

“No hatred or malice, fear or affection”:

Media and sentencing

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Abstract: This paper investigates the effects of coverage of crime and criminal justice on sentencing outcomes, depending on the trial setting, and in particular presence of laypeople versus only experts. We look at the effect of television broadcasting of crime and criminal justice stories on sentencing decisions in criminal courts composed only of professional judges or including civilian jurors. We find that media content on the day before a trial affects sentencing with civilian jurors: sentences are longer after more coverage of crimes, and shorter after stories on judicial errors. However, we find no effect of media coverage on professional judges. This effect is driven by TV exposure to crime: crimes per se have no effect. Media seems to affect jurors' decisions very circumstantially, by making crime more salient, rather than by capturing deeper social currents or reflecting changes in levels of crime: the effects do not last over time. This paper contributes to the analysis of how context affects judicial decisions, and helps distinguish how different types of extraneous factors affect lay people versus experts: unlike for other psychological biases (priming, gambler's fallacy, mental depletion...), experience can help mitigate the effects of domain-pertinent but irrelevant external information, as conveyed by media.

Keywords: courts, media, sentencing, crime, judicial decision, cognitive bias

JEL codes: D83, K4, K14, L82

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1. Introduction

How does television coverage of crime and criminal justice affect sentencing decisions in different institutional settings? The presence, content, and biases of media have been shown to affect many important behaviors, including voting, corruption, or political accountability. In the legal sphere and turning to judges' behaviors, recent empirical evidence has supported theories of legal realism, which posit that psychological or contextual factors might affect sentences, beyond just the facts being examined. At the intersection of these two literatures, we investigate the effect of television coverage of crime and justice on sentencing decisions, depending on the institutional setting.

Jurors play an important role in the criminal justice, representing civil society and capturing its views, thus adding legitimacy to criminal justice. They are expected to deliver impartial judgment, and explicitly asked to act and reflect void of all passions, taking only relevant factors into account. However, this is a striking case where people are asked to make important decisions in a role that is new to them and with no feedback on the effects of their decisions, potentially making their choices very susceptible to context, such as news content. It might also be the case that gruesome incidents trigger unconscious biases (automatic, or Type I thinking), regardless of one's experience with criminal justice – and so, that *any* criminal justice decision would be subject to this kind of priming. Understanding how news coverage affects sentencing is important in the judicial context to capture the potential tradeoffs between legitimacy from civilian representation and sensitivity to local context. Beyond the judicial context, this paper explores how the content of media affects behaviors, and how expertise might mitigate biases.

We examine these questions in the French context, which offers several interesting features. First, all felonies are judged by a jury including civilians. Unlike in the US, there is no possibility for plea bargain, which could affect the sample of eventual juror decisions, in particular if prosecutors or defendants incorporate media content in their plea decisions. Second, French jurors are not only asked to vote on guilt; if the defendant is found guilty, they also vote on a sentence length. This allows us to look at effects of media on both conviction and sentencing. Lastly, professional judges are

not elected in France: they are civil servants, and therefore do not have direct incentives to appeal to voters via media coverage of their decisions. We can therefore single out the “media to trial” channel, without threats of a “trial to media” channel.

We combine two sources of data: administrative data for all criminal records in France between 2004 and 2010, and data on the content of 8PM TV news, which is widely followed in France. Our main identification rests on random variations in the exact timing of trials and offenses covered by the media. We compare outcomes of trials that happened to take place just after more coverage of crime and judicial errors, versus less. Since the exact timing of trials is determined months ahead of time, timing of trials and perpetrated offenses are plausibly independent. We further rule out the reverse causality problem - the fact that TV could mention some upcoming verdicts - by focusing on news on crime perpetrated, that is excluding news stories about trials, courts, verdicts...

We find that news stories on the day before a trial affect sentences in jury trials, but not convictions. For criminal courts including jurors, news stories about felonies increase sentences the following day by an average of 24 days, while stories on judicial errors decrease sentences by an average of 37 days. By contrast, news stories do not affect decisions made only by professional judges. For adults, types of crimes judged by juries versus judges are not exactly the same; so we provide evidence from the juvenile setting that this difference is plausibly driven by presence of jurors. This suggests that experience protects from media bias, and they are not swayed exogenous shocks resulting from current events.

Turning to mechanisms, we find that sentences are not affected by differences in crime rates, but by differences in media coverage of crime: effects are stronger after high audiences, and local variations in crimes do not affect sentences. We also document that sentences are only affected by news about criminal justice and not by other bad news on topics like natural catastrophes or unemployment. Furthermore, we find that stories on crime and criminal justice being “top of mind” seems to matter more than longer-term exposure to crime stories: news stories on the day before a judgment affects sentencing but not further back in time. Along these lines, news stories about crimes that occurred closer to the court have more effect than news stories about crimes further away.

Our paper extends the existing literature on the effect of media on important behaviors, by demonstrating its effect on sentencing decisions. Several papers have investigated the effect of media on voter turnout (Gentzkow, 2006; Cagé, 2013), corruption (Ferraz, and Finan, 2008), political accountability (Snyder and Strömberg, 2010), election results (DellaVigna and Kaplan, 2007; Gerber et al, 2009; Enikolopov et al, 2011), conflicts (Yanagizawa-Drott 2010), or offending (Dahl and DellaVigna, 2009). This paper also contributes to the literature on biases affecting judicial decisions. Judges might be changing their sentencing patterns close to elections (Berdejo and Yuchtman, 2013; Lim et al, 2012) to gain popular support. Their decisions are also affected by several behavioral biases, such as gambler's fallacy (Chen et al 2014), or extraneous factors (Danziger et al 2011; Chen and Spamann, 2014). Beyond professional judges, many studies look at on biases affecting lay people, most of which rely on either surveys or mock juries, in particular because of legal limitation in gathering data on juries' decisions (see a review by Devine et al, 2001). Some recent papers have used data on real convictions. They largely focus on intrinsic characteristics of jurors and defendant: age (Anwar et al, 2012b), race (Anwar et al, 2012a; Gazal-Ayal and Sulitzeanu-Kenan, 2010), or political opinions (Anwar et al 2014). And in non-judicial domains domains, expertise has famously been found not to be a shield against news biases (De Bondt, and Thaler, 1985).

This paper innovates in several ways. First, while most prior work in this area used survey data (Dowler, 2003; Surette, 2014) or built media pressure variables (Lim et al 2012; Lim, 2013), our paper shows the effect of media content on judicial decisions. Second, our paper presents robust evidence that media could affect jurors in two opposite directions, depending on the content of the news: jurors are sensitive not only to coverage of crime, but also of judicial errors. Third, while most papers look at access to media, we use content at a daily level, using a simple and replicable methodology that could be applied to other domains, beyond the criminal justice sphere. Lastly, while most papers study either judges or laypeople, we use the same methodology to contrast the effects of media on professionals and jurors.

Our results are also important from a policy perspective. Understanding biases in judicial decision is crucial, since the right to a fair trial is an essential feature of democratic justice systems. Furthermore, biases observed in criminal justice could affect public policy efficiency. Rizolli and Stanca (2012) show that both type one and

type two errors in convictions decrease deterrence. Over or under sentencing could be viewed as two attenuated version of classical judicial errors, thus they could have an effect on crime. Third, understanding differences between professional judges and lay people is important as systems with juror are costly and their presence or absence in different jurisdictions is frequently debated.²

The rest of the paper is organized as follows. In section 2, we describe French institution and the data we use. Section 3 discusses our identification strategy. Sections 4 and 5 present the effect of media on juror and professional judges. Section 6 explores mechanisms, and section 7 concludes.

2. Institutions and Data Description

2.1. French courts

There are three types of criminal courts in France: correction courts (*tribunal correctionnel*), criminal courts (*cour d'assises*), and juvenile courts (*tribunal pour enfant*). Correction courts examine offenses for which the maximum possible prison sentence is 10 years, so mainly misdemeanors, while the criminal courts are for crimes such as homicides, forcible rape, aggravated assaults, or armed robberies.

Correction courts process about 600,000 cases every year, and professional judges determine both guilt and sentences. Criminal courts examine about 3,000 cases per year. Judicial decisions are made by a jury of 9 (first instance proceedings) or 12 (appeal proceedings) civilian jurors³ and three professional judges⁴. Defendants are typically judged in the county (*département*) where the offense took place. There is one court per county in smaller counties, and several in larger ones, in particular in the areas of Paris, Lille and Marseille.

Jurors are randomly drawn among French citizens registered to vote who are above 23 years old. 40 potential jurors are selected for a court session, which usually lasts about

² In France, jurors were introduced in some criminal courts in 2012, removed from those courts in 2013 and their presence in labor court is a matter of some debate.

³ These are the pre-2012 numbers, for which we do our analyses. There. Since 2012, these numbers went down to 6 and 9 jurors, respectively.

⁴ One of the three judges, the "president", lead the trial. The number of judges and juror has been decided such than professional judges could not avoid conviction if all juror want it.

two weeks, and during which one to ten cases are heard, each trial lasting two to three 2-3 days. All 40 jurors show up to court at the beginning of each case, and members of the jury plus two substitute jurors are randomly drawn. Presence is mandatory and the compensation is around 100 USD per day in court. The defense attorneys and prosecutor are allowed to exclude some jurors (5 and 4 respectively) but unlike in the US, there is no questioning of potential jurors and selection can only rely on available information: name, age, sex, occupation, and what the person looks like as they walk to the bar. Importantly for our work, this means that jurors cannot be excluded based on their perception of media or current events.

The jury decides both culpability and sentence lengths, and both decisions are made on the same day: if the defendant is found guilty, the jury immediately votes on sentence length.⁵ Conviction is decided with a majority rule of two thirds while sentences require a majority plus one vote.⁶ 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.

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

Juvenile courts examine misdemeanor cases for offenders less than 18 at the moment of the crime, and felonies for offenders less than 16 at the time of the crime. Felonies committed by 16 to 18 year olds are judged in criminal court.⁷ Juvenile courts are composed of one professional judges plus two assessors appointed for 4 years and selected for their particular interest in juvenile-related issues. Juvenile courts process about 25,000 cases per year: each year, about 22,000 misdemeanors and 350 felonies committed by offenders under the age of 16 are tried. An additional 300 juveniles aged 16 to 18 years old are judged in criminal court each year.

⁵ Very high-profile cases, in particular linked to terrorism, are judged solely by professional judges.

⁶ This is covered in articles 355 – 365 of the French penal code, which can be found online at <http://www.legifrance.gouv.fr/affichCode.do?idSectionTA=LEGISCTA000006167469&cidTexte=LEGITEXT000006071154>

⁷ They are however not judged as adults. In particular, penalties are less severe than for adults, but the overall structure of the criminal court is similar to that of adults.

