6] The acquittal rate is much higher for offenders who go to jury trial. For example, Anwar et al. (2012) find that about 27 percent of jury trials in Florida lead to no guilty conviction, but less than 10 percent of defendants actually have a jury trial. The remaining 90 percent plead guilty, leading to the very low overall acquittal rates. Since there are no plea bargains in France, the relevant comparison acquittal rate would be acquittals of all felony defendants, and not only those decided in a jury trial.

[7] See table 1. Investigation length is defined as the time between crime and trial, so it is the sum of the time between crime and arrest and time between arrest and trial. While we cannot parse these out in our data, on average nationally, there are 24–26 months between crime and arrest and 34–36 months between arrest and trial (Ministère de la Justice 2012, 127).

[8] Authors' calculations, using court data, presented in table 7 below.

B.   Court Data

Our main data source on sentencing is criminal records (*casier judiciaire*), kept by the French Ministry of Justice. Criminal records have one observation per criminal court conviction. No criminal record is kept if the trial ends in an acquittal. Criminal records are collected for administrative purposes: judges check them at trial, they are (very selectively) used for background checks, and the French Ministry of Justice uses them for statistical purposes. They contain information on the date and county of conviction, offenses (type of offense, date of offense) and sentences, as well as basic sociodemographic information such as age, gender, and nationality. We analyze outcomes for convictions from between 2004 and 2010.

The upper part of table 1 presents descriptive statistics on adult convictions in criminal court between 2004 and 2010. Felony defendants are mostly male (94 percent) and French (88 percent); they are 38.6 years old on average, and 36 percent have had some past conviction. Forcible rape is the most frequent crime tried in criminal courts (47 percent). The average sentence is around 10 years, with up to 15 years for murder.[9]

In order to fully understand how media might influence criminal court outcomes, we sought to obtain data on acquittals and trial length (criminal records report only the date of the verdict). Some court clerks keep this information on annotated court schedules, so we contacted all 95 counties, and 44 were willing to share their court schedules. Of these, 25 counties had annotated schedules, which included information on trial outcomes and allow us to determine whether a defendant was convicted, was acquitted, or appealed the decision.[10] For this subsample, there are 514 acquittals for 6,769 trials (7.6 percent). The remaining 19 counties did not have information on acquittals, but their schedules include trial start dates. In online appendix A, we provide more details on these subsamples. The cases in these subsamples are very similar to our main sample in terms of defendant characteristics, crimes, and sentences. The main results in this paper use the criminal records data set, and we specify when we use the criminal court schedule data sets.

---

[9] Only 3.5 percent of sentences are fully suspended and 84 percent of sentences are only prison time (no part of which is suspended). We code life imprisonment sentences as being equal to 32 years, which is the highest sentence other than life imprisonment. Only 0.64 percent of sentences are life sentences, and we show that our results are robust to dropping life sentences (app. table D4, col. 2).

[10] What exactly appears on the court schedule is left to the court's discretion. Some courts update their schedules after the trial and include information on the outcome of the trial, while others do not. We noted no particular pattern according to which courts update their schedules to include information on trial outcomes.

TABLE 1
SUMMARY STATISTICS ON CONVICTIONS AND 8:00 P.M. NEWS CONTENT, 2004–10

	Mean	Standard Deviation	Median	Maximum
		A. Felonies ($N = 16,342$)		
Number of cases per year	2,335	246	2,471	2,633
Fraction male	.94	.23	1	1
Age	38.62	13.07	37	91
Fraction French	.87	.34	1	1
Had a past conviction	.36	.48	0	1
Appellate case	.13	.34	0	1
Investigation length (years)	5.29	4.73	3.49	37.29
Length pretrial custody (days)	675.58	491.4	700	7,439
Offense:				
Murder	.18	.38	0	1
Aggravated assault	.12	.33	0	1
Forcible rape	.47	.5	0	1
Property crime	.22	.41	0	1
Prison sentence (years):				
Overall	10.16	5.68	9	Life
Murder	15.15	7.06	15	Life
Aggravated assault	8.91	5.46	8	Life
Forcible rape	9.49	4.38	9	Life
Property crime	8.25	4.66	7	Life
		B. Subsample with Acquittal Data ($N = 6,769$)		
Felony acquittal	.076	.27	0	1
		C. 8:00 p.m. TV National News (TF1 and France 2) ($N = 2,557$)		
Number of stories per day on:				
Felonies	1.28	1.65	1	12
Felony perpetrated	.89	1.37	0	10
Judicial errors	.14	.76	0	22
At least one story per day on:				
Felonies	.57	.50	1	1
Perpetrated felonies	.45	.50	0	1
Judicial errors	.07	.26	0	1
Time per day (minutes) on:				
Felonies	1.69	2.29	.93	16.57
Perpetrated felonies	1.19	1.92	0	12.87
Judicial errors	.21	1.24	0	36.77
Audience (million)	13	2.12	13	19

SOURCE.—Authors' calculations based on criminal records, provided by the French Ministry of Justice, and data collected from the National Audiovisual Institute and from French criminal records.

NOTE.—Statistics on the news content reflect stories covered on the 8:00 p.m. national television news (TF1 and France 2). Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. The summary statistics on convictions use all criminal records. The acquittal rate is calculated for the subsample of cases for which we have acquittal information.

C.   French Television: Viewership and Data

Television is a popular source of information in France, with two channels splitting most of the audience shares: TF1, a privately owned, noncable channel, draws between 31.8 percent (2004) and 24.5 percent (2010) of viewers; France 2, a public channel, draws between 20.5 percent (2004) and 16.1 percent (2010) of viewers. In particular, the 8:00 p.m. news programs are very popular and influential, so much so that they are often dubbed the "8:00 p.m. mass." The 8:00 p.m. TF1 and France 2 news programs have average audiences of 8 million and 5 million viewers per day, respectively (from a population around 65 million in France).[11] Both programs last roughly 40 minutes. Programming on free audiovisual media (including TF1 and France 2) is mandated to be nonpartisan. This neutrality is enforced by the Superior Counsel of Audiovisual Media.

The National Audiovisual Institute archives all 8:00 p.m. news broadcasts. We collected data on TF1 and France 2 news broadcasts between 2004 and 2010. For each news story, we have information on the title, date, place, and length of the story and a list of keywords describing its content. Archive staff assign those keywords using a precise standardized list. There is an average of 24 stories per day, per channel. Since we are most interested in the effect of jurors' environment on sentencing, we limit our sample to national news, which represents an average of 15 stories per day.[12] We also obtained from the National Audiovisual Institute information on viewership for each news broadcast, as calculated by Médiamétrie.

We use the keywords describing the content of each story to construct indicators of coverage of crime and criminal justice. We kept keywords that appear more than 20 times between 2004 and 2010 (2,636 words—more than 80 percent of all keywords). We grouped them into categories relevant to measuring coverage of crime and criminal justice: felonies, criminal law, trials, and judicial errors. Using this keyword classification, we then calculate the number of news stories per day that include keywords on felonies and judicial errors and the number of minutes devoted to these subjects.

We also create a measure for stories about crimes committed (henceforth labeled "perpetrated felonies"), which are stories about crimes that do not mention trials or legislation. We identify these as news stories including "felony" keywords but no "criminal justice" keywords, such as "trial," "verdict," "court," "hearing," or "appeal." Appendix table A1 presents the full list of words used to construct these measures.

For each day and each topic, we construct three measures: number of stories, number of minutes, and dummies for whether these topics were covered at all. Stories from both TV channels, TF1 and France 2, are

---

[11] The figures on viewership in this paragraph are based on the authors' calculations using data from Médiamétrie, a French audience measurement company. These data were included in the main media data set.

[12] Authors' calculations, using the main media data set.
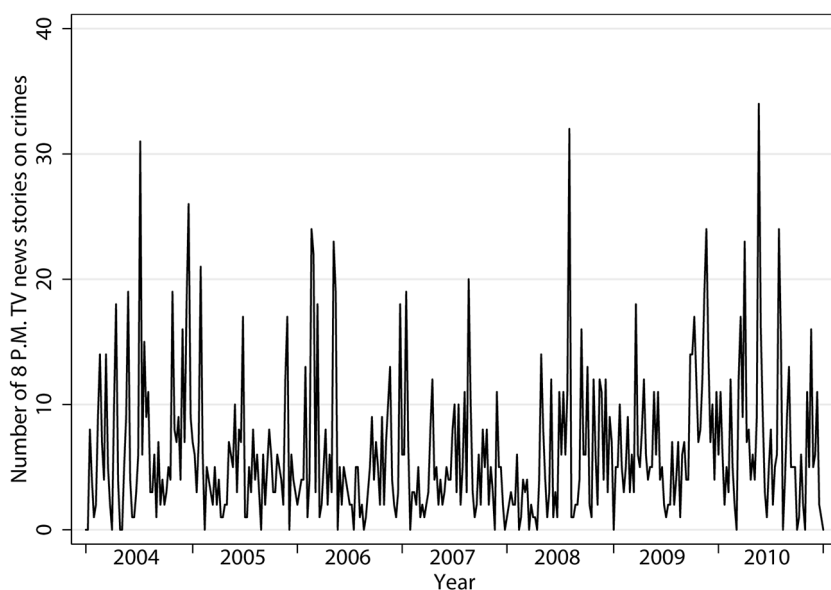
Fɪɢ. 1.—Number of stories about perpetrated felonies on the 8:00 p.m. national television news (TF1 and France 2) per week, 2004–10. Stories about perpetrated felonies are stories about crimes that do not mention trials or legislation. Source: Authors' calculations based on data collected from the National Audiovisual Institute.

grouped together in our measures. Figure 1 illustrates variations over time in stories on crime: there is substantial variation in coverage of crime. Table 1 presents descriptive statistics on the coverage of stories between 2004 and 2010. While there are many stories about crime, with a lot of daily variation, news on miscarriage of justice is rarer, and at least 75 percent of these stories relate to the infamous "Outreau trial,"[13] in which a dozen people had been wrongly convicted for sexual abuse of children on the basis of false witness testimony. Since stories about judicial error most often relate to this single event, our main results focus on coverage of crimes.

