

Measuring Crime Reporting and Incidence: Method and Application to #MeToo

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November 28, 2022

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Abstract

This paper studies the Me Too movement's effects on sex criminality. As many victims do not report to the police, a long-standing empirical challenge with reported crime statistics is that they reflect variations in victim reporting *and* crime incidence. To separate both margins, I develop a duration model that studies the delay between the incident's occurrence and its report to the police. The model accounts for unobserved heterogeneity, never-reporters, and double-truncation in the data. I apply it to the police records of large US cities. Contrary to the widespread view that #MeToo was a watershed moment, I find that sex crime reporting had already been increasing for years before its sudden mediatization in October 2017. Nonetheless, the movement had a positive, persistent impact on victim reporting, particularly for juveniles, racial minorities, and victims of misdemeanors and old crime incidents. The increase in reporting translates into drastically higher probabilities of arrest for sex offenders. Using reported non-sexual crimes as a control group, difference-in-differences estimates suggest the movement also had a sizable deterrent effect.

Keywords: crime reporting, crime deterrence, sex crimes, #metoo, survival analysis, double-truncation

JEL Classification: C18, C24, C41, J16, K14, K42

* germain.gauthier@polytechnique.edu – I am extremely grateful to my advisor Alessandro Riboni for his continuous guidance and encouragement on this project. For helpful discussions and suggestions, I would like to thank Elliott Ash, Christophe Bellego, Christian Belzil, Guillaume Bied, Xavier d'Haultfoeuille, Jean-David Fermanian, Roberto Galbiati, Pauline Rossi, Arne Uhendorff, Bella Vakulenko-Lagun, Gerard Van den Berg, Philine Widmer, and Yanos Zylberberg. I would also like to thank seminar participants at CREST, ETH Zürich, University of St. Gallen, Bocconi University, University Paris-Nanterre, and HEC Paris. This research is supported by a grant of the French National Research Agency (ANR), “Investissements d’Avenir” (LabEx Ecodec/ANR-11-LABX-0047”).

1 Introduction

In October 2017, the Me Too movement led millions of women worldwide to protest against sexual violence. Enthusiastic commentators portrayed the movement as a game-changer in the history of women’s rights.¹ Others, more skeptical, raised concerns about false allegations, backlash effects, and socioeconomic and racial divides.² As for most interventions against crime, a major impediment to evaluating the movement’s impact on crime reporting and incidence is that many victims do not come forward. Between 1995 and 2010, U.S. national surveys estimated over 6 million rape and sexual assault victims, of which 60 to 70% did not report the incident to the police (Plantz et al., 2013). In turn, a long-standing empirical challenge has been to interpret variations in reported crimes as changes in the number of offenses committed or in victims’ propensity to report (Quêtelet, 1831; Levitt, 1998).

This paper proposes a general methodology to disentangle victim reporting and crime incidence from police data. I use this novel approach to provide empirical evidence of the Me Too movement’s impact on sex criminality. I find three key results. First, the victim reporting rate had already been increasing for years before #MeToo. Second, the movement had a positive and persistent effect on victim reporting – particularly for the most vulnerable groups of the population. Third, it had a large deterrent effect on sex offenders.

My analysis relies on the police records of New York City, Los Angeles, Cincinnati, and Seattle between 2010 and 2019. I document that more than half of sex crime charges are filed with a delay relative to the incident date. Reports can occur days, months, and sometimes years after the crime. In addition, I show that there was a large increase in reported sex crimes over the decade but that delayed reports primarily drive it. This pattern is consistent with the progressive depletion of a large stock of unreported sex

¹For example, see AP News (2017); Psychology Today (2017); Berkeley Law (2019).

²For examples, see Forbes (2020); Harvard Business Review (2019); New York Post (2020); New York Times (2017a); AP News (2021); New York Times (2017b).

crimes and suggests victims became more likely to report over time.

Building on this intuition, I develop a reduced-form, flexible duration model to analyze delayed reports over time. Victims enter the study on the incident date and exit upon filing a complaint to the police. As some victims never report, I extend the canonical mixed proportional hazards (MPH) model to explicitly account for never-reporters. This modification does not require additional statistical assumptions. However, it requires researcher knowledge about the share of never-reporters at the beginning of the study period. The duration model allows me to reconstruct all subsequent variations in this share over time. In turn, I can decompose a time-series of reported crimes into two margins of crime reporting and incidence.

I estimate the model by maximizing the likelihood function integrated over the random effects. However, the model's estimation is complicated by double-truncation in the data. Double-truncation is a non-trivial sample selection scheme that arises because I only observe plaintiffs who report a crime to the police during the study period. Plaintiffs with shorter reporting delays are less likely to enter the study, and thus, left truncation leads to a sample biased towards larger reporting delays. Conversely, plaintiffs with longer reporting delays are less likely to report before the end of the study, and thus, right-truncation leads to a study sample biased towards smaller reporting delays. Without a suitable correction, I show that a naive, out-of-the-box implementation of the MPH model returns severely biased estimates. I thus correct the likelihood to account for double-truncation. The correction weights each observation by the inverse of their sampling probability. The identification assumption is that reporting delays are independent of the date of the incident once conditioning on the history of interventions affecting victim reporting (e.g., #MeToo). In Monte Carlo simulations, the proposed estimator largely outperforms the naive estimator, with no bias in estimates.

I then take the model to the data. First, I focus on *plaintiffs*. I show that abstracting from the subset of victims who did not report to the police provides a lower bound on #MeToo's effects on the hazard of reporting. I find that the hazard of filing a complaint

increased by 15 to 20% following the intense mediatization of #MeToo in October 2017. The effect increases over time, suggesting that the movement had a durable impact on reporting norms. Plaintiffs are approximately twice as likely to report past crimes after #MeToo (relative to recent crimes). Thus, encouraging sex crime victims from past decades to reconsider filing a complaint to the police partly allowed older generations to “catch up” with the reporting rate of younger cohorts. Finally, juveniles and racial minorities appear particularly responsive to the intervention. This mitigates concerns of the unequal treatment of age and race in the movement’s media coverage ([Onwuachi-Willig, 2018](#)). These results are robust to various specifications for the baseline hazard, unobserved heterogeneity, incident-level characteristics, and placebo dates.

I further investigate the implications of these findings for the *entire victim population*. In line with national estimates, I assume that 70% of the victims would not have reported incidents for 2010.³ When accounting for never-reporters, I uncover a large, linear, and positive pre-trend between 2010 and 2017. This contradicts the widespread view that #MeToo was a watershed moment. It is not to say, however, that the movement had no impact. The timing of #MeToo appears as a structural break in the time-series and reinforces pre-existing trends. I estimate that the share of victims who eventually report a sex crime to the police more than doubled over the decade, reaching 75%. #MeToo accounts for approximately 25% of the increase in sex crime reporting over the period. Alternative values for the share of never-reporters in 2010 do not revert these trends but affect their magnitude. I show that my main results are robust to a broad range of reasonable parameter values, ranging from 60 to 80% of never-reporters.⁴ Moreover, when focusing on the segment of complaints that lead to arrests, I find qualitatively similar trends, suggesting unfounded allegations do not drive the results.

Next, I reconstruct the time series of sex crime incidence based on estimated reporting rates. Empirical estimates indicate that sex crime incidence decreases by approximately

³Absent any future interventions that may affect victim reporting propensity (such as #MeToo).

⁴This interval encompasses all estimates of the reporting rate of victims of sexual violence by the National Crime Victimization Survey since 2011.

35% between 2010 and 2019. The decomposition of reported sex crimes thus reveals a substantial increase in sex crime reporting and a substantial decrease in sex crime incidence over time. The two margins partly cancel each other out and are thus less apparent in the time series of reported crimes. Using this newly constructed time-series, I assess the Me Too movement’s impact on sex crime incidence. To account for potential confounders, I use reported non-sexual crimes as a plausible control group in a difference-in-differences setup. In the post-treatment period, I find a large and statistically significant deterrent effect of 23% per quarter. I find no effect for placebo dates as well as for non-sexual crimes. My results are also robust to alternative counterfactual models, including an interactive fixed effects model ([Xu, 2017](#)) and the matrix completion method ([Athey et al., 2021](#)). In my baseline specification, the Me Too movement accounts for approximately 12% of the decrease in sex crime incidence over the period.

I consider several channels that may affect victim and offender behaviors. Regarding victims, a plausible explanation is a social norm narrative in which the social cost of reporting a sex crime has decreased. Consistent with this interpretation, I show that the number of tweets surrounding sexual assault has increased over the decade (and so did Google queries). This pattern suggests an increase in sexual violence awareness. Regarding criminal behavior, my results are consistent with a Beckerian model of crime. The reporting rate has increased the probability of arrest for sex offenders, from roughly 15% in 2010 to 37% in 2019. In the data, a one percentage point increase in the probability of arrest is associated with a 0.9 percentage points decrease in sex crimes.

This paper is organized as follows. Section [2](#) reviews the literature. Section [3](#) presents the data sources and a brief history of the Me Too movement. Section [4](#) discusses empirical patterns surrounding #MeToo in light of a simple conceptual framework. Section [5](#) outlines a general method to infer variations in crime incidence and reporting from police records with delayed reports. Section [6](#) presents estimates of the Me Too movement’s effects on victims and offenders. Section [7](#) discusses plausible mechanisms. Finally, Section [8](#) concludes.

2 Related Literature

My results inform several distinct strands of literature. First, I contribute to the nascent literature that studies the origins and consequences of the Me Too movement. Public allegations of sexual misconduct have substantially impacted company valuations on financial markets and labor market outcomes (Borelli-Kjaer et al., 2021; Luo and Zhang, 2022; Sophie Calder-Wang and Sweeney, 2021; Batut et al., 2021; Cici et al., 2021; Bernabe, 2021; Gertsberg, 2022). Closely related to this paper, Levy and Mattsson (2021) estimate that the Me Too movement has led to a 10% increase in sex crime reports in a large sample of OECD countries (including the United States).⁵ I study the Me Too movement's impact on sex criminality with a novel methodology. My empirical strategy presents several advantages, including the ability to jointly study variations in crime reporting and incidence at fine-grained scales (e.g., cities). I provide novel insights into the movement's origins and impact in four ways. First, I show that past studies likely underestimated the impact of the Me Too Movement on sex crime reporting. Second – and related to this first point – my results indicate that the movement's deterrent effect is large and statistically significant. Third, I find that the movement had a differential impact along socio-demographic lines and was particularly effective at fostering reporting among racial minorities, juveniles, and past victims of sex crimes. Fourth, I uncover substantial trends in crime reporting and incidence before the Me Too movement. This last result cautions against interpreting social movements as “as-good-as-random” events. If the timing of #MeToo was largely unforeseen, it nonetheless appears in an opportune context of shifting social norms related to sexual violence.

More generally, I contribute to the literature on the under-reporting of gender-based violence (Cheng and Hsiaw, 2020). Previous studies have found that the election of female politicians and the number of female officers in the police workforce have increased the reporting of gender-based violence in India (Iyer et al., 2012; Miller and Segal, 2019).

⁵Rotenberg and Cotter (2018) also find similar descriptive evidence for Canada.

Displays of public outrage for highly mediatized affairs, such as allegations of pedophilia in the Catholic Church and particularly gruesome rape cases in India, have also increased the number of victims coming forward (Bottan and Perez-Truglia, 2015; Mathur et al., 2019; Sahay, 2021; McDougal et al., 2021). The Me Too movement is arguably the largest and most persistent public awareness campaign on sexual violence. I provide empirical evidence that it successfully encouraged victims to file a complaint to the police. Its impact is not limited to victims, however, as the movement is associated with a large decrease in sex crime incidence.

This last finding has implications for the broader literature on crime deterrence. Dating back to Becker (1968), crime deterrence has been extensively studied by scholars (for reviews, see Nagin, 2013; Chalfin and McCrary, 2017; Doleac, 2019). Through the careful study of fines, arrests, and prosecutions, the crime literature analyzes incentives within the criminal justice system but rarely investigates how private attitudes affect enforcement. A smaller literature in political economy, however, has noted that prevailing social norms often co-exist with the criminal justice system and can both facilitate and hinder law enforcement efforts (Hay and Shleifer, 1998; Berkowitz et al., 2003; Benabou and Tirole, 2011; Young, 2015). In particular, the monitoring and punishment of unlawful behaviors depends to a large extent on the willingness of victims to report incidents to law enforcement agencies (Akerlof and Yellen, 1994; Dyck et al., 2010; Acemoglu and Jackson, 2017). By increasing the probability of arrest and prosecution, my results highlight that changes in reporting norms can successfully enforce socially desirable behaviors when the legal system fails to do so on its own.

This paper also connects to the large amount of literature on the reliability of reported crime statistics. Since the *XIXth* century, scholars refer to the share of crimes that is neither reported to nor recorded by law enforcement agencies as the *dark figure of crime* (Coleman and Moynihan, 1996).⁶ To this day, unobserved crimes pose a serious empirical

⁶The expression *dark figure of crime* is attributed to the Belgian mathematician Adolphe Quêtelet. Though he raised the issue in the first half of the 19th century, it became popular in the 1960s.

challenge for analyzing and interpreting police records.⁷ It is particularly problematic when studying the effectiveness of interventions to fight crime. If such interventions affect both crime reporting and incidence, then estimates of treatment effects based on reported crime statistics will be biased ([Levitt, 1998](#)). Though the dark figure of crime represents a well-documented problem, few approaches attempt to disentangle reporting behaviors from crime rates. As a result, researchers heavily rely on victimization surveys to better monitor victim and offender behaviors. Police databases remain, however, the only source of geographically disaggregated data on crime that allows researchers to exploit geographic and time variation in treatment assignment to identify treatment effects. Developing new methods tailored for these records is thus critical to improving our understanding of crime reporting and deterrence. Several contributions in economics have relied on proxy variables to disentangle both margins (e.g., [Aizer, 2010](#); [Stephens-Davidowitz, 2013](#); [Bellégo and Drouard, 2019](#)). Most frameworks implicitly or explicitly assume crimes are reported in short periods to the police or never at all. This is not the case for sex crimes, domestic violence, and harassment complaints, among others. The existence of delayed reports and the empirical issues they raise remain understudied (for notable exceptions, see [Lee and Suen, 2019](#); [Klemmer et al., 2021](#)). I contribute in three ways to this literature. First, I clarify the econometric implications of delayed reports. Second, I propose a solution to monitor variations in victim reporting and crime incidence for crimes reported over long periods. Finally, I provide real-world evidence that underreporting is a serious empirical threat for practitioners and is a first-order concern for the credible impact evaluation of interventions.

On a more technical note, I contribute to the analysis of doubly-truncated data in survival analysis (see [Dörre and Emura, 2019](#), for an overview). Beyond police data, double-truncation is a sampling scheme that arises in biostatistics ([LAGAKOS et al., 1988](#); [Moreira and de Una-Alvarez, 2010](#); [Emura and Murotani, 2015](#)), engineering ([Ye and Tang,](#)

⁷For example, around 40% of violent victimizations and 33% of property victimizations were reported to the police in 2019 ([Morgan and Thompson, 2021](#)).

2016), astronomy (Efron and Petrosian, 1999), and economics (Dörre, 2020). Previous research has derived semi-parametric Cox regression models for doubly-truncated data (Vakulenko-Lagun et al., 2019; Rennert and Xie, 2018; Mandel et al., 2018). These approaches assume *unconditional* independence between the duration and the truncation times. This is often too restrictive an assumption in the presence of time-varying covariates. For instance, in the case of the Me Too movement, it rules out the possibility that the movement would have affected victims' reporting of sex crimes to the police. Furthermore, these models assume homogeneous populations. In practice, unobserved heterogeneity is common, and modeling it is important for valid causal inference (Abbring and Van den Berg, 2003). I thus develop parametric yet very flexible duration models for doubly-truncated data that solve these two methodological shortcomings. First, I relax the unconditional independence assumption to a more realistic *conditional* independence assumption. Controlling for the Me Too movement's viral outbreak on social media thus makes it possible to assess its impact on victim reporting. Second, I extend the model to unobserved heterogeneity.

3 Data and Context

3.1 Data Sources

City-level Police Data My main data source is detailed incident-level police records for New York City, Los Angeles, Seattle, and Cincinnati, between 2010 and 2019.⁸ These cities represent a combined population of approximately 13 million Americans. The records are official administrative data. The data collection is meant to be rigorous and systematic, and is sent later to the FBI for consolidation. Importantly, the records distinguish the incident date from the date of its report to the authorities.⁹ This will be crucial for my

⁸I stop the data collection in December 2019, because lockdowns and other restrictions to fight the COVID pandemic may have affected sex criminality from 2020 onwards.

⁹The Uniform Crime Reporting (UCR) yearly data provides yearly aggregate reports per agency, but no incident-level observations. The National Incident-Based Reporting System (NIBRS) provides incident-

empirical strategy (see Section 5). For Los Angeles and New York City, I also download offender-level arrest datasets over the same period. This allows me to compute arrest rates per crime category.

I process the data in the following way. I manually classify offenses as *sexual* or *non-sexual*. I exclude sexual offenses related to *pornography*, *indecency*, *loitering*, *sexting*, and *prostitution*. When possible, I further distinguish sex offenses between *misdemeanors* and *felonies*.¹⁰ For non-sexual offenses, I focus on four broad categories: *burglary*, *robbery*, *assault*, and *murder*. I exclude all other non-sexual offenses from the analysis. For a complete list of offenses considered and excluded, see Appendix A. I also observe the socio-demographic characteristics of plaintiffs and alleged offenders, such as their self-declared race, age, and sex. The sex variable has three groups: *male*, *female*, and *unknown*. For the self-declared race, I form four categories: *black*, *hispanic*, *white*, and *other/unknown*. For the age of plaintiffs and offenders, I create a dummy variable *juvenile* that takes values zero for adults (above 18 years old) and one for children (below 18 years old). The age recorded is the plaintiff's (offender's) age upon filing the complaint. Finally, I exclude from the sample all complaints with incoherent dates for the incident or its report to the police.¹¹

The FBI Consolidated Databases The FBI's Uniform Crime Reporting (UCR) program provides the general public with a broad range of statistics from local law enforcement agencies. To compare my results to national reported crime statistics, I collect official consolidated crime databases between 1976 and 2019. Reported crimes are harmonized into the Uniform Crime Reporting Summary Reporting System (SRS) (Kaplan, 2021). The

level observations, but solely records the incident date and omits the report date to the police. I rely on city-level datasets (Police Data Initiative, 2021). I choose cities that provide (i) incident-level data for the period 2010–2019, (ii) include sex crime reports, and (iii) distinguish the date of the incident and the date of its report to the police. I further exclude Austin and Tucson from the sample, because the distribution of reporting delays in these cities is extremely skewed around the first day and raises data quality concerns. The main results are qualitatively similar when including these two cities in the estimations.

¹⁰New York City makes a clear distinction between the two categories. Other cities do not.

¹¹In some cases, the incident's date is later than the date of its report, or one of the two dates is missing. These represent 1.5% of the raw data.

SRS is a crime report database aggregated by month, agency, and crime category.¹² It is the most comprehensive database on offenses known and clearances by arrest in the United States.¹³ The UCR also provides supplementary reports on homicides (Kaplan, 2019). This allows me to compute sexual and non-sexual homicide rates.

The National Crime Victimization Survey To compare my results to national survey estimates, I collect the National Crime Victimization Survey (NCVS) responses between 2010 and 2019. The Bureau of Justice Statistics (BJS) each year surveys a nationally representative sample of about 240,000 persons in approximately 150,000 households. Respondents are explicitly asked about their experiences of criminal victimization and whether they reported the incidents to the police. Thus, one can compute direct estimates of crime reporting and incidence.

Google Queries on Sex Criminality I download from Google the web search interest for the topics “sexual assault” and “Me Too movement” in the United States between 2010 and 2019. The query counts are normalized so that the largest number of queries over the period equals one hundred. I interpret this dataset as a proxy for sexual violence awareness.

Tweets on Sex Criminality I download all tweets related to sexual assault on the 1st of each month between 2013 and 2019. I also add tweets from the 15th of October 2017 (i.e.,

¹²In 2013, the FBI changed its national definition of rape to include a broader range of sexual offenses. On the FBI's website, one may read: "*The old definition was 'The carnal knowledge of a female forcibly and against her will.' Many agencies interpreted this definition as excluding a long list of sex offenses that are criminal in most jurisdictions, such as offenses involving oral or anal penetration, penetration with objects, and rape of males. The new Summary definition of Rape is: 'Penetration, no matter how slight, of the vagina or anus with any body part or object, or oral penetration by a sex organ of another person, without the consent of the victim.'*" This change in definition is unlikely to affect my city-level estimates because I rely on the more detailed city-level classification of offenses to define sexual and non-sexual crimes. In Column 8 of Table 2, I show that restricting the sample to 2014 – 2019 does not substantially change my main result.

¹³The National Incident-Based Reporting System (NIBRS) is a more recent data collection effort implemented to improve the overall quality of crime data collected by law enforcement. For each incident, it records the date of its occurrence (but not the date of its report to the police). I do not rely on this database because it has limited geographical coverage relative to the SRS. According to the FBI, in 2017, it covered 33% of the US population.

the day when #MeToo went viral on social media). I identify tweets on sexual assault based on a large set of explicit keywords, such as: “rape”, “sexual assault” and “#metoo”. The traffic on Twitter has increased drastically over the years, going from hundreds of daily tweets at its creation in 2006 to 500 million daily tweets in 2021. To make meaningful comparisons over time, I collect the total number of tweets posted on each day of data collection. The complete list of keywords and further details may be found in Appendix A. Once again, I interpret this dataset as a proxy for growing sexual violence awareness.

3.2 Descriptive Statistics

Summary statistics on the city-level dataset are provided in Table A.3. Between 2010 and 2019, there were over 2 million crime incidents reported. Among those, approximately 110,000 were sexual crimes. Sexual criminality remains largely gender-specific, with 87% of reports filed by women in the sample (95% when excluding incidents without this information). In terms of declared race, black plaintiffs and Hispanics form the bulk of sex crime reports. Furthermore, children and teenagers are particularly exposed to sexual criminality, with around 43% of plaintiffs declaring being below 18 when filing the complaint. Alleged offenders, on the other hand, are mainly adult males belonging to a racial minority.

A striking feature in the data is the incidence of delayed reporting: approximately 30% of the total number of offenses are not reported on the day of the incident (I refer to these complaints as *delayed* reports as opposed to *direct* ones). But this figure hides substantial heterogeneity across offenses. 42% of sex crimes are direct reports as opposed to 79% for non-sexual assaults and 83% for robberies. The difference is also more sizable for mean reporting delays. The average time to report a sex crime is 197 days, as opposed to less than six days for non-sexual assaults, robberies, and burglaries.¹⁴ The standard deviation in reporting delays is almost sixteen times larger for sex crimes relative to non-sexual

¹⁴Murders have rather long reporting delays when the police record them with a delay. Those correspond to cold cases.

assaults, robberies, or burglaries. Figure A.1 presents the sample distribution of delayed sex crime reports. Approximately 40% of plaintiffs report on the day of the incident, 80% within the first month, and 90% within the first year.

Delayed reporting also varies along socio-demographic lines. Longer reporting delays are typically expected for subgroups of the population with higher costs to filing a complaint. Figure A.2 displays boxplots of reporting delays per age group and declared plaintiffs' race. Juveniles report over larger periods than adults. Hispanic and black plaintiffs also display longer reporting delays than other racial categories.

3.3 The Me Too Movement

The Me Too movement is a social movement against all forms of sexual misconduct where people share and publicize allegations of sex crimes. Its explicit goal is to raise awareness of the pervasiveness of sexual violence in society. Social activist Tarana Burke launched the movement on MySpace in 2006. For over ten years, the campaign focused on female minorities (mainly black women) and benefited from limited media coverage. On the 15th of October 2017, it was popularized by actress Alyssa Milano in reaction to the Harvey Weinstein affairs. She tweeted: *"If you've been sexually harassed or assaulted write 'me too' as a reply to this tweet."* In the following days, the hashtag #MeToo spread virally on social media and was posted millions of times on Twitter, Facebook, and other platforms worldwide.