2.2. Court data

Our main source of judicial data is criminal records (*casier judiciaire*), informed and kept by the French Ministry of Justice, from 2004 to 2010. These criminal records have one observation per criminal court conviction, and no information for acquittals: for people found “not guilty”, no criminal record is kept and no record of the case remains. Criminal records are collected for administrative purposes: judges have access to them as part of a case, they are very selectively used for background checks, and the French Ministry of Justice uses them to do statistics. They contain information on date and county of trial, detailed information on offenses (type of offense, date of offense) and sentences, as well as socio-demographic information on age, gender, and nationality⁸.

Table 1 presents descriptive statistics on convictions between 2004 and 2010. "Most severe misdemeanors" are defined as misdemeanors with a 10 years maximum sentence. Importantly for our identification, we need the date of sentencing. Guilt and sentencing are determined at the same time in criminal courts, but in correction courts, sentencing often happens later. However, if the trial took place at maximum of 7 days after the offense, this indicates that there was a procedure – *comparution immédiate* – where conviction and sentencing were immediate. To precisely identify sentencing dates, we limit our sample to these cases.

Offenders who commit a felony are 37 years old on average and mostly male (93%), adults (92%) and French (88%). Forcible rape is the most frequent crime (45%)⁹. Sentences are overall much shorter in France than in the US: for example, the average sentence for murder is 14 years.

People convicted for the most severe misdemeanors and juvenile are also mostly male. They are mainly convicted for property crimes and their sentences are much lower than those for felonies (around 11 months for the most severe misdemeanor,

⁸ The list of county for which we collect additional information is presented in appendix A.

⁹ Armed robberies are supposed to be felonies and therefore tried in criminal court. However, in practice only the most severe ones are judged as felonies, the rest are re-qualified as misdemeanor and bench tried. This explains why only 20% of offenses tried in criminal court, and therefore in our main sample, are property crimes.

less than 3 months for juveniles).

For 39 counties¹⁰, we additionally collected felony courts' schedules. For these, we have the precise dates of the beginning and the end of each session, and the order of trials within sessions. For 22 counties, the schedules also contain precise dates of acquittals¹¹. In this subsample, there are 260 acquittals (4.77%) over 5194 trials.

2.3. French television: viewership and data

Television is a very popular source of information in France. Two television channels are most watched in France: TF1, a privately owned, non-cable channel has between 32.3% (2004) and 24.5% (2010) of viewers; and France 2, a public channel has between 20.5% (2004) 16.1% (2010) of viewers¹². The 8PM news bulletins are very popular and influent, so much so that they have been dubbed the "8PM mass". The 8PM news bulletins have average respective audiences of 8 million (TF1) and 5 million (France 2) viewers per day (for 60 millions inhabitant in France). Both programs last for roughly 40 minutes.

France is very centralized, and although there are local channels, regional news bulletins have fewer viewers than national bulletins. This also has a practical implication when we look at the effects of news on judicial decisions: most of the news that people watch on television takes places in counties far from where they live, and in particular, news stories which might affect jurors' perception of crime and law enforcement will rarely be local.

The National Institute for Audiovisual media (INA) archives all 8PM news bulletins. We collected data on news bulletins of TF1 and France 2 presented between 2004 and 2010. For each news story, we have information on date, place, length (in seconds), and title of the story, and a list of keywords describing its content. We have one observation per news story, for an average of 24 observations per day, per channel.

¹⁰ We contacted all 95 courts in France. 39 courts answered and accepted our request to provide schedules. For the list of courts, see annex 1.

¹¹ Unlike most procedures in France, what exactly appears on the court's schedules is left to their discretion. Some courts update their schedules after the facts and include information on the outcome of the trial, while others do not. There is no particular pattern in which courts update their schedules to include information on the outcomes of trials.

¹² Source: "Médiamat Annuel" published by Mediamétrie

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.

We construct indicators of coverage of crime and criminal justice in the news by using keywords that appear more than 20 times between 2004 and 2010 (2,636 words – which represents more than 80% of all keywords). We then group them into several categories relevant to measuring coverage of crime and criminal justice: judicial errors; felonies; misdemeanors; criminal law; and trials. All words used are presented in appendix B. We also have indicators for stories about actual crimes committed (labeled “perpetrated offenses” moving forward), i.e. crime stories which do not also mention trials or laws. In order to isolate these, we look for news stories on offenses, which do not contain keywords such as: trial, verdict, court, hearing, appeal... (see appendix B, point 5 and 6 for the list of the keywords). For each day and each topic, we can thus construct indicators, for the number of stories and number of minutes on the topic; as well as a dummy if a topic was covered at all.

Figures 1a and 1b illustrate variations over time in stories on crime and judicial errors: there is quite substantial variation in number of stories covering crime. While there are many stories about crime, with a lot of daily variation, news on miscarriage of justice is more rare, and concentrated mainly around the infamous “Outreau trial”, during which it was revealed that a dozen people had been convicted for sexual abuse on children, based on false witnesses. Table 1 presents descriptive statistics on the coverage of these stories between 2004 and 2010. There are very frequently stories on crime, but a lot of variation in the number of reports on a given day.

2.4. Police data

Our final data source is official police statistics, which report the monthly number of offenses per county¹³. We aggregate this data to obtain the number of felonies and misdemeanors by month and county. Note that police and court definitions of offense differ slightly. In particular, the court database is at the offender level, while police

¹³ Those data are publicly available on the open data platform of the French government (www.data.gouv.fr).

report crimes, which could have been committed by several persons; so court statistics yield slightly higher overall numbers than police statistics.

Using police data, there are on average 18.3 felonies and 3157 misdemeanors per month and county, amounting to 1736 felonies and around 300,000 misdemeanors per month in France (excluding driving offenses). Both felonies and misdemeanors are highly concentrated in and around the biggest cities (Paris, Marseille, Lille and Lyon).

3. Identification strategy

Our main identification strategy exploits variations in the exact (daily) timing of news stories and trials. We compare outcomes of cases examined just after more coverage of crime or judicial errors to that of cases following lesser media coverage of crime and justice stories. Since trial dates are chosen months in advance, we can assume that the exact (daily) timing of a trial is unrelated to the content of news on the day before the trial (more discussion of this below). We estimate equations of the form:

$$Y_{i,t,j} = \alpha Media_{t-k} + \beta X_{t,j} + \gamma Z_i + \varepsilon \quad (1)$$

Where $Y_{i,t,j}$ is the trial outcome for person i judged at time t in county j ; $Media_{t-k}$ captures measures¹⁴ of media coverage on relevant topics at time $t - k$; $X_{t,j}$ controls for time and place dependent variables¹⁵; Z_i controls for individual characteristics¹⁶. Both media and trials present systematic differences across months and days of the week: lower audiences during the weekend and over the summer; fewer trials in August, and more serious cases at the end of sessions (i.e. on Friday). We thus include controls for day of the week and month. We present the correlation between sentence length and individual characteristics in Appendix table C1.

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

¹⁴ Number of stories; dummy for the existence of a story on crime or judicial errors; number of minutes

¹⁵ County fixed effects, number of felonies and misdemeanors per month and county (police statistics) and controls for days of the week, month, and year.

¹⁶ Offense, type of court (appellate court, minor court, normal court), age, gender, French citizenship, pre-trial custody (number of days), investigation length (number of days), number of prior convictions in the past five years.

stories on the day before a trial. Using the same basic structure, we can define placebo exposure by looking at the effect of media *after* trial on trial's outcome, since posterior events cannot influence the trial's outcomes.

Finally, we also run specifications with news stories for both $t - 1$ and $t + 1$, which has two advantages. First, they summarize the main effect and the placebo. Second, they help address the fact that news stories might be correlated over time: an event might be covered several days in a row, and $media_{t+1}$ could be correlated with Y_{it} through the correlation between $media_{t-1}$ and $media_{t+1}$. Empirically, coverage of felony, misdemeanor and judicial errors on a given day increases the number of report on that subject the following day by 0.32, 0.33 to 0.43 respectively. However, the correlation is much weaker two days later, around 0.07; and there is no longer any correlation after this. This is suggestive that on average, events are covered for a couple of days at a time.¹⁷

Our main identification rests on the assumption that the exact timing of trials is not related to TV coverage of crime and criminal justice. This assumption is plausible for several reasons. First, most felonies – and even more so, most trials – do not make national news. Second, 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. Table 1 shows that the average length of time that goes by between crimes and court dates is of 5.27 years, and the median is 3.5 years. One might worry that some lawyers factor in public sentiment when choosing trials date: they could try to game trial dates to avoid periods of higher crime coverage. However, trial dates are set several months in advance, as a function of availabilities of judges, lawyers and the courts. So while season or month could potentially be manipulated, last minute planning to avoid a trial in an unfavorable climate seems very unlikely. Jurors must be summoned at least 30 days before the beginning of the trial, so timing trials at the day level is not plausible.

To address the potential concern that media could cover information on upcoming judicial decision, we use two different strategies. First, we look at the effect of news stories on crimes and violent offenses *excluding* stories on trials and legislation,

¹⁷ Since our main outcomes of interests, conviction and sentence length, are determined at the individual level, we need not worry about correlations in error terms, even while including covariates that might be temporally correlated.

keeping only stories on crimes that took place around the trial date¹⁸. Second, we check our results removing stories on crimes that took place in the same county as the trial, to make sure that there could not be overlap between case tried and story covered. As we mentioned, France is very centralized and people from all over tune into national news and they will typically get information on crimes and other events taking place in counties where they do not live.

We investigate the effect of media on two main outcomes: conviction and sentence length. The second outcome is only observed if a defendant is found guilty. We then have a system of two equations:

$$Acquittal_{i,t,j} = \alpha_1 Media_{t-k} + \beta_1 X_{t,j} + \gamma_1 Z_{i,t} + \varepsilon \quad (2)$$

$$Prison_{i,t,j} = \alpha_2 Media_{t-k} + \beta_2 X_{t,j} + \gamma_2 Z_{i,t} + \vartheta \quad (3)$$

The main coefficients of interest are α_1 and α_2 , which capture the effects of media on jurors' decisions.

Sentence length is only observed in case of conviction. If media has an effect on acquittals, then $\alpha_1 \neq 0$ in equation (2). In this situation, we observe sentences for a selected subsample of trials. For example if news on felonies increases the probability of being found guilty, we observed *more* sentences after news coverage of felonies. Using simple OLS would lead to biased estimates of α_2 . In the previous example, the marginal conviction would plausibly have shorter average sentences, if least severe cases were more likely to be swayed by media. Selection would thus induce a downwards bias to our results.

If media has no effect on acquittals, then $\alpha_1 = 0$. If this is the case, the sample of cases for which we observe sentences will not be different depending on the content of the news, and α_2 is identified in equation (3). This equation captures the relation between media and sentence for all trials.