D.   Crime

We compute the number of crimes per county using two data sources. First, we use official police statistics, which report the monthly number of offenses per county. These data are publicly available on the open data platform of the French government (www.data.gouv.fr). We calculate the number of felonies per county and per month. There are, on av-

---

[13] We calculate this percentage by looking at the number of stories on judicial errors that include one of two words: "Outreau" or "Burgaud" (the judge in charge of that case). This gives us a lower bound for the percentage of stories on judicial errors that are about the Outreau case, since some other coverage on Outreau may not have included direct mentions of either.
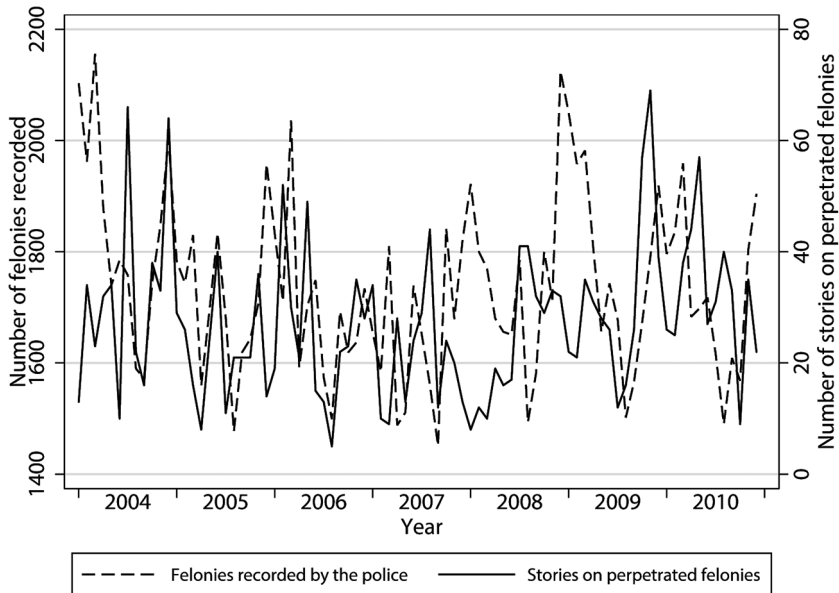
Fig. 2.—Monthly crimes on TV and reported by the police. The solid line (right axis) presents the number of stories on perpetrated felonies on the 8:00 p.m. national television news (TF1 and France 2), per month, 2004–10. Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. The dashed line (left axis) presents the number of felonies recorded by the police, per month, from 2004 to 2010. Source: Authors' calculations based on data collected from the National Audiovisual Institute and from police statistics publicly available on the open data platform of the French government.

erage, 18 felonies per county and per month recorded by the police, amounting to about 1,736 felonies per month in France. In figure 2, we present the monthly variation in number of felonies and number of media stories on crime between 2004 and 2010. The variation in number of media stories on crime in a given month does not directly match the variation in number of offenses.[14]

Our second measure of crime comes from the criminal records data presented in Section II.B. Criminal records include the date on which the crime was committed. Using data for 2004–15, we can determine the number of crimes that led to a conviction in the following years, at the day and county levels.[15]

---

[14] At the yearly level, the number of news stories on crimes is correlated with the number of actual felonies, as shown in app. fig. B1. It is only in the short run that variations in crimes differ from media coverage thereof.

[15] We exclude offenses that take place on the first day of the month, since these capture all offenses whose date is uncertain. Fifty-eight percent of the cases are tried within 5 years of the offense date and 80 percent within 7 years. Our "criminal records" measure of offending captures more offenses at the beginning of the 2004–10 period than at the end of that period.

The two data sets have different strengths and weaknesses. The police data record all crimes but are available only at the month level. Criminal records capture offending at the day level but keep track only of crimes for which a person was convicted. Note also that court data are at the offender level, while police data are at the crime level; therefore, court statistics yield slightly higher overall numbers than police statistics, since one crime could have multiple perpetrators.

E.   Perceptions of Crime

Finally, we use a national victimization survey to get a measure of people's perception of safety and insecurity. This survey has been administered each year since 2007 by the French National Institute of Statistics and Economic Studies (INSEE), and we used data collected from 2008–10.[16] Roughly 17,000 people are surveyed each year, so there are 50,817 respondents over the period 2008–10. We obtained from INSEE a specific data set containing the exact date each survey was administered. In order to measure people's perception of safety, we use the answer to the following question: "Do you sometimes feel insecure in your neighborhood?" The possible responses are never (77.6 percent), rarely (9.5 percent), sometimes (9.5 percent), and frequently (3.2 percent).

III.   Empirical Strategy

Our main identification strategy exploits variation in the exact timing of news stories and trials. We compare verdicts handed down just after more coverage of crime to verdicts handed down following less media coverage of crime. We investigate the effect of media on two main outcomes: conviction and sentence length. Since trial dates in France are set months in advance, the exact day of a trial is plausibly unrelated to the content of news on the day before the verdict (more discussion of this below). We estimate equations of the form

$$Y_{i,t,j} = \alpha \text{Media}_{t-k} + \beta X_t + \gamma W_j + \delta Z_i + \varepsilon_{i,t,j}. \tag{1}$$

Under the assumption that the conditional mean of the errors is zero,

$$E\big(\varepsilon_{i,t,j}|\text{Media}_{t-k}, X_t, W_j, Z_i\big) = 0. \tag{2}$$

The term $Y_{i,t,j}$ is the trial outcome (conviction or sentence length) for person i whose verdict happened at date t in county j; Media_{t-k} captures

---

[16] A complete description of the survey (in French) can be found at http://www.insee.fr/fr/methodes/default.asp?page=sources/ope-enq-victimation.htm.

measures of media coverage on relevant topics k days before the verdict; X_t controls for time (day of the week, calendar month, and year fixed effects); W_j controls for county of trial; and Z_i controls for certain defendant characteristics. These characteristics are offense and type of court (appellate court or first-instance court), age, gender, a dummy for French citizenship, pretrial custody (number of days), investigation length (number of days), and number of prior convictions in the past 5 years. Standard errors are clustered at the county level unless otherwise specified.

In our main specifications, we define Media as the number of news stories on a given topic, and we present results for $k = 1$: in this case, we measure the effect of news stories on the day before the verdict. Using the same basic structure, we can conduct a "placebo experiment" by looking at the effect of media after the verdict on a trial's outcome, since posterior events cannot influence that trial's outcome. We also run specifications that include measures of news stories on the day before and the day after the verdict. In our main analyses, we do not include media content on the day of the verdict, since its effect is a priori ambiguous. While the 8:00 p.m. news broadcast that takes place on the day after the verdict could not influence a trial's outcome, some of the events covered on the 8:00 p.m. news on the day of the verdict might also have appeared in morning papers and on radio shows. Therefore, this news on the day of the verdict cannot be seen unequivocally as news "before" or "after" a verdict.

Identification rests on the assumption that the exact timing of trials is not related to TV coverage of crime and criminal justice. One might worry about reverse causality: more serious trials are more likely to be covered on TV. Note first that most felonies and most trials do not make national news. Furthermore, several years typically go by between offenses and trials, so on any given date, crimes that are being covered are not those that are being tried. Another concern is selection bias: attorneys could factor in public sentiment when choosing trial dates, trying to avoid periods of higher crime coverage. However, trial dates are set several months in advance, as a function of availabilities of judges, attorneys, and courtrooms. Trial length is determined before the schedule is set, based on the number of experts and witnesses. Jurors are summoned at least 30 days before the trial starts. So while season or month (which we control for) could potentially be manipulated, last-minute planning to avoid a trial in an unfavorable media climate seems very unlikely.

Table 2 presents the correlation between characteristics of cases being tried and coverage of felonies and judicial errors at $t - 1$. Each panel represents independent regressions; we examine the effect on sentence length of three different types of news: news about perpetrated felonies (which we define as stories about crimes that do not mention trials or legislation), about all felonies (including stories on trials and legislation), and about judicial errors. For most covariates, differences across

TABLE 2
CHARACTERISTICS OF CASES BEING TRIED, BY MEDIA COVERAGE ON THE DAY BEFORE THE VERDICT

| | Type of Offense Being Tried | | | Number of News Stories on | | | | | | | |
| | | | | Trial Characteristics | | | | | Sociodemographics | | |
	Homicide (1)	Sexual Assault (2)	Property (3)	Investigation Length (4)	Pretrial Custody (5)	Appeals Court (6)	Trial Length (7)	Past Conviction (8)	Age (9)	Male (10)	French (11)
				A. Effect of News about Perpetrated Felonies							
Perpetrated felonies $t-1$	−.000790	−.000365	.00388	−.151	2.309	.00296	.0442	−.000228	−.169*	.000185	.00321
	(.00335)	(.00413)	(.00394)	(10.67)	(3.093)	(.00208)	(.0297)	(.00309)	(.0922)	(.00144)	(.00213)
				B. Effect of All News about Felonies							
Felonies $t-1$	−.00170	−.000225	.00255	16.94*	2.518	.00150	.0138	.000891	−.0428	−.000216	.00227
	(.00310)	(.00324)	(.00330)	(9.910)	(2.777)	(.00190)	(.0208)	(.00227)	(.0713)	(.00111)	(.00188)
				C. Effect of News about Judicial Errors							
Judicial errors $t-1$	−.00745	−.000837	.00720	−4.851	−4.416	−.00745*	−.0492	.00184	−.0981	.00478***	.00538
	(.00454)	(.00547)	(.00532)	(17.36)	(5.663)	(.00417)	(.0460)	(.00460)	(.138)	(.00149)	(.00365)
Observations	16,342	16,342	16,342	16,342	16,342	16,342	7,324	16,342	16,342	16,342	16,342
Mean	.300	.469	.216	1930	675.6	.135	3.556	.358	38.62	.231	.870

SOURCE.—Authors' calculations based on criminal records, provided by the French Ministry of Justice, and data collected from the National Audiovisual Institute and from French criminal records.

NOTE.—The dependent variable of each regression is specified in the column header. Panel A presents results by coverage of perpetrated felonies, panel B of felonies, and panel C of judicial errors. We regress case characteristics on the number of news stories on each topic. Regressions include day of the week fixed effects. Each cell presents regression coefficients for a different regression. Estimates in col. 7 are calculated using data for which we have information on session length (subsample 1, as defined in online app. A). News stories are those covered on the 8:00 p.m. national television news on TF1 and France 2. Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. Standard errors are clustered at the county level.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

media contexts are not statistically significant, and when significant, the point estimates are small. In particular, observables are balanced across media coverage of perpetrated offenses, which is our main measure of crime coverage. For our secondary measure of media coverage of crime—which includes trials and laws—investigations are longer after coverage of crime. The reason could be that trials after long investigations have a higher probability of being covered in the media. However, importantly for our identification strategy, column 11 of table 2 shows that trial length is not correlated with media content on the day before the verdict, confirming that it is not strategically manipulated on the basis of news content. These analyses validate our identification strategy: the exact timing of cases is orthogonal to news coverage of offenses.

To further address the potential concern that media could cover information on upcoming judicial decisions, our preferred estimates look at the effect of news stories about current crimes and violent offenses, excluding stories about trials (perpetrated felonies). As part of our robustness checks, we also exclude stories about crimes that took place in the same county as the trial, to make sure that there could not be overlap between the case tried and news stories examined. Appendix table E6 shows that only 2 percent of stories on crimes took place in the same county as the trial and only 7 percent took place in an adjacent county.

Another concern could be that there would be more coverage of (unrelated) crime stories when there are more high-profile court cases, because the public's interest in crime would increase.[17] Online appendix B shows that there is little empirical relationship between the amount of news on crime and the amount of news on trials.

We investigate the effect of media on two main outcomes: conviction and sentence length. Sentence length is observed only in cases of conviction. If media have an effect on acquittals, then our estimates of the effect of news on sentencing would likely be biased by selection (Lee 2009).

IV. Media Coverage and Jurors' Decisions in Criminal Court

A. *Media Coverage and Jury Conviction*

We first measure the effect of news coverage of crime on acquittals. This information is available only for the subsample of 25 counties that provided data on acquittals, since criminal records in France report only convictions (see online app. A for more details). Results are presented in columns 1–4 of table 3. Each panel of the table represents independent

---

[17] Conversely, time constraints could make news about trials limit coverage of perpetrated felonies.

regressions: the effect of news about felonies perpetrated, about all felonies, and about judicial errors is examined separately.

News stories about perpetrated felonies (panel A) have no impact on convictions. News stories about felonies in general (panel B, including news on upcoming trials) at $t − 1$ are marginally associated with more acquittals, but the effect is small and not robust.[18]

We now move to the effect of media on sentence length. Since media do not affect acquittals, the sample of defendants for whom we observe sentences is similar regardless of coverage of crime and judicial errors on the day preceding the verdict.[19]

B.   *Media Coverage and Sentence Length: Jury Decisions*

To measure the effect of media content on sentence length, we use the full sample of criminal records, since jurors determine sentences for all convictions. Our main results are presented in columns 5–8 of table 3. As for acquittals, each panel presents results for independent regressions. Column 5 presents regression results with no controls, and columns 6–8 include controls for case characteristics.