Following its mass mediatization, mentions of the hashtag #MeToo have dwarfed past references to the Me Too movement (see Figures A.7). However, this large discontinuity in the time series should not oust the social and historical context in which the movement emerged. Several pieces of anecdotal evidence suggest that attitudes toward sex crimes had been changing for over a decade. The movement appeared 11 years before it became popular. The traditional media also brought many affairs to the spotlight during the 2000s: examples include sex crime allegations in the Catholic Church, the Bill Cosby

sexual assault cases, and allegations concerning Harvey Weinstein before 2017. On social media, several hashtags denouncing violence against women preceded #metoo but were less viral (e.g., #YesAllWomen, #IAmNotAfraidToSpeak, #myHarveyWeinstein and #BeBrave). Given these early signs of changing attitudes towards women, one would expect positive time trends in crime reporting before October 2017. I find empirical support for this hypothesis in Section 6.

Debated on television and in the newspapers, the Me Too movement also raised critiques as it gained momentum. Some pointed to the risk of false allegations.¹⁵ Others claimed the movement failed to recognize the heightened vulnerability that women of color frequently face.¹⁶ Finally, proponents warned against the potential backlash.¹⁷ I address these concerns in my empirical assessment of the movement's impact on victims and offenders. Overall, in the scope of this study, false allegations are unlikely to be the drivers behind the large increase in sex crime reports, the reporting rate increased more for black, Hispanic, and juvenile victims, and there was no backlash effect in sex crime incidence (see Section 6).

4 Conceptual Framework

"I do not fear to say that all we possess of statistics of crime and misdemeanors would have no utility at all if we did not tacitly assume that there is a nearly invariable relationship between offenses known and adjudicated and the total unknown sum of offenses committed."

– Quêtelet (1831)

This section reviews the empirical issues researchers encounter when studying police data. I focus more specifically on the large share of crimes that are not reported or

¹⁵For example, see [Forbes \(2020\)](#) and [New York Post \(2020\)](#).

¹⁶For example, see [New York Times \(2017a\)](#) and [AP News \(2021\)](#).

¹⁷For example, see [Harvard Business Review \(2019\)](#).

reported with a delay and their consequences for causal inference.

4.1 The Canonical Problem

Consider an analyst disposing of reports recorded by the police between τ_1 and τ_2 , respectively the first and last calendar data collection dates. She aims to study the impact of an intervention (e.g., the Me Too movement, an increase in the number of police officers, a harsher institutional penalty) on the number of crimes C_t . The intervention takes place in period $t^* \in [\tau_1, \tau_2]$. For each period t , she observes R_t , the number of crimes recorded by the police. As a share of victims does not report the incident to the police, reported crimes R_t generally do not equate to the total number of crimes C_t . Let r_t denote the victim reporting rate. Assuming no delayed reporting, we have

$$R_t = r_t \times C_t. \quad (1)$$

It becomes apparent that reported crimes R_t are a function of two latent variables and that a simple linear regression framework will be subject to an omitted variable bias. Let D_t be a dummy variable that takes the value one in periods after the intervention (i.e., $t \geq t^*$) and zero otherwise. We have

$$\log(C_t) = a + bD_t + \varepsilon_t - \log(r_t), \quad (2)$$

where a is an intercept term, b is the coefficient associated with D_t , and ε is an error term.

If the share of unreported crimes r_t is correlated to the treatment D_t , then estimates of b will be biased. In many applications, researchers explicitly or implicitly assume that the reporting rate r_t is orthogonal to D_t to conduct inference. Though this is a convenient assumption, it is also unlikely to hold in practice. On the contrary, interventions aimed at fighting crime often increase the probability of arrest or the severity of sentencing. Theory suggests that these factors matter for victims and offenders, thus one would expect such

interventions to lower crime rates and increase reporting rates.

4.2 Adding Delayed Reports

In the absence of delayed reports, reported crimes R_t may be understood as in Equation 1. In practice, however, delayed reports are common in police data, particularly for sex crimes (as documented in Subsection 3.2).

Delayed reports require a careful understanding of the reporting guidelines of law enforcement agencies. There are now two dates to consider: the incident date and the date of its report to the police. Aggregating reported crimes on the date of their report or the incident date will generally lead to different counts (and ultimately estimates). When both dates are available, it is straightforward to show that crime trends are typically sensitive to the choice of the date (see Appendix A for examples based on police data).¹⁸

One can extend Equation 1 to account for delayed reports. Let R_{t,τ_1,τ_2} denote the number of crimes committed in period t that were reported between τ_1 and τ_2 , τ_0 the calendar date of the earliest incident in the data, Y the time to report to the police, F its associated cumulative distribution function, and χ_t the history of interventions. R_{t,τ_1,τ_2} is now

$$\mathbb{E}[R_{t,\tau_1,\tau_2}] = p_t \times C_t, \quad (3)$$

where $p_t = F(\tau_2 - t \mid \chi_t) - F(\max(\tau_1 - t, 0) \mid \chi_t)$ is simply the probability of reporting a crime that occurred at date t within the study period. The omitted variable bias in the classical regression analysis remains (Equation 2). Furthermore, p_t depends on the study period considered (i.e., on τ_1 and τ_2). Thus, it is also mechanically correlated to D_t . For instance, the closer t is to the end of the study period τ_2 , the smaller the probability of reporting a crime that occurred in period t before the end of data collection. This implies a spurious decreasing time trend in observed reported crimes R_{t,τ_1,τ_2} . As a result, in the

¹⁸In most cases, both dates are not available. For instance, in the United States, the Uniform Crime Reporting (UCR) data focuses on the report date, whereas the National Incident-Based Reporting System (NIBRS) relies on the incident date.

classical regression framework (Equation 2), estimates of the marginal effect of D_t will generally be biased (see Appendix A for an example based on police data).¹⁹

4.3 Reported Crimes Around #MeToo

Given never-reporters and delayed reporting, understanding the effects of the Me Too movement's sudden mass mediatization is not straightforward. City-level and national reported crime statistics indicate that sex crime reports have increased between 2010 and 2019. In Figure A.5, I plot national reported crime trends for sexual and non-sexual crimes over time. The trends are largely similar to those found in my sample of cities (see Panel A of Figure 1). Reported sex crimes at the national and city level increase by approximately 40%. Interestingly, the surge starts before #MeToo. Following Equation 1, this can be rationalized either as an increase in sex crime reporting or an increase in sex crime incidence, if not variations in both latent variables.

Often, researchers turn to victimization surveys for direct estimates of sex crime incidence and reporting. Thus, in Figure A.4, I present estimates from the NCVS between 2010 and 2019. Survey evidence indicates that increased sex crime incidence mainly drives the rise in sex crime reports (+300% over the decade). The increase is particularly large after #MeToo, suggesting a backlash effect.

At the same time, this narrative appears inconsistent with several pieces of evidence in police records. First, reported non-sexual crimes at the national level decreased by approximately 20% (and they remain stable in the city-level dataset). If we assume that sex crime incidence is positively correlated with non-sexual crimes, then this suggests a decrease in sex crime incidence. Second – and perhaps even more convincingly – homicides related to sex crimes decrease by approximately 70% between 2010 and 2019, whereas homicides unrelated to sex crimes remained stable. Sex crime homicides are very few (less than a hundred cases per year), but arguably do not suffer from under-reporting.

¹⁹Interestingly, they will be biased even if the intervention D_t has no effect on crime incidence C_t or crime reporting p_t .

Once again, if we assume that sex crime incidence is positively correlated to sex crime homicides, this suggests a decrease in sex crime incidence. Third, in Panel B of Figure 1, I distinguish delayed and direct reports for sex crimes. Delayed reports increased twice as much as direct reports over the period. This stylized fact is consistent with the depletion of a large stock of unreported sex crimes being progressively reported to the police. Thus, it also suggests an increase in reporting behaviors rather than an increase in sex crime incidence.

5 Empirical Framework

Given the prevalence of delayed reporting in police records, the timing of sex crime reporting is an attractive dependent variable to separate sex crime incidence and reporting. Following Equation 3, the method proposed in this paper aims at estimating the probability distribution of times to report Y (which is potentially affected by a series of interventions) to recover an estimate of p_t . To this end, I develop reduced-form duration models to estimate the reporting hazard a sex crime to the police. In this section, I discuss the models, their estimation, and validate my estimators via Monte Carlo simulations.

5.1 The Structure of Crime Reports

To begin, I focus on the data structure of police records. Some of its peculiarities are relevant for survival analysis. Figure 2 presents a graphical summary of police data as duration data. The study window goes from τ_1 to τ_2 (solid vertical lines), with an intervention D_t in between represented by a dashed vertical line. Some plaintiffs report before the intervention's implementation and form the control group (non-treated observations). Still, others are affected by the intervention and form the treated group (treated observations). Some plaintiffs report before the start of the study and are unobserved (left-truncated data points). Some plaintiffs have not yet reported a crime to the police by the end of the study but will in the future and are unobserved (right-truncated data

points). Finally, some victims may decide never to report and are unobserved. We will call them never-reporters. This graphical depiction raises two empirical challenges to correctly estimate the probability distribution of times to report Y .

First, the data is doubly-truncated (on the left and the right). Though left-truncation is common in economic applications of survival analysis, right-truncation is a relatively understudied truncation scheme that requires special attention. To account for double-truncation, I provide an analytical correction of the log-likelihood, which I explain in greater detail in Subsection 5.5.

Second, the model needs to account for the share of never-reporters. This implies that the cumulative distribution function of times to report Y will be improper and have a positive mass as y tends to infinity. In bio-statistics, such models are referred to as “cure models” (see [Amico and Van Keilegom, 2018](#), for a review). I propose a promotion time model to account for this stylized fact, which I explain in greater detail below (see Subsection 5.3).

5.2 Plaintiff Reporting Hazard

Let (Y, X) denote a random vector where $Y \in \mathbb{R}^+$ is the time to report of a victim and $X \in \mathbb{R}^d$ contains observed covariates. The standard Mixed Proportional Hazards (MPH) model assumes the conditional reporting hazard at time y has the form²⁰

$$h_{it}^{(p)}(y | \gamma_i, x_{ity}) = h_0(y) \exp(\beta' x_{ity}) \gamma_i, \quad (4)$$

where $h_0 : \mathbb{R}^+ \rightarrow \mathbb{R}^+$ is the baseline hazard function, $(\beta') \in \mathbb{R}^{d+1}$ is the vector of regression coefficients and $\gamma_i \in \mathbb{R}$ is an individual-specific, time-invariant, unobserved random effect (i.e., often referred to as unobserved heterogeneity or frailty). When $\gamma_i = 1$ for all observations, the model boils down to the canonical Cox model ([Cox, 1972](#)). The core assumptions of this model are (i) proportional hazards and (ii) the multiplicative decom-

²⁰I write random variables in uppercase and their realizations in lowercase.

position between the baseline hazard, the effect of observed covariates, and the effect of individual-specific unobserved heterogeneity (Van den Berg, 2001). Note that covariates x_{ity} may be time-varying. In such cases, their values depend on the calendar incident date t and the duration y (e.g., if $t + y \geq \text{Oct.2017}$, then $\text{MeToo}_{ity} = 1$, otherwise $\text{MeToo}_{ity} = 0$).

In an ideal empirical setting, a researcher would observe all victims, and it would be straightforward to estimate the hazard of reporting a crime to the police. Unfortunately, as many crimes go unreported, fitting the model in Equation 4 to police records only provides estimates of the reporting hazard a crime to the police among *plaintiffs* (see Panel A of Figure 3). It thus focuses on the duration of reporting delays. There are compelling reasons for focusing on this dependent variable as many studies have highlighted the benefits of early formal disclosure (Klemmer et al., 2021).²¹

Despite the importance of studying the duration of delays, the duration alone falls short of informing us of the reporting rate in the general population of *victims* (see Panel B of Figure 3). At the same time, one would assume that the reporting rate correlates with reporting delays. If plaintiffs report *faster* to the police, then perhaps victims also report *more*. In what follows, I present a modification of the MPH model that builds on this intuition.

5.3 Victim Reporting Hazard

We can explicitly account for never-reporters if we enforce the baseline hazard function as a density function and add an intercept to the regression model of Equation 4:

$$h_{it}^{(v)}(y | \gamma_i, x_{ity}) = f_0(y) \exp(\alpha + \beta' x_{ity}) \gamma_i, \quad (5)$$

²¹Tavarez (2021) summarizes: “Delayed reporting of sexual violence can adversely impact medical treatment, case outcomes, and the overall mental state of survivors. [...] Survivors reporting and seeking support within the first 24 hours have better treatment outcomes and their medical examinations can provide the best possible forensic evidence to assist prosecution [...] forensic evidence for potential drugging, collected through the survivor’s hair, can only be collected approximately within one month of the rape [...] Therefore, victims who formally disclose sooner (approximately within 72 hours) have been found to experience a broader extent of available options and better case outcomes, such as evidence collection, emergency medical care, and apprehension of the perpetrator.” In addition, longer reporting delays are often associated with repeated, longer periods of abuse. Formal reporting may ensure the safety of victims and prevent future offenses.

where f_0 is a proper density function²² and $(\alpha, \beta') \in \mathbb{R}^{d+1}$ is the vector of regression coefficients. In this model, α accounts for the share of never-reporters at baseline, β synthesizes covariate effects on the response, and f_0 models the duration influence. The model incorporates the main stylized facts mentioned previously: (i) a share of victims report with a delay, and (ii) a share of victims will never report.²³ In the simplest case of no time-varying covariates, the baseline proportion of victims who will never report a crime to the police is²⁴

$$\lim_{y \rightarrow \infty} S_i^{(v)}(y | \gamma_i, x_i) = \exp(-\gamma_i \exp(\alpha + \beta' x_i)).$$

Like the MPH model, the model's core assumption is proportional hazards. In fact, when the baseline hazard is left unspecified, the two models produce numerically similar estimates for β .²⁵ The MPH model would, however, require observing the never-reporters as right-censored observations to study the victim reporting propensity. By construction, never-reporters are unobserved in police records. The promotion time model allows me to specify the share of never-reporters at baseline (e.g., in the pre-intervention period) through the plugin parameter α . Figure 3 presents a graphical intuition on the differences between the two models when applied to police records.

In Subsection 5.7, I investigate via simulations the sign of the bias when misspecifying α . I find that setting a lower value than the true share of never-reporters biases estimates of covariates on crime reporting toward zero.²⁶ In the application to the Me Too movement (see Section 6), I set the plugin parameter based on empirical evidence from the National Crime Victimization Survey, which suggests that roughly 70% of sexual crimes are never reported to the police. I also plot crime and reporting trends for a broad range of

²²I systematically distinguish the baseline distribution's cumulative, hazard, density and survival functions (i.e., F_0, h_0, f_0, S_0) from the functions related to the distribution of times to report Y of victims (i.e., $F^{(v)}, h^{(v)}, f^{(v)}, S^{(v)}$), and the distribution of times to report Y of plaintiffs (i.e., $F^{(p)}, h^{(p)}, f^{(p)}, S^{(p)}$).

²³The model has a theoretical interpretation as a standard Poisson counting process. See Appendix C.

²⁴See Appendix C for the extension to time-varying covariates.

²⁵In the MPH regression, the baseline survival function is $S_{it}^{(p)}(y | \gamma_i, x_i) = \exp(-\gamma_i \exp(\beta' x_i) H_0(y))$. The baseline cumulative hazard H_0 is unspecified so that it can account for α and F_0 in Equation 5, hence the numerically close estimates.

²⁶The intuition for this is that ignoring never-reporters compresses differences between the cumulative distribution functions of delayed reports (see Figure 3).

parameter values as robustness checks (see Appendix D). In practice, the model rescales the observable part of the distribution of times to report Y . The choice of α thus changes the magnitude of the effects uncovered but not the underlying trends.

Based on Equation 5, it is straightforward to recover an estimate of sex crime incidence. Recall C_t the total number of crimes committed in period t , R_{t,τ_1,τ_2} the total number of reported incidents for date t by the end of the study period, and $F^{(v)}$ the cumulative distribution function of times to report in the victim population. Then we have²⁷

$$\hat{C}_t = \frac{R_{t,\tau_1,\tau_2}}{\widehat{F^{(v)}}(\tau_2 - t \mid \chi_t) - \widehat{F^{(v)}}(\max(\tau_1 - t, 0) \mid \chi_t)}. \quad (6)$$

The resulting estimate \hat{C}_t accounts both for double-truncation in the data and never-reporters, the two main empirical challenges presented in Section 5.1. It also accounts for variations in the hazard rate related to observable (potentially time-varying) covariates (e.g., a policy intervention).

5.4 Modeling Uncertainty

Randomness is modeled through unobserved heterogeneity and the baseline hazard. A common critique of duration models is that their results often depend on too restrictive parametric assumptions. To minimize parametric assumptions, I specify flexible functional forms for the baseline hazard and unobserved heterogeneity.

Baseline Hazard A priori, the baseline hazard of delayed reports h_0 in Equations 4 and 5 is unknown. To infer it from the data, I model the hazard as a piece-wise constant function (Yin and Ibrahim, 2005). Formally, given a set of time points $y_1, \dots, y_{m+1} \in (\mathbb{R}^+)^{m+1}$ such that $y_1 = 0 < y_2 < \dots < y_m < y_{m+1}$, and marginal hazard rates $h_{01}, \dots, h_{0m} \in (\mathbb{R}^+)^m$, the

²⁷In some cases, multiple charges may be filed for one offender. My estimates of sex crime incidence measure the number of sex crimes committed in a period and remain agnostic on the number of offenders involved.

hazard h_0 may be written

$$h_0(y) = \sum_{l=0}^m h_{0l} I_l(y) \quad \text{with} \quad I_l(t) = \begin{cases} 1 & \text{if } y < y_{l+1} \\ 0 & \text{otherwise.} \end{cases} \quad (7)$$

This allows for a flexible yet fully parametric baseline reporting hazard. With a unique time point $k_1 = 0$, the model reduces to an exponential cure rate model. With a large number of time points (and a large number of observations), the model resembles semi-parametric models (for which the baseline hazard is left unspecified).²⁸ Furthermore, the large amount of observations in the data allows me to specify a large number of breaks m , making the baseline hazard flexible in theory and practice.

Unobserved Heterogeneity In my baseline results, I assume unobserved heterogeneity is gamma-distributed with mean one. In a large class of frailty models, the frailty distribution among survivors converges to a gamma distribution under mild regularity assumptions ([Abbring and Van Den Berg, 2007](#)). This makes it an attractive parametric distribution to account for time-invariant, unobserved determinants of crime reporting.

I also consider an alternative, computationally more intensive approach. I model the population under study as consisting of K sub-populations with different frailties $\{\gamma_k\}_{k \in \{1, \dots, K\}}$ and respective shares within the population $\{s_k\}_{k \in \{1, \dots, K\}}$. I impose that all parameters are strictly positive and that the sum of the shares is one. I set one frailty effect to one ($\gamma_1 = 1$), so that other frailty effects are relative to this subpopulation. This formulation is a general specification of unobserved heterogeneity, which can account for various distributions. It is well-known that such a parametrization correctly accounts for the bias induced by unobserved heterogeneity, but does not accurately estimate the

²⁸It may also prove helpful to study the effects of statutes of limitations. A statute of limitations is a time limit for a person to file a charge. If the time limit expires, the individual who wishes to bring forth a claim is barred from doing so. In other terms, the alleged offender may not be prosecuted anymore. A statute of limitations may have two effects on delayed reports. First, it sets a hard threshold for the time to report, beyond which the reporting hazard is null. This is not a problem for the duration model. Second, it could increase the reporting hazard as the time limit draws nearer. Researchers can formally test this by specifying breaks in the baseline hazard h close to the time limit.

distribution of unobservables ([Heckman and Singer, 1984b](#)).

5.5 Estimation

Likelihood Contrary to common applications in the economics literature, police records present an additional empirical challenge as they raise the issue of double-truncation: crime incidents are observed if they are reported within the study period. Some reports may occur after the end of the study (i.e., right-truncation), and others may occur before its start (i.e., left-truncation). Right truncation implies an oversampling of shorter durations. Conversely, left truncation means an oversampling of longer durations. Without an appropriate correction, a naive estimation will lead to biased estimates ([Dörre and Emura, 2019](#)).

Formally, let T denote the incident date, Y the time to report to the police, $U = \max(0, \tau_1 - T)$ the left-truncation time, and $V = \tau_2 - T$ the right truncation time. Recall that τ_1 and τ_2 are respectively the start and end of the study period. Note also that $V = U + d$ where $d = \tau_2 - \tau_1$.²⁹ Finally, let f and g denote the density functions of Y and U . Under double-truncation, I observe n incidents indexed by i from the probability distribution (T, Y) given $U \leq Y \leq V$. The density of each data point (u_i, y_i, v_i) is

$$P(U = u_i, Y = y_i | U \leq Y \leq U + d).$$

In general, when subjects have unequal probabilities of selection, then the observed sample will not be representative of the underlying target population. The associated likelihood is

$$L(\Theta) = \prod_{i=1}^n \frac{f(y_i)g(u_i)}{\int_u \left(\int_u^{u+d} f(y) dy \right) g(u) du}.$$

This likelihood is complex, but under the assumption of independence between Y and

²⁹In the case of police records, V is thus entirely determined by U and d . This is referred to as *fixed-length* double-truncation, but the results presented below also hold for more general double-truncation schemes.

U , one can decompose it into two, somewhat more tractable conditional likelihoods:

$$L(\Theta) = \prod_{i=1}^n \frac{f(y_i)}{\int_{u_i}^{u_i+d} f(y) dy} \times \prod_{i=1}^n \frac{\left(\int_{u_i}^{u_i+d} f(y) dy \right) g(u_i)}{\int \left(\int_u^{u+d} f(y) dy \right) g(u) du}.$$

I use the first conditional likelihood to make an inference on f . This first term is relatively intuitive. It is an inverse-probability weighting approach in which observations are weighted by the inverse of their sampling probability. Furthermore, a major advantage of focusing on the first conditional likelihood is that I do not specify the distribution of the truncation time U . The likelihood to maximize eventually simplifies to

$$L(\Theta) = \prod_{i=1}^n \frac{f(y_i)}{F(u_i + d) - F(u_i)}.$$

Conditioning on observed covariates is straightforward. The covariates can be time-varying (e.g., policy interventions) and thus relax the independence assumption between U and Y to a more realistic, conditional independence assumption.³⁰ If specified, random effects are integrated out. I estimate the models by full maximum likelihood. For further details, see Appendix C.

Identifiability The identifiability of proportional hazards models with unobserved heterogeneity has been widely studied (for detailed discussions and proofs, see [Elbers and](#)

³⁰Recently, inverse-probability weighting approaches have been proposed for fitting the Cox model to doubly-truncated data ([Mandel et al., 2018; Rennert and Xie, 2018](#)), of which right-truncated data is a special case ([Vakulenko-Lagun et al., 2019](#)). They rely on the non-parametric maximum likelihood estimators of the selection probabilities proposed by [Efron and Petrosian \(1999\)](#) and [Shen \(2010\)](#). The main assumption is that Y , U and V are *unconditionally* quasi-independent. This would imply that the incident date does not affect the plaintiffs' time to report in the context of crime reports. This runs precisely counter to my research prior: the time to report likely varies with the incident date, as plaintiffs are more or less likely to report a crime to the police over time (e.g., before/after the MeToo movement). I formally test this quasi-independence assumption for each city in my dataset ([Martin and Betensky, 2005](#)). I reject the null hypothesis for all cities that survival and truncation times are quasi-independent at all standard significance levels. Thus, these methods are not a good fit in this empirical context. My approach relies on a less demanding and more realistic *conditional* independence assumption. However, it comes at the expense of specifying a parametric baseline hazard. To limit the impact of this parametrization, I specify flexible, piece-wise constant baseline hazards (see Subsection 5.2).