¹⁸ This strategy is valid if the number of news stories on trials is not correlated with news about "perpetrated felonies". This need not be the case: journalists might report more on crime overall when major trials are being covered; or conversely, time constraints could make news about trials limit the amount of news on perpetrated felonies. We check for those possibilities. One additional report on trials increase the number of news stories on felony perpetrated by 0.07. This coefficient is very small and not significant when we control for day in the week and year fixed effects, which are included in our regressions.

4. Media coverage, criminal courts and jurors' decisions

4.1. Timing of news and trials

First, we check that media content is not correlated with the type of cases being tried. Table 2 presents the correlation between some characteristics of cases being tried and coverage of felonies or judicial error at $t-1$.¹⁹ As we had institutional reasons to believe, we observe no detectable difference in the type of offenses being tried.

Investigations are significantly longer after coverage of crime. However the difference is no longer significant after stories on crime committed, i.e. netting out news stories on trials. This suggests that trials after long investigations have a higher probability of being covered in the media. In our main analyses, we control for time between offense and trial, and focus on media coverage of perpetrated offenses to limit risks of coverage of offenses being tried. This validates our identification strategy: the exact timing of cases is orthogonal to news coverage of offenses.

4.2. Media coverage and jurors' conviction

We first measure the effect of media coverage of crime on acquittals. This information is not recorded for the full data, since criminal records in France reflect only convictions. To explore this question, we use the subsample we collected in 22 counties for which we have data on acquittals.

Results are presented in the first four columns of the table 3. Each cell of the table represents an independent regression. This means that the effect of news about felony, felony perpetrated... is examined separately. Media at $t-1$ (columns 1 and 2) or $t+1$ (placebo, column 3) has no impact on conviction rates and non-robust, marginally significant effects using both $t-1$ and $t+1$ (column 4).

¹⁹ Additional specifications are available in the appendix C, table 2.

Note that overall, only about 8% of defendants are found “not guilty” in criminal court.²⁰ This low number might be due to the fact that investigating judges have to decide that there is enough evidence against the defendant for the case to be pursued in criminal court: it is only beyond a certain threshold that jurors examine cases. Though these numbers are not directly comparable, for felony cases in US (See Reaves, 2013) only 1% of adjudication outcomes are an acquittal.²¹ In England and Wales 11.9% of offenders judged by Crown courts were found not guilty.²²

These results might suggest that acquittal might happen too rarely to have enough variation to be detected with our design. However, the upper bound of the confident interval at 5% is around 0.004. This means that the effect of media coverage of crime is, at most, quite small, in the sense that the sample of people for whom we observe sentences should not vary dramatically due to differential media coverage. As we mentioned in the identification strategy section, since media at most marginally change the sample of people for whom we observe sentences, we can identify the effect of media on sentences by looking at differences in sentences by media content.

We test the robustness of this result by using two indirect measures of acquittals. First, we look at the number of cases per day, which is a good proxy since acquittals result in the *absence* of a criminal record for that case. Second, we look at re-qualification of offenses to misdemeanor. Any case judged in criminal court was considered as a felony at the end of the investigation period. If a person was judged in a criminal court and convicted for a misdemeanor, part of their charges must have been dropped or re-qualified. We use this “partial acquittal” as a proxy for “full acquittal”. Those two strategies are presented in Appendix C3. They confirm that media have no effect on acquittal.

²⁰ Calculated by the authors using official statistics on overall outcomes of trials from the Ministry of Justice, p.127 of the [Annuaire Statistique de la Justice, Édition 2011-2012](#). Chaussebourg and Lumbroso (2008) look at appellate cases, and find a rate of acquittal of 7% for this subset of cases – which is low, given the selection.

²¹ The acquittal rate is much higher for offenders who receive a jury trial. For example, Anwar et al (2012) find that about 27% of jury trials in Florida lead to no guilty conviction, but less than 10% of defendants actually have a jury trial. The remaining 90% plead guilty, leading to the very low actual conviction rates. Plea bargains are not an option in France, so the relevant comparison acquittal rate would be acquittals of all felony defendants, and not only those receiving a jury trial.

²² Criminal Justice Statistics Quarterly Update to September 2012. Ministry of Justice Statistics bulletin https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/220090/criminal-justice-stats-sept-2012.pdf

4.3. Media coverage and sentence length: jurors' decisions

We now present our main results: the effect of media content on sentence length. First regression results are presented in columns 5 to 8 of table 3. As for results on acquittals, each cell presents a result for an independent regression. Column 5 presents regression results with no controls, and columns 6 to 8 include controls for case characteristics. We find significant responses of sentence length to content of news, for both crimes and judicial errors. For each extra story on felonies, sentences are 24 days longer the day after. This is not driven by the fact that media announce upcoming decision for the most severe case. Results for felonies perpetrated yield similar results. Conversely, one additional story on judicial errors decreases sentences by 37 days. Stories on misdemeanors have no effect - or a limited one - on sentences, suggestive that news on lesser offenses does not impact outcomes of cases being tried. All those results hold with or without control variables.

Reassuringly, we find no differences in sentences if the news story was the day after: none of the coefficients on content of media the day after are correlated with sentences (column 7 and 8), with small point estimates and large standard errors. This indicates that jurors of civilians 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 on crimes and shorter after media reports on judicial errors.

Professional judges and lawyers are supposed to limit juror biases, by reminding them that they should only consider relevant factors. It is then reassuring that the effect, although very significant, is not very large. The one-month variation that we observe due to one extra report on crimes or judicial mistakes represents roughly 1% of the average prison sentence.

4.4. 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 crime (from the defense) or criminal justice (from the prosecutors). Overall, 13.8% 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 only observe cases that were not appealed. We investigate this using the date of the first instance proceeding, which is also recorded in the court data. We restructure our data, and for cases that were appealed, we take as the trial date the date of the first instance proceeding.²³ All trial dates are thus now first appearance dates, and we create a dummy for cases that were appealed, which is our outcome of interest here.

Results are presented in the first column of table 4. All coefficients are non significant and point estimates are extremely small. News stories do not impact appeals. This confirms the idea that media does not impact conviction, and only affects sentencing. Furthermore, the effects we find of media on sentences are robust, but small, and likely not detectable at the case level by the prosecution or the defense.

We then run additional robustness checks to show that that our results are robust to variations in the definition of what offenses to consider, and how to define media content.

Columns (2) to (4) of table 4 vary sample and model. While our main specification focused on felonies, which represent the bulk of offenses judged in criminal court, column (2) looks at the effect of media on sentences for *all crimes* judged by criminal court with juror, including misdemeanors judged at the same time as felonies and re-qualification. Even if sentences for the non-felony crimes are far smaller, results go in the same direction. Column (3) present results when life sentences are dropped. In the fourth column, standard errors are clustered by day. In all those specifications results are extremely closed to those obtain in section 4.3.

In columns (5) and (6) we vary our measure of media coverage and exposure. In our main results, we used number of news stories covering felonies or judicial errors. Here, we use alternative measures of media coverage: length of media coverage (minutes); dummies measuring any exposure. Results remain unchanged²⁴.

In the last three columns we vary time controls. In column (7) we add county specific time trend. In column (8) we add year*month fixed effects and in column (9) we add

²³ 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.

²⁴ Results are also similar if use only TF1 or only France 2, if we restrict the sample to adults or if we weight by audiences (not shown).

session fixed effects. For this last specification, the database is restricted to the 39 counties for which we have sessions. Session fixed effects control for time but it also control for some court characteristics, such as professional judges are constant through court sessions. Standard errors increase because part of the variability is captured by the additional controls. However, coefficients keep the same signs and order of magnitude, and they are often still significant. While we cannot include presiding judge fixed effects since we don't have data on that, this last analysis is a more conservative version of that. Indeed, the presiding judge stays the same for the whole session, and so the session fixed effect absorbs president fixed effects.

In the French context, there is no extensive jury selection process. As jurors are called up, defense lawyers and prosecutors can choose to reject them, but they only have a few seconds to decide and the only information on jurors is their name, gender, profession, and physical appearance and misdemeanor as they walk up to the bar. This process could only affect our result if the parties select juror differently the day after news stories about crime or judicial error, which seems unlikely as it is unclear how this would be taken into account with so little information in choosing jurors. We nonetheless test this hypothesis by running the main regressions on the subsample of trials which last more than one day. For those trials, jury selection took place before the 8PM news bulletins of the day before sentencing, and could not be influenced by news. Results (presented in appendix C4) are not affected by this restriction.

Heterogeneity across county (juror pool) and defendant characteristics is presented in appendix E. Overall, we find that older and more conservative counties react more to TV news. Furthermore, younger defendants, as well as foreigner and defendants with past convictions, tend to be more affected by the media coverage of crime and criminal justice. This suggests that people who already tend to have longer sentences for a given offense and criminal history, are also more affected by TV coverage.

5. Professional judges and media: correctional and juvenile courts

With civilian juror participation, sentencing is affected by the content of media. How does media affect decisions made by professional judges alone? In order to look at

this, we turn to decisions taken by professional judges only, in correctional and juvenile courts.

5.1. Correctional and juvenile courts overall

Similarly to section 4, we first measure the effect of crime coverage in the media on acquittals. Since this information is not recorded in the data set, we use an indirect measure of acquittals: the number of criminal records. This measure would increase if there were less acquittals, and conversely with more acquittals, since each acquittal results in there *not being any* criminal record for this case.

Results are presented in the first three columns of table 5. Column (1) presents the results for all misdemeanors tried within one week of the date of offenses.²⁵ The sample is restricted to immediate trials (*comparution immédiate*) since only for these can we precisely date sentencing. Elsewise, misdemeanors sentences are pronounced after the trial. Column (2) presents results for “most serious” misdemeanors, which are likely to be more similar to cases tried in courts of sessions. Column (3) present results for juvenile courts.²⁶ News stories about felonies or judicial error have no impact on conviction rates. However, there seem to be some correlation between news about misdemeanors and the number of cases in court – both on the day before, *and* on the day after, leading us to think that for misdemeanors, this might be driven by reverse causality: most severe misdemeanor cases also receive most media coverage. This is the weakness of the strategy based on trials taking place in the week after the crime. While it allows us to precisely date the decision, the effect of news on misdemeanors on the number of crime could be biased, for example when there is riots during several days, which lead more media coverage because of more convictions. For this reason, the juvenile courts – which as criminal cases have a long investigation period, offer a better identification strategy.

²⁵ In general, sentences for misdemeanors are not pronounced on the same day as the trial, except for immediate trials (*comparution immédiate*). To capture those, we look at trials that took place within one week of offense date, and we can say with fair certainty that trial and sentencing date coincide.