Panels A and B, respectively, present the effect of news about perpetrated felonies (excluding stories on trials and legislation) and all news about felonies. We find significant impacts on sentence length of news about crimes. For each additional story on felonies, sentences handed down the day after the stories run are 24 days longer on average. Results are similar for perpetrated felonies. The difference between these two measures is that one includes news coverage of trials and the other does not. This indicates that coverage of crimes, not only coverage of trials, affects sentences. These results hold with and without controls.

Reassuringly, we find no difference in sentences depending on the content of news stories on the day after the verdict (cols. 7 and 8): none of the coefficients are significant. Point estimates are small, with large standard errors. In addition, point estimates for news at $t − 1$ and $t + 1$ are marginally statistically different for perpetrated felonies (p-value = .0651). This indicates that civilian jurors respond to the context in which they are making judicial decisions: cases that are otherwise similar tend to get longer sentences when tried after more media reports.[20]

Why are sentences, but not convictions, affected by media? One candidate explanation stems from the difference in how juries vote on convictions and sentences. Since a single vote determines conviction, the

[18] Robustness tests are presented in app. tables D1 and D2.
[19] We discuss this point further in online app. D (table D3 and discussion).
[20] Results are similar when we use the subsample for which we have additional information and, in particular, the subsample used in Sec. IV.A for results on acquittals (see app. table D4).

TABLE 3
News Content, Acquittals, and Sentence Length: Jury Trials

		Number of News Stories on Outcome						
	Acquittal (Subsample)				Sentence Length			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Effect of News about Perpetrated Felonies								
Perpetrated felonies $t-1$	.00358	.00281		.00245	28.28**	25.96**		25.82**
	(.00324)	(.00287)		(.00280)	(14.06)	(10.28)		(9.992)
Perpetrated felonies $t+1$			.00228	.00187			4.615	.770
			(.00346)	(.00344)			(10.08)	(9.885)
p-value difference $t-1/t+1$				.900				.0651
B. Effect of All News about Felonies								
Felony $t-1$	.00471**	.00382*		.00363*	33.25***	25.25***		23.94***
	(.00213)	(.00187)		(.00188)	(12.53)	(7.947)		(7.760)
Felony $t+1$			.00161	.000953			11.03	6.833
			(.00277)	(.00282)			(8.315)	(8.199)
p-value difference $t-1/t+1$				.473				.128

2150

	C. Effect of News about Judicial Errors						
Judicial errors $t − 1$	.000224 (.00470)	−.00181 (.00540)		−.00274 (.00603)	−63.77*** (21.96)	−39.27** (15.87)		−39.70** (15.77)
Judicial errors $t + 1$			.00578 (.00747)	.00601 (.00777)			−.436 (13.60)	3.346 (13.34)
p-value difference $t − 1/t + 1$				.488				.0395
Controls	Day of week	All	All	All	Day of week	All	All	All
Observations	6,769	6,719	6,719	6,719	16,342	16,342	16,342	16,342
Mean	.0759	.0759	.0759	.0759	3,656	3,656	3,656	3,656

Source.—Authors' calculations based on criminal records, provided by the French Ministry of Justice, and data collected from the National Audio-visual Institute and from French criminal records.

Note.—Panel A presents the effect of coverage of perpetrated felonies, panel B of felonies, and panel C of judicial errors. Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. These regressions are estimated for the subsample of cases for which we have information on acquittals (defined in online app. A). The outcome in cols. 1–4 is a dummy for acquittal. Controls in cols. 2–4 are for gender, aggregate type of offense, county, and dummies for month, day of week, and year, which are the variables available for this sample. The outcome in cols. 5–8 is sentence length in days. Regressions are estimated for all criminal records. Controls in cols. 6–8 are for age, gender, nationality (French or other), length of pretrial detention, type of offense, number of prior convictions in the past 5 years, type of court (appellate court, court of first instance), county, length of time between offense and trial, and dummies for month, day of week, and year. News stories are those covered on the 8:00 p.m. national television news on TF1 and France 2. All standard errors are clustered at the county level.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

marginal voter would have to be affected by media for there to be an effect on conviction. Conversely, sentences are reached through a convergence process, and so any juror may be able to influence the ultimate decision through this iterative voting. Another interpretation could be that news stories are more likely to change the perception of crimes' severity, and not the strength of the evidence in the case at hand.

C.   Robustness Checks

The first concern that we address is that the effect of media on sentences could be counterbalanced by more appeals after coverage of crimes (from the defense) or criminal justice (from the prosecutors). Overall, 14.4 percent of first-instance cases are appealed. This could be driving the null result on convictions and lead to an understatement of the effects of media on sentencing, if we observe only cases that were not appealed. We investigate this using the date of the first-instance proceeding, which is recorded in the court data: when cases were appealed, we keep only the first-instance proceeding.[21] All verdict dates are thus now first-appearance dates, and we create a dummy equal to one if a case was appealed, our outcome of interest here. Results are presented in column 1 of table 4. All coefficients are nonsignificant and point estimates are extremely small. News stories do not affect appeals. This also confirms the idea that media do not affect conviction.

In column 2, we present the effect of news on log sentences, this measure being more robust to outliers. News at $t - 1$ remains significant while news at $t + 1$ is not. In column 3, we replicate our results excluding trials that took place in the same county as stories about crime covered the day before or after, as an additional step to make sure that there could be no possible overlap between the case tried and news stories examined. Results are extremely close to those presented in table 3.

Another concern might be around differential selection of jurors, based on exposure to coverage of crime. As was outlined in Section II, in the French context, potential jurors can be excluded only on the basis of minimal information. In particular, people are not questioned before being included in a jury, and so attorneys have no information on their relation to media or current events. One might still worry that a different set of jurors could be selected the day after news stories about crime than would have been selected the day before. While unlikely, we control for this scenario by running our main regressions for a subsample of trials

---

[21] Since appeals generally take time, we look at appeals judged up to 2012 for cases whose first instance was prior to 2010. This allows us to capture virtually all appeals cases between 2004 and 2010.

that last more than 1 day, among the sample of cases for which we know trial length. For those trials, jury selection took place before the 8:00 p.m. news broadcasts of the day before sentencing and could not be influenced by the news. Results are presented in column 4 of table 4. They are similar to those obtained for all cases.

The remainder of table 4 shows that our results are robust to variations in the exact kind of media coverage and time controls. In column 5, we control for session fixed effects, for the subsample for which we have information on session (see online app. A for the description of the subsample). This is a very stringent specification, since each session contains four trials on average. In column 6, standard errors are clustered by day. In column 7, we add county-specific time trends. Results are close to those obtained in Section IV.C.

In column 8, we vary our measure of media coverage and exposure. In our main results, we used number of news stories covering felonies. Here, we use the length of media coverage in minutes. Our results tell the same story with this measure of news. Results are similar if we look at the effect of each TV channel—TF1 or France 2—alone (see app. table E5).

D.   Magnitude of Effects

Our main results imply that each additional news story on crime increases sentence length by 26 days on average, for a 0.7 percent increase in sentence length per additional story on crime. Column 1 of table 5 presents a regression of sentence length on a dummy equal to one if there is at least one news story on crime on the day before the verdict. We find that being tried after a news story on crime versus no story on crime increases sentences by 3 months (86 days), which is an average of a 2.4 percent increase in sentence length. In this subsection, we first contrast these magnitudes to those found in the experimental literature and in field evaluations of the effect of extraneous factors on sentencing and then unpack these average effects in terms of sentence distribution.

Our average effects are small relative to those in lab experiments testing the effect of biases on sentencing. To take a few examples, Englich and Mussweiler (2001) and Englich, Mussweiler, and Strack (2006) find sentences to be 20–33 percent higher, both for professionals and for laypeople, after being presented a high versus low sentence anchor. Ogloff and Vidmar (1994) find that mock jurors exposed to TV excerpts of testimonies of sexual abuse victims chose hypothetical sentences 30 percent higher than mock jurors exposed to less emotional narratives. The fact that our results are qualitatively similar to those obtained in lab experiments, but quantitatively smaller, is in line with the lab study of many other mechanisms (Dahl and DellaVigna 2009; see also Kessler and Vesterlund 2015).

TABLE 4
News Content and Sentence Length: Robustness Checks

| | | | Number of News Stories on Outcome | | | | | | |
| | | | | | | Sentence Length | | | |
	Appeal (1)	Log (Sentence) (2)	Without Trial in County of the News (3)	Trial Length Longer than 1 Day (4)	With Session Fixed Effects (5)	With Error Clustered per Day (6)	With County-Specific Time Trend (7)	With Media in Minutes (8)	Same Regression (9)
A. Effect of News about Perpetrated Felonies									
Perpetrated felonies $t-1$	−.00153	.00711***	23.41**	31.82**	60.11***	25.89**	25.45**	15.52**	24.96**
	(.00221)	(.00262)	(10.41)	(12.56)	(19.97)	(12.53)	(11.98)	(7.504)	(10.24)
Perpetrated felonies $t+1$	−.000649	−.000490	6.410	18.65	23.47	.77	−1.305	3.661	−.882
	(.00208)	(.00278)	(10.59)	(14.40)	(17.06)	(9.249)	(9.417)	(6.937)	(9.887)
p-value difference $t-1/t+1$	.752	.0502	.237	.464	.110	.113	.0870	.241	.0624
Felony other than perpetrated $t-1$									20.68
									(13.59)
Felony other than perpetrated $t+1$									23.23
									(14.64)
p-value difference $t-1/t+1$									.908
B. Effect of All News about Felonies									
Felony $t-1$	−.00189	.00577**	24.21***	33.51***	45.89***	23.94***	23.42**	11.08**	
	(.00187)	(.00227)	(8.409)	(9.462)	(15.72)	(9.413)	(9.114)	(5.552)	
Felony $t+1$	.00098	.000739	9.120	13.09	24.68*	6.833	6.131	6.040	
	(.00193)	(.00219)	(8.705)	(12.28)	(13.76)	(7.562)	(7.563)	(5.554)	
p-value difference $t-1/t+1$	.292	.132	.229	.230	.249	.174	.170	.453	

	C. Effect of News about Judicial Errors								
Judicial errors $t − 1$	−.00440	−.0130**	−40.09**	−23.87	−24.18	−35.65***	−35.65***	−23.20**	−39.57***
	(.00403)	(.00530)	(15.74)	(25.06)	(41.26)	(11.08)	(11.08)	(9.571)	(15.61)
Judicial errors $t + 1$	−.00167	.00322	2.985	6.197	16.49	4.394	4.394	1.236	5.039
	(.00230)	(.00383)	(13.31)	(21.90)	(36.18)	(8.104)	(8.104)	(7.816)	(13.38)
p-value difference $t − 1/t + 1$	.592	.0127	.0389	.461	.402	.00566	.00566	.0583	.0322
Observations	16,636	16,340	Varies	6,462	7,903	16,342	16,342	16,313	16,342
Mean	.149	8.058	Varies	3,654	3,619	3,656	3,656	3,656	3,656

SOURCE.—Authors' calculations based on criminal records, provided by the French Ministry of Justice, and data collected from the National Audiovisual Institute and from French criminal records.