Ridder, 1982; Heckman and Singer, 1984a; Abbring and Van den Berg, 2003). In general, the baseline hazard and the distribution of unobserved heterogeneity are identified if the mean of the unobserved heterogeneity is finite and there is at least one regressor (Elbers and Ridder, 1982). I thus impose this constraint on the random effects.

A perhaps less well-known issue is the identifiability of proportional hazards models under double-truncation. Let us first consider the case without left-truncation. Then, if the right-truncation time is greater than the maximum victim time to report, f is (non-parametrically) identifiable. This assumption is referred to as positivity (Vakulenko-Lagun et al., 2019). On the other hand, if the maximum truncation time is less than the maximum time to report, then f is only identifiable up to a constant of proportionality (Seaman et al., 2021). In this case, the selection mechanism does not allow us to observe values of Y greater than the maximum truncation time. Thus, we do not know what proportion of individuals experience a time to report greater than the maximum truncation time. The inclusion of left truncation often allows us to observe the entire distribution of delays. For instance, I observe reports filed 30 years after the incident in my data. It appears reasonable to assume this is the maximum time to report and that the entire distribution of times to report of *plaintiffs* can be estimated from the data. However, even in the presence of left-truncation, the entire distribution of times to report of *victims* cannot be estimated from the data. By construction, right-truncation does not allow us to observe the share of never-reporters. This is why the latter, represented by the parameter α , enters the model as a plugin parameter in Equation 5.

5.6 Assumptions

The core assumption of the models in Equations 4 and 5 is proportional hazards. I assume that the effect of covariates does not depend on the time to report Y . More formally, for all delays y , I assume that

$$\beta(y) = \beta.$$

In practice, the assumption of proportional hazards can be tested and relaxed on the observable part of the data by allowing for time-dependent covariate effects. The model on plaintiffs (Equation 4) can be made fully flexible by interacting all covariate effects with a transform of time to report (e.g., bins of durations or their log). The same can be done for the model on victims (Equation 5). We cannot, however, observe the share of never-reporters from police records alone. This implies that one cannot formally test if and how the speed at which plaintiffs report is related to the share of never-reporters.³¹

Though unlikely in reality, it is always possible to construct models in which the two are unrelated. Consider for instance a model that separates the population of victims into two categories: a share p of victims report to the police (potentially with a delay) and a share $1 - p$ never do. If we allow covariates to affect both the share of victims who report p and the shape of the survival function for this category S , then we have

$$S_{it}^{(v)}(y | x_i) = 1 - p(x_i) + p(x_i)S(y | x_i).$$

In such a model, delayed reporting is unrelated to the share of never-reporters. For example, an extreme case is when covariates do not affect the share of victims who eventually come forward: $P(x_i) = p$. Then the distribution of observed times to report is uninformative of the share of never-reporters, and police data with delayed reports is of no help to researchers.

5.7 Monte Carlo Evidence

To assess the performance of my estimator, I run a series of Monte Carlo simulations. I benchmark my models and estimators against an out-of-the-box implementation of the MPH model with gamma-distributed frailty. The implementation is from the R pack-

³¹To further test the model's adequacy, criminal victimization surveys could distinguish the incident date from the date of its police report. The share of never-reporters would then be identifiable from the data, and one could formally test for proportional hazards. The data to do this is currently unavailable as the National Crime Victimization Survey asks respondents about their experiences of criminal victimization in the past six months before the interview.

age *FrailtyEM* ([Balan and Putter, 2019](#)). The package handles right-censoring and left-truncation. However, it is not designed (and does not claim) to handle double-truncation. If right-truncation presents a serious empirical issue in our context, this implementation should return biased estimates.

I first simulate datasets from Equation 4. The hazard h_0 is modeled as a piece-wise constant exponential function with mean $\lambda_1 = 0.2$ for the first period and mean $\lambda_2 = 0.01$ for all other periods. Three covariates do not vary over time. Their effects on the hazard are respectively $-0.5, 0.3$, and 0 (on the log-scale). To capture the double-truncation scheme, observations are exponentially truncated on the right at rate $\lambda = 0.005$ and on the left at rate 0.05 . For 1000 data points, this leaves me with approximately 400 observations per simulation. I simulate 1000 datasets. Results are presented in Panel A of Figure 4. Estimates of the MPH model without the analytical correction are severely biased (except for the null effect, which seems correctly estimated). On the other hand, the proposed correction performs well and results in unbiased estimates. This first set of evidence indicates the analytical correction of the likelihood appropriately accounts for double-truncation.

This is reassuring, but police records stem from a complex and messier data-generating process. I thus turn to more demanding and realistic simulations that include the presence of time-varying covariates (e.g., interventions), unobserved heterogeneity, and never-reporters (see Equation 5). I simulate time series of crime reports over 200 periods. At each period t , ten offenses are committed. The hazard h_0 associated with F_0 is modeled as a piece-wise constant exponential function with mean $\lambda_1 = 0.2$ for the first period and mean $\lambda_2 = 0.01$ for all other periods. This captures the fact that a large share of crimes is reported on the day of the incident. 50% of the victims never report the crime to the police between periods 0 and 75. In period 75, an intervention D_1 permanently decreases the reporting hazard by -0.5 and increases the number of offenses committed to 12. In period 100, an intervention D_2 permanently increases the reporting hazard by 0.3 and decreases the number of offenses committed to 8. In period 125, an intervention does not affect victims and offenders. Unobserved heterogeneity is assumed gamma-distributed

with variance 0.3. To capture the double-truncation scheme, I only keep observations for reported incidents between periods 50 and 200, which corresponds to roughly 800 reports out of 4,000 crimes per simulation. I simulate 1,000 datasets. Results are summarized in Panel B of Figure 4. Once again, the out-of-the-box MPH regression estimator is severely biased. In a dynamic setting, it does not capture null effects correctly anymore. On the other hand, the proposed estimator for a correctly specified promotion time model presents no bias in estimates (as measured by a t-test).

Given that the share of never-reporters in the first data period is a researcher's input, I also investigate the sensitivity of estimates to model misspecification in another set of simulations. I estimate additional models: a corrected MPH model that assumes all victims eventually report and several corrected promotion time models that assume various shares of never-reporters in the first data period. Results suggest undershooting the share of never-reporters at baseline leads to estimates of covariate effects biased towards zero (see Table C.1). The bias is relatively small in the Monte Carlo Simulations. However, one should not conclude that the plugin parameter α has no impact on downstream empirical results. A model that assumes 0% of never-reporters largely underestimates the impact of interventions on the share of never-reporters. Conversely, a model that assumes 80% of never-reporters overestimates the impact of interventions. For example, for an intervention effect of 0.3, assuming a share of never-reporters of 50% (80%) implies $0.5^{\exp(0.3)} \approx 39\%$ ($0.8^{\exp(0.3)} \approx 74\%$) of never-reporters post-intervention.

6 Empirical Results

This section studies sex crime incidence and reporting between 2010 and 2019, with a focus on the Me Too movement's impact. I first examine the impact of #MeToo on the plaintiff reporting hazard (see Equation 4). I then investigate what these variations imply for the reporting rate of victims (see Equation 5). This also allows me to compute estimates of sex crime incidence over the period (see Equation 6). Finally, I isolate the effect

of #MeToo on sex crime incidence.

6.1 Plaintiffs

I first estimate trends in the plaintiff reporting hazard over the period (see Equation 4). By abstracting from never-reporters, I focus on the observable part of the distribution of times to report Y . From the Monte Carlo simulations, we know that we cannot interpret the resulting hazard ratios as increases in the victim reporting hazard. These estimates are downward-biased if a share of victims never reports to the police. Thus, they may be understood as a lower bound for victims' propensity to report.

Main Results The dependent variable is the number of days elapsed between a sex crime being committed and its report to the police. The breaks in the piece-wise constant baseline hazard are set after 1, 30, 90, 180, and 365 days. I assume unobserved heterogeneity is gamma-distributed. My baseline specification is

$$h_{itc}^{(p)}(y) = h_0(y) \exp \left(\delta_c + \sum_{k=\text{Oct.15,2010}}^{\text{Oct.15,2019}} \beta_k \mathbb{1}(t+y \geq k) \right) \gamma_i. \quad (8)$$

δ_c is a city fixed effect that accounts for variations in reporting delays across cities. γ_i is a gamma-distributed unobserved heterogeneity term. For each year, the dummy variables take the value one from the 15th of October onwards. The main coefficients of interest are the yearly betas. I interpret β_k as the additional (higher or lower) propensity to report a sex crime to the police in year k among plaintiffs. Figure 5 presents the evolution of the hazard ratio over the decade.³² Overall, the plaintiff reporting hazard appears flat until October 2017 (i.e., when the Me Too movement went viral on social media). It then increases by approximately 23% in the following two years (significant at all standard

³²Figure D.1 presents the estimated baseline survival functions per city. For comparison, Figure C.1 also presents the evolution of the hazard ratio for a model that does not account for double-truncation. This naive model has unstable coefficient estimates that are very sensitive to the choice of the end of the study period. In all cases, it estimates that the reporting hazard increased from approximately 70% to 150%. This is, of course, mainly spurious but highlights the risks of failing to account for double-truncation.

significance levels).³³ This finding confirms that #MeToo coincides with a large change in victim reporting behaviors. Overall, the Me Too movement appears to gain momentum, with no apparent decrease by the end of the study period. A closer inspection at the quarterly level confirms this observation (see Figure D.2), suggesting that #MeToo is not simply a one-point-in-time event that briefly increased the saliency of sexual violence in society. On the contrary, the movement coincides with a durable change in reporting norms.

Heterogeneity I then turn to heterogeneity analysis. To investigate #MeToo's effects on incident-level characteristics, I focus on approximately 30,000 observations from the New York Police Department. This represents roughly one fourth of the total number of observations. One could worry about selection effects. However, the magnitude and sign of the unconditional #MeToo effect are extremely similar for this subsample than for the overall sample (see Table 2). My specification on this restricted sample is:

$$h_{itc}^{(p)}(y) = h_0(y) \exp \left(\delta_c + \zeta X_i + \phi \text{MeToo}_{ity} + \Omega X_i \times \text{MeToo}_{ity} + \Psi \text{Old}_{ity} \times \text{MeToo}_{ity} \right) \gamma_i. \quad (9)$$

X_i is a vector of time-invariant incident characteristics. It includes the victim and suspect's race and sex, a dummy variable for juveniles, and whether the crime is a felony or a misdemeanor. The Me Too movement went viral on social media on the 15th of October 2017. Thus, MeToo_{ity} is a dummy variable equal to one after this date. Finally, Old_{ity} is a dummy variable that takes value one if the incident occurred more than 365 days ago (and 0 otherwise). Figure 6 presents estimates of Φ , Ω , and Ψ . The baseline #MeToo effect Φ is for white women plaintiffs, filing a complaint against a white suspect for a sexual felony less than a year old. #MeToo has no statistically significant effect on this popula-

³³This indicates #MeToo is associated with a much larger increase in the reporting hazard than in previous periods. However, given that all these estimates are downward-biased, this should not be interpreted as strong evidence for the absence of pre-trends in the pre-treatment period. As we will see in the next section, once accounting for the share of never-reporters, there are sizable, linear, and positive pre-trends to the movement's mediatization.

tion, but it has a large and statistically significant effect on old crime incidents (i.e., more than a year old). The intervention appears particularly effective at encouraging victims of past crime incidents to report to the police. However, this is not to say that the movement had no effect on recent crime incidents for all crime types and socio-demographic groups. Misdemeanors are more likely to be reported than felonies at the 10% significance level. Juvenile, Hispanic, and black victims are also more responsive to the intervention. I find no statistically significant effect for male victims, black, Hispanic, and male suspects. Juvenile suspects are far less likely to be reported after #MeToo. This mirrors the increase in juvenile plaintiff reporting, who are more likely to file complaints against adult suspects. Overall, #MeToo had a larger, positive effect on the most vulnerable groups of the victim population, as well as on victims of misdemeanors and past crimes.

Robustness I conduct a series of robustness exercises (see Table 2). First, I assess the sensitivity of estimates to alternative specifications for unobserved heterogeneity. Columns 1, 2, and 3 present estimates of the #MeToo effect on the plaintiff reporting hazard respectively without unobserved heterogeneity, with a gamma-distributed random effect, and with a discrete random effect (à la Heckman and Singer, 1984a). Overall, all specifications indicate a positive impact on the reporting hazard and point estimates are very close to one another, suggesting the specification of unobserved heterogeneity is unlikely to heavily affect point estimates. Second, I show that my estimates are robust to the inclusion of linear and quadratic time-trends that account for pre-trends (see Columns 4 and 5). Third, I assess the sensitivity of estimates to different breaks in the baseline hazard. Columns 6 to 9 estimate the #MeToo effect on reporting for a broad range of breaks in the baseline hazard. Adding flexibility to the baseline hazard mildly decreases the size of the effect at first, but then point estimates remain remarkably similar past three breaks (and gains in the likelihood become negligible).³⁴ Fourth, the FBI changed its definition of rape in 2013. City-level police records rely on a different categorization of offenses, which should

³⁴In practice, many victims report on the first day, so the break on day 1 is the break that matters to estimate reasonable baseline survival functions.

not affect my results. To remove any doubts, the #MeToo effect remains when I restrict the sample to crimes reported after 2013 (see Column 10). Fifth, I assess the sensitivity of estimates to control variables (see Columns 11 and 12). I find that controlling for all crime incident characteristics barely changes the estimated #MeToo effect. I also run my analysis on non-sexual crimes. The latter are not a particularly relevant control group, because they are generally reported with short delays to the police (see Table A.3). Nonetheless, I interpret resulting point estimates as placebo tests that may capture more general changes in police recording practices. The size of the effects is negligible and suggests that my results are unlikely driven by such dynamics (see Columns 13 to 15). Finally, to relax the proportional hazards assumption, I allow for a differential effect of the yearly dummies on recent crime incidents (of less than 365 days) and older crime incidents (of more than 365 days). Figure D.5 presents the results. I find empirical support for time-dependent effects (as the heterogeneity analysis already indicated). However, in Figure D.6, I show that focusing only on recent crime incidents does not revert the estimated trends in the plaintiff reporting hazard. If anything, it indicates a larger positive pre-trend since 2010.

6.2 Victims

Next, I investigate what these trends imply for sex crime reporting in the general population of victims (see Equation 5). I estimate the victim reporting hazard between 2010 and 2019. Consistent with estimates of the NCVS, I set α so that the share of never-reporters in 2010 is approximately equal to 70%. I pool all cities and specify the hazard as follows:

$$h_{itc}^{(v)}(y) = f_0(y) \exp \left(\alpha + \delta_c + \sum_{k=\text{Oct.15,2010}}^{\text{Oct.15,2019}} \beta_k \mathbb{1}(t+y \geq k) \right) \quad (10)$$

As in Equation 8, the main coefficients of interest are the yearly betas. I interpret β_k as the additional (higher or lower) propensity to report a sex crime to the police in year k among victims.³⁵ Note that Equation 10 also provides us with the total share of victims

³⁵The main difference is that I do not specify a random effect term for unobserved heterogeneity. This

who will eventually report a sex crime to the police for a given year k (and in the absence of future events that may shift victim reporting behaviors).

Main Results Figure 7 decomposes reported sex crimes to the police into estimates of sex crime incidence and reporting (based on Equations 6 and 10). Accounting for never-reporters uncovers relatively large pre-trends before the Me Too movement. Estimates suggest the share of victims who eventually report increased from 30% in 2010 to 55% before October 2017. The Me Too movement coincides with a reinforcement of these broader trends as the share of reports reaches 75% in 2019. Thus, my estimates suggest that the reporting rate more than doubled during the decade. This also implies a large decrease in sex crime incidence over the same period. These substantial pre-trends partly cancel each other out in the time series of reported crimes and translate into an increase in reported sex crimes (aggregated at the incident date) of approximately 50%.

These results highlight the importance of separating crime incidence and reporting for empirical research on crime. As many have suggested since Quêtelet (1831), reported crime statistics are likely but the tip of the iceberg. Figure 7 also questions the context in which successful social movements emerge. Though the precise date on which #MeToo went viral was unforeseen, the movement also appears to take place in the context of a deeper societal change in sex crime reporting and incidence.

Robustness I conduct a series of robustness exercises (see Appendix D). First, I present estimated trends under different values for the share of never-reporters in 2010. The model rescales the observable part of the distribution of times to report Y . The choice of α thus changes the magnitude of the effects uncovered, but not the underlying trends (see Figure D.4). I show that a broad range of reasonable parameter values for α – rang-

is because the promotion time model often fails to identify the presence of unobserved heterogeneity. Yet, it still recovers the simulated treatment effects with seemingly no bias in estimates in the Monte Carlo simulations. Furthermore, we know that a gamma-distributed unobserved heterogeneity barely changes the point estimate for the Me Too movement’s impact on the plaintiff reporting hazard. Thus, I do not expect it to largely affect results.

ing from 60 to 80% of never-reporters at baseline – leaves the main decomposition result qualitatively unchanged (see Figure D.3).³⁶ Second, given the presence of time-dependent effects, I assess my estimates' sensitivity to this specific violation of proportional hazards. In Figure D.7, I show that focusing on recent crime incidents does not qualitatively change the estimated trends in sex crime incidence and reporting.

A remaining concern is that my main results assume truthful and founded crime reports. In practice, false allegations of a crime are a rampant concern for the criminal justice system – in particular when it comes to sex offenses. It is notoriously difficult to assess the incidence of false accusations.³⁷ Recent estimates suggest that baseless rape allegations represented approximately 5% of total rape charges in the United States between 2006 and 2010 (De Zutter et al., 2017). Though a precise estimate of such allegations is out of reach of researchers, one can still ponder their implications for interpreting my estimates. If the rate of false allegations is positively correlated to #MeToo, then the model will overestimate the movement's effects on the victim reporting rate. As a result, it will also inflate the size of its extrapolated deterrent effect. This is a plausible scenario, particularly if the expected benefits of filing a charge increase for plaintiffs after #MeToo.³⁸ To assess the impact of unfounded allegations on my estimates, I restrict my sample to sex crimes that resulted in an adult arrest (note that this information is only available for Los Angeles). These reports are more likely to have presented compelling evidence. Estimates from this seriously restricted sample can be understood as highly conservative estimates of sex crime incidence and reporting. Yet they are qualitatively similar to my main results for Los Angeles (see Figure D.8). This suggests unfounded allegations are not driving my results.

³⁶Note that this interval is very large, as it encompasses all estimates of the victim reporting rate by the National Crime Victimization Survey since 2011.

³⁷Previous studies have found estimates ranging from 1.5% to 90% of total rape charges filed to the police are false (Rumney, 2006). Despite this apparent uncertainty in estimates, a closer look finds that many studies are outdated, lack statistical power, have worked with heterogeneous definitions, and sometimes have questionable methodologies. Maclean (1979) deemed reports false if, for instance, the victim did not appear "dishevelled", while Stewart (1981) works with a sample of 18 charges.

³⁸Through larger financial compensations or higher probabilities of sentencing, for instance.

6.3 Offenders

The careful decomposition of reported sex crimes indicates that sex crime incidence has decreased over the period. I attempt to isolate the contribution of #MeToo to this trend. As #MeToo potentially affected all cities in the United States and worldwide, credible control groups for causal inference are limited. However, the crime literature suggests crime categories are subject to cyclical fluctuations, part of which has been explained by weather conditions, economic downturns, labor market conditions, alcohol consumption, and sports events (Markowitz, 2005; Jacob et al., 2007). For these reasons, reported non-sexual crimes are a plausible control group. I thus construct several counterfactuals for quarterly sex crime incidence based on quarterly non-sexual crime reports. For simplicity, my baseline empirical strategy is a difference-in-differences. The specification for crime i , in quarter t , in city c is:

$$\log(\text{Crimes})_{itc} = \beta \text{MeToo}_{it} + \delta_i + \delta_c + \delta_t + \varepsilon_{itc}. \quad (11)$$

δ_i , δ_t , and δ_c are respectively crime, time, and city fixed effects. MeToo_{it} is a dummy variable that takes value one for sex crimes after October 2017 and thus β is the marginal effect of #MeToo on sex crimes. To construct valid confidence intervals, I sample with replacement observations from the dataset and estimate the duration model, sex crime incidence, and the counterfactuals at each iteration. Sex crime incidence is an extrapolation based on the duration model's output and the bootstrap procedure accounts for this additional source of randomness.

The difference-in-differences estimates may be interpreted causally under the assumption that reported non-sexual crimes and sex crimes would have followed similar trends absent the Me Too movement's sudden mediatization. Despite some noise in the data, the inspection of pre-trends suggests this is a plausible assumption (see Figure 8). In addition, given that #MeToo was specifically related to sex crimes, I assume that the reporting rate of non-sexual crimes is uncorrelated to the timing of #MeToo.

Main Results Table 2 presents the main results. Across specifications, I find a strong, negative, statistically significant decrease in sex crime incidence after #MeToo ($\approx -23\%$, see Column 1). I compare my results to estimates to using reported sex crimes as a treated unit (instead of my estimates of sex crime incidence). I use incident dates (see Column 8) or report dates (see Column 9) to aggregate crime reports. When studying incident dates with no correction, one also finds a positive yet statistically insignificant increase in sexual crime reports (+20%). When studying report dates, one finds a large increase in sexual crime reports (+32%). The latter is statistically significant at all standard significance levels. Both estimates have been previously interpreted as an increase in the reporting rate of victims (Levy and Mattsson, 2021).

This warrants three remarks. First, in the presence of delayed reports, estimates are sensitive to the date used for aggregate reported crime statistics (i.e., the incident date or of its report). In our empirical context, the size of the effect and its statistical significance vary drastically. Second, one cannot interpret these estimates as an increase in reporting behaviors without making additional, strong, and unverifiable assumptions. Third, if the Me Too movement simultaneously increases victim reporting and decreases sex crime incidence, then both estimates will underestimate the movement's impact as a whole. According to my estimates, this is clearly the case.

Robustness I conduct a series of robustness exercises. First, as presented in Table 2, my results are robust to alternative counterfactual models that relax the parallel trends assumption. This includes an interactive fixed effects (IFE) model (Xu, 2017) and the matrix completion method (Athey et al., 2021). I find that the IFE model with five factors best fits the pre-treatment period (see Figure D.9). In practice, however, both alternative models barely change point estimates of the movement's effects on sex crimes (see Columns 2 and 3). Second, I sequentially drop each non-sexual crime used for the counterfactual. In all cases, a strong, negative, statistically significant effect on sex crime incidence remains (see Columns 4 to 7). Third, I replace sex crime incidence as the treated unit with one

of the non-sexual crimes used for the counterfactual. For all placebo crimes, I find no statistically significant effect of the Me Too movement (see Columns 10 to 13).

7 Discussion and Mechanisms

7.1 Social Norms

The recurring public debate over the incidence of false allegations is a reminder that the decision to report a crime to the police is also influenced by social norms and beliefs. Victims may incur social costs for reporting a sex crime to the police in at least two ways. First, libeler narratives can undermine the credibility of the charges and question the motives behind them. In turn, beliefs on the incidence of sex crimes and libelers in one's society may weigh in a victim's decision.³⁹ Second, social conformity concerns may also influence a victim's decision. If victims care about what other victims do, then a repeated coordination game may easily result in persistent, sub-optimal equilibria.

Unfortunately, there is no systematic empirical evidence of the discourse surrounding sex crimes, and the attitudes of the broader public on the matter.⁴⁰ As a proxy for these narratives, I collect and analyze tweets mentioning sex crimes between 2010 and 2019. I present the time-series of tweet counts (in logs) in Figure A.7. The hashtag #MeToo was virtually never used before October 2017 and may lead us to believe the discourse surrounding sex crimes was also very limited (see Panel A). In fact, the topic was already discussed on Twitter its share of the total number of tweets has been increasing since 2010 (see Panel B). Similar observations are made with Google Trends data (see Figure A.8). This suggestive evidence is consistent with the pre-trends in crime reporting uncovered in Section 6.