²⁶ As there is, by construction, only a very short time between facts and trials we only present results using news on felonies. The effect of news about misdemeanor is affected by reverse causality problem and results would be biased

We now turn to the effect of media on sentence length. Results are presented in the last three columns of table 5 for misdemeanor, “most serious” misdemeanors and juveniles. Coefficients are non-significant and very small. Whereas sentences are on a scale from 1 to 10 between felonies and misdemeanors, the point estimates are on a scale of 1 to 100. Given our sample size, we have a well estimated zero effect.

5.2. Singling out professionalism: felonies committed by juveniles

The difference between the effect of media on criminal court and its absence for misdemeanors and juvenile courts could have two causes. First, media might only affect certain types of decisions, and the differential effects might be driven by the differences in cases examined. Second, professional judges might be less swayed by media stories than civilian jurors lacking experience. Those two hypotheses are hard to disentangle for adults, since civilian jurors are only involved in felony trials.

Juvenile procedures for felonies allow us to contrast directly the effects of media on professionals versus laypeople. For felony offenses, age only determines whether a case is judged in juvenile court or in criminal court: offenders under 16 are judged by a juvenile court, and by a criminal court between 16 and 18. The data confirms this distinction to hold empirically: 98.5% of felony offenders less than 16 years old are tried in juvenile court, and 95% of youth 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 including a jury of laypeople. To be charged as felonies, offenses committed by juveniles have to be very severe. If we specifically compare offenders who are 15 or 16 years old, we find that the crime structure is similar (around 70% of sexual crimes), investigations are always very long, socio-economic status are similar (95% male and 95% French) and sentences are about 1.8 times longer in criminal court comparing to juvenile court (instead of 12 times longer for misdemeanors). Descriptive statistics are presented in appendix F.

The effects of media on felonies committed by juveniles are presented in table 6. The first four columns compare the effect of media in juvenile court - with only professionals – to the effect in criminal courts, for defendants less than 18 years old – which include nine layperson jurors. Even if the samples are very small, the effect of

media on juveniles judged by jurors is significant and of similar in magnitude to that presented in section 3. The effect of media on juveniles judged by professional judges is not significant and small in magnitude. The last four columns compare more specifically the effect among offenders at the 16-year-old age cutoff. Again the effect is significant and sizable for 16-year-old offenders, while it is not for 15-year-olds. This consolidates our interpretation of this null effect being driven by professionalism, rather than differences in case contents.

5.3. Why are professionals not subject to media biases?

Why don't sentences by professionals respond to media, when judges have been found to be subject to many other biases? One hypothesis could be linked to judicial independence in France. As we mentioned earlier, judges in France are not elected. When thinking about judicial independence, we mainly think of the way in which judges might respond to coverage of *their own decisions*. The French context allows us to investigate another mechanism: how coverage of crime overall might affect particular sentences, when these decisions in return cannot affect their careers. Our results with professional judges are interesting to contrast to the findings of Lim et. al. (2012), who find that appointed judges are affected by media coverage of their cases.

It might be the case that judges are different from people who compose juries. In France, judges are highly educated, increasingly female (about 80% of incoming classes), and these differences in observable characteristics might explain differences in effects of media. Another possible interpretation would be that when people have more experience in making judicial decisions, they are less swayed by media coverage of crimes irrelevant to the case at hand. This difference between judges and jurors could be interpreted as experience helping tune out domain-specific but case-irrelevant information. Conversely, in new situations, people might extract more cues from the external world – and in particular, from media information.

To see if experience can be acquired during one's time as a juror, we contrast the effect of media on sentencing at the beginning or at the end of a session. At the beginning of a session, all jurors are novices, while in later cases, some will have had some experience making conviction and sentencing decisions. However, we find no

significant differences in media biases towards the beginning or end of sessions (see appendix table D4). This suggests that the length of a trial session is too short to allow for learning that would counter biases from the media.

6. Interpretations and Mechanisms

6.1. Crime versus crime exposure

The results we present in section 4 could be driven by two very different mechanisms. Coverage of felonies is plausibly correlated with frequency of crime, and the effect of coverage of crime could be a proxy for levels of crime affecting people. Alternatively, jurors might simply be responding to differences in media exposure, conditional on given levels of crime or judicial mistakes.

In figures 2a and 2b we present the yearly and monthly variations in number of felonies and number of media stories on crime, between 2004 and 2010. There is a yearly correlation between felonies and news stories on crime (figure 2a). Turning to monthly variations²⁷, figure 2b shows that this correlation is less obvious at a finer temporal level: there is a lot of idiosyncratic variation in number of media stories on crime that does not directly match the variation in number of offenses²⁸.

The correlation between crime and crime exposure has been addressed in previous sections. The numbers of misdemeanor and felonies within a month and a country, as measured by police forces, are included in the control variables. Models including these variables do not differ from estimates without controls. However, police measures of crime are at the monthly level, while media variables are at the daily level. Our media variables might be capturing smaller variations in the number of crimes at the county and day level. We investigate this question by measuring the effect of crimes committed during the 30 days before the trial.

This information comes from criminal records, which include offending dates. Using information from future trials, we can construct the number of offenses per day. In

²⁷ Month is the finest level of variation that we can obtain using publicly available police data.

²⁸ When we regress number of felonies on number of news stories, we obtain a statistically significant correlation between those two variables. However, the R2 of the regression is only 0.04

order to have a constant definition for this measure of crimes per day, we keep felonies judged in the following 5 years (71% of felonies). Since we have criminal records data until 2012, we build a database containing the number of felonies per day between 2004 and 2007 – roughly half of our main sample.²⁹

The main estimates are presented in the first four columns of table 6. Columns (1) and (3) replicate the estimates of section 4.3 presenting the effect of the number of misdemeanors and felonies registered by the police in the county during the month. Misdemeanors have no effect and felonies have a limited and marginally significant effect. Columns (2) and (4) present the estimates from regressions including the number of felonies - judged in the following five years - committed in the precise 30 days before the trial. They have no effect on sentences. Moreover they do not change the effect of media. Standard errors are bigger and some coefficients are no longer significant, but point estimates stay in the same orders of magnitude. This result highlights the importance of media coverage in itself, beyond the number of crimes.

To better document that the effects are being driven by media coverage and not by crime itself, we look at potential different effects of viewership, exploring whether events covered when audiences are higher have greater effects on sentences. We run our main regressions for trials after above-average and below-average audiences.³⁰ If our measure is a proxy for media exposure the estimates should be the same across audience rates. On the contrary if the effect is driven by TV in particular it should be bigger when more people watch. Results are presented in columns (5) to (8) of table 7. The effect is always bigger when audiences are high. Those results further suggest that our results are mainly driven by coverage rather than the events themselves.

6.2. Bad news versus crime-specific information

Several papers have documented the effect of irrelevant but upsetting information - bad weather, defeat of the local sports team - on people's behavior (Card and Dahl, 2011; Chen and Spamann, 2014). The effect of media coverage of crime on

²⁹ We drop the 1st of each month because when the exact offending date is missing, it appears as the having taken place on the 1st of that month.

³⁰ The two audiences are very much correlated, they are both above or both below average in more than 80% of the cases.

sentencing could be another form of bad news: news on crime might just be one type of upsetting news, leading to an increase in sentences because of bad mood. Note first of all that the negative effect on sentencing of news on judicial error is hard to interpret in that direction, since judicial errors are arguably bad news as well.

In order to investigate the general effect of bad news on sentences, we constructed a database of keywords appearing more than 200 times between 2004 and 2010³¹. We report on a daily basis the number of stories containing each keyword. Table 8 presents the effect of several types of bad news at t-1 and t+1 on sentences. Columns (1) to (4) document the effect of news stories on strikes, natural disaster, social conflict and unemployment³² while columns (5) and (6) present the effect of news about judicial error and murder - the two most common keyword in the judicial error and felony aggregates. For this subset of frequent keywords, only bad news related to criminal justice have an effect on sentences.

In a more systematic exercise, we run similar regressions with all keywords used more than 200 times. Results are discussed in appendix G. More than 97% of the words unrelated to criminal justice do not present results consistent with an effect on sentences. Those who fill the basic requirement do not form a coherent semantic group and usually have high point estimates at t+1. Average point estimates at t-1 are equal to 0.07 among words unrelated to criminal justice while it is 7.78 among keywords related to crime and criminal justice.

6.3. Saliency vs. information gathering

Media could affect sentencing decisions through two main causal chains. Media might help people update their beliefs on the risk that a felony or a judicial error might occur. In this case, news should have a lasting impact on judicial decisions, and sentences should be higher when there is more crime, regardless of the intensity of media coverage.

³¹ Those keywords represent less than 2% of the keyword used in the database but around 60% of the occurrences (the vast majority of the keywords are only used once).

³² Keywords have been chose because they are used more than 1000 times and represent clear bad news.

The second hypothesis is that media affects sentencing by making one type of event more salient. In that hypothesis, felonies or judicial errors are not viewed as more frequent, they are just more “top of the mind”. Media should then only affect decisions in a brief time window, and the effect should be different depending on how it is presented. News on facts close from people's home should be more salient. On the contrary, if people are used to see information on felonies, each additional media story could be less salient.

Figure 3a and 3b present coefficient of the regression of sentences on media at $t-k$ for different value of k . Interestingly, results diverge depending on the type of news. For news stories about felonies, it is only what happened on television on the day before the trial that affects sentencing: more news coverage of crimes at $t-2$ or in the previous week does not change sentencing. On the contrary, the effect of news stories about judicial errors last for some time and only vanished after four days.

This allows us to enter a little bit more into the mechanisms through which sentencing is affected by media: this immediacy in the relation between news and sentencing is informative of the span in which media affects decisions. It also allows us to lean towards people seeing news stories about felonies as bringing events to the top of the mind, rather than informing deep social trends: if this were to be the case, then news stories would have stronger effects over longer periods of time.

Finally, we look at the effects of media depending on whether news stories about crime are close or far from the county. We use two definition of “close”: felony occurred in the same administrative region, but not in the same county;³³ and felonies occurred in an adjacent county. For each trial we measure the effect of news close or further from the county. Results are presented in table 9. Whatever the definition is, the effect of close news stories is bigger. This confirms the idea that people react most to news stories nearer to home, possibly because they identify more to these stories.

³³ There is 22 region divided in 95 counties in France. We remove stories that took place in the same county to make sure these results are not subject to the reverse causality discussed earlier.

7. Conclusion

As with other important behaviors, the content of media affects jurors' decisions: sentences in jury trials are longer following more coverage of crime, and shorter after coverage of judicial mistakes. This effect is very localized: only media coverage on the day before affects sentencing decisions. By contrast, we find no effect of media on professional judges' decisions: in this domain, it seems that professional experience cancels out the way in which media might influence decisions.

Whereas several recent papers pointed out several biases that professional judges might be subject to, such as mental depletion or priming, reacting to extraneous good or bad news, or responding to electoral cycles, we show one bias which they are not subject to: news coverage of crime and justice. This 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 jurors of laypeople); and more generally, training or experience could help overcome some biases. If these results were to carry out to other domains, it could help inform the interaction between citizen decision-making (for example, use of ballots) and media information.