NOTE.—Panel A presents the effect of coverage of perpetrated felonies, panel B of coverage of felonies, and panel C of coverage of judicial errors. Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. These estimates include controls for age, gender, nationality (French or other), length of pretrial detention, type of offense, number of prior convictions in the past 5 years, county, length of time between offense and trial, and dummies for month, day of week, year, and type of court (appellate court, court of first instance) except for col. 1. In col. 1, we look at the effect of media on the day before the first-instance verdict, and not on the final conviction. The sample includes all first-instance trials between 2004 and 2010 and no appellate trials. In col. 2, the outcome is log sentences. In col. 3, we exclude stories that took place within the same county as the trial, and so the sample size and outcome mean vary across panels. Panel A has 15,956 observations (mean sentence: 3,658 days), panel B has 15,856 observations (mean sentence: 3,655 days), and panel C has 16,331 observations (mean sentence: 3,655 days). In cols. 4 and 5, we include only data for which we have information on session length (subsample 1, as defined in online app. A), limiting our sample to trials that lasted 1 day in col. 4. Column 5 includes court session fixed effects. Standard errors are clustered at the day level in col. 6 and at the county level in other columns. Column 7 includes county-specific time trends. In col. 8, media is measured in minutes instead of in number of stories. Column 9 presents the effect of stories on felonies, felonies perpetrated, and judicial errors in a single regression. News stories are those covered on the 8:00 p.m. national television news on TF1 and France 2.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

TABLE 5
EVIDENCE ON MECHANISMS AND HETEROGENEITY OF EFFECTS

OUTCOME: SENTENCE LENGTH

	Dummy for News Coverage (1)	Controlling Crimes (Police) (2)	Controlling Crimes (Courts) (3)	Audience below Median (4)	Audience above Median (5)	Trial Length ≥ 2 Days (6)	Intensive and Extensive Margin (7)	Survey: Felt Unsafe in Your Neighborhood (8)
A. Effect of News about Perpetrated Felonies								
Perpetrated felony t − 1 (dummy)	86.10*** (24.33)						76.76** (35.29)	.0210** (.0103)
Perpetrated felony t + 1 (dummy)	10.81 (28.28)						15.97 (35.34)	−.00366 (.00833)
Perpetrated felonies t − 2						−1.374 (16.56)		
Perpetrated felonies t − 1		25.60** (10.09)	24.90** (10.08)	19.85 (15.75)	29.65* (14.93)	32.21** (13.99)	5.177 (14.58)	
Perpetrated felonies t + 1		.955 (9.762)	2.111 (9.814)	.894 (13.45)	−.891 (13.91)	18.72 (14.56)	−3.048 (12.59)	
Crimes: police measure		−3.137* (1.599)						
Crimes: court measure			.321 (38.15)					
p-value t − 1/t + 1 (dummy)	.0584						.254	.0845
p-value t − 1/t + 1 (continuous)		.0694	.0757	.378	.108	.461	.656	
p-value t − 1 audience					.639			
B. Effect of All News about Felonies								
Felony t − 1 (dummy)	91.86*** (25.76)						67.55* (37.87)	
Felony t + 1 (dummy)	28.82 (25.52)						17.19 (36.12)	

2156

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Felony $t-2$					−18.60		
					(12.40)		
Felonies $t-1$	23.89***	24.28***	17.72	27.07**	38.92***	11.01	.00186
	(7.737)	(7.575)	(11.87)	(11.72)	(10.69)	(11.57)	(.00747)
Felonies $t+1$	6.954	7.766	3.289	13.62	13.76	3.985	−.00363
	(8.131)	(7.895)	(13.19)	(11.24)	(12.36)	(11.45)	(.00711)
Crimes: police measure	−3.169*						
	(1.614)						
Crimes: court measure		−.156					
		(37.94)					
p-value $t-1/t+1$ (dummy)	.0791						
p-value $t-1/t+1$ (continuous)	.129	.110	.450	.410	.162	.669	
p-value $t-1$ audience			.574			.336	.637
Observations	16,342	15,926	7,378	8,041	6,462	16,342	50,749
	3,656	3,656	3,585	3,587	3,654	3,656	3,656
Mean							1.383

SOURCE.—Authors' calculations based on criminal records, provided by the French Ministry of Justice, and data collected from the National Audiovisual Institute and from French criminal records, from publicly available police statistics, and from a survey on victimization, conducted by the French National Institute of Statistics and Economic Studies (INSEE).

NOTE.—Panel A presents the effect of coverage of perpetrated felonies and panel B of felonies. Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. For judicial outcomes (cols. 1–7), estimates include controls for age, gender, nationality (French or other), length of pretrial detention, type of offense, number of prior convictions in the past 5 years, type of court (appellate court, court of first instance), county, length of time between offense and trial, and dummies for month, day of week, and year; standard errors in columns are clustered at the county level. In col. 1, we include a dummy for the presence of news stories on crimes. In col. 2, we include controls for the number of felonies per county and per month, measured using publicly available police data. In col. 3, we include controls for the number of crimes per county and per day that led to a conviction by 2015, as reflected in criminal records. We calculate this using the date of conviction that appears on criminal records, excluding cases tried on the first day of each month, since crimes are dated on the first day of the month if the exact date is unknown. In col. 4 (5), we limit our sample to cases tried on a day when the audience for the 8:00 p.m. news was below (above) the median in audiences for that period. In col. 6, we include only data for which we have information on session length (subsample 1, as defined in online app. A) and for which the trial lasted 2 days or more. In col. 7, we include a dummy for the presence of news stories on crimes and the number of news stories on crimes. Estimates in col. 8 are calculated for responses to a survey on perceived safety. The outcome is the answer to the question "Do you feel insecure in your neighborhood?" on a scale from 1 (never) to 4 (always). This column includes controls for day of the week and region. News stories are those covered on the 8:00 p.m. national television news on TF1 and France 2.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

The effects that we find are closer in magnitude to those found in field evaluations of the effect of extraneous factors on sentencing.[22] Eren and Mocan (2016) find that juvenile sentences increase by 35 days, or 6 percent, after "surprise losses" of the football team from the judge's alma mater. Abrams and Yoon (2007) find that an attorney with 11 years of experience will, on average, obtain sentences that are 1.2 months shorter than someone with only 1 year of experience, representing a 17 percent decrease in sentence length. Lim et al. (2015) find that a one standard deviation increase in coverage of court activities increases the average sentence length of nonpartisan elected judges for homicides, sexual assaults, and robberies by about 5.7 months (3.4 percent).

Average effects do not fully capture how media affect sentencing and, in particular, may not be the best way to interpret magnitudes. Figure 3 plots the sentence distribution, depending on whether there was some coverage of crime on TV on the day before the verdict or not. This graph indicates that, in France, sentences for crimes are full years of prison. So another way to interpret the average effects is that media content increases the likelihood that a person will be sentenced to a higher sentencing bracket of at least 1 year.[23]

Figure 3 also suggests that the difference in sentences after news about crime appears for longer sentences. This graphical intuition is confirmed using quantile regressions, using controls for offender characteristics, offense, and day of the week fixed effects (online app. C). These results suggest that media coverage of crime affects sentences in the top two-thirds of the distribution. We estimate, for example, that an additional news story increases the 90th percentile sentence by more than 150 days. Note that except for the top deciles of the treatment effect of news stories on crime, most of the quantile regression coefficients are not significantly different from one another when we include controls.

E.   Judicial Errors

In panel C of tables 3 and 4, we look at the effect of media coverage of judicial errors on sentencing. As with crimes, we find that news stories on judicial errors have no impact on convictions (table 3, panel C, cols. 1–4). Moving to sentence length, one additional story on judicial errors (table 3, panel C, cols. 5–8) decreases sentences by 40 days. Those results

---

[22] Many recent empirical papers look at convictions rather than sentence length (Anwar et al. 2012, 2014; Chen et al. 2016), and so we cannot directly compare magnitudes of effects. The same applies to a lot of the mock jury work, which focuses on convictions (for reviews, see Greene [1990] or Devine et al. [2001]).

[23] The average increase of prison sentences by 3 months after coverage of crime in fact means that one-quarter of defendants received an additional year in prison, if we assume that news on TV does not cause rank reversals in sentences.
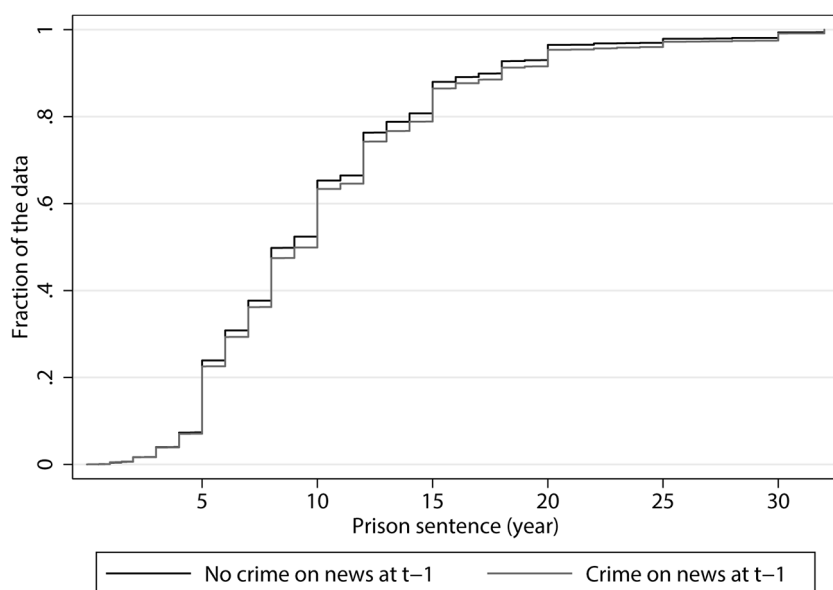
Fɪɢ. 3.—Distribution of sentence length by coverage of crime on TV. The dark line (light line) presents the cumulative fraction of defendants with a sentence shorter than any sentence length if there were any stories (no stories) on perpetrated felonies on the 8:00 p.m. national television news (TF1 and France 2) on the day before the verdict. Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. Source: Authors' calculations based on criminal records provided by the French Ministry of Justice and data collected from the National Audiovisual Institute and from French criminal records.

are robust to many different specifications (table 4, panel C). This suggests that news can sway jurors in both directions: toward more severity after coverage of crimes and less severity after coverage of judicial errors. In column 9 of table 4, we jointly test the effect on sentencing of news stories about perpetrated felonies, felonies other than perpetrated felonies (coverage of legal discussions and trials), and judicial errors. The effect of coverage of perpetrated felonies and judicial errors remains similar to what was observed before. The effect of news stories about "felonies other than perpetrated" is not significant.

However, as mentioned earlier, stories on judicial errors almost exclusively relate to the Outreau case, in which false testimonies resulted in several innocent suspects spending years in jail, and one defendant who even died while incarcerated. The Outreau miscarriage of justice was covered at various points in time (see app. fig. F1), and our identification exploits various moments when Outreau came up. However, this context does not allow us to say whether our results would hold for less

extreme cases of miscarriage of justice. By contrast, news about felonies is frequent and refers to many different events.

We now turn to mechanism. While the first-order results on judicial errors are robust, our analysis of mechanisms for judicial errors is not as robust. In the remainder of the paper, we focus on the effects of coverage of crime. Similar analyses for judicial error are presented in online appendix F.