³⁹Given the considerable uncertainty on the matter, these beliefs need not be true or unbiased.

⁴⁰To the best of my knowledge, the Views of the Electorate Research (VOTER) survey is the only national survey with explicit questions on sex criminality, but they only ask respondents about sexual harassment (which I do not study in this paper).

7.2 Odds of Punishment

Figure A.3 compares the evolution of arrest rates for sex crimes to non-sexual crimes between 2010 and 2019 for New York City and Los Angeles.⁴¹ Overall, arrest rates remain relatively stable over the period, with a slight decrease for sexual offenses.

Combining arrest rates with my estimates of sex crime incidence, I also compute the unconditional probability of arrest for committing a sex crime:

$$P_t(\text{Arrest} = 1) = P_t(\text{Arrest} = 1 \mid \text{Report} = 1) \times \hat{F}_t^{(p)}(+\infty \mid \chi_t). \quad (12)$$

Figure 9 presents the evolution of the probability of arrest for sex offenders over the period. It increases from 16% to 37% between 2010 and 2019. The decrease in sex crime incidence is thus consistent with a Beckerian argument. Between 2010 and 2019, a one percentage point increase in the probability of arrest is associated with a 0.9 percentage point decrease in sex crime incidence. These findings suggest that sex offenders react to the odds of punishment, and thus that increasing the probability of arrest through increased reporting may effectively prevent future offenses.

8 Conclusion

In the past two centuries, underreporting has been a major empirical challenge in making sense of reported crime statistics. This paper proposes a methodology to separate sex crime incidence and reporting. My empirical strategy leverages the largely understudied presence of delayed reports in police records. The latter raises new empirical issues but also allows researchers to work with tools from survival analysis to study variations in the reporting hazard over time.

I then study sex crime incidence and reporting surrounding the Me Too movement. I

⁴¹Seattle and Cincinnati do not provide arrest data for fine-grained categories and only report arrest data for crimes in the Uniform Crime Reporting program.

present three key results on its origins and impact. First, #MeToo largely and persistently increased the propensity to report of victims. The effect is larger for juveniles, racial minorities, victims of misdemeanors and past crime incidents. Second, it had a deterrent effect on sexual offenders. Third, the movement appears in a general context of decreasing sex crime rates and increasing sex crime reporting. These substantial trends partly cancel each other out and are less apparent in the time series of reported crimes.

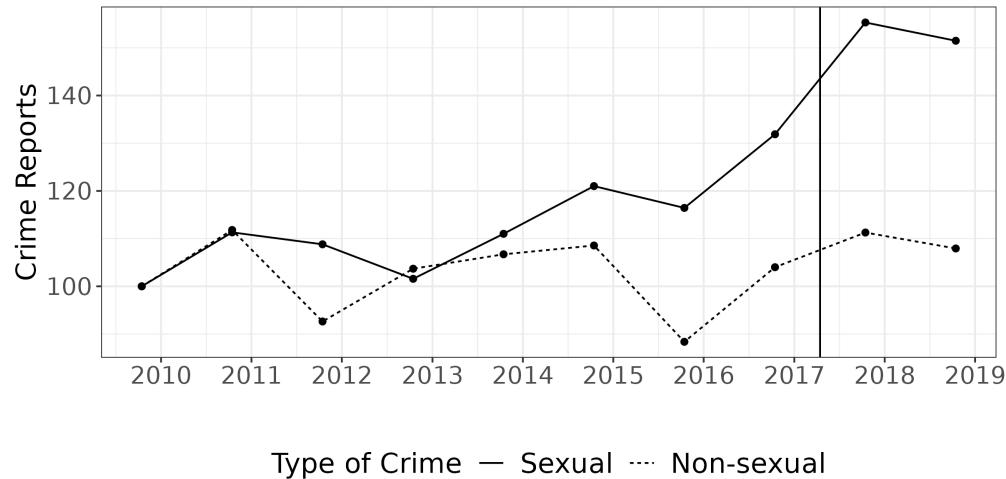
This last finding highlights the importance of disentangling crime incidence and reporting in police data. Though many competing explanations may rationalize my estimates, they are unlikely to be driven by false allegations and changes in the recording guidelines of law enforcement agencies. I present suggestive evidence that rapidly changing social norms increased the reporting rate of victims and, ultimately, the likelihood of arrest for sex offenders.

My results suggest that increasing the reporting rate of victims may significantly deter criminal behavior. They further highlight that social norms can successfully enforce socially desirable behaviors when the legal system fails to do so on its own. The broader legal, political, and economic consequences of the Me Too movement remain unknown and represent an opportunity for future research.

Figures and Tables

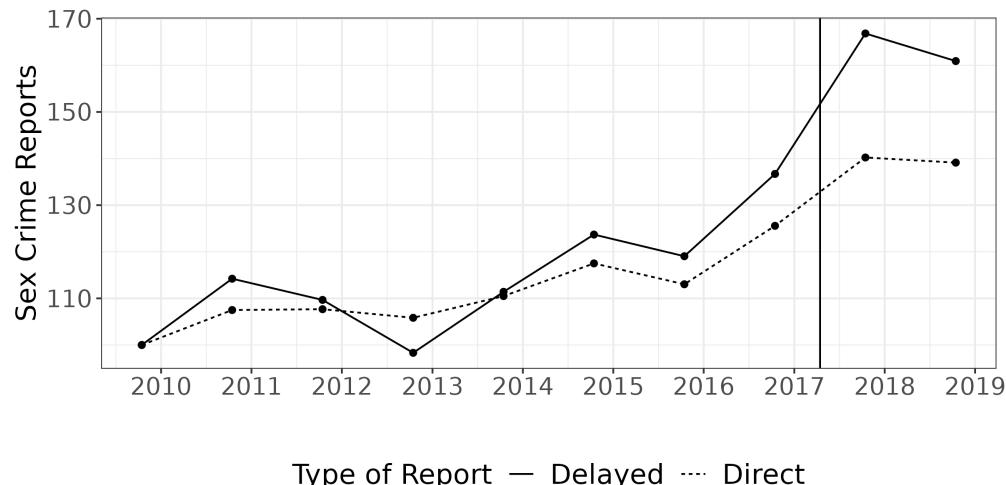
Figure 1: Trends in Reported Crimes

A. Sexual / Non-Sexual Crime Reports



Type of Crime — Sexual Non-sexual

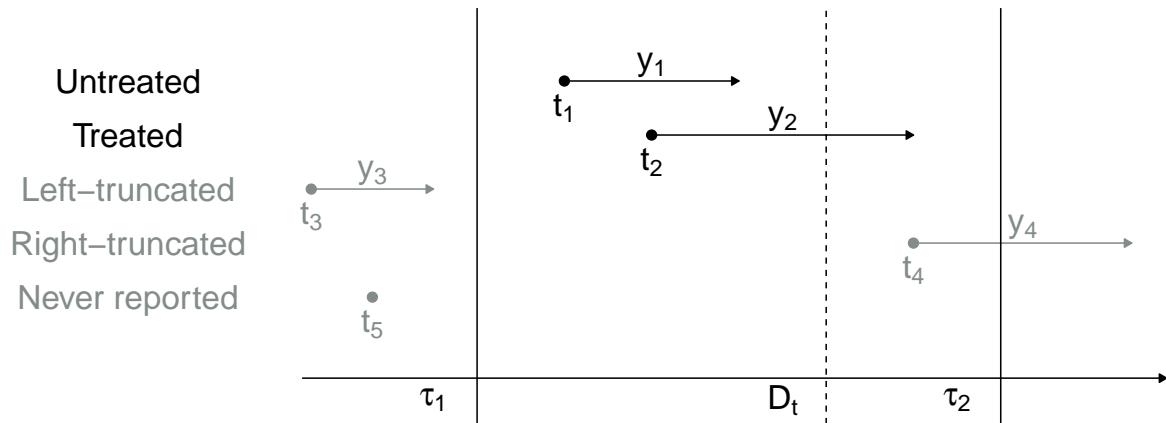
B. Direct / Delayed Sex Crime Reports



Type of Report — Delayed Direct

Notes: This figure presents trends in reported crimes. Crime reports are aggregated on the report date. Crimes reported on the first day are labeled as a direct report. The vertical solid line is set one period before #MeToo (Oct 2017). Panel A compares sexual crime reports to non-sexual crime reports. Panel B compares direct sexual crime reports to delayed sexual crime reports. A report is labeled as a direct report if it is reported on the day of the incident.

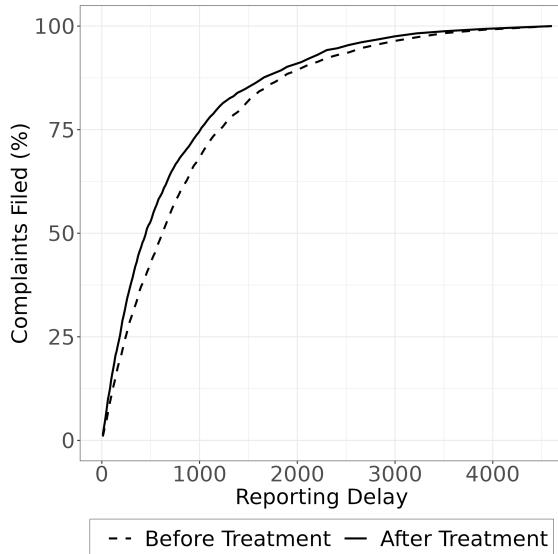
Figure 2: The Structure of (Delayed) Crime Reports



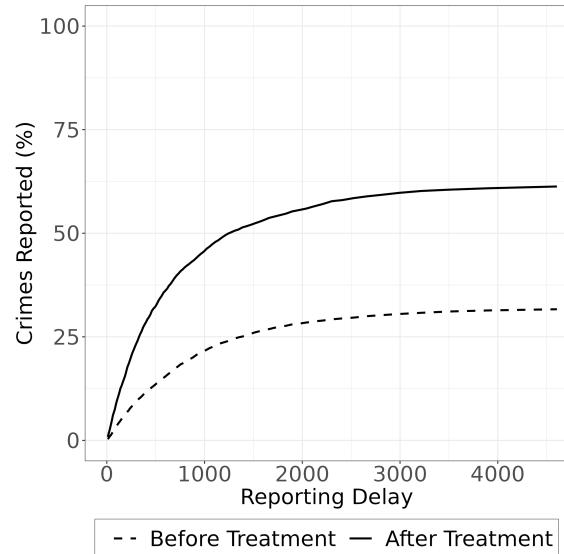
Notes: A graphical depiction of time to report data based on police records. The study window goes from τ_1 to τ_2 (solid vertical lines), with an intervention D_t in between represented by a dashed vertical line (e.g. #MeToo). Elements in gray are unobserved. Elements in black are observed. Some plaintiffs will have reported before the intervention's implementation and form the control group (non-treated observations). Others will be potentially affected by the intervention and form the treated group (treated observations). Some plaintiffs have reported before the start of the study and are unobserved (left-truncated data points). Some plaintiffs have not yet reported a crime to the police by the end of the study period but will in the future and are unobserved (right-truncated data points). Finally, a fraction of victims decide to never report and are never observed (i.e., never-reporters). Victims who never report are modeled as never-reporters (but note that this quantity is not identifiable from police records).

Figure 3: From Observed Delays to Victim Reporting Rates

A. Plaintiffs



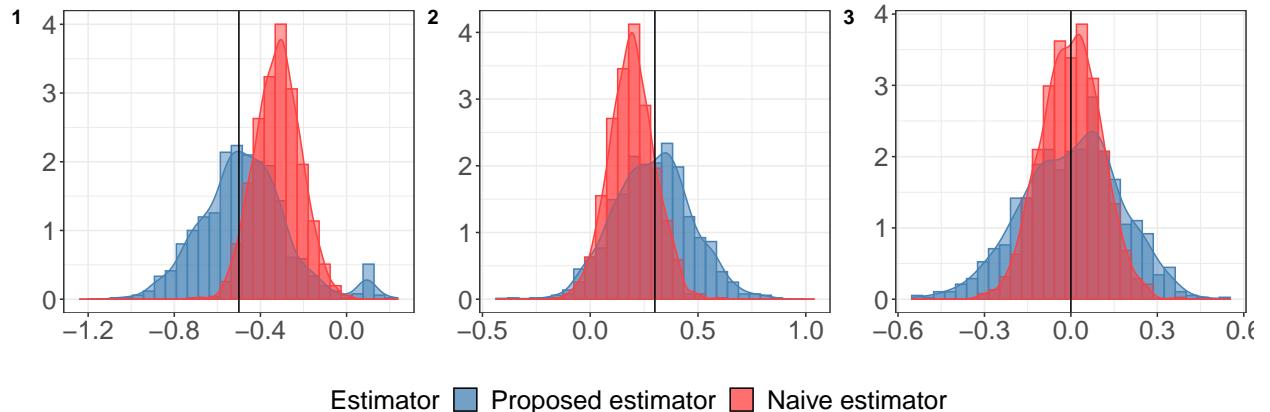
B. Victims



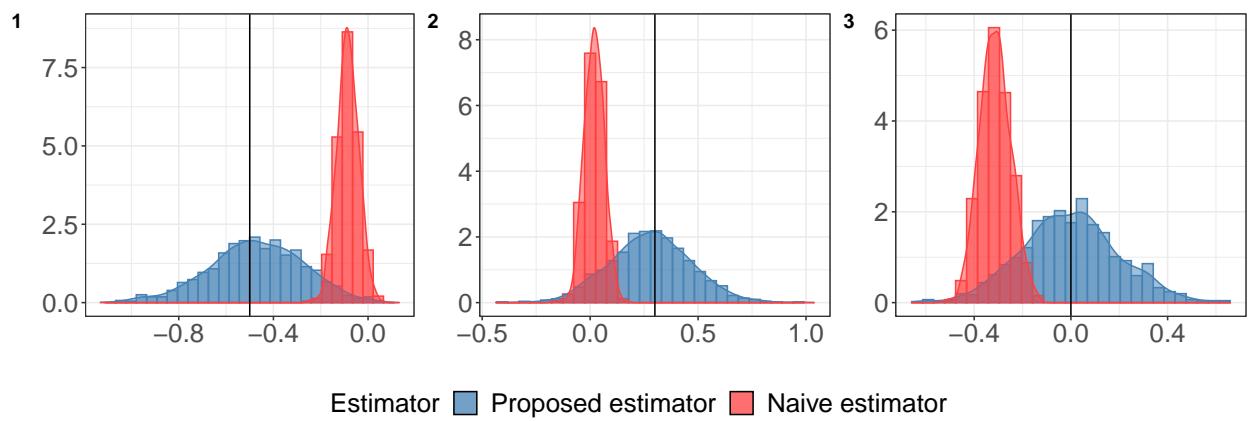
Notes: These simulated distributions provide graphical intuition on the duration models developed in the paper. A treatment/intervention increases the propensity to report of victims. The plots compare cumulative distribution functions (CDFs) before (dashed line) and after (solid line) the treatment. Panel A plots the CDF of reporting delays among plaintiffs. The researcher observes this in police records. It corresponds to Equation 4. Panel B plots the CDF of the entire victim population and corresponds to Equation 5. The CDFs do not sum to 100% because a share of victims will never report to the police. Those victims are unobserved. However, suppose one knows the victim reporting rate before the treatment (here 30%). In that case, the observed distribution of reporting delays is sufficient to infer the victim reporting rate after the treatment (here around 60%). This intuition can be generalized to multiple treatments/interventions. For instance, fitting a linear spline over time recovers variations of the reporting rate over time (see the application to #MeToo in Section 6). Note also that ignoring never-reporters (Panel A) tends to underestimate the differences between CDFs in the victim population (Panel B), hence the bias towards zero when setting too low a share of never-reporters at baseline (relative to the ‘true’ share).

Figure 4: Monte Carlo Simulations

A. Static Simulations



B. Dynamic Simulations



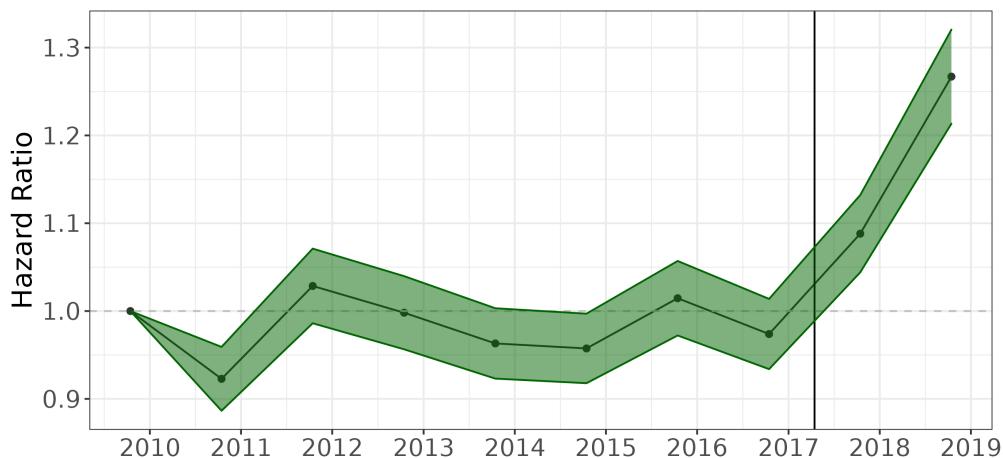
Notes: Results of 1000 Monte Carlo simulations. Panel A simulates datasets with time-invariant covariates and double-truncation (based on Equation 4). The blue densities present the distribution of estimates with a likelihood that appropriately accounts for double-truncation. The red densities present estimates of the out-of-the-box MPH model as implemented in the R package *frailtyEM* ([Balan and Putter, 2019](#)). This implementation does not account for double-truncation in the data. Subpanel 1 is for the first intervention with effect -0.5. Subpanel 2 is for the second intervention with effect 0.3. Subpanel 3 is for the third intervention with no effect. Each panel's solid vertical black line is the 'true' parameter value. The results indicate the proposed likelihood correctly accounts for double-truncation in the data by weighting observations by the inverse of their sampling probability. Panel B simulates more realistic datasets of police complaints with time-varying covariates, unobserved heterogeneity, never-reporters, and double-truncation (based on Equation 5). In both panels, the densities of the proposed estimator are centered around the true parameter value, whereas the naive estimator is biased.

Table 1: #MeToo Effects on Crime Reporting – Lower Bounds and Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Plaintiff Hazard	Sex Crimes												Assaults	Robberies	Burglaries
#MeToo: October 15, 2017	0.154 (0.009)	0.169 (0.011)	0.11 (0.01)	0.13 (0.012)	0.10 (0.017)	0.224 (0.012)	0.170 (0.011)	0.166 (0.011)	0.166 (0.011)	0.135 (0.013)	0.196 (0.020)	0.196 (0.019)	0.0018 (0.011)	0.03 (0.011)	0.05 (0.011)
Baseline Hazard															
Day 0+	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Day 1+	X	X		X	X	X	X	X	X	X	X	X	X	X	X
Day 30+	X	X		X	X		X	X	X	X	X	X	X	X	X
Day 90+	X	X		X	X			X	X	X	X	X	X	X	X
Day 180+	X	X		X	X				X	X	X	X	X	X	X
Day 365+	X	X		X	X					X	X	X	X	X	X
Controls															
Crime Category													X		
Victim Characteristics													X		
Suspect Characteristics													X		
City Fixed Effects	X	X	X	X	X	X	X	X	X	X	X		X	X	X
Time-Trends															
Linear				X	X										
Quadratic					X										
Unobserved Heterogeneity	Gamma	Discrete	Gamma	Gamma	Gamma	Gamma	Gamma	Gamma	Gamma	Gamma	Gamma	Gamma			
τ_1	2010	2010	2010	2010	2010	2010	2010	2010	2010	2014	2010	2010	2010	2010	2010
τ_2	2019	2019	2019	2019	2019	2019	2019	2019	2019	2019	2019	2019	2019	2019	2019
Cities	All	All	All	All	All	All	All	All	All	NYC	NYC	All	All	All	All
N Observations	111869	111869	111869	111869	111869	111869	111869	111869	111869	72730	32442	50000	50000	50000	
Algorithm	BFGS	BFGS	rgenoud	BFGS	BFGS	BFGS	BFGS	BFGS	BFGS	BFGS	BFGS	BFGS	BFGS	BFGS	BFGS
Log-Likelihood	-419895	-416769.5	-421380	-419889	-419886	-420521	-417059	-416848	-416811	-173196	-121129	-119837	-78038	-68458	-108183

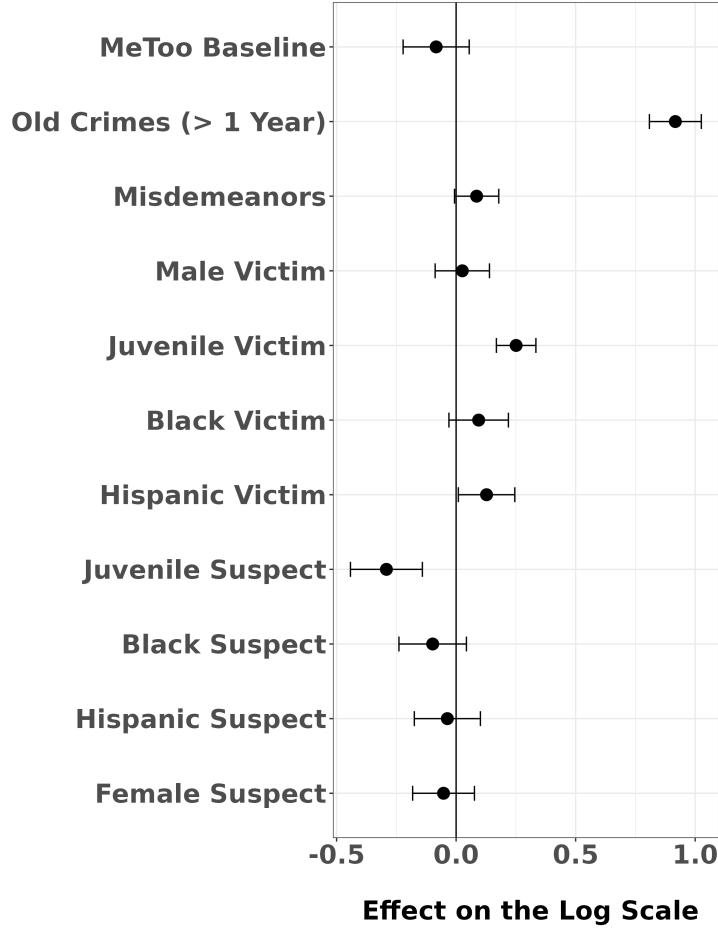
Notes: Effect of the Me Too movement’s sudden mediatization in Oct. 2017 on the plaintiff reporting hazard (see Equation 4) for various specifications and samples. Estimates are presented on the log scale. I focus on plaintiffs and abstract from never-reporters. Thus, the estimates reported in this table are lower bounds for the #MeToo effect on crime reporting. Standard errors are in parentheses. Column 1 presents the baseline estimate. Columns 2 and 3 add a random effect in the estimation to account for time-invariant unobserved heterogeneity. Columns 4 and 5 respectively account for linear and quadratic time-trends in reporting. Columns 6 to 9 sequentially increase the number of breaks in the baseline hazard. Column 10 restricts the study period to 2014–2019. Column 11 presents estimates for New York City. Column 12 controls for incident-level characteristics for New York City. Overall, the effect size is very stable across specifications. Columns 13 to 15 study the hazard of plaintiffs for non-sexual crimes and are interpreted as placebo tests (estimates are based on a random sample of 50,000 incidents). Overall, effect sizes are much smaller than for sex crimes.