The diffusion of this result during juror's mandatory training would be a good way to reduce the problem, while being mindful that there might be tradeoffs with "boomerang effects" of increasing sensitivity to coverage of crime and justice by drawing jurors' attention to potential biases. Overall, these results suggest that juries of lay people might lead to greater noise in outcomes of trials, if over and beyond behavioral biases, current events context might impact sentencing. If defendants are risk-averse, this extra source of variability of outcomes might increase the appeal of plea bargains when they are available.

Finally, by investigating the effect of media on sentencing in a jury trial, we are able to look at a particularly localized, contextual outcome. Whereas for behaviors like voting, citizens might actively be looking for information in the media; or conversely, elected judges might be looking to influence the media through their decisions, jurors are plausibly neither seeking feedback, nor trying to influence other outcomes beyond the trial. Yet even in this setting, media affects jurors' decisions, in a very localized manner, highlighting the contextual influences of media.

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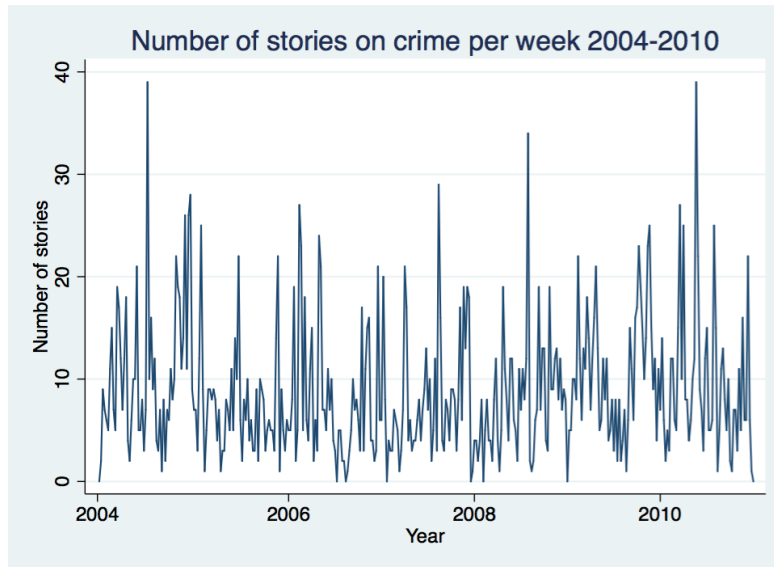


Figure 1a: Number of news stories on crime per week: 2004 – 2010

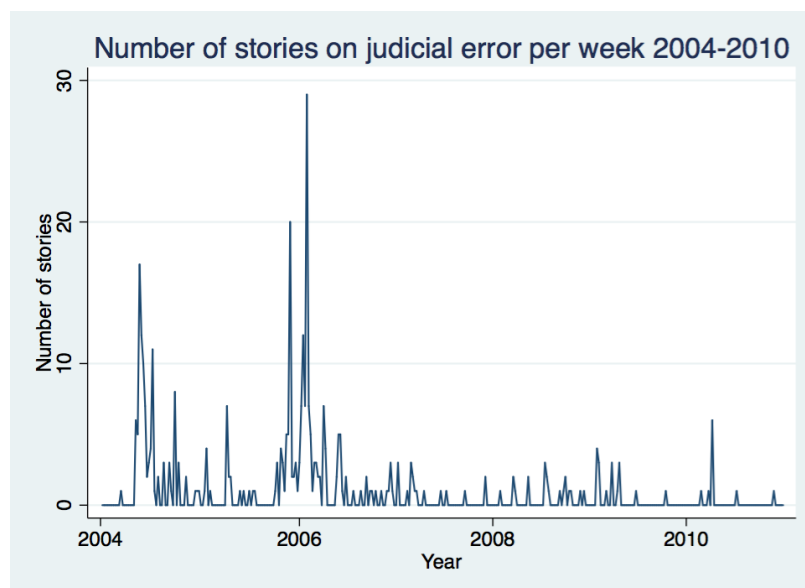


Figure 1b: Number of news stories on judicial errors per week: 2004 – 2010

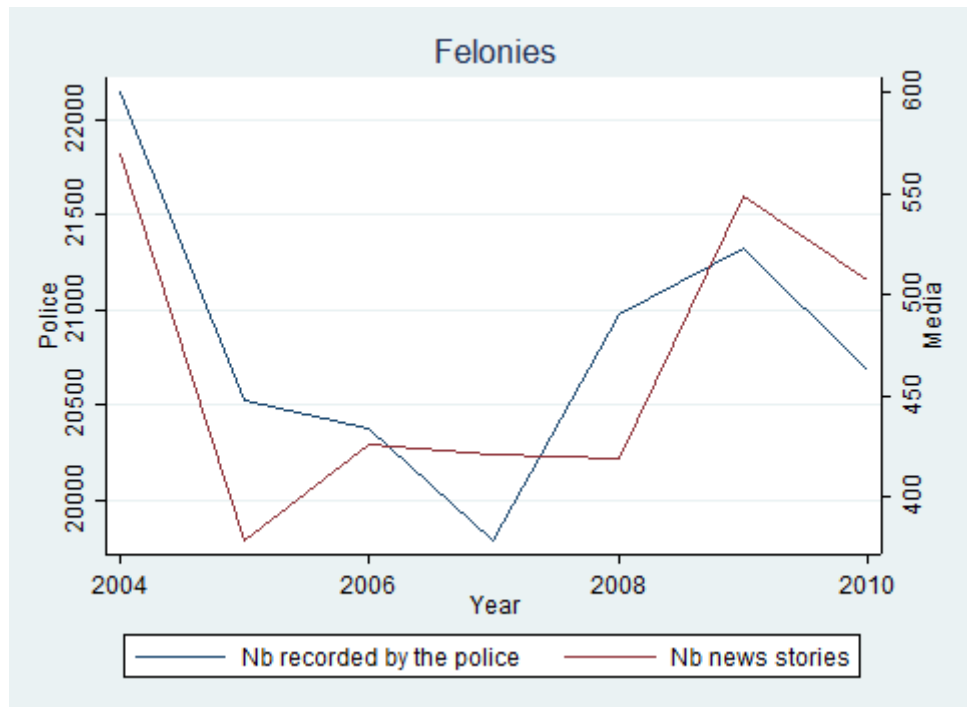


Figure 2a: Number of felonies recorded by the Police or covered by the news per year: 2004 – 2010

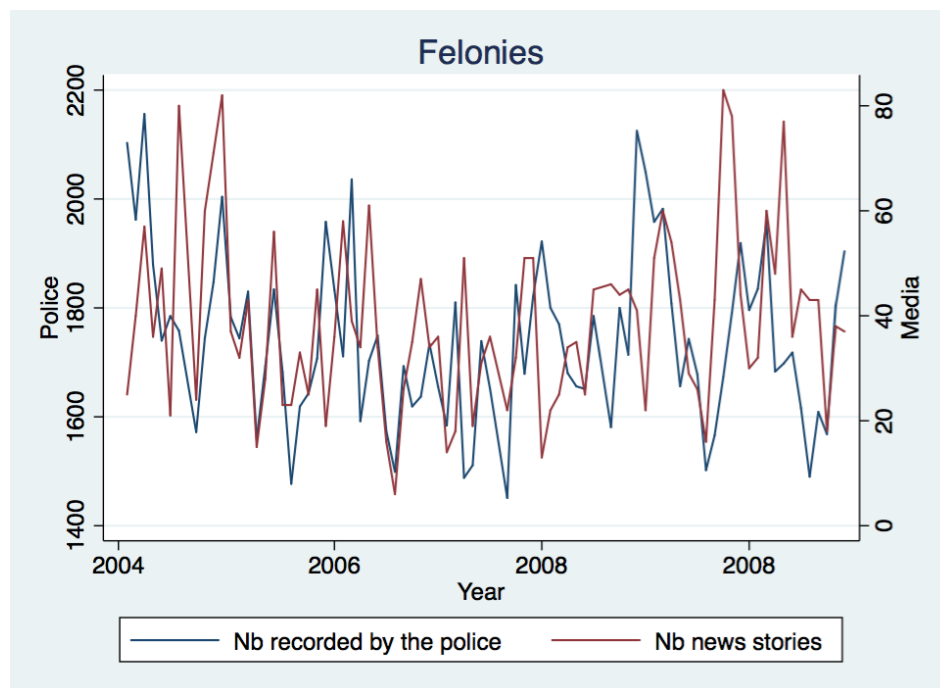


Figure 2b: Number of felonies recorded by the Police, and number of news stories on crimes on TV, by month

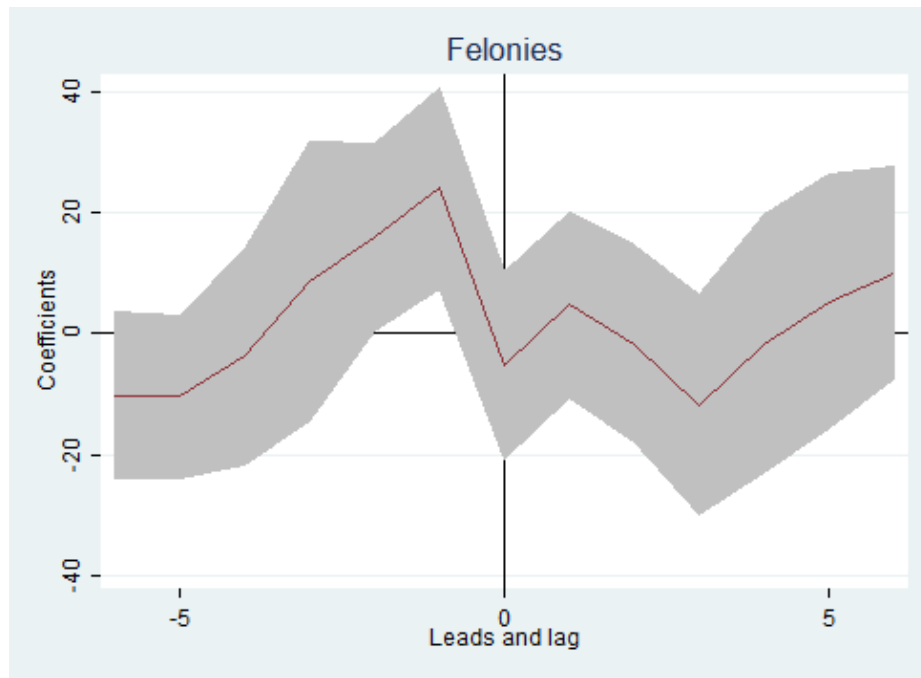


Figure 3a: effects of the number of report on felonies at $t+k$ on sentences at t for different value of k . *Note: regressions include all control variables. 10% confidence interval in grey.*