V.   Mechanisms of Media Influence: Social Trends, Mood, or "Top of the Mind"?

The effect on sentencing decisions of exposure to news about crime and criminal justice could be explained by several mechanisms. Jurors might be responding to changes in the prevalence of crime, media coverage might alter perceptions of crime or criminal justice, media coverage could affect people's overall mood, or media coverage could increase what is "top of the mind" at a given moment.

In this section, we show that news coverage of crime, beyond increases or decreases in the prevalence of crime, affects judicial decisions. We further show that media content has a very short-lived effect: only news on the day before a verdict affects sentencing, but news further back in time does not. Using a nationally representative survey on perceptions of safety and security, we show that the same holds true for people's perception of insecurity. Finally, we show that this effect is not driven by mood changes: we find that coverage of other "bad news" does not affect sentencing.

A.   Crime versus Media Coverage of Crime

Are jurors responding to real changes in crime that are reflected in the news, or are they affected by crime showing up on the news? If coverage of felonies is correlated with frequency of crime, our results could be explained by two causal chains. When there is more crime (and greater TV coverage of crime), jurors might be more likely to have been victims or to know victims of crime, or be more generally concerned about longer-term trends in crime, and to reflect that in their sentencing choices. Alternatively, media coverage may increase the saliency or visibility of crime or trigger emotional responses. In that case, the effect would be driven by news coverage and not by the prevalence of crime.

A first way to explore whether people are reacting to crime firsthand or coverage of crime is to contrast the effect of stories on crimes within one's county or outside. As a reminder, column 3 of table 4 shows that point estimates do not change much when looking at crime stories that took place outside one's county. When further breaking out news "out-

side one's county" to news about crimes in adjacent counties or further away, the latter remains significant and similar to our main estimates.[24]

A second way to distinguish the effect of news from the effect of crime is to include controls for crime. As discussed in Section II.D, figure 2 shows that news on crime and crimes registered by the police were not correlated in the short run, giving us leverage to tease out the effect of crime versus coverage of crime. In columns 2 and 3 of table 5, we include measures of numbers of felonies. Column 2 controls for month by county number of felonies using police data, and column 3 controls for day by county numbers of crimes that led to a conviction, using court data (see Sec. II.D for more details on these measures). Controlling for crimes does not affect our media point estimates. The effect of local crime on sentences is open to more than one interpretation. Daily crime measured using court data has no effect on sentences, but monthly felonies measured using police data have a negative, marginally significant, effect on sentences.

B. Effects of Media Exposure

We next explore whether news stories covered when TV audiences are higher have a greater effect on sentences. We split TV audiences around the median and look at the effect of media on sentences when the verdict is after audiences below the median (col. 4) or above the median (col. 5). Effects are bigger and point estimates are always significant when audiences are above the median, while they are not when audiences are below the median. Although this difference in point estimates is not significant (the *p*-value is equal to .67 for the effect of news about perpetrated crime and .57 for all news about crime), this suggests that more exposure to news on crimes increases the likelihood of sentences responding to crime.[25] Note that audience rates could be correlated to what is covered on TV—for example, more people might watch the news if it covers horrible crimes—and so we cannot exclude that crimes covered on high audience days differ from those covered on low audience days.

---

[24] This result, consistent with the idea that media coverage, not crime, is affecting sentences, is presented in app. table E7. While estimates for coverage of crime within one's county or a neighboring county are imprecise (potentially because of the rarity of such events, shown in table E6), this indicates that our main results are not driven by coverage of crime to which a juror may directly or indirectly have been exposed.

[25] There are, on average, 15.1 million viewers when the audience is above the median and 12.6 million viewers when the audiences is below the median. In the "above-median" sample, the probability that at least one juror watched the 8:00 p.m. news the day before is 91 percent vs. 86 percent for the "below-median" sample. The average number of viewers among nine jury members is 2.1 in the above-median sample and 1.9 in the below-median sample.

*C.   Saliency versus Information Gathering:
     Short- and Long-Term Effects*

In the previous subsections, we showed that news coverage of crime matters independent of possible effects of trends in crime. Turning to an analysis of the duration of the effects, we show that our results are most consistent with short-lived changes in perceptions of crime.

Figure 4 presents coefficients for a regression that includes both leads and lags in news coverage of crime, relative to the date of the verdict. We find that only TV news on the day before a verdict affects sentencing: more news coverage of crimes at $t - 2$ or further back does not affect sentencing. One could argue that this pattern comes from the fact that judges and jury members pay attention to information only when it is rel-
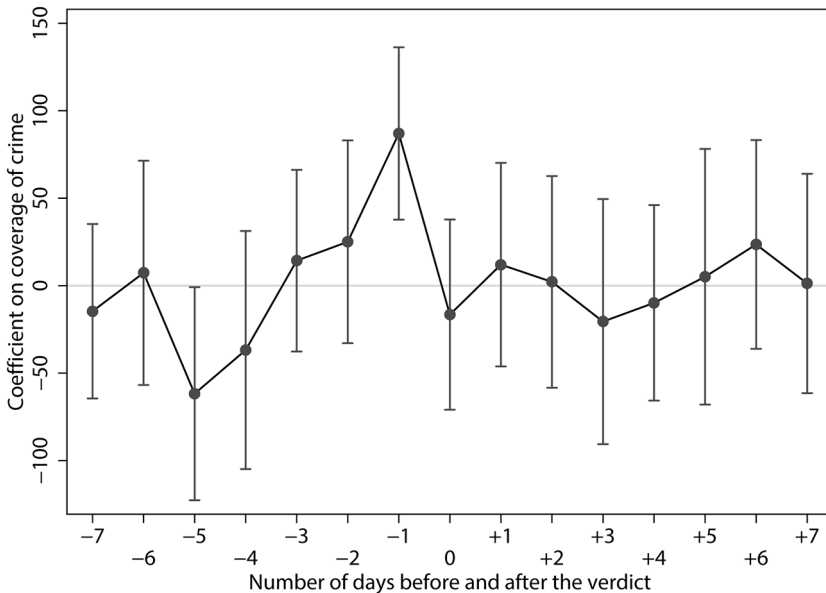
FIG. 4.—Duration of the effect of media coverage of crime on sentences: regression coefficients for perpetrated felonies, 7 days before and after sentencing. The measure for perpetrated felonies is a dummy equal to one if there were any news stories about crimes on the 8:00 p.m. national television news (TF1 and France 2). Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. The reported coefficients are for a single regression, which also includes controls for age, gender, nationality (dummy for French citizenship), length of pretrial detention, type of offense, number of prior convictions in the past 5 years, type of court (appellate court, court of first instance), county, length of time between offense and trial, and dummies for month, day of week, and year. Standard errors are clustered at the county level. Bars represent the 95 percent confidence interval. Source: Authors' calculations based on criminal records, provided by the French Ministry of Justice, and data collected from the National Audiovisual Institute and from French criminal records.

evant to their decision making, that is, when they have to make a judgment. We explore this in two ways.

First, if seeking to collect information before making a judicial decision, people would likely pay attention to news during the whole length of the trial and not just on the day before the verdict. Using the subsample of cases for which we have a trial start date and for which the trials lasts more than 2 days, we test this hypothesis by measuring the effect of news at $t - 1$ and $t - 2$ on the verdict. Results are presented in column 6 of table 5. We find that while news on the day before the verdict affects sentences, news stories during the trial but prior to the day before the verdict have no effect. The sample is smaller than our main sample, and the difference between news at $t - 2$ and news at $t - 1$ is not significant for news about perpetrated crimes (p-value = .3) but is significant at the 5 percent level for news on crime in general.

In order to further test the role of information gathering, we look at the effect of media on the intensive versus extensive margin. We regress sentences on a dummy equal to one if there was media coverage of crime on the day before the verdict and on the number of news stories (table 5, col. 7). All the effect comes from the extensive margin: while having been exposed to stories on crime before the verdict affects sentences, there is no additional contribution of each additional story. Assuming that each additional story would provide more information on crimes, this result is less consistent with learning and seems to us more consistent with news putting crime on top of people's mind.

D.   Effects of Crime News on Salience of Crime

To further uncover what mechanisms may be explaining our results, we use responses to a nationally representative survey on perceptions of public safety and insecurity from between 2008 and 2010. This allows us to explore whether media coverage of crime directly influences perceptions of crime. The survey takes 3–4 months to administer, and some people will be surveyed after more coverage of crime and others after less. We exploit this variation to see if media content affects responses. Results are presented in column 8 of table 5 and in figure 5. First, survey respondents declare more frequently that they feel insecure in their neighborhood on the day after news stories about perpetrated felonies. Moreover, this effect is limited in time; only news on the day before the survey affects responses. The fact that we replicate our main findings in this survey sheds light on mechanisms. First, a random sample of the population is affected by media content. This indicates that the effect of media on sentences cannot be fully explained by jurors paying more attention to news stories on crime because they are making judicial decisions, since people did not know ahead of time that they would be ad-
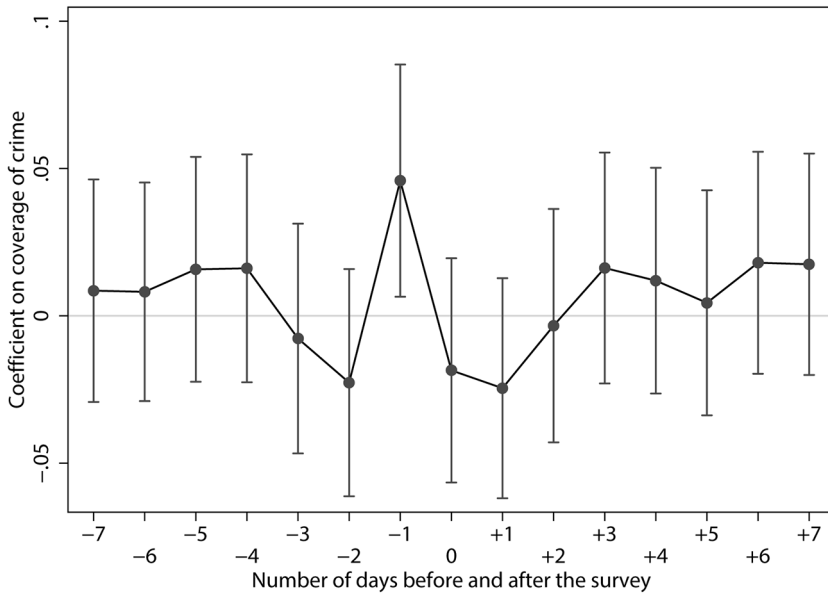
FIG. 5.—Duration of the effect of media coverage of crime on "feeling unsafe": regression coefficients for perpetrated felonies, 7 days before to 7 days after a survey of perceived insecurity. The measure for perpetrated felonies is a dummy equal to one if there were any news stories about crimes on the 8:00 p.m. national television news (TF1 and France 2). Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. The outcome is the answer to the question "Do you feel insecure in your neighborhood?" on a scale from 1 (never) to 4 (always). Coefficients are for a single ordered logit regression, which includes controls for day of the week and region. Bars represent the 90 percent confidence interval. Source: Authors' calculations using data from a survey on victimization conducted by the French National Institute of Statistics and Economic Studies (INSEE) and data collected from the National Audiovisual Institute.

ministered this survey and had no reason to factor in media content in their responses. Second, this makes it unlikely that the influence is fully mediated by judges or attorneys leveraging these news stories in their recommendations to the jury. This analysis of timing and the survey evidence suggest that the effect of media coverage of crime is mediated by crime news making crime more salient to jurors.