Figure 5: Yearly Hazard Ratios – Plaintiffs



Notes: Yearly estimates of the plaintiff reporting hazard based on Equation 8. I focus on plaintiffs and abstract from never-reporters. Thus, the estimates reported in this table are lower bounds for the yearly marginal effects on crime reporting. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. Unobserved heterogeneity is assumed gamma-distributed. 95% confidence intervals (in grey) are computed with a numerical estimation of the hessian combined with the delta method. The vertical solid black line corresponds to the Me Too movement's mediatization.

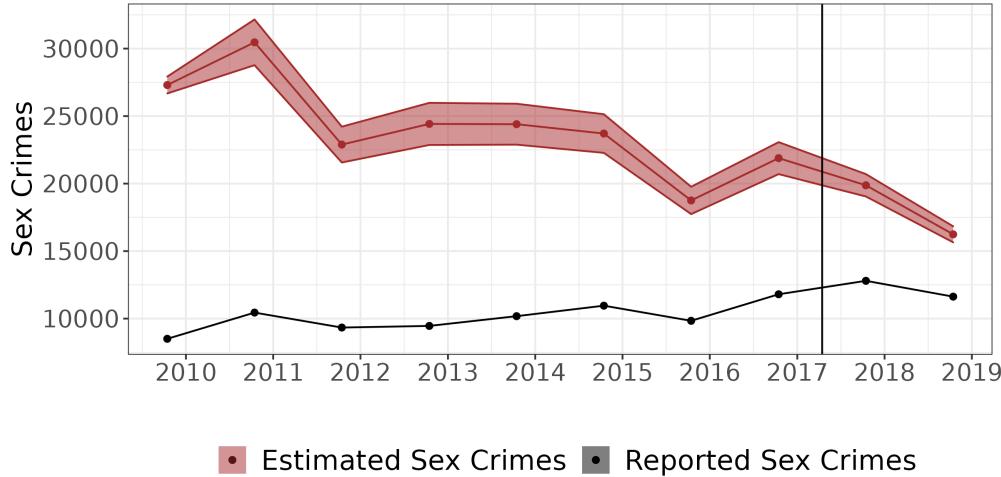
Figure 6: #MeToo Effects on Crime Reporting – Heterogeneity Analysis



Notes: Estimates of the baseline #MeToo effect and additional interaction effects (based on Equation 9). The baseline #MeToo effect is for white women plaintiffs, filing a complaint against a white male suspect, for a sexual felony less than a year old. The vertical solid black line corresponds to a null effect. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. Unobserved heterogeneity is assumed gamma-distributed. Estimates are presented on the log scale. 95% confidence intervals.

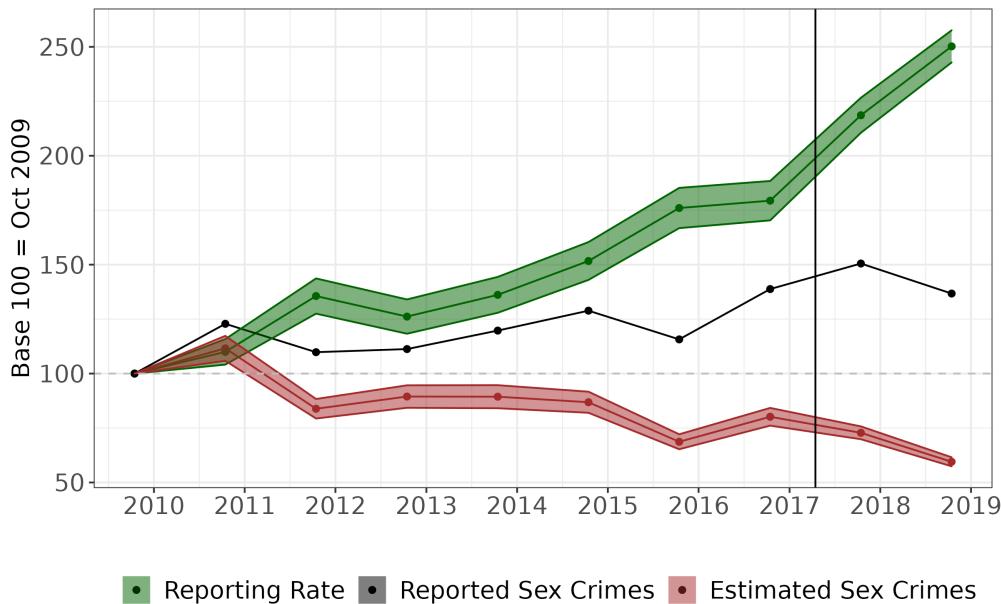
Figure 7: Trends in Sex Crime Reporting and Incidence

A. Nominal Values



■ Estimated Sex Crimes ■ Reported Sex Crimes

B. Base 100 = Oct. 15, 2009



● Reporting Rate ■ Reported Sex Crimes ■ Estimated Sex Crimes

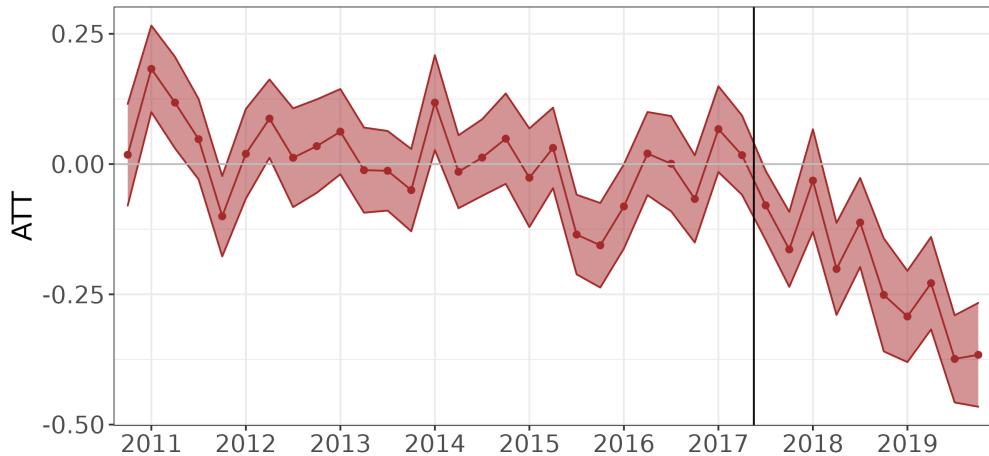
Notes: Panel A presents estimates of sex crime incidence relative to the observed, reported sex crimes to the police over the period. Panel B decomposes reported sex crimes into an extensive margin (crime reporting) and an intensive margin (crime incidence) based on Equations 5 and 6. The green line are yearly estimates of the reporting rate of victims of sex crimes. It rescales Figure 5 to account for never-reporters. For the baseline specification, I assume 30% of victims would have eventually reported sex crimes committed in 2010 (absent future interventions such as #MeToo). The black line are yearly reported sex crimes. The red line are yearly estimates of sex crime incidence. To obtain these results, breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. No unobserved heterogeneity. 95% confidence intervals are constructed with a bootstrap procedure and 200 iterations. The vertical solid black line corresponds to the Me Too movement's mediatization.

Table 2: #MeToo Effect on Sex Crime Incidence – Main Results and Robustness

Treated Group	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Estimated Sex Crimes							Reported Sex Crimes (Incident Date) (Report Date)		Murders	Assaults	Robberies	Burglaries
	#MeToo: October 15, 2017	-0.23 (0.02)	-0.23 (0.03)	-0.21 (0.015)	-0.21 (0.016)	-0.19 (0.02)	-0.27 (0.02)	-0.26 (0.02)	0.20 (0.105)	0.32 (0.10)	0.11 (0.15)	0.17 (0.12)	-0.16 (0.19)
Model	DID	IFE	MC	DID	DID	DID	DID	DID	DID	DID	DID	DID	DID
Fixed Effects													
City Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X	X
Time Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X	X
Crime Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X	X
Control Groups													
Murders	X	X	X		X	X	X	X	X		X	X	X
Assaults	X	X	X	X		X	X	X	X			X	X
Robberies	X	X	X	X	X		X	X	X		X	X	
Burglaries	X	X	X	X	X	X		X	X		X	X	X
Standard Errors	Bootstrap	Bootstrap	Bootstrap	Bootstrap	Bootstrap	Bootstrap	Bootstrap	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered
N Observations	740	740	740	592	592	592	592	740	740	592	592	592	592

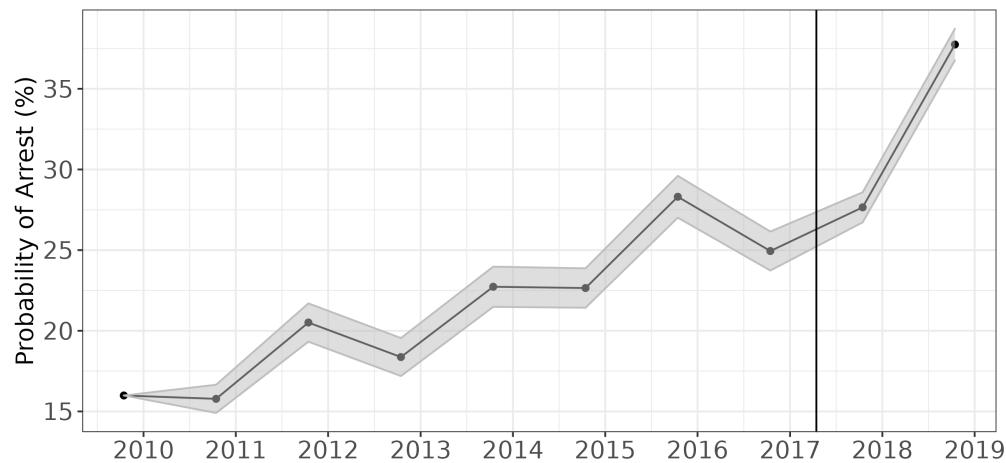
Notes: This table presents the Me Too movement's effects on sex crimes for various specifications. The focus is on the Average Treatment Effect on the Treated (ATT). Dependent variables are on the log scale. The panel data is aggregated at the quarterly level. Standard errors are in parentheses. When the dependent variable involves estimated sex crimes (based on Equation 6), I rely on a bootstrap procedure with 200 iterations. Otherwise, standard errors are computed analytically and clustered at the city, crime, and period levels. Column 1 presents the baseline #MeToo effect on sex crime incidence using a difference-in-differences (DID) model. Columns 2 and 3 use alternative counterfactual models that relax the parallel trends assumption. Column 2 relies on the interactive fixed effects (IFE) model (Xu, 2017) and Column 3 on the Matrix Completion (MC) method (Athey et al., 2021). Columns 4 to 7 sequentially drop one non-sexual crime from the control group. Column 8 presents the observed effect on reported crimes to the police (aggregated at the incident date). Column 9 does the same exercise but aggregates crime reports at the report date. Columns 10 to 14 sequentially use reported non-sexual crimes as the treated unit and are interpreted as placebo tests.

Figure 8: #MeToo Effect on Sex Crime Incidence – Event Study



Notes: Quarterly estimates of the Average Treatment for the Treated (ATT). The main results are presented in Table 2. I present here the results from a standard two-way fixed effects event-study. 95% confidence intervals are constructed with a bootstrap procedure and 200 iterations. The vertical solid black line corresponds to the Me Too movement's mediatization. For other counterfactual models that relax the parallel trends assumption, see Figure D.9.

Figure 9: Probability of Arrest for Sex Offenders



Notes: Yearly estimates of the probability of arrest for sex offenders based on Equation 12 for New York City and Los Angeles. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. No unobserved heterogeneity. 95% confidence intervals (in grey) are constructed with a bootstrap procedure and 200 iterations. The vertical solid black line corresponds to the Me Too movement's mediatization.

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A Data Sources

A.1 City-level Police Records

City-level crime data was obtained through the *Police Data Initiative*. The main website is <https://www.policedatainitiative.org/datasets/>. The links to the raw datasets may be found below. In addition, I provide a list of all offenses classified as sexual / non-sexual offenses and sex offenses excluded from the analysis.

New York City I download the police records from <https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Historic/qgea-i56i> and the arrest data from <https://data.cityofnewyork.us/Public-Safety/NYPD-Arrests-Data-Historic-/8h9b-rp9u>.

Los Angeles I download the police records from <https://data.lacity.org/Public-Safety/Crime-Data-from-2019-to-Present/2nrs-mtv8> and the arrest data from <https://data.lacity.org/Public-Safety/Arrest-Data-from-2010-to-2019/yr6-6re4>.

Cincinnati I download the data from <https://data.cincinnati-oh.gov/safety/PDI-Police-Data-Initiative-Crime-Incidents/k59e-2pvf>.

Seattle I download the data from <https://data.seattle.gov/Public-Safety/SPD-Crime-Data-2008-Present/tazs-3rd5>.

Table A.1: Classification of Non-Sexual Offenses

Classification	Offense Label
Assault	assault, aggravated driveby shooting, assault, aggravated other domestic violence, assault, aggravated other, assault, aggravated peace officer (non serious inj), assault, aggravated peace officer (serious injury), assault minor injury domestic violence, assault minor injury, assault no injury domestic violence, assault no injury, agg assault, agg assault by public servant, agg assault fam date violence, agg assault on peace officer, agg assault on public servant, agg assault with motor veh, agg robbery by assault, aggravated assault, aggravated assault weapon or ordnance, aggravated vehicular assault, assault, assault knowingly harm victim, assault recklessly harm victim, assault school personnel, assault 3 & related offenses, assault by contact, assault by contact fam dating, assault by threat, assault by threat fam dating, assault of a pregnant woman, assault offenses, assault on peace officer, assault on public servant, assault w injury fam date viol, assault with deadly weapon on police officer, assault with deadly weapon, aggravated assault, assault with injury, assault, aggravated driveby shooting, assault, aggravated other, assault, aggravated other domestic violence, assault, aggravated peace officer (non serious inj), assault, aggravated peace officer (serious injury), assault minor injury, assault minor injury domestic violence, assault no injury, assault no injury domestic violence, battery simple assault, child abuse (physical) aggravated assault, child abuse (physical) simple assault, crash intoxication assault, expired deadly assault, expired att robbery by assault, felonious assault, felonious assault victim seriously harmed, felonious assault weapon or ordnance, felony assault, intimate partner aggravated assault, intimate partner simple assault, negligent assault, other assault, robbery by assault
Burglary	burglary attempted forcible entry, burglary forcible entry, burglary unlawful entry no force, agg burglary armed w deadly weapon, ordnance, aggravated burglary, aggravated burglary inflict harm, att burglary non residence, att burglary of residence, burglary, burglary from vehicle, burglary from vehicle, attempted, burglary non residence, burglary of coin op machine, burglary of residence, burglary of shed detached garage storage unit, burglary of veh no suspect fu, burglary of vehicle, burglary trespass likely occ struct commit offense, burglary trespass occ struct to commit offense, burglary trespass occ likely occ struct to commit offense, burglary trespass struct to commit offense, burglary, attempted, burglary attempted forcible entry, burglary breaking&entering, burglary forcible entry, burglary unlawful entry no force
Murder	homicide manslaughter, homicide murder, aggravated murder, aggravated murder felony, aggravated murder premeditated, capital murder, crash crim neg homicide, crash intox manslaughter, crash manslaughter, crash murder, crash negligent homicide, crim neg homicide non traffic, criminal homicide, expired att capital murder, expired att murder, homicide offenses, homicide negligent vehicle, homicide negligent,unclassifie, homicide manslaughter, homicide murder, invol manslaughter result of misdemeanor, involuntary manslaughter, justified homicide, manslaughter, manslaughter, negligent, murder, murder & non negl. manslaughter, negligent homicide, reckless homicide, voluntary manslaughter
Robbery	robbery banks, robbery carjacking, robbery commercial house, robbery convenience store, robbery highway, robbery miscellaneous, robbery residence, robbery service station, agg robbery armed, deadly ordnance, agg robbery armed, deadly weapon, agg robbery inflict attempt serious harm, agg robbery deadly weapon, aggravated robbery, attempted robbery, expired att agg robbery weap, robbery, robbery dangerous weapon, ordnance, robbery use threaten immed use of force, robbery by threat, robbery inflict attempt threat ser phys harm, robbery banks, robbery carjacking, robbery commercial house, robbery convenience store, robbery highway, robbery miscellaneous, robbery residence, robbery service station

Notes: This table presents non-sexual offenses used for the empirical analysis. I manually classify them into four broad categories: *assault*, *burglary*, *murder*, and *robbery*.

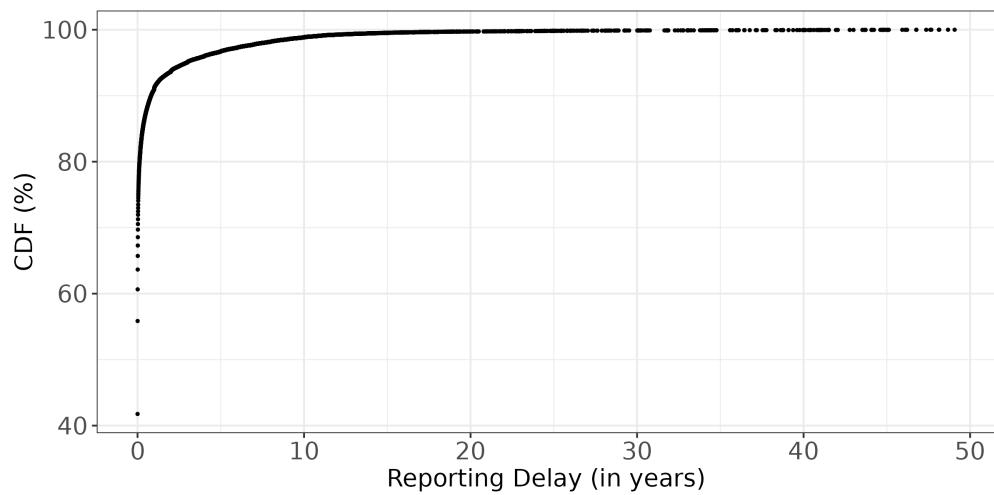
Table A.2: Classification of Sexual Offenses

Status	Offense Label
Excluded	commercialized sex other, commercialized sex pandering, prostitution, vagrancy loitering, agg promotion of prostitution, bestiality, crime against nature sexual asslt with anim, child pornography, commercialized sex house of ill fame, commercialized sex other, commercialized sex pandering, compelling prostitution, curfew loitering vagrancy violations, expiredatt agg sex asslt child, failure to reg as sex offender, fel asslt sexual conduct w o disclosing hiv knowledge, gross sex imp vict mental physical cond, human trafficking, human trafficking commercial sex acts, human trafficking commercial sex acts, indecency with child exposure, indecent exposure, kidnapping engage in sexual activity, loitering, loitering for drug purposes, loitering in public park, loitering on school prop, loitering deviate sex, loitering gambling (cards, dic, miscellaneous penal law, off. agnst pub ord sensblty &, pornography obscene material, poss promo child pornography, promotion of prostitution, prostitution, prostitution & related offenses, prostitution offenses, public indecency, public indecency appear to be sex act, public indecency engage in sex act, public indecency exposure, purchasing prostitution, sex bat vic minor, off tmp occ discip contr, sex offender registrant out of compliance, sexting depicting a minor, sexting transmit sexual photos, sexual performance by child, vagrancy loitering
Included	sex offenses child molesting, sex offenses exposure, sex offenses lewd & lascivious acts, sex offenses molesting, sex offenses obscene phone calls, sex offenses other (adultery,incest,stat rape,etc), sex offenses peeping tom, sexual assault attempted rape, sexual assault forcible rape, sexual assault other, agg forced sodomy, agg forced sodomy of child, agg rape, agg rape of a child, agg sexual assault child objec, agg sexual assault w object, agg sodomy, assault contact sexual nature, battery with sexual contact, burg of res sexual nature, cont sex abuse of child, expired att agg sexual assault, expired att forced sodomy, expired att rape, expired att rape of a child, expired att sexual assault, expired att sexual asult child, expired attaggforcesodomychild, felony sex crimes, forced sodomy, forced sodomy of child, gross sexual imposition, gross sexual imposition < 13 yrs, statutory, gross sexual imposition force, improper contact sex aslt vict, incest (sexual acts between blood relatives), incest prohibited sex conduct, indecency with a child contact, indecent assault, rape, rape force, threat of, rape substantially impair judgment, rape victim < 13, non forcible, rape victim mental or physical disability, rape of a child, rape, attempted, rape, forcible, sex crimes, sex offenses, sex offenses, consensual, sex offenses child molesting, sex offenses exposure, sex offenses lewd & lascivious acts, sex offenses molesting, sex offenses obscene phone calls, sex offenses other (adultery,incest,stat rape,etc), sex offenses peeping tom, sex,unlawful(inc mutual consent, penetration w frgn obj, sexual assault of child object, sexual assault w object, sexual assault with an object, sexual assault attempted rape, sexual assault forcible rape, sexual assault other, sexual battery, sexual battery mistake for spouse, sexual battery parent or guardian, sexual battery school person of authority, sexual battery victim coerced, sexual coercion, sexual imposition, sexual imposition offensive contact, sexual imposition victim 13, 14, 15, sexual imposition victim impaired, sexual penetration w foreign object, sodomy sexual contact b w penis of one pers to anus oth, statutory rape of child, unlawful sexual conduct with a minor, viol po sexual aslt victim

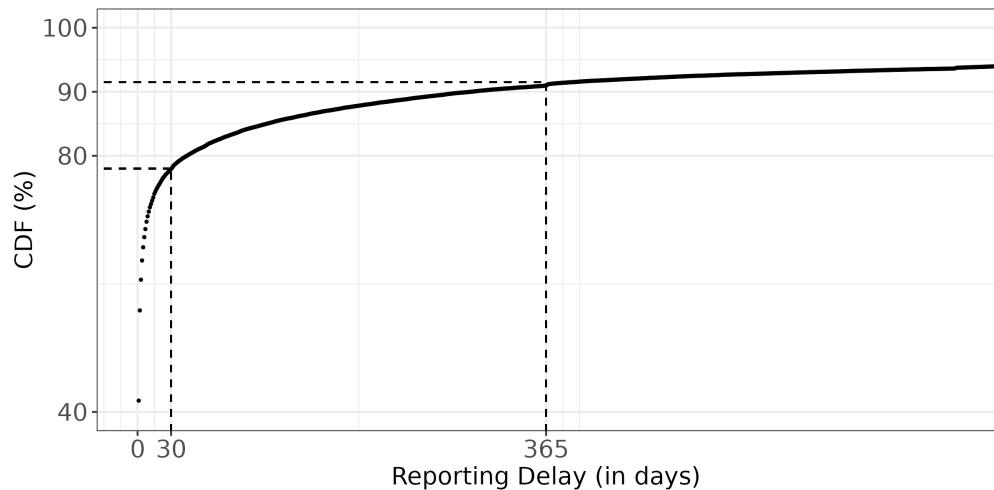
Notes: This table presents which sex offenses are used for the empirical analysis. I exclude sex offenses related to *pornography, indecency, loitering, sexting, and prostitution*.

Figure A.1: Distribution of Reporting Delays for Sex Crimes

A. Entire Distribution



B. A Closer Look at Shorter Delays



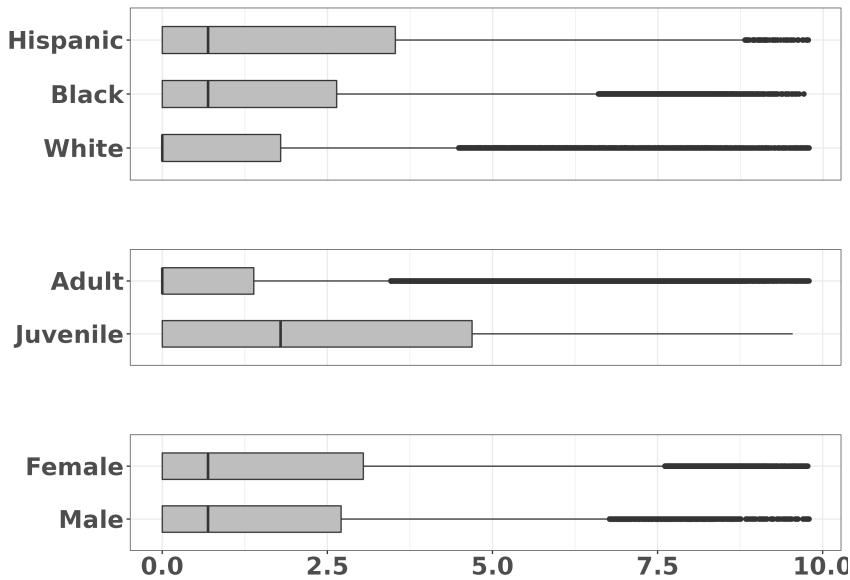
Notes: Distribution of observed reporting delays for sex crimes. Approximately 40% of plaintiffs report on the day of the incident, 80% within the first month, and 90% within the first year.