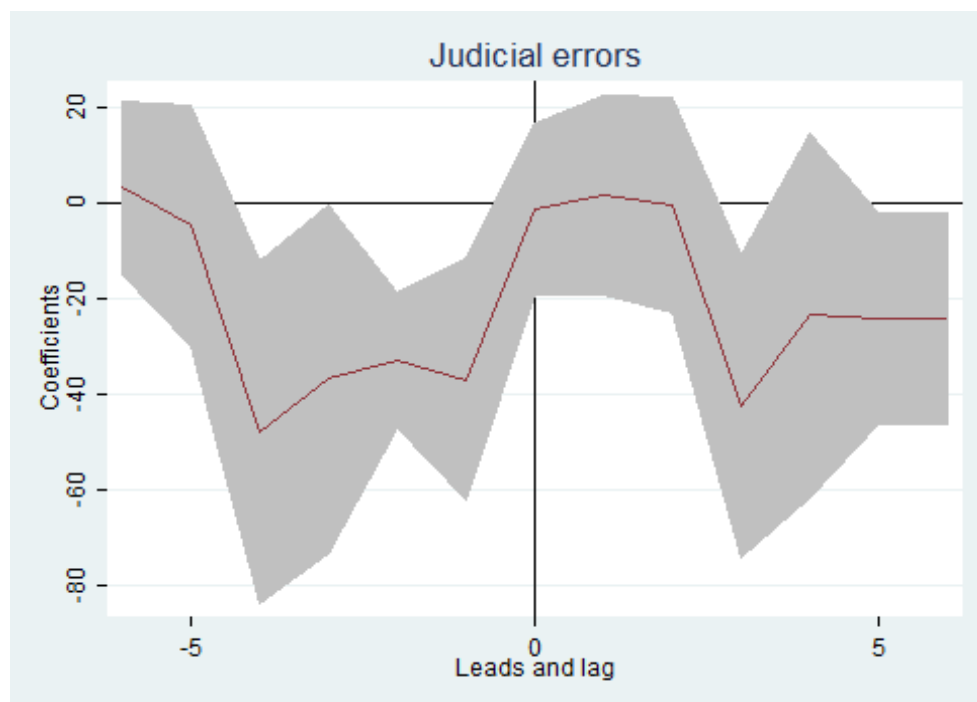


Figure 3b: effects of the number of report on judicial errors at $t+k$ on sentences at t for different value of k . *Note: regressions include all control variables. 10% confidence interval in grey.*

		Mean	sd	median	max
Felonies (N=17,755)	Fraction male	.94	.23		
	Fraction French	.08	.27		
	Age	37.28	13.41	35	91
	Investigation length (years)	5.11	4.58	3.42	37.3
	Murder	.17	.38		
	Violence	.12	.32		
	Rape	.48	.5		
	Property crime	.22	.41		
	Prison murder (years)	14.98	7.06	15	life
	Prison violence (years)	8.73	5.41	8	life
Worse misdemeanors (N=19,638)	Prison sexual assault (years)	9.05	4.41	8	life
	Prison property crime (years)	7.97	4.6	7	life
	Fraction male	.95	.22		
	Fraction French	.75	.43		
	Age	27.18	8.69	24	74
	Assault	.02	.12		
	Theft	.25	.43		
All misdemeanors (N=204,615)	Drug	.7	.46		
	Prison (year)	.92	.92	.67	7
	Fraction male	.96	.2		
	Fraction French	.73	.45		
	Age	30.03	10.06	28	87
	Assault	.18	.39		
	Theft	.36	.48		
Juveniles (N=152,788)	Drug	.08	.28		
	Prison (year)	.57	.6	.42	7
	Fraction male	.93	.26		
	Fraction French	.94	.23		
	Age	17.18	1.57	17	39
	Assault	.22	.41		
	Theft	.61	.49		
Media (N=2,557)	Drug	.07	.26		
	Prison (year)	.2	.34	.08	5.08
	Number of stories per day on...				
	Felonies	1.28	1.65	1	12
	Felony perpetrated	0.89	1.37	0	10
	Misdemeanors	2.22	2.23	2	20
	Misdemeanor perpetrated	1.85	1.97	1	19
	Judicial errors	0.14	0.76	0	22
	Time per day (in minutes) on...				
	Felonies	1.72	2.34	.95	16.87
	Felony perpetrated	1.21	1.96	0	13.08
	Misdemeanors	3.15	3.33	2.3	32.57
	Misdemeanor perpetrated	1.85	1.97	1	19
	Judicial erros	0.22	1.36	0	44.7
	Audience numbers (million)	13	2,15	13	19

Table 1: Summary statistics for conviction and media, 2004-2010

	Type of offense being tried									Investigation length		
	Homicide			Sexual assault			Property					
Felony t-1	-0.00152 (0.00307)			0.00140 (0.00311)			0.00105 (0.00316)			16.23* (8.924)		
Felony perpetrated t-1	-1.96e-05 (0.00316)			0.00137 (0.00396)			0.00336 (0.00370)			2.031 (11.38)		
Judicial error t-1	-0.00207 (0.00443)			-0.00489 (0.00496)			0.00165 (0.00550)			-10.54 (14.87)		
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128
Sample mean	0.267	0.267	0.267	0.445	0.445	0.445	0.202	0.202	0.202	1865	1865	1865

Table 2: Content of cases, by media coverage of felonies, felonies perpetrated, and judicial errors on the day before the trial.

Note: Regressions include controls for: age, gender, nationality (French or other), length of pre-trial detention, type of offense, county, length of time between offense and trial, dummies for month, day of week and year; as well as monthly number of offenses reported by police at the county level. Standard errors are clustered by county. Unless specified otherwise, this is the case in all regressions presented.

	Acquittals (sub-sample)				Sentence length			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Felony t-1	0.00205*	0.00163		0.00205*	33.97***	23.58***		22.57***
	(0.00113)	(0.00109)		(0.00119)	(11.82)	(7.834)		(7.752)
Felony t+1			-0.00175	-0.00213			9.203	5.211
			(0.00123)	(0.00133)			(7.655)	(7.608)
Felony perpetrated t-1	0.00148	0.00118		0.00174	35.15**	23.95**		23.76**
	(0.00172)	(0.00183)		(0.00204)	(13.67)	(10.02)		(9.901)
Felony perpetrated t+1			-0.00289**	-0.00317*			4.607	1.082
			(0.00134)	(0.00158)			(9.335)	(9.240)
Misdemeanor t-1	-0.000198	0.000130		-0.000430	-4.076	10.61*		10.83
	(0.00122)	(0.00130)		(0.00153)	(7.960)	(6.125)		(6.839)
Misdemeanor t+1			0.00189	0.00204			2.555	-0.884
			(0.00127)	(0.00155)			(6.061)	(6.980)
Misdemeanor perpetrated t-1	-0.000538	-0.000416		-0.00104	-3.776	8.469		8.733
	(0.00125)	(0.00137)		(0.00167)	(9.622)	(6.679)		(7.580)
Misdemeanor perpetrated t+1			0.00176	0.00214			1.944	-0.973
			(0.00140)	(0.00175)			(6.653)	(7.731)
Judicial error t-1	0.00274	0.00158		0.00174	-48.25**	-37.02**		-37.69**
	(0.00344)	(0.00419)		(0.00412)	(20.77)	(15.40)		(15.27)
Judicial error t+1			-0.00101	-0.00114			1.662	5.221
			(0.00313)	(0.00307)			(12.65)	(12.37)
Control	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	5,453	5,453	5,453	5,453	17,755	17,755	17,755	17,755
Sample mean	0.0475	0.0475	0.0475	0.0475	3522	3522	3522	3522

Table 3: Effect of the number of new stories at t-1, t+1 or t-1 and t+1 on acquittal (columns 1-4) and sentence length (columns 5-8) .

Note: coefficients in each different cell correspond to different regressions. Each column regroup the results of 5 different regressions concerning different types of news. Number of observations, mean or sd are identical among columns

	Appeal	With misdemeanor	Without life imprisonment	With error clustered per day	With Media in minute	With dummies	With county specific time trend	With year*month fixed effect	With session fixed effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Felony t-1	-0.00120 (0.00176)	20.31*** (7.316)	19.87** (7.595)	22.57*** (8.748)	15.07*** (5.485)	91.88*** (24.69)	23.68*** (8.216)	23.39*** (8.075)	45.61*** (13.95)
Felony t+1	0.00177 (0.00183)	4.732 (7.269)	7.823 (7.280)	5.211 (7.105)	2.479 (4.882)	27.93 (24.73)	4.836 (8.226)	14.07* (7.499)	15.38 (13.40)
Felony perpetrated t-1	-0.00104 (0.00221)	22.08** (9.088)	21.19** (9.339)	23.76** (11.57)	17.85** (8.388)	82.68*** (24.39)	26.35** (10.32)	24.72** (10.68)	57.94*** (17.51)
Felony perpetrated t+1	-2.30e-05 (0.00199)	0.857 (8.871)	3.884 (9.050)	1.082 (8.695)	-0.135 (6.163)	11.46 (26.45)	-1.182 (9.942)	11.12 (9.642)	19.89 (17.66)
Judicial error t-1	0.00147 (0.00405)	-31.98** (14.88)	-39.95*** (15.18)	-37.69*** (10.76)	-19.23** (8.610)	-80.07* (40.48)	-27.03* (15.59)	-20.45 (16.53)	-15.58 (37.13)
Judicial error t+1	-0.000658 (0.00238)	2.712 (11.87)	8.405 (11.80)	5.221 (6.923)	1.879 (6.284)	35.71 (51.27)	9.823 (12.24)	20.56 (13.51)	18.67 (32.85)
Control	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	19,468	19,128	17,636	17,755	17,755	17,755	17,171	17,755	7,392
Sample mean	0.138	3334	3468	3522	3522	3522	3522	3522	3511

Table 4: News and sentence length: robustness check. *Note: regressions are independent by pair. We regress sentence length on Felony at t-1 and t+1, then sentence length on felony perpetrated at t-1 and t+1...*