E. Bad News versus Crime-Specific Information

Several papers have documented the effect of irrelevant but upsetting information on people's behavior (Card and Dahl 2011; Chen 2017). News about crime could be just another type of upsetting news, and any bad news might increase sentence length because of the bad mood of an individual playing a role in determining the sentence.

In order to investigate the general effect of bad news on sentence length, we construct a database of keywords appearing more than 200 times between 2004 and 2010.[26] We report the number of stories that contain each keyword per day. Appendix table E1 presents the effect of several types of bad news at $t-1$ and $t+1$ on sentences. Columns 1–4 document the effect of news stories on strikes, natural disasters, social conflict, and unemployment,[27] while columns 5 and 6 present the effect of news about judicial error and murder—the two most common keywords in the judicial error and felony aggregates. For this subset of frequent keywords, only bad news related to criminal justice—crimes or judicial errors—has an effect on sentencing.[28]

F. Heterogeneous Effects by Type of Offense Tried and on TV

In table 6, we present the effect of news by type of crime covered and by type of offense tried.[29] Panel A presents the effect of news on all perpetrated felonies, by type of offense being tried; panel B distinguishes between news on sexual crimes (0.15 news story per day), news on murders (0.5 news story per day), and news on other kinds of crimes (0.26 news story per day). Column 1 presents results for all offenses being tried, and columns 2–5 present the effect of different news on sentences for different types of crimes: murder, aggravated assaults (including involuntary manslaughter and torture), sexual crimes, and property felonies (mainly armed robbery). Panel A shows that sexual crimes and property crimes are more influenced by news than violent crimes. Moving to column 1 of panel B, it appears that only news on sexual felonies perpetrated at $t-1$ appears to be significant. Even if coefficients are not significantly different from that for coverage of murders, sexual crimes seem to have a greater effect on sentencing.

Results by crime type (cols. 2–5) suggest that verdicts are not affected by news about crimes of the same type as the one judged. Stories on sexual crimes have significantly more effect on trials for violent crimes than

---

[26] Those keywords represent less than 2 percent of the keywords used in the database but around 60 percent of the occurrences (the vast majority of the keywords are used only once).

[27] These keywords have been chosen because they are used more than 1,000 times and capture bad news.

[28] The coefficients for the regression of sentences on news at $t-k$ for different values of k are presented in online app. H.

[29] Further explorations of heterogeneous treatment effects, in particular according to position of the news stories in the 8:00 p.m. news lineup and defendant and juror sociodemographic characteristics, are presented in online app. E. We find no significant differences relative to those factors.

TABLE 6
HETEROGENEITY OF THE EFFECT OF MEDIA ON SENTENCES, BY TYPE OF CRIME
COVERED ON TV AND BY TYPE OF OFFENSE BEING TRIED

	OUTCOME: SENTENCE LENGTH				
	All Trials (1)	Murder (2)	Aggravated Assault (3)	Sexual Crimes (4)	Property Crimes (5)
	A. Effect of News about Perpetrated Felonies				
Perpetrated felonies $t-1$	25.82**	30.37	−32.52	27.99**	57.64**
	(9.992)	(37.72)	(37.84)	(13.41)	(24.22)
Perpetrated felonies $t+1$	0.770	6.638	9.781	−7.337	10.19
	(9.885)	(33.72)	(26.57)	(12.65)	(24.07)
p-value difference $t+1$	.0651	.559	.369	.0649	.258
	B. Effect of News about Perpetrated Felonies, by Type of Crime Covered				
Number of news stories on perpetrated felonies at $t-1$ on:					
Murder	14.71	−39.70	−89.97**	41.07**	72.63*
	(13.63)	(40.72)	(41.99)	(19.43)	(43.65)
Sexual crimes	56.24*	129.2	166.5**	10.70	27.62
	(32.64)	(107.2)	(82.30)	(34.27)	(54.88)
Other	21.90	97.44	−74.92	15.71	42.56
	(16.85)	(63.17)	(64.10)	(22.22)	(29.75)
Number of news stories on perpetrated felonies at $t+1$ on:					
Murder	−16.93	−20.33	−5.223	−23.68*	−10.65
	(14.03)	(42.30)	(32.73)	(13.26)	(39.45)
Sexual crimes	21.53	116.3	−20.54	15.93	12.53
	(23.24)	(85.96)	(74.86)	(29.95)	(52.45)
Other	18.62	−.639	71.87	.130	36.77
	(20.47)	(82.21)	(59.37)	(23.02)	(31.48)
Observations	16,342	2,913	1,983	7,661	3,529
Mean	3,656	5,455	3,209	3,416	2,968

NOTE.—The outcome in all regressions is sentence length in days. Panel A presents the effect of coverage of perpetrated felonies and panel B of felonies. Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. These estimates are calculated using all criminal records and include controls for age, gender, nationality (French or other), length of pretrial detention, type of offense, number of prior convictions in the past 5 years, type of court (appellate court, court of first instance), county, length of time between offense and trial, and dummies for month, day of week, and year. News stories are those covered on the 8:00 p.m. national television news on TF1 and France 2. Standard errors are clustered at the county level.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

stories on murders. The opposite is true for trials for sexual crimes, though the difference is not significant.

News stories about murders have no effect on sentences for trials for murder (col. 2) but have a negative effect on aggravated assaults (col. 3). This could come from a contrast effect. Indeed, crimes aggregated in column 3 are similar to but less severe than the murders covered by the news.

VI.   Professional Judges and Media

Thus far, our analyses reflect decisions made by a jury that includes civilian jurors who have no particular legal or criminal justice experience. Now we ask, how do media affect decisions made by professional judges alone? On the one hand, media might sway civilian jurors particularly, because they have little experience from which to draw other reference points. On the other hand, a lengthy literature has shown that professional judges' decisions are affected by extraneous factors. Ideally, we would like to compare how sentences vary by news coverage for similar cases, depending on whether a case is judged by only judges or also by a jury that includes civilians. Unfortunately, such a setup does not exist, so we present different strategies to get at this question.

Our first strategy is to look at misdemeanors, which professional judges examine alone. We focus on the most severe misdemeanors but acknowledge that differential effects might be driven by the difference in cases examined. We then turn to juvenile procedures: before 16, youths are tried in juvenile court (with no lay jurors), and after age 16, they are tried in criminal court for felonies. We can exploit this discontinuity to contrast the effects of media on professionals versus laypeople. Finally, we compare first-instance proceedings to appeals, the latter having more experienced teams of judges and attorneys.

Columns 1 and 2 of table 7 present characteristics of cases tried in corrections courts, for all severe violent misdemeanors (defined as crimes for which the maximum sentence in the French Criminal Code is at least 7 years; col. 1) and for the most severe sexual misdemeanors (sexual aggressions for which the maximum sentence in the French Criminal Code is at least 7 years; col. 2).[30] Defendants are similar to criminal court defendants in terms of gender and nationality (see table 1 for comparison), but they tend to be younger (35.4 years old vs. 38.6 years old). Investigation length is shorter (3 years vs. 5 years), and sentences are much shorter (1 year vs. 10 years), especially when focusing on prison sentences, excluding suspended sentences.[31]

---

[30] In app. table G1, we provide statistics on all cases judged in corrections courts. Many rapes are requalified as "sexual aggressions" because of lack of evidence or in order to avoid long investigations and trials. In recent years, several bills were introduced to reduce this practice and ensure that all rapes are tried in criminal courts (see Senate Bill 249, on January 11, 2012, or Senate Bill 368, on February 13, 2014). These cases are therefore cases that legislators are moving to see tried more consistently in criminal court.

[31] In corrections courts, sentences are often a mix of a prison sentence and a suspended sentence (e.g., 6 months of imprisonment and 18 months of suspended sentence). Using our sample on corrections courts, we calculate that, on average, one-third of the total sentence is suspended. Only 20 percent of defendants have a sentence that is only imprisonment, with no suspended sentence. By contrast, in criminal court, on average, 90 percent of the sentence length is imprisonment, and 84 percent of sentences contain only imprisonment with no suspended prison time.

TABLE 7
JURORS VS. PROFESSIONAL JUDGES: DESCRIPTIVE STATISTICS

	SAMPLE: CORRECTIONS COURTS (Professional Judges Only)		SAMPLE: JUVENILES					
	All Severe Misdemeanors (1)	Severe Sexual Misdemeanors (2)	Felonies in Juvenile Court (No Jurors) (3)	Juveniles in Criminal Court (Jurors) (4)	Diff. Col. 3 and Col. 4: p-Value (5)	15 Years Old (No Jurors) (6)	16 Years Old (Jurors) (7)	Diff. Col. 6 and Col. 7: p-Value (8)
Number of cases per year	5,051	2,977	316	263	.09	85	121	.17
Male	.92	.95	.97	.97	0	.96	.97	.38
French	.84	.85	.97	.94	0	.96	.95	0
Age (at crime)	35.48	39.6	13.98	17.01		15	16	0
Investigation length (days)	1,129	1,645	1,851	1,752	.04	1,426	2,005	0
Sentence (including suspended)	596	679	800	2,068	0	978	1,809	0
Sentence in prison	290	317	170	1,478	0	331	1,141	0
Murder and violence	.41	0	.05	.17	0	.09	.13	.013
Sexual assaults	.59	1	.83	.53	0	.7	.69	.72
Armed robbery	0	0	.12	.3	0	.21	.18	.11
Observations	35,358	20,842	2,212	1,842		594	845	

NOTE.—Descriptive statistics on corrections courts and on felonies judged in juvenile court or in juvenile criminal court. We define "severe misdemeanors" as crimes for which the maximum sentence in the French Criminal Code is at least 7 years. We define "severe sexual misdemeanors" as sexual offenses for which the maximum sentence in the French Criminal Code is at least 7 years.

Columns 1 and 2 of table 8 present the effect of media on correctional case outcomes. As sentences and imprisonment are usually different in correctional courts, we present the effect of media on the two different outcomes: the effect of media on overall sentence length is presented in panel A, and the effect of media on imprisonment is presented in panel B. We find that there is no effect of media coverage on either the most severe misdemeanors (col. 1) or severe sexual misdemeanors (col. 2). Not only are the coefficients nonsignificant; they are also very small, even in terms relative to the average sentence (less than 0.4 percent). Given our sample size, we have a well-estimated zero effect. Once again, we cannot rule out the fact that this difference is due to differences in the kind of offenses being tried, but this does suggest that decisions by professionals are not affected by media.

Trials for juveniles in France offer a potentially cleaner research design. For felony offenses, age at offense determines whether a case is judged in juvenile court or in criminal court: before 16, youths are judged in juvenile court, and between 16 and 18 years old they are judged in juvenile criminal court, which includes nine civilian jurors.[32] The data confirm that this rule holds empirically: 98.5 percent of felony offenders less than 16 years old are tried in juvenile court, and 95 percent of youths more than 16 years old are tried in criminal court, though not as adults. This setup allows us to compare similar cases, which based on the age of the defendant will be judged by a jury including laypeople. To be charged as felonies, offenses committed by juveniles have to be very severe. Columns 3–8 of table 7 compare characteristics of juveniles tried in criminal court versus juvenile court. While offending profiles are different (cols. 3 and 4) for the overall sample, when we limit ourselves to 15- and 16-year-olds (cols. 6 and 7), the crime structure is similar (around 70 percent of sexual crimes), investigations are always very long, and sociodemographic statuses are similar (col. 8 presents the p-value of the difference). The downside of this strategy, however, is that the sample is much smaller, with 2,212 youths tried in juvenile court and 1,842 youths tried in juvenile criminal court between 2004 and 2010.[33]

In general, a juvenile's maximum sentence is half that of an adult; but if a juvenile offended between ages 16 and 18, a court can decide not to

---

[32] Criminal courts for juveniles include nine jury members and three professional judges (like criminal courts for adults). In contrast to adult criminal courts, two magistrate assistants have to be specialized in juvenile crimes, and the trial is not public. The criminal court for juveniles also judges adults who committed crimes with juveniles. In our main specifications, we include only young adults (under 21). This threshold corresponds to the age after which prison time must be served in adult facilities. We provide more information on juvenile courts in online app. G.