Table A.3: Descriptive Statistics on Police Records

Characteristic	Sex Crime	Murder	Assault	Robbery	Burglary
N	110,591	7,478	1,239,729	295,097	536,312
Report Type					
Delayed	58%	12%	21%	17%	54%
Direct	42%	88%	79%	83%	46%
Time to Report (days)					
Mean	197.19	105.47	4.40	2.68	6.05
Median	2.00	1.00	1.00	1.00	2.00
SD	857.99	948.55	57.12	52.73	62.07
City					
Cincinnati	4.5%	12%	4.4%	9.7%	6.4%
Los Angeles	24%	31%	30%	28%	51%
New York	68%	53%	58%	57%	29%
Seattle	3.8%	3.6%	7.7%	5.2%	14%
Victim Sex					
Female	87%	17%	53%	30%	46%
Male	13%	83%	47%	70%	54%
Victim Age					
Adult	57%	92%	91%	85%	95%
Juvenile	43%	7.5%	9.0%	15%	4.5%
Victim Race					
White	22%	9.9%	16%	23%	42%
Black	40%	67%	48%	40%	37%
Hispanic	38%	23%	36%	37%	20%
Suspect Sex					
Female	8.0%	7.8%	25%	6.4%	8.7%
Male	92%	92%	75%	94%	91%
Suspect Age					
Adult	97%	98%	98%	97%	100%
Juvenile	3.4%	2.3%	2.2%	3.2%	0.3%
Suspect Race					
White	14%	7.7%	11%	5.5%	16%
Black	49%	65%	57%	73%	61%
Hispanic	37%	27%	32%	21%	23%

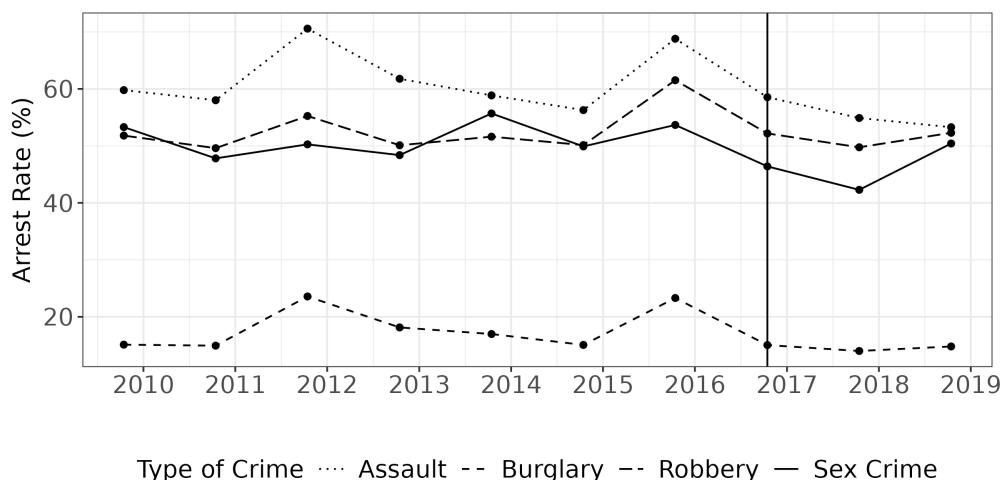
Notes: Descriptive statistics for incident-level police records of New York City, Los Angeles, Seattle, and Cincinnati, between 2010 and 2019.

Figure A.2: Reporting Delays per Incident and Victim Characteristics



Notes: Boxplots of sex crime report delays per incident characteristics. The darker the gray scale, the longer the delayed reports for each incident characteristic. Reporting delays (in days) are presented on the log scale.

Figure A.3: Arrest Rates

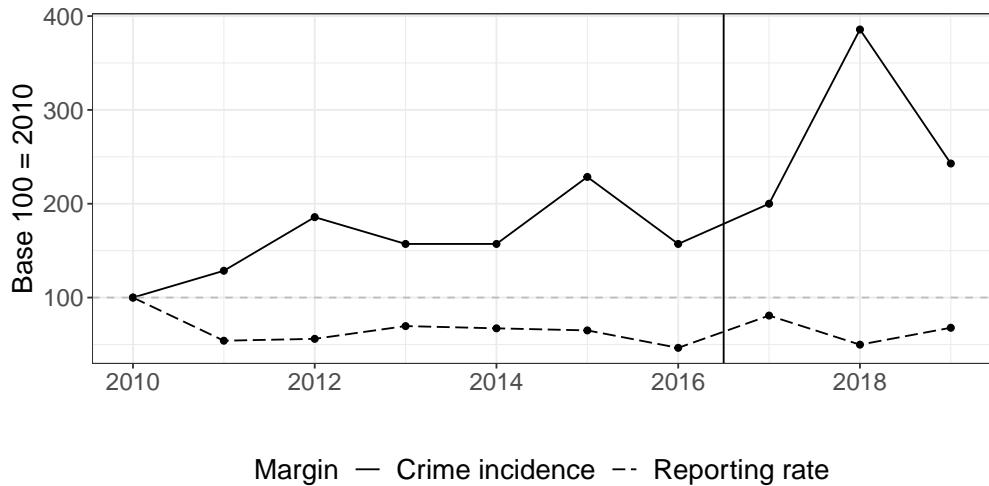


Notes: Arrest rates across crime categories between 2010 and 2019 for New York City and Los Angeles.

A.2 National Crime Victimization Survey (NCVS)

Figure A.4 presents national estimates of sex crime reporting and incidence based on the National Crime Victimization Survey between 2010 and 2019.

Figure A.4: National Survey Estimates of Sex Crime Reporting and Incidence



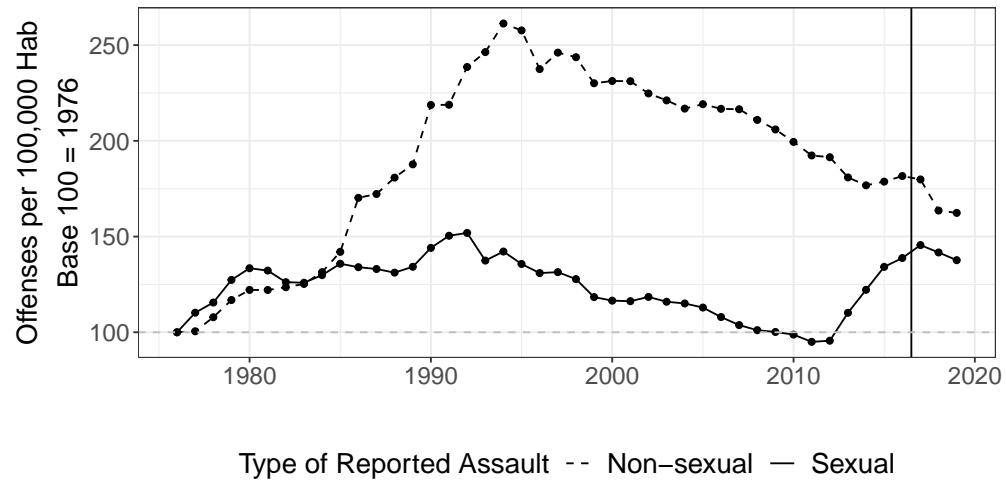
Notes: The vertical solid line is set before #MeToo (the viral social media episode took place in Oct 2017).
Source: National Crime Victimization Survey (2010 – 2019)

A.3 FBI Datasets

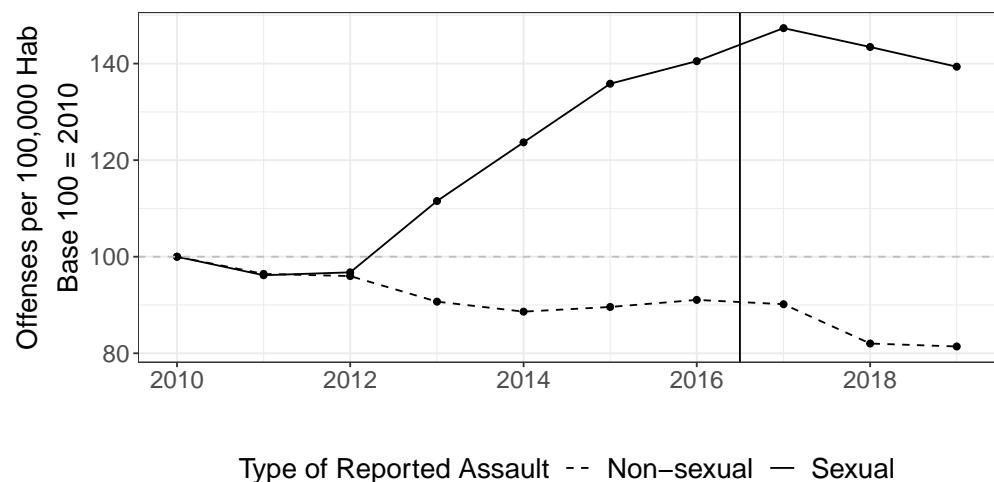
Figure A.5 plots trends in reported sexual and non-sexual assaults. Figure A.6 plots trends in homicides related and unrelated to sexual criminality. These trends are suggestive evidence that the reporting rate of victims of sexual criminality has increased while sex crime incidence has decreased in the U.S. past and more recent history.

Figure A.5: National Trends in Reported Crimes

A. Long-run (1970-2019)



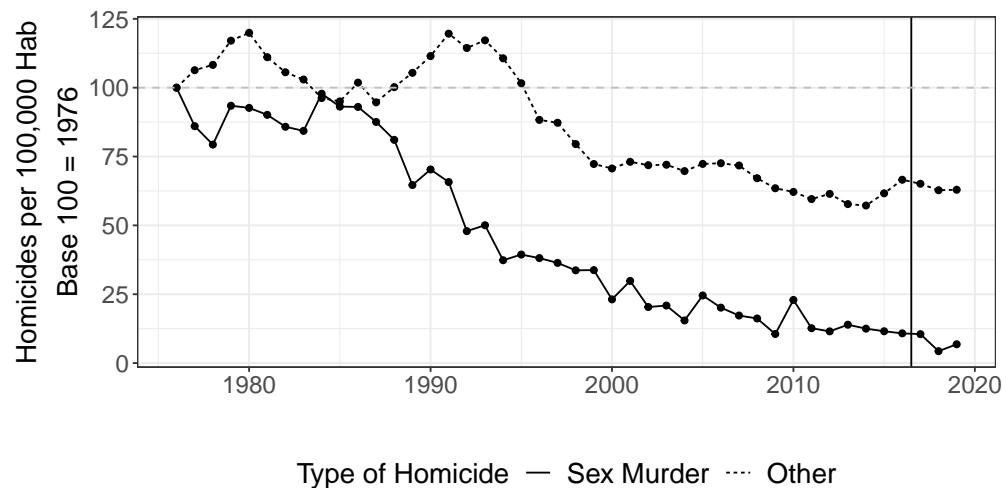
B. Short-run (2010-2019)



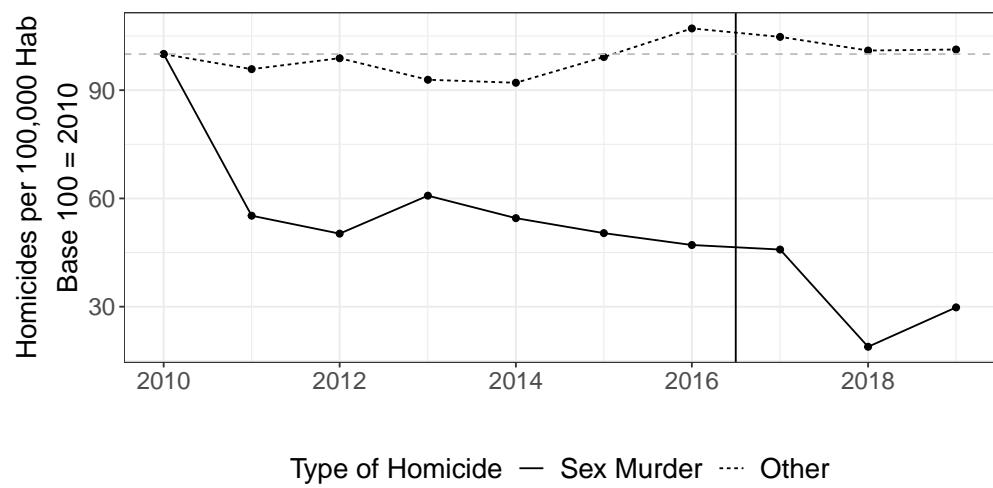
Notes: This figure presents national trends in reported crime rates per 100,000 people. Crime reports are aggregated on the report date. Panels A and B distinguish between non-sexual assaults (dashed line) and sexual assaults (solid line). The time-series in Panel A starts in 1970. The time-series in Panel B starts in 2010.

Figure A.6: National Trends in Homicides

A. Long-run (1970-2019)



B. Short-run (2010-2019)



Notes: This figure presents national trends in homicides per 100,000 people. Crime reports are aggregated on the report date. Panels A and B distinguish between non-sexual homicides (dashed line) and sexual homicides (solid line). The time-series in Panel A starts in 1970. The time-series in Panel B starts in 2010.

A.4 Twitter

Keyword Search for Tweets on Sex Crimes I collect all tweets related to sex crimes for the first day of each month between 2010 and 2019. I also added the 15th of October 2017 since the hashtag #MeToo went viral at that time. The exact query sent to the Twitter API is:

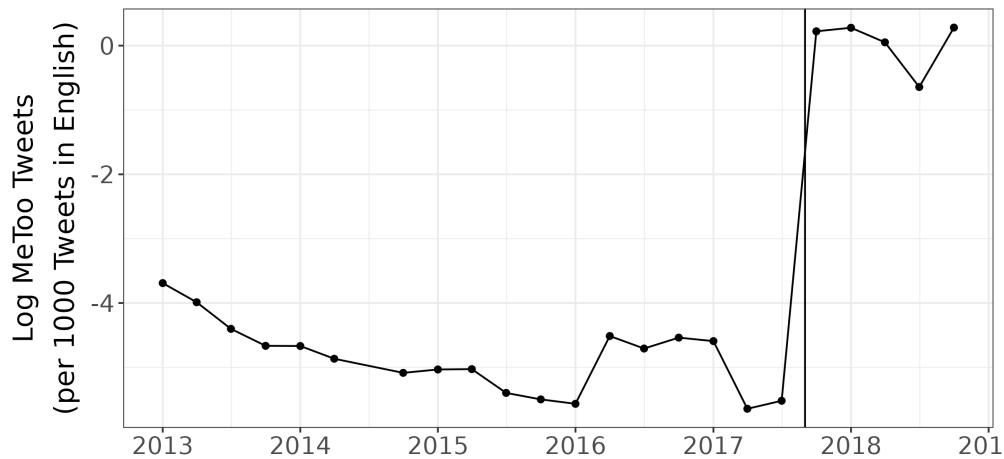
```
'''(sexual assault) OR (sexually assault) OR (sexually assaults)  
OR (sexually assaulted) OR (sexually assaulting) OR (sexual harassment)  
OR (sexually harass) OR (sexually harasses) OR (sexually harassed)  
OR (sexually harassing) OR (sexual abuse) OR (sexually abuse)  
OR (sexually abuses) OR (sexually abused) OR (sexually abusing)  
OR rape OR rapes OR raping OR raped OR #metoo'''
```

Estimates of Twitter Traffic To obtain estimates of Twitter traffic, I count the number of English tweets containing the word “a” and/or “the” for the first day of each month between 2010 and 2019. For consistency, I also add the 15th of October 2017. The exact query sent to the Twitter API is:

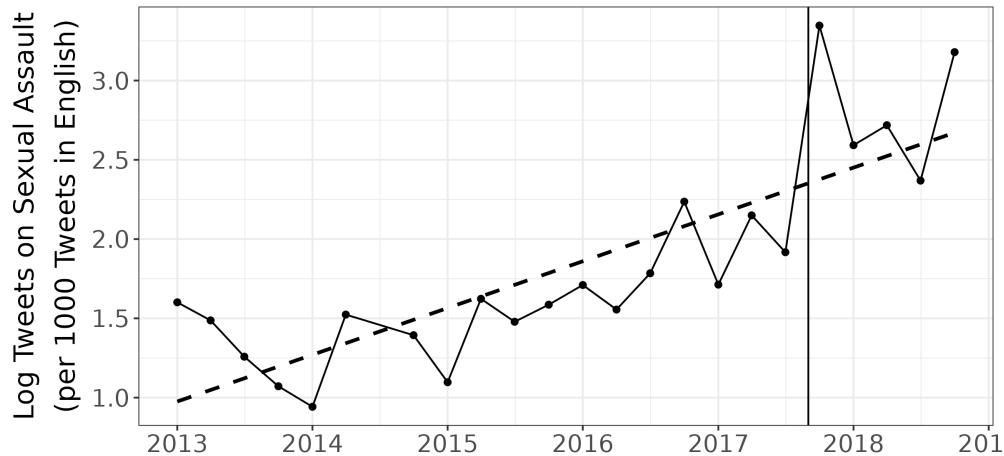
```
"(a OR the) lang:en"
```

Figure A.7: Tweets Related to Sex Crimes

A. Me Too Tweets



B. Sex Crime Tweets

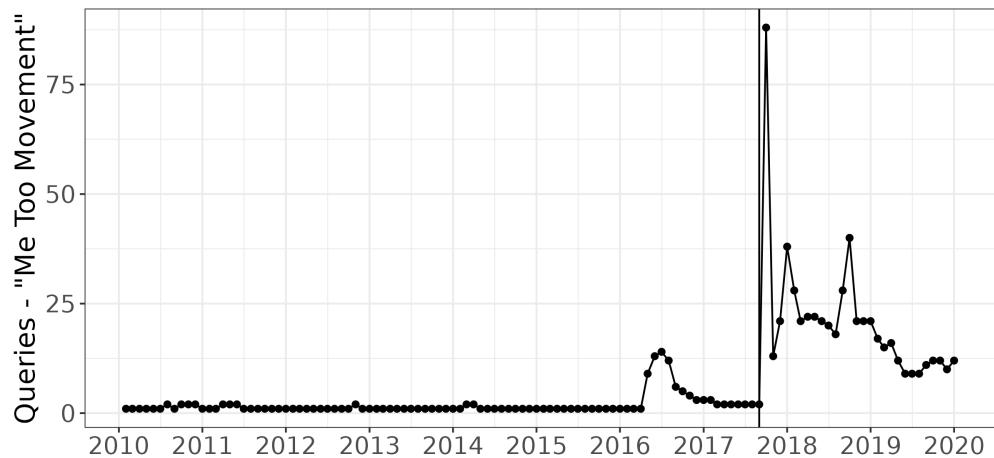


Notes: This figure presents trends in the number of tweets with #MeToo (Panel A) and referring to sex crimes more generally (Panel B). The dashed line in Panel B is a linear fit. The structural break in Panel A indicates that the Me Too movement's sudden mediatization brought sex crimes to the forefront of the public debate. However, Panel B nuances this interpretation, as there were clear pre-trends in the number of tweets related to sex crimes before #MeToo was used as a coordination device to combat sexual violence. Before October 2017, the hashtag was marginal on Twitter and rarely referred to sex crimes. The vertical solid line is set one period before #MeToo (Oct 2017).

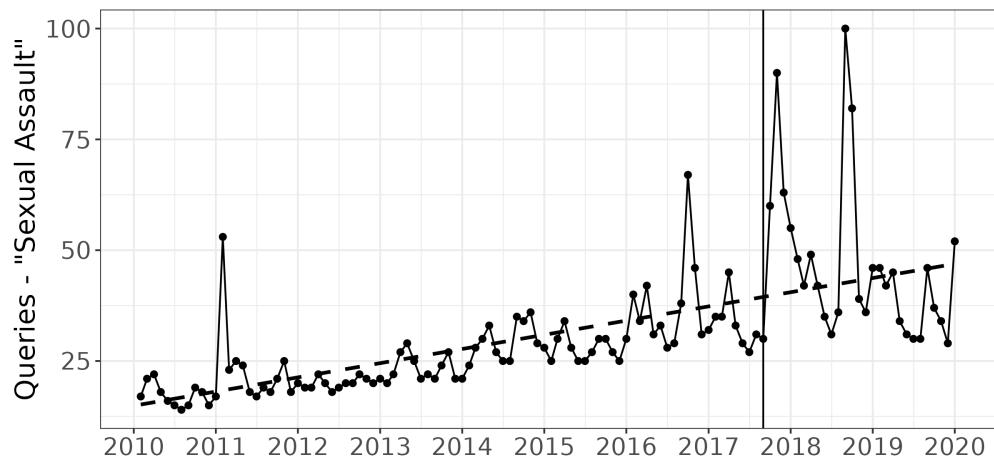
A.5 Google Trends

Figure A.8: Google Queries Related to Sex Crimes

A. Queries for Topic “Me Too Movement”



B. Queries for Topic “Sexual Assault”



Notes: This figure presents trends in the number of queries for the topic “Me Too Movement” (Panel A) and for the topic “Sexual Assault” (Panel B). The dashed line in Panel B is a linear fit. The structural break in Panel A indicates that the Me Too movement’s sudden mediatization brought sex crimes to the forefront of the public debate. However, Panel B nuances this interpretation, as there were clear pre-trends in the number of queries related to sex crimes before #MeToo was used as a coordination device to combat sexual violence.

B Empirical Issues with Police Data

Delayed reports complicate the analysis of reported crimes R_t . There are now two dates to consider: the incident date and the date of its report to the police. Aggregating reported crimes on the date of their report or the incident date will generally lead to different counts (and ultimately estimates). When both dates are available, it is straightforward to show that crime trends are typically sensitive to the choice of the date. Figure B.1 plots reported sex crimes aggregated at the incident or the report date. Counts markedly vary across measures, in particular at the beginning and the end of the study period.

This observation can be formalized as follows. First, consider the incident date as the main date for analyzing reported crime statistics. Let Y denote the time to report to the police, F its associated cumulative distribution, and χ_t the history of interventions. R_{t,τ_1,τ_2} is then

$$R_{t,\tau_1,\tau_2} = \left(F(\tau_2 - t \mid \chi_t) - F(\max(\tau_1 - t, 0) \mid \chi_t) \right) \times C_t.$$

Reported crimes depend on the study period (i.e., on τ_1 and τ_2). To illustrate this phenomenon, Figure B.2 plots reported sex crimes that occurred in August 2010 depending on the end of the study period τ_2 . The closer the incident date to the end of the study period, the smaller the number of reported sex crimes for this date. This is an entirely spurious correlation related to the structure of the data (i.e., right-truncation bias in this case).