	Number of conviction			Sentence length		
	All misdemeanor (1)	Most severe misdemeanor (2)	Juvenile (3)	All misdemeanor (4)	Most severe misdemeanor (5)	Juvenile (6)
Felony t-1	-0.0607 (0.305)	-0.0164 (0.0550)	0.300 (0.451)	0.176 (0.257)	-1.715 (1.295)	-0.0568 (0.214)
Felony t+1	-0.0591 (0.305)	-0.0376 (0.0549)	0.765* (0.450)	0.171 (0.335)	2.941* (1.515)	0.274 (0.190)
Felony perpetrated t-1	0.0302 (0.365)	0.0490 (0.0659)	0.0687 (0.540)	-0.00806 (0.270)	-1.597 (1.406)	0.0318 (0.284)
Felony perpetrated t+1	0.105 (0.366)	-0.0329 (0.0659)	0.317 (0.541)	-0.787** (0.376)	-0.543 (1.906)	0.405* (0.220)
Misdemeanor t-1	0.515** (0.239)	0.212*** (0.0426)	0.390 (0.353)	0.104 (0.229)	0.0678 (1.032)	0.117 (0.139)
Misdemeanor t+1	0.326 (0.239)	0.160*** (0.0426)	0.736** (0.353)	0.230 (0.239)	-0.167 (1.092)	0.0343 (0.175)
Misdemeanor perpetrated t-1	0.377 (0.268)	0.245*** (0.0477)	1.237* (0.666)	-0.0446 (0.236)	-0.292 (1.023)	0.108 (0.160)
Misdemeanor perpetrated t+1	0.447* (0.267)	0.211*** (0.0476)	3.332*** (0.666)	0.175 (0.267)	0.0531 (1.182)	0.107 (0.183)
Judicial error t-1	-0.284 (0.652)	0.125 (0.117)	0.0520 (0.963)	-0.352 (0.588)	2.297 (2.175)	0.0546 (0.439)
Judicial error t+1	-0.0672 (0.651)	-0.162 (0.117)	1.659* (0.963)	0.418 (0.518)	-0.903 (2.516)	0.325 (0.330)
Control	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,555	2,555	2,555	204,455	19,657	152,787
Sample mean	80.02	7.68	60.35	205.3	332.4	78.14

Table 5: Professional judges; effect of the number of new stories at both t-1 and t+1 on number of conviction (columns 1-3) and sentences (columns 4-6).

	Felonies in juvenile court (without jurors)		Juveniles in criminal court (with jurors)		15 years old (without jurors)		16 years old (with jurors)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Felony t-1	12.61 (10.87)	16.00 (10.88)	39.12* (20.34)	39.81* (21.53)	3.139 (23.26)	7.993 (23.20)	12.62 (18.01)	18.86 (18.99)
Felony t+1		-13.88** (6.552)		-3.139 (27.56)		-16.32 (12.12)		-28.34 (20.10)
Felony perpetrated t-1	11.20 (12.39)	14.68 (12.57)	56.69** (23.08)	58.71** (24.65)	-3.037 (24.06)	2.102 (24.96)	42.30* (22.57)	47.67** (23.60)
Felony perpetrated t+1		-16.41* (8.497)		-10.87 (26.79)		-21.37 (15.17)		-28.79 (28.19)
Judicial error t-1	-14.02 (11.42)	-10.84 (12.03)	-33.08 (32.56)	-32.03 (32.08)	1.321 (16.31)	-0.986 (17.90)	-23.53 (41.24)	-26.48 (41.62)
Judicial error t+1		-14.91 (14.67)		-4.792 (20.85)		17.66 (18.54)		16.42 (19.42)
Observations	2,508	2,508	2,024	2,024	610	610	850	850
Sample mean	758.26	758.26	2169.07	2169.07	979.7	979.7	1798.8	1798.8

Table 6: Juvenile felonies: Effects of media, depending on the presence of juries of laypeople.

Note: These regressions do not include controls. With controls, point estimates are similar in magnitude, but given the small sample sizes, results are not all significant with controls.

	Control for the number of crimes in the same month and county				Audience			
	(1)	(2)	(3)	(4)	< mean (5)	> mean (6)	< mean (7)	> mean (8)
Felony t-1	22.57*** (7.752)	20.87* (10.91)			9.793 (10.83)	30.66*** (10.91)		
Felony t+1	5.211 (7.608)	7.145 (10.73)			10.09 (11.86)	7.190 (10.18)		
Felony perpetrated t-1			23.76** (9.901)	19.42 (14.74)			18.60 (16.02)	26.56** (12.83)
Felony perpetrated t+1			1.082 (9.240)	-3.370 (13.64)			9.288 (13.80)	-3.624 (11.81)
Misdemeanors (police)	-0.0195 (0.0219)		-0.0201 (0.0217)					
Felonies (police)	-3.275* (1.654)		-3.229* (1.644)					
Felonies (justice)		-11.44 (16.72)		-11.38 (16.69)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,755	10,583	17,755	10,583	7,799	9,956	7,799	9,956
Sample mean	3522	3481	3522	3481	3606	3457	3606	3457

Table 7: Crimes and crimes exposure. Misdemeanors and felonies are measured per month and per county.

	Strike (1)	Natural disaster (2)	Social conflict (3)	Unemployment (4)	Judicial error (5)	Murder (6)
Media at t-1	4.855 (5.422)	-3.181 (6.687)	8.215 (6.967)	-15.46 (11.41)	-37.17** (17.08)	26.74* (15.19)
Media at t+1	-1.703 (5.940)	6.102 (12.14)	-2.856 (6.819)	-1.048 (12.70)	0.735 (13.12)	-3.858 (11.91)
Control	yes	yes	yes	yes	yes	yes
Observations	17,755	17,755	17,755	17,755	17,755	17,755
Sample mean	3522	3522	3522	3522	3522	3522

Table 8: Sentence length and news on criminal justice versus other bad news.

	(1)	(2)	(3)	(4)
Felony same region t-1	49.34** (19.83)			
Felony other region t-1	16.17* (8.572)			
Felony same region t+1	9.630 (17.79)			
Felony other region t+1	7.778 (7.700)			
Felony perpetrated Same region t-1		29.83 (18.75)		
Felony perpetrated Other region t-1		20.53* (11.64)		
Felony perpetrated Same region t+1		1.938 (15.46)		
Felony perpetrated Other region t+1		8.113 (10.30)		
Felony adjacent county t-1			40.40* (20.62)	
Felony non-adjacent county t-1			18.68* (9.624)	
Felony adjacent county t+1			16.29 (19.98)	
Felony non-adjacent county t+1			6.355 (8.215)	
Felony perpetrated Adjacent county t-1				42.17* (22.93)
Felony perpetrated Non-adjacent county t-1				18.50 (12.07)
Felony perpetrated Non-adjacent county t+1				15.25 (17.91)
Felony perpetrated Non-adjacent county t+1				5.539 (10.27)
Controls	Yes	Yes	Yes	Yes
Observations	17,230	17,442	17,230	17,442
Sample mean	3522	3522	3522	3522

Table 9: The proximity effect: effect of media on sentence, by distance between geographic location of offense covered on TV and county of trial.

Appendix A

List of courts for which we got court schedules and information on acquittals:

Ain (1), Alpes Maritimes (6), Hautes-Garonne (31), Gironde (33), Indre (36), Loire (42), Loiret (45), Maine et loire (49), Haute Marne (52), Meurthe-et-Moselle (54), Pas de Calais (62), Pyrénées-Orientales (66), Haute Savoie (74), Paris (75), Yvelines (78), Somme (80), Tarn (81), Vendée (85), Vienne (86), Haute Vienne (87), Essonne (91), Seine-Saint Denis (93)

List of courts for which we got court schedules, but no information on acquittals:

Allier (3), Cantal (15), Doubs (25), Eure (27), Gard (30), Hérault (34), Isère (38), Haute-Loire (43), Lot-et-Garonne (47), Moselle (57), Nord (59), Puy-de-Dôme (63), Bas-Rhin (67), Savoie (73), Seine-et-Marne (77), , Val-de-Marne (94), Val-d'Oise (95)

Appendix B

List of words used to define main aggregate measures of media coverage of crime and criminal justice. Number of appearances in the news 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); hold-up (200); viol (321); mœurs (334); enlèvement (335); pédophilie (522); meurtre (1435); violence (1620) (if used with "décès").

2. Judicial errors: erreur judiciaire (235); Outreau (262); réhabilitation judiciaire (36); Burgaud Fabrice (58).

3. Misdemeanors: trafic d'armes (20); disparition (cavale) (22); clandestin (Sans papier) (24); travail au noir (26); contrebande (27); escroquerie (caisse noire) (27); harcèlement moral (27); trafiquant (29); manifestant (casseur) (31); drogue (cannabis) (31); abus de biens sociaux (31); Musulin Toni (32); racket (33); état d'urgence (37); délit d'initie (39); travail clandestin (40); couvrir feu (41); délit (53); homicide involontaire (53); proxénétisme (54); arme à feu (64); campement (illégal) (65); cocaïne (72); bavure policière (76); clandestin (Sans papiers) (76); piraterie (83); cambriolage (87); bagarre (89) (if used without "décès"); banque (Clearstream) (91); clandestin (sans papier) (91); contrefaçon (98); infraction (105); bande de jeunes (107); corruption (120); profanation (136); délinquant (141); dégâts (dégradation) (144); maltraitance (165); trafic de drogue (180); drogue (186); insécurité (210); vandalisme (261); escroquerie (283); fraude (339); délinquance (362); vol-infraction (369); délinquance juvénile (385); agression (705); violence (1620) (if used without "décès").

5. 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); accusé (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).

6. 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).

Appendix C: Additional Regression tables

	Sentence length
Past convictions (dummy)	72.01** (31.64)
Number of past convictions	19.38** (9.575)
Nb misdemeanors in the county in the month	-0.0282 (0.0214)
Nb felonies in the county in the month	-3.381** (1.387)
Age	14.31*** (1.282)
Sex	682.6*** (61.79)
French (dummy)	-66.07 (74.15)
Investigation length	0.0395** (0.0191)
Pre-trial custody (number of days)	1.427*** (0.0894)
Thursday (dummy)	-111.7* (60.65)
Wednesday (dummy)	49.67 (67.03)
Thursday (dummy)	110.5* (62.50)
Friday (dummy)	175.9*** (62.28)
Saturday (dummy)	111.6 (114.9)
Sunday (dummy)	-144.8 (711.7)
Juvenile	-664.9*** (42.55)
Appeal	487.4*** (71.04)
Appeal for juveniles	-247.6 (203.9)
Violence (dummy)	-1,775*** (76.94)
Forcible Rape (dummy)	-1,703*** (69.58)
Armed Robbery (dummy)	-2,182*** (63.50)
Year fixed effects	Yes
Month fixed effects	Yes
County fixed effects	Yes
Observations	17,499
Sentence mean	3525
Sentence sd	2037

Table C1: Correlation between sentence time and control variables

	Age			Male			Past conviction			Pre trial custody		
Felony t-1	-0.0358 (0.0770)			-0.00154 (0.00116)			0.000783 (0.00239)			1.253 (2.437)		
Felony perpetrated t-1	-0.148 (0.103)			-0.000505 (0.00154)			-0.000500 (0.00335)			2.307 (2.816)		
Judicial error t-1	-0.107 (0.129)			0.00525*** (0.00163)			0.00753* (0.00440)			-8.955* (5.103)		
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128	19,128
Sample mean	37.02	37.02	37.02	0.930	0.930	0.930	0.405	0.405	0.405	619.1	619.1	619.1

Table C2: Socio-demographic characteristics of defendants, by media coverage of felonies, felonies perpetrated, and judicial errors.