[33] We exclude from the analysis offenders who were below the age of 13 (no prison sentence possible) or above 21 (necessarily incarcerated in adult facilities, even if judged and convicted together with other juveniles). We also exclude judicial decisions that are not made the day of the trial (only 57 cases), since we cannot precisely date the verdict.

TABLE 8
EFFECTS OF MEDIA, DEPENDING ON THE PRESENCE OF JURIES OF LAYPEOPLE: CORRECTIONS COURTS AND JUVENILES

OUTCOME: SENTENCE LENGTH

| | Sample: Corrections Courts (Professional Judges Only) | | Sample: Juveniles | | | |
	All Severe Misdemeanors (1)	Severe Sexual Misdemeanors (2)	Felonies in Juvenile Court (No Jurors) (3)	Juveniles in Criminal Court (Jurors) (4)	15 Years Old (No Jurors) (5)	16 Years Old (Jurors) (6)
A. Effect of News about Perpetrated Felonies on Sentences (Including Suspended)						
Perpetrated felony $t-1$ (dummy)	1.534	−2.532	34.75	146.9**	68.18	107.9*
	(4.814)	(6.085)	(28.69)	(69.40)	(64.04)	(63.80)
Perpetrated felony $t+1$ (dummy)	2.643	2.162	−20.57	−24.76	−94.51*	−120.4
	(4.509)	(6.243)	(26.14)	(94.41)	(53.60)	(76.22)
p-value $t-1/t+1$	.880	.588	.167	.172	.0878	.0397
p-value comparison $t-1$			.061		.670	
Mean sentence (including suspended)	595.8	679	800	2,068	978	1,809

		B. Effect of News about Perpetrated Felonies on Sentence in Prison			
Felony $t-1$ (dummy)	.883	10.58	165.8*	12.90	69.73
	(3.657)	(18.23)	(94.45)	(60.23)	(81.74)
Felony $t+1$ (dummy)	−2.592	20.33	−22.12	−78.73	−47.16
	(3.445)	(20.79)	(125.4)	(56.30)	(97.18)
p-value $t-1/t+1$	.511	.342	.269	.361	.395
p-value comparison $t-1$		.0802		.603	
Mean sentence in prison	290.3	170.4	1,478	331.3	1,141
Observations	35,358	2,212	1,842	594	845

NOTE.—The outcome in all regressions is sentence length in days. In panel A, the outcome is sentence length (including suspended sentences). In panel B, the outcome is the imprisonment sentence. All regressions include controls for day of week. Columns 1 and 2 contains additional controls for age, gender, nationality (French or other), length of pretrial detention, type of offense, number of prior convictions in the past 5 years, county, length of time between offense and trial, and dummies for month, year, and type of court (appellate court, court of first instance). We define "severe misdemeanors" as crimes for which the maximum sentence in the French Criminal Code is at least 7 years. We define "severe sexual misdemeanors" as sexual aggressions for which the maximum sentence in the French Criminal Code is at least 7 years. News stories are those covered on the 8:00 p.m. national television news on TF1 and France 2. Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. Standard errors are clustered at the county level.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

apply this rule. Another major difference is in the fraction of the sentence that is suspended: table 7 shows that in juvenile court, 22 percent of one's sentence length is imprisonment (the remainder being a suspended sentence) versus 74 percent in juvenile criminal court. Sentences are about 1.8 times longer in juvenile criminal court, compared to juvenile court. Because of deterrence, this change in expected sentences could induce differences in unobserved characteristics. While we cannot directly speak to this, the deterrence effect of the increase in sentence time among juveniles seems to be quite low (Lee and McCrary 2005). Note also that at least for adults, we do not find the effect of media on sentences to change a lot with observable offender characteristics (see app. table E3); so even if youths are unobservably different across courts, this exploration of heterogeneity for adults suggests that it would not affect our estimates.

Results are presented in columns 3–6 of table 8. Panel A presents results for sentences including suspended sentences, and panel B presents results for imprisonment sentences only. Columns 3 and 4 show the effect of media on sentences in juvenile criminal court (with civilian jurors), using the number of news stories on crime or a dummy for crime being covered. As with adults, sentences are longer after more coverage of crimes. Even if the sample is small, the effect of media on the sentences for juveniles judged by jurors is significant and similar in magnitude to that presented in Section III. However, the results are different in juvenile court, where there are only professional judges, as shown in column 4: estimates are small in magnitude and not significant.[34] The difference between the effect in criminal court and the effect in juvenile court is significant when we use dummies (1 percent level for news about perpetrated crime, 10 percent level when using all news about crime).

Columns 5 and 6 compare more specifically the effect among offenders at the 16-year-old age cutoff. The effects of media are less clean, as the samples get very small. However, they remain sizable and significant (for news about perpetrated felonies) for 16-year-old offenders, while they are not for 15-year-olds. These results are more imprecise, and the difference between the effects on prison sentences at 16 and 15 years old is not significant (p-value = .596 for sentences including suspended sentences; p-value = .603 for imprisonment only).

The full distribution of the effect broken out by age at infraction is presented in figure 6. While the estimates are noisy because of the small sample sizes, the general picture is similar to the one presented in columns 3–6 of table 8. Media coverage of crime does not affect sentences for offenders who were younger than 16 when offending and so were

---

[34] Some coefficients for $t + 1$ are significant. However, they are marginally significant (10 percent), are not robust, and go in the opposite direction of those found throughout the paper.
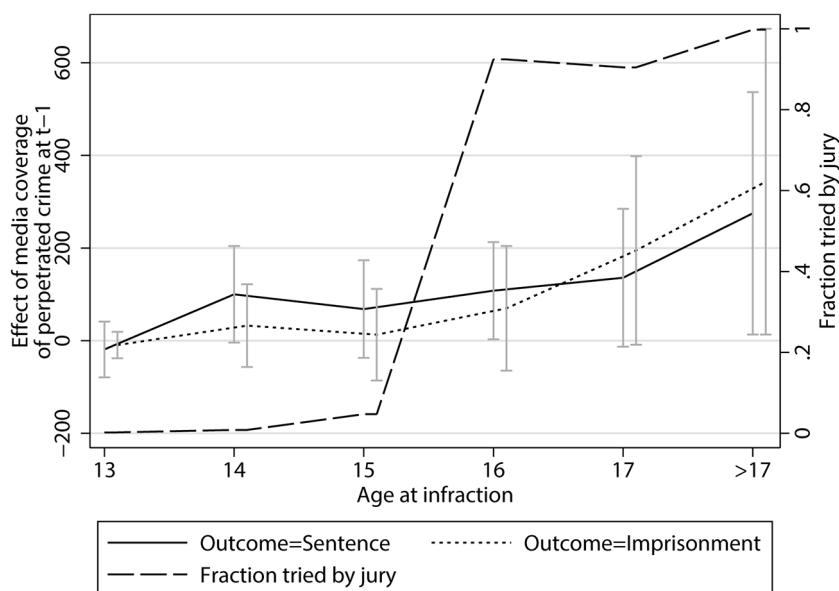
FIG. 6.—Effect of the coverage of perpetrated felonies on sentencing, by age of the defendant and presence of lay jurors. This figure presents the coefficient of news on perpetrated felonies on two outcomes: sentence length, including suspended sentences (solid line), and incarceration length (dotted line). We consider all youths judged in both juvenile court and criminal court who were no more than 21 years old when they committed the offense. The measure for perpetrated felonies is a dummy equal to one if there were any news stories about perpetrated felonies on the 8:00 p.m. national television news (TF1 and France 2). Stories on perpetrated felonies are stories about crimes that do not mention trials or legislation. The dashed line presents the fraction of trials judged by a jury (right axis). Each coefficient is for a separate regression, which also includes controls for sociodemographic and crime characteristics. Bars represent the 90 percent confidence interval. Source: Authors' calculations based on criminal records, provided by the French Ministry of Justice, and data collected from the National Audiovisual Institute and from French criminal records.

tried only by professional judges. Point estimates for prison sentences (excluding suspended sentences) are especially small. Conversely, the effect of media is significant or close to significant for offenders aged 16 or more.

Finally, we compare outcomes for first-instance proceedings and appellate courts separately. Procedural rules are slightly different in the two contexts. There are more jury members (nine in first-instance proceedings and 12 in appellate courts at the time of this study). Appellate courts judge crimes that were not committed in the county. Judges and attorneys working in appellate courts are usually more experienced than those in first-instance courts. Finally, appellate court cases have already been judged, and so juries already have a reference point regarding sentences.

Results presented in appendix table E2 indicate that news stories on crimes have no effect on sentence decisions in appeal courts. This could be due to more careful deliberations for appellate decisions, to the existence of a reference point provided by the preceding decision, or to the presence of more experienced professionals, who can guide jurors more effectively to ignore the news.

Overall, these three sets of results suggest that the effects of media on sentences are not present when there are professional judges or when these are more experienced. While we cannot rule out fully that these differences in media effects are due to differences in case contents or in the probability that one member of the court has seen the news,[35] these results over different contexts suggest that at least in the legal profession, experience can reduce the effect of media on decision making.

VII.   Conclusion

In this paper, we show that news content affects criminal justice decisions: sentences in jury trials are longer following more coverage of crime and shorter after coverage of judicial errors. We find that only media coverage of crime and criminal justice (not of other upsetting topics) affects sentences and that only coverage of crimes on the day before a sentence is handed down (not on other prior days) affects sentencing decisions. By contrast, we find no effect of media on professional judges' sentencing decisions.

Our results are important from a policy perspective. Understanding biases in judicial decisions is crucial, since the right to a fair trial is an essential feature of democratic justice systems. Our main result that sentences, but not convictions, are affected by media is in line with the finding in Kahneman, Schkade, and Sunstein (1998) of "shared outrage but erratic awards" in punitive damages. Understanding differences between decision making by judges and by laypeople is important, as jury trials are costly and there are debates as to what jurisdictions they should be present in.

Recent papers have identified several biases that judges might be subject to, such as mental depletion (Danziger et al. 2011), gambler's fallacy (Chen et al. 2016), or mood of the day (Chen 2017). Our paper suggests that professional expertise can limit the effect of media biases, which otherwise have been shown to matter for many behaviors (and, in our case, for juries that include laypeople). This could be a reason for the French judicial system to include laypeople only in conviction decisions and not in sentencing decisions if these are more susceptible to external factors.

---

[35] In criminal courts, the court is composed of 12 people, but only of three people in juvenile and in correctional courts. It may also be the case that judges are less likely to watch the 8:00 p.m. news.

The diffusion of this result during juror training could be a good way to reduce the problem; however, we should be mindful of the potential trade-off of a "boomerang effect": drawing jurors' attention to potential biases may increase sensitivity to coverage of crime and justice. Overall, these results indicate that juries of laypeople might lead to greater noise in outcomes of trials if, beyond behavioral biases, the current-events context also affects sentencing.