Next, consider the date of the report as the main date for the analysis of reported crime statistics. Let f denote the victim population density function of times to report Y . Then R_t is

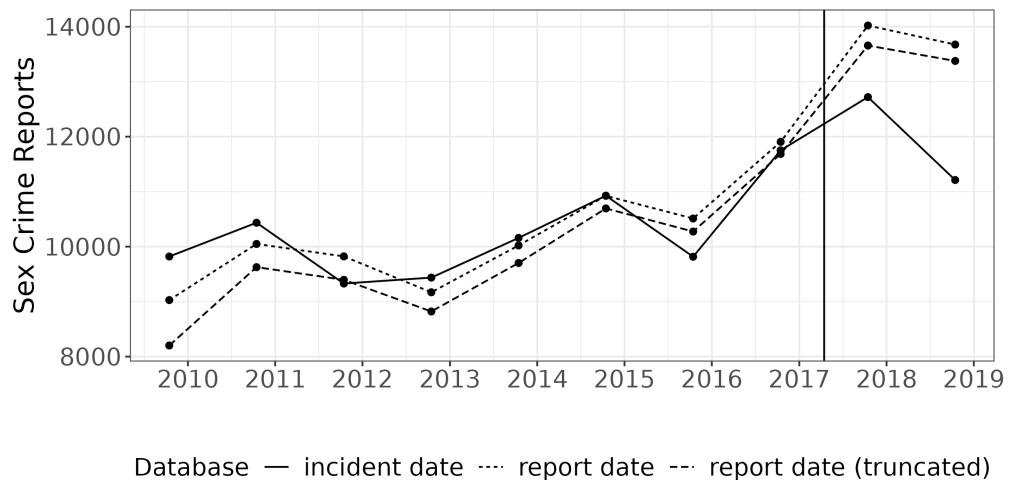
$$R_t = \sum_{j=1}^t f(t-j) \times C_j.$$

Turning to the linear regression analysis, we have

$$\log \left(\sum_{j=1}^t f(t-j) \cdot C_j \right) = \alpha + \beta_1 D_t + \epsilon_t.$$

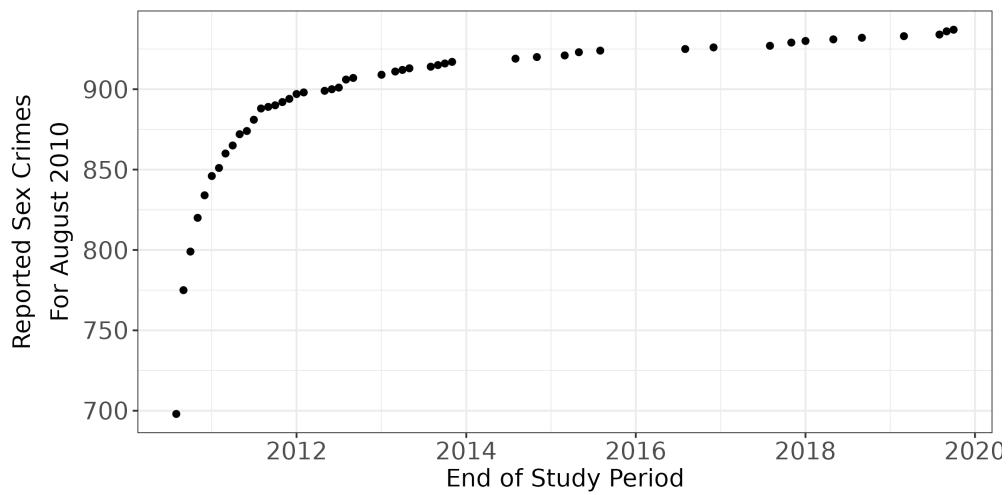
Contrary to the incident date, reported crimes aggregated at the report date are not dependent on the study period considered. However, in this last equation, there is a sum in the log transform, and the standard omitted variable framework from Equation 1 breaks down. It becomes very hard to measure the effect of the intervention D_t on crime incidence.

Figure B.1: Sex Crime Reports for Different Police Reporting Guidelines



Notes: This figure presents reported sex crime counts for different police reporting guidelines. The solid line aggregates reports by incident date. The dotted line aggregates reports by the report date. The dashed line aggregates reports by the report date, but the data is only records incidents that took place after October 2009. Overall, depending on police recording guidelines, counts may vary substantially.

Figure B.2: Sex Crime Counts for the Same Incident Date But Different Study Periods

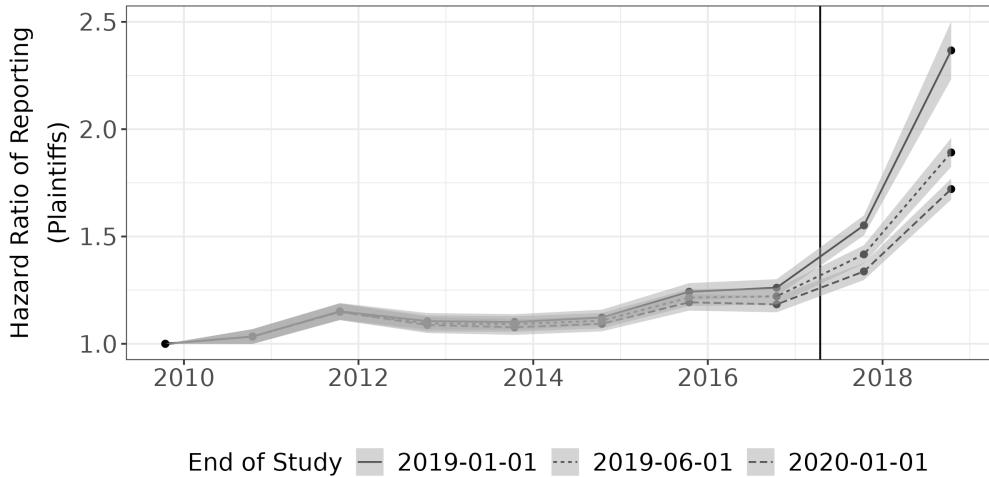


Notes: This figure presents reported sex crime counts for the same incident date but different study periods. Each data point corresponds to reported sex crime counts for August 2010 for various end of data collection dates. The closer the incident date to the end of the study period, the more biased downwards the reported sex crime counts. This is because victims often report with a delay.

C Duration Model

C.1 Illustration of the Right-truncation Bias

Figure C.1: Right-truncation leads to a large upward bias.



Notes: Yearly estimates of the victim reporting hazard using a naive Cox regression model (i.e., that does not account for right-truncation). Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. 95% confidence intervals. Without an appropriate correction for right-truncation, estimated hazard ratios are heavily dependent on the end of the study period τ_2 .

C.2 Theoretical Motivation for the Promotion Time Model

If a crime is committed, victim i chooses whether to report the incident in subsequent periods. The psychology literature has highlighted several influential factors in the decision to report (see [Tavarez \(2021\)](#) for a review). Barriers to reporting include internal psychological barriers (e.g., trauma, guilt, and fear), social interactions (e.g., social stigma, relationship with the perpetrator), and the criminal justice system (e.g., negative police interactions, low perceived odds of success in court). In addition, victims are sometimes unaware of resources available and where to report, and it may take time to understand that the situation encountered was in fact rape or sexual assault. Juvenile victims, in particular, lack the level of knowledge needed to recognize and the ability to articulate that a sex crime occurred. All these factors influence both the probability of eventually re-

porting and reporting delays. To capture internal deliberations of victims in a simple and tractable framework, I assume victim j is exposed to K_t potential decisive arguments to voice out upon a crime being committed.⁴²

$$K_t \sim Pois(\theta_t).$$

These reasons are assumed independent and identically distributed. The time for each argument to trigger a report to the police is drawn from a distribution F_0 .⁴³ It is then straight-forward to show that the hazard and survival functions of times to report of victims are respectively

$$h_t^{(v)}(y) = \theta_t f_0(y) \quad \text{and} \quad S_t^{(v)}(y) = \exp\left(-\theta_t F_0(y)\right).$$

See below for a formal proof. Note that the survival function has a positive mass as y tends to infinity, which represents the share of victims who will never report to law enforcement agencies:

$$S_t^{(v)}(+\infty) = \exp(-\theta_t).$$

⁴²An alternative modeling strategy is to formulate the decision process of victims as an optimal stopping problem. It reads as follows. In addition to the costs and benefits of numerous institutional factors (e.g., expected probabilities of success, social pressure), victim i knows her personal circumstances may change over time. At each period following the incident, she chooses to file a complaint or to postpone the report in the hope of obtaining more favorable circumstances in the future. This is reminiscent of job search models (Mortensen, 1986).

⁴³Similar models have been used to model duration data in fertility studies and cancer studies (Lambert and Bremhorst, 2019).

Formal Proof

$$\begin{aligned}
S_t^v(y) &= P(Y > y) \\
&= P(N = 0) + P(W_1 > y \cap \dots \cap W_N > y \cap N \geq 1) \\
&= \exp(-\theta_t) + \sum_{N=1}^{\infty} (1 - F(y))^N \exp(-\theta_t) \frac{\theta_t^N}{N!} \\
&= \sum_{N=0}^{\infty} (1 - F(y))^N \exp(-\theta_t) \frac{\theta_t^N}{N!} \\
S_t^v(y) &= \exp(-\theta_t F(y))
\end{aligned}$$

C.3 Derivation of the Log-Likelihood

Recall that Y is the distribution of times to report, U is the distribution of left-truncation times, d is the length of the study period (i.e., $\tau_2 - \tau_1$), X is a random vector of observed covariates, and h_0 , H_0 are respectively the baseline hazard and the baseline cumulative hazard of the duration model. In the case of the MPH model, the baseline hazard is a piece-wise constant function. In the case of the promotion time model, the baseline hazard is the density of a distribution of which the hazard is modeled as a piece-wise constant function. Sample observations are indexed by i and Θ is the vector of parameters to estimate. Finally, I refer to random variables with upper-case letters and to their realizations with lower-case letters.

Time-invariant covariates I start with the case of time-invariant covariates and no unobserved heterogeneity. I observe a sample of n realizations $\{(x_i, y_i)\}_{i \in \{1, \dots, n\}}$ for inference. Under the assumption that Y and U are independent conditional on observed co-

variates X , we have

$$L(\Theta | x) = \prod_{i=1}^n \frac{P(y_i | x_i)}{P(u_i \leq y_i \leq u_i + d | x_i)},$$

which gives

$$L(\Theta | x) = \prod_{i=1}^n \frac{\exp(\beta' x_i) h_0(y_i) \exp\left(-\exp(\beta' x_i) H_0(y_i)\right)}{\exp\left(-\exp(\beta' x_i) H_0(u_i)\right) - \exp\left(-\exp(\beta' x_i) H_0(u_i + d)\right)},$$

after some factorization, we obtain

$$L(\Theta | x) = \prod_{i=1}^n \frac{\exp(\beta' x_i) h_0(y_i) \exp\left(-\exp(\beta' x_i)(H_0(y_i) - H_0(u_i))\right)}{1 - \exp\left(-\exp(\beta' x_i)(H_0(u_i + d) - H_0(u_i))\right)}.$$

Time-varying covariates Next, I extend the model to time-varying covariates. In this case, as soon as one of the covariate changes, the covariate vector needs to be updated. Assume that those variations occur at $J_i - 1$ occasions $y_{i1}, \dots, y_{iJ_i-1} \in \mathbb{R}^{J_i-1}$. Among those, the first $J'_i + 1$ are observed variations, whereas the remaining are *counterfactual* values for the right-truncation time⁴⁴, such that: $y_{i0} = u_i \leq y_{i1} \leq \dots \leq y_{iJ'_i} = y_i \leq \dots \leq y_{iJ_i} = v_i \leq \infty$, yielding the sequence of covariate vectors: $\chi_i = \{x_{i1}(y_{i0}), \dots, x_{iJ_i}(y_{iJ_i-1})\} \in \mathbb{R}^{J_i}$. For $j = 1, \dots, J_i$, $y_{ij} - y_{ij-1}$ is the time spent by the i^{th} subject in his or her j^{th} covariate configuration $x_{ij}(y_{ij-1})$. Then we obtain

$$L(\Theta | \chi) = \prod_{i=1}^n \frac{\exp(\beta' x_{iJ'_i}) h_0(y_{iJ'_i}) \exp\left(-\sum_{j=1}^{J'_i} \exp(\beta' x_{ij}(y_{ij})) (H_0(y_{ij}) - H_0(y_{ij-1}))\right)}{1 - \exp\left(-\sum_{j=1}^{J_i} \exp(\beta' x_{ij}(y_{ij})) (H_0(y_{ij}) - H_0(y_{ij-1}))\right)}.$$

Note that these last two equations do not require knowing the history of covariates between the incident date and the beginning of follow-up (i.e., between 0 and the left-

⁴⁴For example, if the victim had not reported to the police in September 2016, she would have been eventually affected by the Me Too movement in October 2017, setting the dummy variable's value to one.

truncation time). They require, however, knowledge of the covariates until the end of the study period (i.e., for the follow-up time, but also up to the right-truncation time).

Unobserved Heterogeneity Next, I extend the model to unobserved heterogeneity. I assume the frailty is a random effect γ . Because the frailty term is unobserved at the individual level, it is necessary to consider the population level and to integrate it out of the likelihood. The likelihood of time-invariant covariates is

$$L(\Theta | \gamma, x) = \prod_{i=1}^n \mathbb{E}_\gamma \left[P(y_i | u_i \leq y_i \leq u_i + d, x_i) \right].$$

Applying Bayes rule, we have

$$L(\Theta | x) = \prod_{i=1}^n \frac{\mathbb{E}_\gamma \left[P(y_i | x_i) \right]}{\mathbb{E}_\gamma \left[P(u_i \leq y_i \leq u_i + d | x_i) \right]}.$$

Replacing expressions with the model parameters gives

$$L(\Theta | \gamma, x) = \prod_{i=1}^n \frac{\mathbb{E}_\gamma \left[\gamma \exp(\beta' x_i) h_0(y_i) \exp \left(-\gamma \exp(\beta' x_i) H_0(y_i) \right) \right]}{\mathbb{E}_\gamma \left[\left\{ \exp \left(-\gamma \exp(\beta' x_i) H_0(u_i) \right) - \exp \left(-\gamma \exp(\beta' x_i) H_0(u_i + d) \right) \right\} \right]}.$$

Similar (cumbersome) expressions may be obtained for time-varying covariates. Note that unobserved heterogeneity requires the researcher to know the history of covariates between the incident date and the beginning of follow-up (i.e., between 0 and the left-truncated time). For time-varying covariates, this can be challenging and likely involves some speculation. For simplicity, I assume there were no interventions that affected victim reporting before the beginning of the study period. As a robustness check, I also estimate the models without left-truncated observations and find qualitatively similar results.

Parametric Unobserved Heterogeneity In the case of well-known parametric frailty distributions, the terms can be expressed in terms of the Laplace transform \mathcal{L}_γ and its first derivative $\mathcal{L}_\gamma^{(1)}$:

$$L(\Theta | \gamma, x) = \prod_{i=1}^n \frac{-\exp(\beta' x_i) h_0(y_i) \mathcal{L}_\gamma^{(1)}\left(\exp(\beta' x_i) H_0(y_i)\right)}{\left\{\mathcal{L}_\gamma\left(\exp(\beta' x_i) H_0(u_i)\right) - \mathcal{L}_\gamma\left(\exp(\beta' x_i) H_0(u_i + d)\right)\right\}}.$$

In the main results, I assume unobserved heterogeneity is gamma distributed with variance Σ ([Vaupel et al., 1979](#); [Abbring and Van Den Berg, 2007](#)). To ensure that the model is identifiable, I use a parameter restriction for the gamma distribution, such that its mean equals one. For gamma distributions, we know that

$$\mathcal{L}_\gamma(s) = (1 + \Sigma s)^{-\frac{1}{\Sigma}} \text{ and } \mathcal{L}_\gamma^{(1)}(s) = -(1 + \Sigma s)^{-(\frac{1}{\Sigma} + 1)}.$$

There are theoretical reasons to assume gamma-distributed unobserved heterogeneity. In a large class of frailty models, the frailty distribution among survivors converges to a gamma distribution under mild regularity assumptions ([Abbring and Van Den Berg, 2007](#)).

Non-parametric Unobserved Heterogeneity Nonetheless, in practice, parametric frailties are mainly driven by computational efficiency concerns rather than theoretical justifications. An alternative to parametric distributions is a non-parametric estimation of unobserved heterogeneity introduced by [Heckman and Singer \(1984\)](#). Assume that the population under study consists of K sub-populations with different frailties $\{\gamma_k\}_{k \in \{1, \dots, K\}}$ and respective shares within the population $\{s_k\}_{k \in \{1, \dots, K\}}$. Further, I impose that all parameters are strictly positive and that the sum of their shares is one. Just like the piecewise constant function to model baseline hazards, this formulation is a general specification of unobserved heterogeneity, which can account for many distributions. The likeli-

hood is then

$$L(\Theta | \gamma, x) = \prod_{i=1}^n \frac{\sum_{k=1}^K \left[\gamma_k \exp(\beta' x_i) h_0(y_i) \exp\left(-\gamma_k \exp(\beta' x_i) H_0(y_i)\right) \right]}{\sum_{k=1}^K \left[\left\{ \exp\left(-\gamma_k \exp(\beta' x_i) H_0(u_i)\right) - \exp\left(-\gamma_k \exp(\beta' x_i) H_0(u_i + d)\right) \right\} \right]}.$$

Code Implementation For models with parametric (or without) unobserved heterogeneity, I use the BFGS algorithm, which builds a picture of the surface to be optimized based on the log-likelihood and its gradient (Nocedal and Wright, 1999). For models with non-parametric unobserved heterogeneity, to maximize the odds of finding a global maximum, I rely on a variant of simulated annealing (Bélisle, 1992). For simulated annealing and BFGS, I use the *maxLik* package in R (HenningSEN and Toomet, 2011). As an alternative to simulated annealing, an evolutionary algorithm combined with a derivative-based quasi-Newton method may also be used (Mebane Jr and Sekhon, 2011). The evolutionary algorithm is implemented in the R package *rgenoud*.

C.4 Additional Insights from Monte Carlo Simulations

Table C.1: Estimates for Correct and Misspecified Values for α

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment 1	0.443 (0.044)	0.455 (0.044)	0.459 (0.045)	0.466 (0.046)	0.474 (0.048)	0.477 (0.051)	0.112 (0.026)
Treatment 2	-0.131 (0.040)	-0.132 (0.041)	-0.141 (0.042)	-0.149 (0.043)	-0.156 (0.045)	-0.165 (0.046)	-0.039 (0.026)
Treatment 3	-0.008 (0.045)	-0.004 (0.047)	-0.011 (0.048)	-0.010 (0.050)	-0.008 (0.052)	-0.009 (0.054)	-0.299 (0.026)
Baseline hazard							
Day 1	X	X	X	X	X	X	X
Day 2+	X	X	X	X	X	X	X
Never-reporters at baseline (%)	0%	10%	30%	50%	70%	90%	0%
Model	MPH	PT	PT	PT	PT	PT	MPH
Correction for double-truncation	Yes	Yes	Yes	Yes	Yes	Yes	No
Algorithm	BFGS						
N Observations	7059	7059	7059	7059	7059	7059	7059
LogLik	-26026	-26024	-26025	-26025	-26026	-26027	-95716

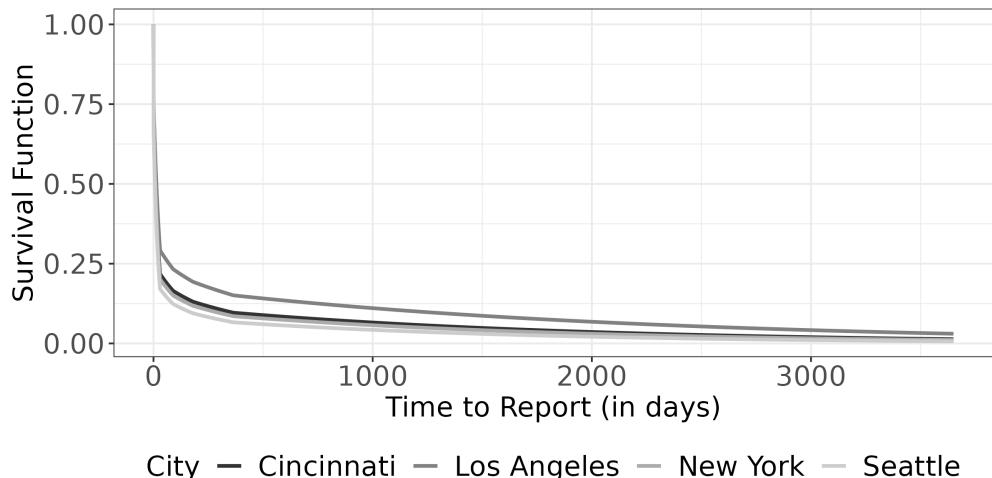
Notes: Estimates and standard errors for a simulated police records dataset (based on Equation 5). The baseline hazard is 0.2 on the first day, then drops to 0.01. 50% of the victims eventually report to the police at baseline. There is no unobserved heterogeneity. Three interventions/treatments affect the propensity to report of victims by respectively 0.5, -0.2, and 0 on the log-scale. The plugin parameter α determines the share of never-reporters at baseline (i.e., for the first data period). Misspecifying its value has an impact on estimates. Relying on a model that assumes all victims eventually report (see Equation 4) leads to coefficients biased towards zero (see column 1). Relying on the promotion time model, setting too low a share of never-reporters (relative to the ‘true’ value) leads to estimates of treatment effects biased towards zero (see columns 2 to 6). Column 4 is the correctly specified model. In all cases, the resulting bias remains small relative to a naive Mixed Proportional Hazards (MPH) model that does not account for double-truncation in the data (see column 7).

D Robustness and Additional Empirical Results

In this appendix, I present additional details and several robustness checks for the main empirical application.

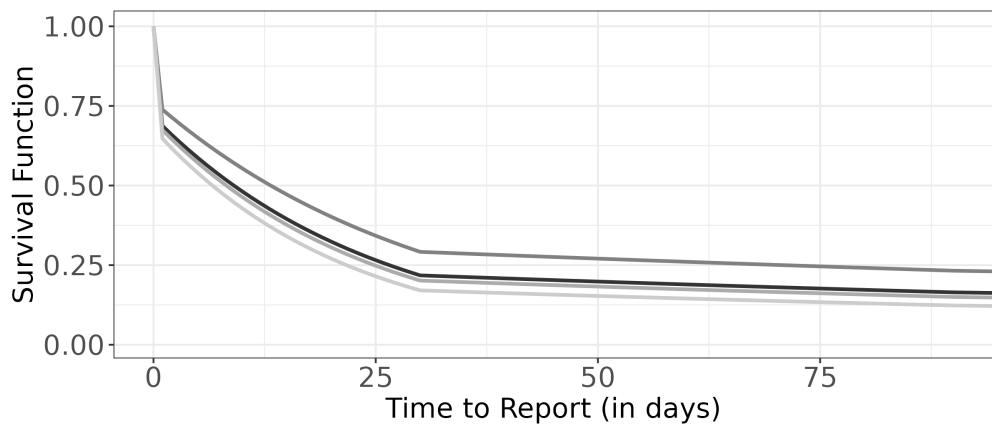
Figure D.1: Estimated Baseline Survival Functions

A. Over Ten Years



City — Cincinnati — Los Angeles — New York — Seattle

B. Zoom In on the First 90 Days

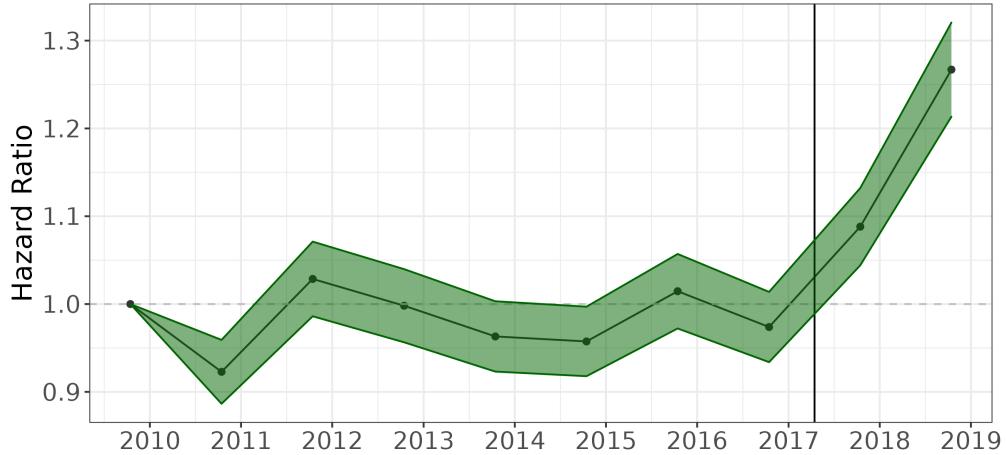


City — Cincinnati — Los Angeles — New York — Seattle

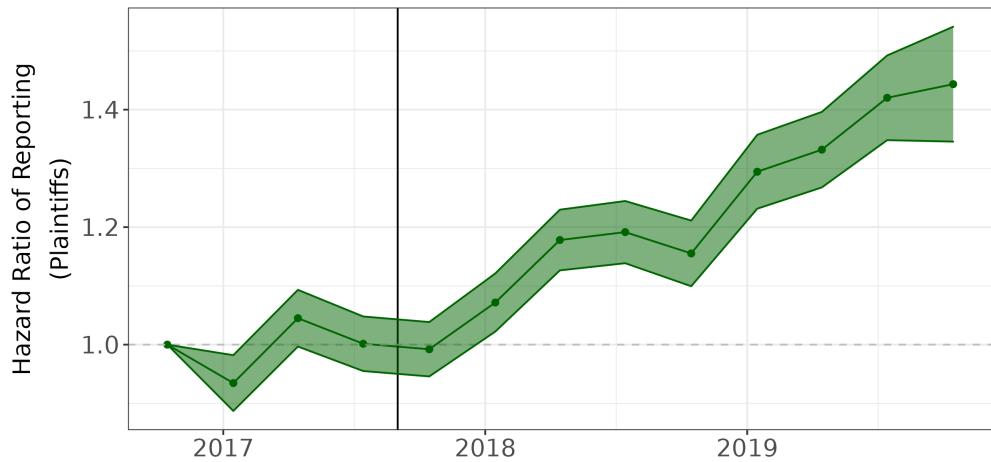
Notes: This figure presents estimates of the baseline survival functions implied by Equation 4 for all four cities in the sample. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. Panel A presents the first ten years. Panel B zooms in on the first 90 days.