	Number of cases				Re-qualified			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Felony t-1	0.127 (0.0793)	0.0790 (0.0522)		0.0645 (0.0534)	0.000436 (0.000861)	0.000754 (0.000849)		0.000699 (0.000914)
Felony t+1			0.0819 (0.0522)	0.0682 (0.0534)			2.92e-05 (0.000695)	-7.73e-05 (0.000770)
Felony perpetrated t-1	-0.0622 (0.0956)	0.0303 (0.0625)		0.0205 (0.0640)	3.60e-05 (0.000900)	0.000202 (0.000851)		0.000198 (0.000887)
Felony perpetrated t+1			0.0500 (0.0625)	0.0455 (0.0641)			-0.000443 (0.000900)	-0.000478 (0.000929)
Misdemeanor t-1	0.487*** (0.0579)	0.116*** (0.0396)		0.0996** (0.0418)	-0.000306 (0.000457)	-0.000205 (0.000431)		-0.000406 (0.000513)
Misdemeanor t+1			0.0819** (0.0396)	0.0497 (0.0418)			0.000911 (0.000660)	0.00108 (0.000748)
Misdemeanor perpetrated t-1	0.359*** (0.0660)	0.0544 (0.0444)		0.0391 (0.0470)	-0.000313 (0.000578)	-0.000151 (0.000533)		-0.000278 (0.000582)
Misdemeanor perpetrated t+1			0.0594 (0.0443)	0.0466 (0.0469)			0.000511 (0.000797)	0.000656 (0.000853)
Judicial error t-1	0.540*** (0.173)	0.144 (0.114)		0.123 (0.114)	0.000756 (0.00222)	0.00134 (0.00216)		0.00135 (0.00218)
Judicial error t+1			0.212* (0.113)	0.199* (0.114)			0.00134 (0.00216)	-0.000735 (0.000780)
Control	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	2,555	2,555	2,555	2,555	13,780	13,780	13,780	13,780
Sample mean	5.393	5.393	5.393	5.393	0.0237	0.0237	0.0237	0.0237

Table C3: Media on acquittal, robustness check.

	Sentence length		
	(1)	(2)	(3)
Felony t-1	18.43** (7.888)		
Felony t+1	5.729 (8.093)		
Felony perpetrated t-1		20.40** (9.493)	
Felony perpetrated t+1		1.207 (9.637)	
Judicial error t-1			-34.47** (15.86)
Judicial error t+1			1.880 (11.80)
Control	Yes	Yes	Yes
Observations	15,591	15,591	15,591
Sample mean	3554	3554	3554

Table C4: Effect of the number of news stories at both t-1 and t+1 on sentences using trial which last more than one day.

	Beginning session (1)	End session (2)	Beginning session (3)	End session (4)	Beginning session (5)	End session (6)
Felony t-1	32.20*** (8.904)	48.27*** (15.73)				
Felony t+1	15.42 (15.10)	2.624 (16.62)				
Felony perpetrated t-1			34.71** (13.78)	42.14** (18.01)		
Felony perpetrated t+1			28.91 (21.19)	8.848 (17.49)		
Judicial error t-1					-9.207 (32.14)	-38.76 (33.21)
Judicial error t+1					12.31 (23.91)	16.24 (23.78)
Control	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,816	3,992	3,816	3,992	3,816	3,992
Sample mean	3483	3483	3483	3483	3483	3483

Table D4: Jurors' experience: the effect of media by rank of trial within the court session. The sample includes the 39 counties for which we have court schedules.

Appendix E: Heterogeneity of the effect, by defendant characteristics

In this appendix, we explore heterogeneity of our result along three main dimensions. First, we look for differential effects along offender characteristics (table E1). Foreigners and young people, appeared to be more affected by media than other people. Repeat offenders are more affected upward (by news about felony) but less affected downward (by judicial error).

Second, we differentiate on news characteristics (table E2). We distinguish between news about sexual crimes, property crimes and violence. As a large part of news stories include multiple offenses, there is a lot of overlap. Sexual crimes seem to have the biggest effect on sentencing.

We then differentiate across counties (table E3). We have no information on jury members but since they are randomly selected from the electoral role we can make the assumption that on average, their characteristics match county characteristics. Counties where the share of conservative vote is higher than average, as well as counties with more people older than 65, are more affected by coverage of felonies.

Finally, we contrast the effect of media by type of offense being judged (table E4). The effect of news stories about felonies is bigger for property crime than for violence. Conversely, the effect of judicial error is more important for the most severe crimes. This could reflect substantial differences in how media affects different offenses; or capture upwards (downward) deviations from relatively short (long) sentences for murder (sexual offenses and property crimes).

Appendix E: heterogeneity of the effect, by defendant characteristics

	Citizenship				Age of defendant				Past convictions			
	French	Other	French	Other	<median	>median	<median	> median	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Felony perpetrated t-1	20.21*	49.74			26.14**	25.99			16.75	30.94**		
	(11.02)	(32.15)			(12.83)	(16.42)			(13.53)	(14.51)		
Felony perpetrated t+1	0.496	6.274			1.877	6.070			0.166	2.957		
	(8.682)	(23.92)			(12.70)	(12.28)			(11.33)	(12.18)		
Judicial error t-1			-30.52**	-62.82**			-32.26**	-38.56			-46.34**	-20.64
			(14.96)	(29.15)			(15.59)	(25.12)			(18.96)	(21.10)
Judicial error t+1			-0.653	32.58			14.81	-1.050			11.74	-8.722
			(12.97)	(37.30)			(16.19)	(13.91)			(14.89)	(17.80)
Observations	15,547	2,208	15,547	2,208	9,045	8,710	9,045	8,710	11,240	6,515	11,240	6,515
Sample mean	3491	3746	3491	3746	3135	3925	3135	3925	3574	3433	3574	3433

Table E1: Effect of content of news on sentence length, by socio-demographic characteristics of the defendant. The median age of defendants is 35 years old threshold was chosen as the median age. Past convictions are defined as having a prior conviction in one's criminal record

	(1)	(2)	(3)	(4)	(5)
Homicide t-1	21.18** (9.412)			19.53** (9.343)	
Homicide t+1	-8.032 (9.407)			-8.471 (9.215)	
Sexual crime t-1		70.57** (27.55)		66.57** (27.77)	
Sexual crime t+1		38.76** (17.85)		39.23** (17.49)	
Property felony t-1			28.01 (29.38)	26.62 (29.36)	
Property felony t+1			28.46 (35.67)	26.67 (35.32)	
Control	Yes	Yes	Yes	Yes	Yes
Observations	17,755	17,755	17,755	17,755	17,755
Sample mean	3522	3522	3522	3522	3522

Table E2: Effect of content of news on sentence length, by type of crime in the news.

	Share vote conservative				Share older than 65				Share Unemployment			
	< mean	> mean	< mean	> mean	< mean	> mean	< mean	> mean	< mean	> mean	< mean	> mean
Felony perpetrated t-1	17.07 (13.94)	28.57** (13.81)			19.63 (12.17)	37.25* (18.98)			20.46 (16.27)	25.99* (13.74)		
Felony perpetrated t+1	-0.615 (13.83)	6.518 (12.59)			-2.854 (11.19)	13.52 (17.74)			3.250 (14.86)	4.373 (11.79)		
Judicial error t-1			-34.41 (22.08)	-32.64 (19.59)			-32.02* (18.41)	-45.85* (23.04)			-44.74** (20.50)	-34.03 (21.34)
Judicial error t+1			13.20 (17.41)	-6.691 (16.44)			5.342 (14.89)	7.452 (21.33)			-3.891 (14.32)	13.72 (17.48)
Observations	8,879	8,876	8,879	8,876	13,061	4,694	13,061	4,694	6,679	11,076	6,679	11,076
Sample mean	3493	3551	3493	3551	3474	3657	3474	3657	3576	3490	3576	3490

Table E3: Effect of content of news on sentence length, by average characteristics of the population in the county. *Note: juror are randomly drawn from the county's population (via electoral roles). However both prosecutor and defendant attorney could exclude some juror.*

	Sexual assault			Violence			Property crimes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Felony t-1	18.32*			23.35			37.12**		
	(9.827)			(20.70)			(16.49)		
Felony t+1	4.107			5.376			14.58		
	(9.195)			(20.61)			(18.29)		
Felony perpetrated t-1		22.15*			5.259			52.02**	
		(12.71)			(24.72)			(22.23)	
Felony perpetrated t+1		-8.416			5.291			16.73	
		(12.23)			(22.60)			(22.73)	
Judicial error t-1			-22.44			-71.67**			-27.10
			(16.56)			(33.72)			(32.86)
Judicial error t+1			11.41			18.77			-36.90
			(13.59)			(39.03)			(25.70)
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,518	8,518	8,518	5,116	5,116	5,116	4,121	4,121	4,121
Sample mean	3259	3259	3259	4464	4464	4464	2897	2897	2897

Table E4: Effect of content of news on sentence length, by type of crime judged

Appendix F:
Descriptive statistics felonies committed by juvenile

	Felonies in juvenile court (without juror)	Juveniles in criminal court (with juror)	15 years old (without juror)	16 years old (with juror)
Percent male	.97	.96	.96	.97
French	.97	.93	.96	.95
Investigation length (days)	1892	1746	1419	1995
Prison (days)	758	2169.07	980	1799
Murder & violence	.05	.18	.09	.13
Rape	.84	.51	.70	.68
Armed robbery	.11	.3	.21	.18

Appendix G

Robustness check: Systematic analysis of keywords used more than 200 times

Over all keywords used more than 200 times, 32 words are related to crime, 2 are related to judicial errors and 327 are related to other topics. We run regressions of sentences on the number of news stories using each keyword at t-1 and t+1, with and without control variables.

Results are consistent with an effect of media on sentences if coefficients at t-1 are significant and have the same sign with and without control variables while coefficients at t+1 are not significant. 14 words fill this criteria. Four are related to crime (among 32, 12.5%) and all have positive coefficient at t-1; one is related to judicial error (among 2, 50%), and 9 are not related to justice (among 327, 2.75%). Among those 9 words unrelated to justice, one keyword, "racism" is strongly correlated with keywords concerning crime. The other 8 words ("building", "closure", "labor conditions", "travelers", "back-to-school", "air transport", "road transport", "second world war"), do not belong to similar semantic fields (except the two on transport which have coefficients in the opposite directions). Even if not significant, point estimates at t+1 are high for these placebo words, while they are not for words related to crime or judicial errors. Average point estimates at t-1 are equal to 0.07 among the words unrelated to justice while it is 7.78 among keywords related to crime and -43 among the two keywords related to judicial errors.