By investigating the effect of media on sentencing in a jury trial, we are able to look at a particularly localized, contextual outcome. For behaviors like voting, citizens might be actively looking for information in the media. Conversely, elected judges might be looking to influence the media through their decisions. Jurors, on the other hand, are plausibly neither seeking feedback nor trying to influence other outcomes beyond the trial in which they are serving. Yet even in this setting, we find that media affect jurors' decisions, underscoring the contextual influences of media.

Appendix

Keywords Used in Our Definitions of Crimes and Criminal Justice Stories

Table A1 shows an example of what the raw media data look like. For each story on the 8:00 p.m. news, the National Audiovisual Institute provides its title, length, time of broadcast, and a list of keywords associated with that story. We use these keywords to determine what stories cover crime and judicial errors.

Below is the list of words that we used to define the aggregate measures of media coverage of crime and criminal justice, with number of days that these words appear in parentheses.

1. Crime: enfant (Mathias) (20); Mouzin Estelle (26); Evrard Francis (26); crime (sexuel) (26); enfant (Valentin) (28); enfant (Jonathan) (31); enfant (Antoine) (36); bandit (39); inceste (40); Bodein Pierre (41); Fourniret Michel (44); cadavre (48); Louis Emile (50); gang (Gang des barbares) (57); crime (68); infanticide (76); bagarre (89) (if used with " décès"); prise d'otage (93); Treiber Jean Pierre (96); séquestration (98); Giraud Géraldine (99); banditisme (113); Erignac Claude (113); Colonna Yvan (117); meurtrier (127); assassinat politique (134); fusillade (162); holdup (200); viol (321); moeurs (334); enlèvement (335); pédophilie (522); meurtre (1435); violence (1620) (if used with the word death).
2. Judicial errors: erreur judiciaire (235); Outreau (262); réhabilitation judiciaire (36); Burgaud Fabrice (58).
3. Trial: reconstitution judiciaire (20); audience-procès (21); réquisition (29); conseil d'Etat (29); procédure d'appel (31); justice (recours) (35); Cour de cassation (37); palais de justice (38); accuse (39); non lieu (39); tribunal de grande instance (49); relaxe (58); acquittement (100); cour d'appel (112);

tribunal correctionnel (123); tribunal (128); verdict (300); cour d'assises (372); prison (402); procès (2173)

4. Law: projet de loi (anticipation et prévention des conflits) (22); parlementaire (24); gouvernement (Fillon, 4eme) (24); gouvernement (Fillon, 2eme) (26); parlement (28); gouvernement (Fillon, 3eme) (28); projet de loi (cohésion sociale) (33); loi (relatif aux libertés des universités) (44); amendement (63); débat parlementaire (66); gouvernement (Fillon) (66); droit pénal (91); député (93); sénat (114); Assemblée nationale (401); loi (599); projet de loi (1032).

TABLE A1
EXAMPLE OF THE STRUCTURE OF RAW MEDIA DATA, FOR THREE DIFFERENT NEWS STORIES

Title	Channel	Length	Keywords	Day	Time
[Barack Obama elected president of the United States]	TF1	00:02:19	Presidential election; United States; Obama Barack; Democratic Party—United States; domestic policy; election results	05/11/08	08:01:15
[A family murder in Hendaye]	TF1	00:01:25	Confessions; child; family; wife; resident (neighbor); Hendaye; murder; father; Pyrenees Atlantiques; testimony	02/16/04	08:12:22
[Meeting between Dominique Perben and the Outreau acquitted]	TF1	00:01:22	Acquittal; judicial error; compensation; Outreau; pedophilia; judicial rehabilitation; meeting	09/27/04	08:00:54

References

Abrams, David S., and Albert H. Yoon. 2007. "The Luck of the Draw: Using Random Case Assignment to Investigate Attorney Ability." *Univ. Chicago Law Rev.* 74 (4): 1145–77.

Anwar, Shamena, Patrick Bayer, and Randi Hjalmarsson. 2012. "The Impact of Jury Race in Criminal Trials." *Q.J.E.* 127 (2): 1017–55.

———. 2014. "The Role of Age in Jury Selection and Trial Outcomes." *J. Law and Econ.* 57 (4): 1001–30.

———. 2015. "Politics in the Courtroom: Political Ideology and Jury Decision Making." Working Paper no. 21145, NBER, Cambridge, MA.

Cagé, Julia. 2013. "Media Competition, Information Provision and Political Participation." Working paper, Harvard Univ.

Card, David, and Gordon B. Dahl. 2011. "Family Violence and Football: The Effect of Unexpected Emotional Cues on Violent Behavior." *Q.J.E.* 126 (1): 103–43.

Chaussebourg, Laure, and Sonia Lumbroso. 2008. "L'appel des décisions des cours d'assises: Conséquences sur la déclaration de culpabilité." *Infostat Justice* 100.

Chen, Daniel L. 2017. "Mood and the Malleability of Moral Reasoning." Manuscript, Soc. Sci., Res. Network. https://ssrn.com/abstract=2740485.

Chen, Daniel L., Tobias J. Moskowitz, and Kelly Shue. 2016. "Decision Making under the Gambler's Fallacy: Evidence from Asylum Judges, Loan Officers, and Baseball Umpires." *Q.J.E.* 131 (3): 1181–1242.

Dahl, Gordon, and Stefano DellaVigna. 2009. "Does Movie Violence Increase Violent Crime?" *Q.J.E.* 124 (2): 677–734.

Danziger, Shai, Jonathan Levav, and Liora Avnaim-Pesso. 2011. "Extraneous Factors in Judicial Decisions." *Proc. Nat. Acad. Sci.* 108 (17): 6889–92.

DellaVigna, Stefano, and Ethan Kaplan. 2007. "The Fox News Effect: Media Bias and Voting." *Q.J.E.* 122 (3): 1187–1234.

DellaVigna, Stefano, and Eliana La Ferrara. 2015. "Economic and Social Impacts of the Media." Working Paper no. 21360, NBER, Cambridge, MA.

Devine, Dennis J., Laura D. Clayton, Benjamin B. Dunford, Rasmy Seying, and Jennifer Pryce. 2001. "Jury Decision Making: 45 Years of Empirical Research on Deliberating Groups." *Psychology, Public Policy, and Law* 7 (3): 622–727.

Dowler, Kenneth. 2003. "Media Consumption and Public Attitudes toward Crime and Justice: The Relationship between Fear of Crime, Punitive Attitudes, and Perceived Police Effectiveness." *J. Criminal Justice and Popular Culture* 10 (2): 109–26.

Englich, Birte, and Thomas Mussweiler. 2001. "Sentencing under Uncertainty: Anchoring Effects in the Courtroom." *J. Appl. Soc. Psychology* 31 (7): 1535–51.

Englich, Birte, Thomas Mussweiler, and Fritz Strack. 2006. "Playing Dice with Criminal Sentences: The Influence of Irrelevant Anchors on Experts' Judicial Decision Making." *Personality and Soc. Psychology Bull.* 32 (2): 188–200.

Enikolopov, Ruben, Maria Petrova, and Ekaterina Zhuravskaya. 2011. "Media and Political Persuasion: Evidence from Russia." *A.E.R.* 101 (7): 3253–85.

Eren, Ozkan, and Naci Mocan. 2016. "Emotional Judges and Unlucky Juveniles." Working Paper no. 22611, NBER, Cambridge, MA.

Ferraz, Claudio, and Frederica Finan. 2008. "Exposing Corrupt Politicians: The Effects of Brazil's Publicly Released Audits on Electoral Outcomes." *Q.J.E.* 123 (2): 703–45.

Gazal-Ayal, Oren, and Raanan Sulitzeanu-Kenan. 2010. "Let My People Go: Ethnic In-Group Bias in Judicial Decisions—Evidence from a Randomized Natural Experiment." *J. Empirical Legal Studies* 7 (3): 403–28.

Gentzkow, Matthew. 2006. "Television and Voter Turnout." *Q.J.E.* 121 (3): 931–72.

Gerber, Alan S., Dean Karlan, and Daniel Bergan. 2009. "Does the Media Matter? A Field Experiment Measuring the Effect of Newspapers on Voting Behavior and Political Opinions." *American Econ. J.: Appl. Econ.* 1 (2): 35–52.

Greene, Edith. 1990. "Media Effects on Jurors." *Law and Human Behavior* 14 (5): 439–50.

Kahneman, Daniel, David Schkade, and Cass Sunstein. 1998. "Shared Outrage and Erratic Awards: The Psychology of Punitive Damages." *J. Risk and Uncertainty* 16 (1): 49–86.

Kearney, Melissa S., and Phillip B. Levine. 2015. "Media Influences on Social Outcomes: The Impact of MTV's *16 and Pregnant* on Teen Childbearing." *A.E.R.* 105 (12): 3597–3632.

Kessler, Judd, and Lise Vesterlund. 2015. "The External Validity of Laboratory Experiments: The Misleading Emphasis on Quantitative Effects." In *The Handbook of Experimental Economic Methodology*, edited by Guillaume Fréchette and Andrew Schotter. New York: Oxford Univ. Press.

La Ferrara, Eliana, Alberto Chong, and Suzanne Duryea. 2012. "Soap Operas and Fertility: Evidence from Brazil." *American Econ. J.: Appl. Econ.* 4 (4): 1–31.

Lee, David S. 2009. "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects." *Rev. Econ. Studies* 76 (3): 1071–1102.

Lee, David S., and Justin McCrary. 2005. "Crime, Punishment, and Myopia." Working Paper no. 11491, NBER, Cambridge, MA.

Lim, Claire S. 2015. "Media Influence on Courts: Evidence from Civil Case Adjudication." *American Law and Econ. Rev.* 17 (1): 87–126.

Lim, Claire S. H., James M. Snyder Jr., and David Strömberg. 2015. "The Judge, the Politician, and the Press: Newspaper Coverage and Criminal Sentencing across Electoral Systems." *American Econ. J.: Appl. Econ.* 7 (4): 103–35.

Ministère de la Justice. 2012. *Annuaire statistique de la Justice: Édition 2011–2012.* http://www.justice.gouv.fr/art_pix/stat_annuaire_2011–2012.pdf.

Ministry of Justice. 2013. *Criminal Justice Statistics: Quarterly Update to September 2012.* https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/220090/criminal-justice-stats-sept-2012.pdf.

Ogloff, James, and Neil Vidmar. 1994. "The Impact of Pretrial Publicity on Jurors: A Study to Compare the Relative Effects of Television and Print Media in a Child Sex Abuse Case." *Law and Human Behavior* 18 (5): 507–25.

Ray, Debraj, and Arthur Robson. 2016. "Certified Random: A New Order for Coauthorship." Working Paper no. 22602, NBER, Cambridge, MA.

Reaves, Brian A. 2013. "Felony Defendants in Large Urban Counties, 2009—Statistical Tables." US Dept. Justice, Bur. Justice Statis., Washington, DC.

Shayo, Moses, and Assaf Zussman. 2011. "Judicial Ingroup Bias in the Shadow of Terrorism." *Q.J.E.* 126 (3): 1447–84.

Snyder, James M., and David Strömberg. 2010. "Press Coverage and Political Accountability." *J.P.E.* 118 (2): 355–408.

Surette, Ray. 2014. *Media, Crime, and Criminal Justice.* Stamford, CT: Cengage Learning.

Yanagizawa-Drott, David. 2014. "Propaganda and Conflict: Evidence from the Rwandan Genocide." *Q.J.E.* 129 (4): 1947–94.