Figure D.2: Hazard Ratios Over Time – Plaintiff Reporting

A. Yearly Hazard Ratios



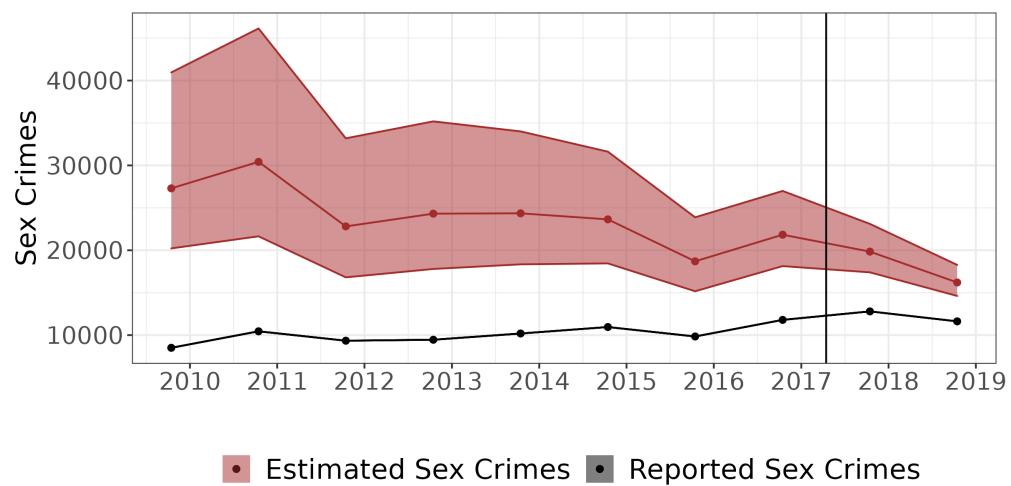
B. Quarterly Hazard Ratios



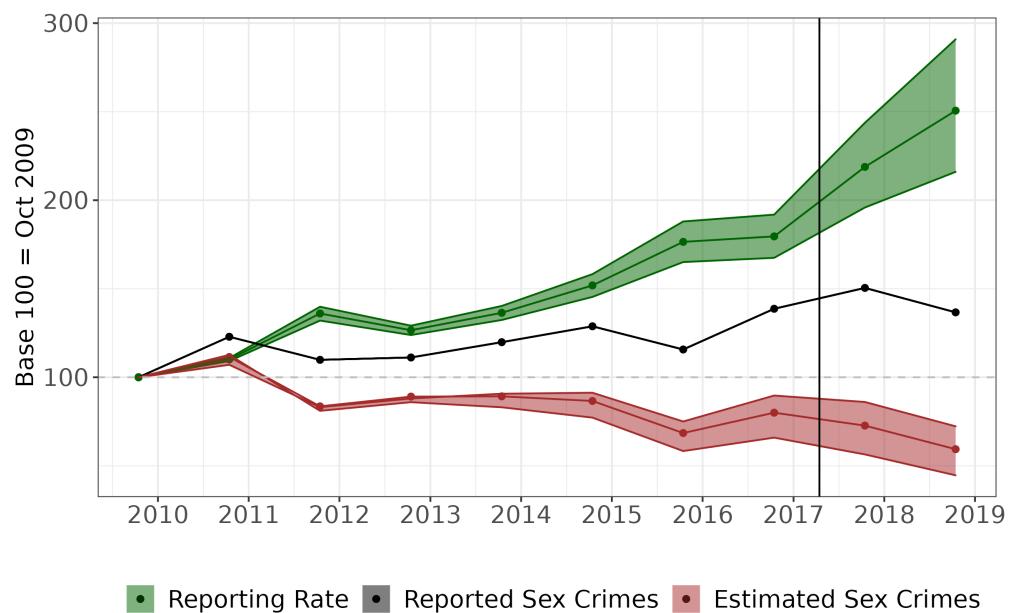
Notes: This figure presents estimates for #MeToo's effect on the plaintiff reporting hazard (see Equation 4). Panel A is at the yearly level. Panel B zooms in around #MeToo's mediatisation and is at the quarterly level. The solid vertical line represents the Me Too movement's intense mediatisation on social media (Oct. 2017). Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. Unobserved heterogeneity is assumed gamma-distributed. Numerical estimation of the hessian combined with the delta method is used to compute standard errors. 95% confidence intervals.

Figure D.3: Trends in Sex Crime Incidence and Reporting for Different Values of α

A. Nominal Values



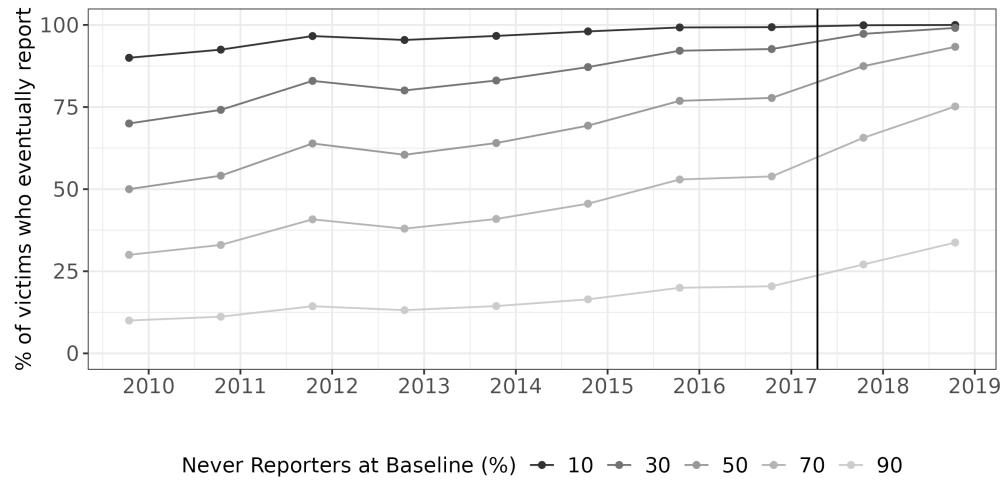
B. Base 100 = Oct. 15, 2009



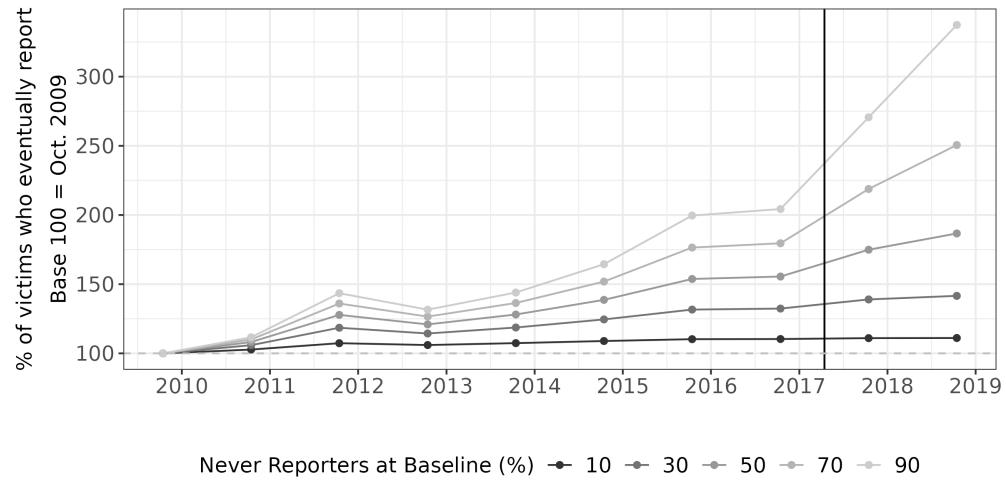
Notes: This figure presents estimates of sex crime incidence and victim reporting for different values of α . The point estimates assume 70% of never-reporters as in the main text. The confidence intervals are built by varying the share of never-reporters at baseline from 60% to 80%. This is in line with the National Crime Victimization Survey's estimates of the victim reporting rate since 2011.

Figure D.4: Share of Never-reporters for Different Values of α

A. Nominal Values

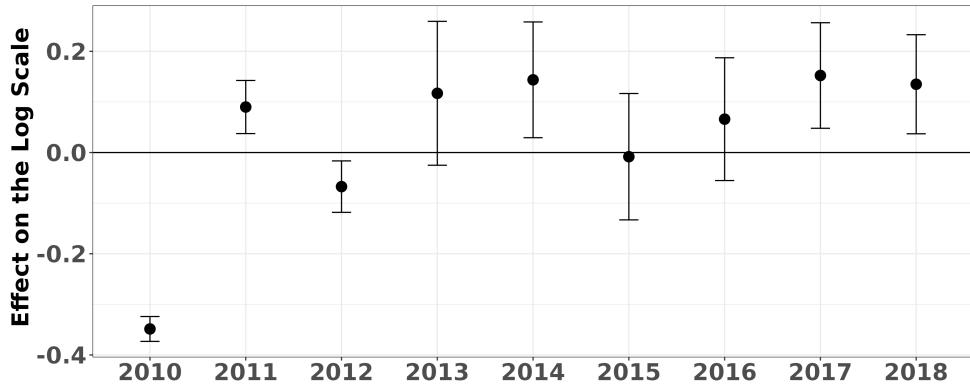


B. Base 100 = Oct. 15, 2009



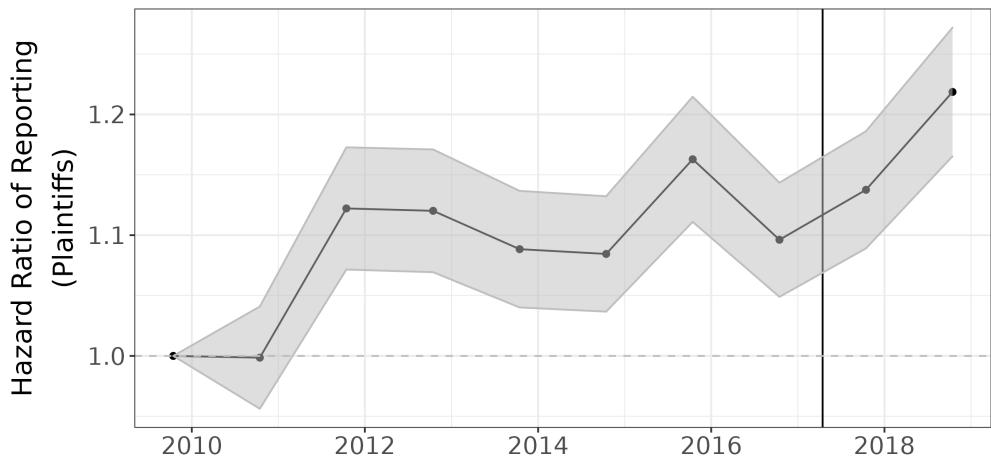
Notes: This figure presents the estimated share of never-reporters for different values of the share of never-reporters in the first period of the study (2010), ranging from 10% to 90%. These trends are to be compared to the share of never-reporters in the main part of the paper (70%). In all cases, I find an increase in the propensity of victims to report a crime to the police. However, depending on α , the magnitude of the increase varies. The smaller the share, the larger the implied trends in the reporting rate. The interactive fixed effects (IFE) model in column (4) has five factors.

Figure D.5: Test for Time-Dependent Effects



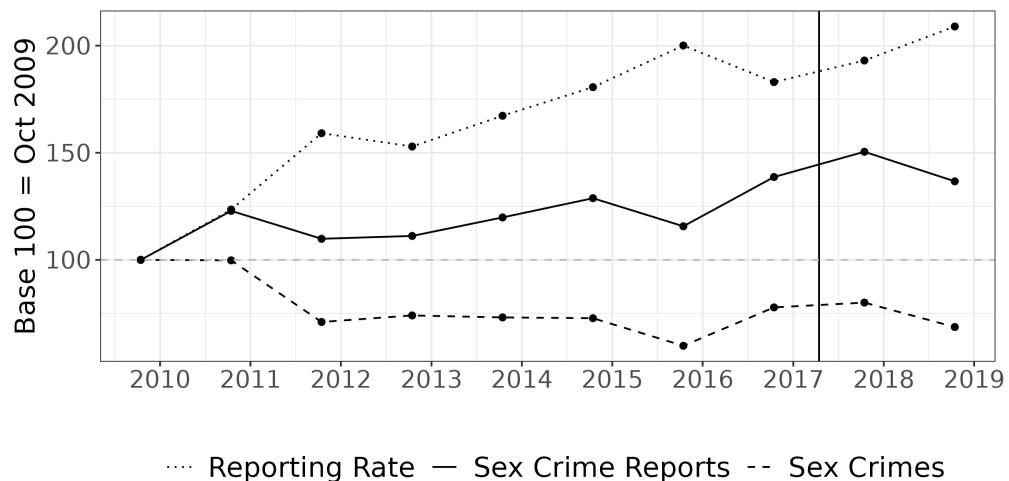
Notes: I distinguish yearly hazard ratios for recent sex crime incidents (less than 365 days) and older sex crime incidents (more than 365 days). This figure presents the additional marginal yearly effect on older crime incidents. A statistically significant effect suggests the presence of time-dependent effects and thus a violation of the proportional hazards assumption. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. Unobserved heterogeneity is assumed gamma-distributed. 95% confidence intervals.

Figure D.6: Yearly Plaintiff Hazard Ratios Under Time-Dependent Effects



Notes: Yearly estimates of the plaintiff reporting hazard based on Equation 8. To account for time-dependent effects, I only keep the effect of year dummies on recent incidents (defined as less than 365 days old). The solid vertical line represents the Me Too movement's intense mediatization on social media (Oct. 2017). Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. Unobserved heterogeneity is assumed gamma-distributed. Numerical estimation of the hessian combined with the delta method is used to compute standard errors. 95% confidence intervals.

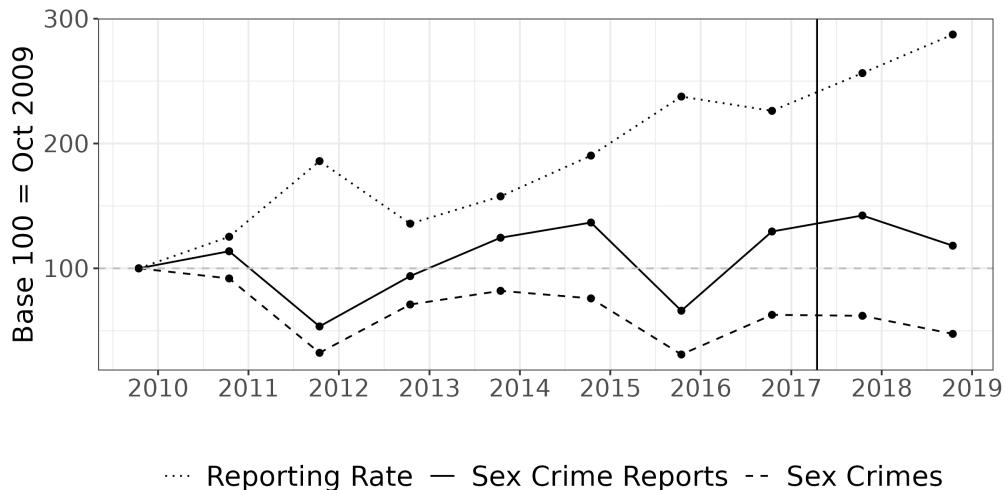
Figure D.7: Trends in Sex Crime Incidence and Reporting Under Time-Dependent Effects



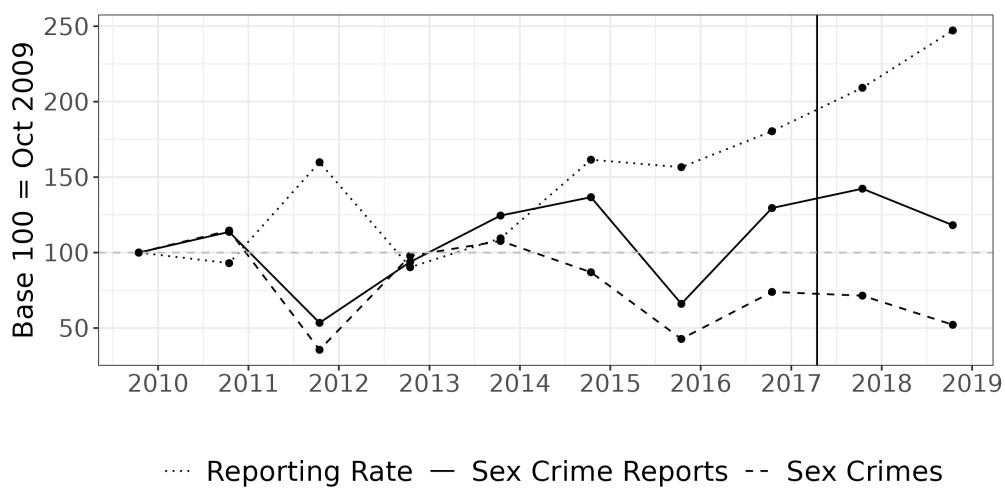
Notes: This figure decomposes reported sex crimes into an extensive margin (crime reporting) and an intensive margin (crime incidence) based on Equations 5 and 6. To account for time-dependent effects, I only keep the effect of year dummies on recent incidents (defined as less than 365 days old). The dotted line are yearly estimates of the reporting rate of victims of sex crimes. The solid line are yearly reported sex crimes. The dashed line are yearly estimates of sex crime incidence. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. Unobserved heterogeneity is assumed gamma-distributed.

Figure D.8: Robustness to Unfounded Allegations

A. Trends for Los Angeles – Full Sample



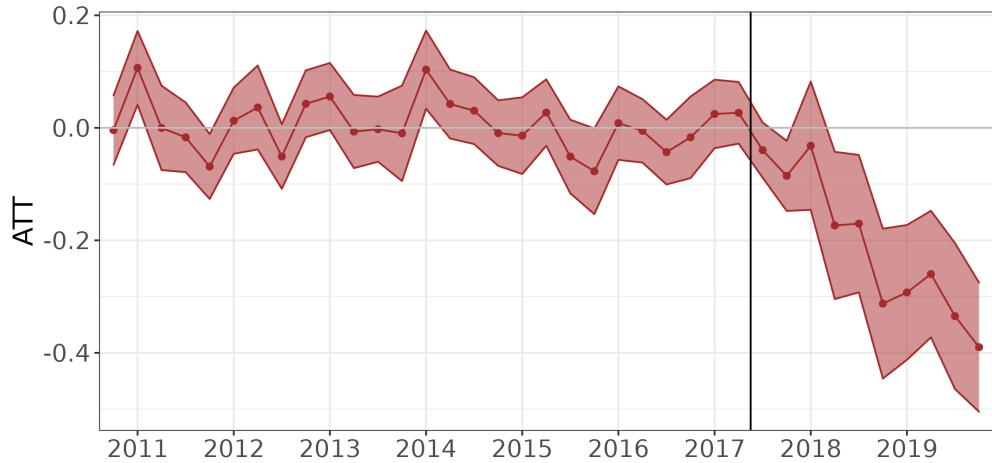
B. Trends for Los Angeles – Sample Restricted to Arrests



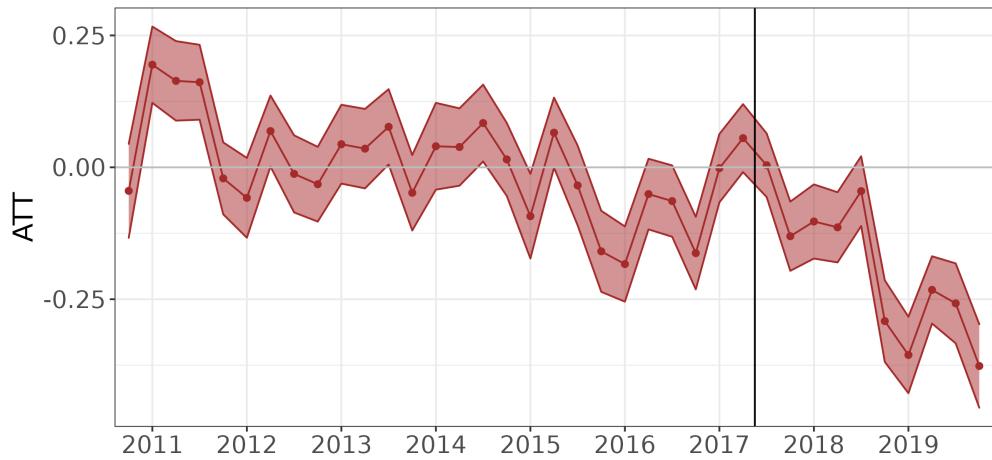
Notes: This figure decomposes reported sex crimes into an extensive margin (crime reporting) and an intensive margin (crime incidence) based on Equations 5 and 6. Panel A estimates trends for the universe of complaints filed at the Los Angeles Police Department. Panel B restricts the sample to sex crime reports that lead to an adult arrest. In doing so, I focus on a subset of reported incidents that are unlikely to be unfounded. I find relatively similar trends as in the main text and Panel A, suggesting false allegations are not driving my results. The dotted line are yearly estimates of the reporting rate of victims of sex crimes. The solid line are yearly reported sex crimes. The dashed line are yearly estimates of sex crime incidence. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. Unobserved heterogeneity is assumed gamma-distributed.

Figure D.9: #MeToo Effect on Sex Crime Incidence

A. Interactive Fixed Effects (IFE)



B. Matrix Completion



Notes: Quarterly estimates of the Average Treatment for the Treated (ATT). The main results are presented in Table 2. Panel A uses an interactive fixed effects (IFE) model ([Xu, 2017](#)). Panel B relies on the Matrix Completion method ([Athey et al., 2021](#)). 95% confidence intervals are constructed with a bootstrap procedure and 200 iterations. The vertical solid black line corresponds to the Me Too movement's mediatization. The counterfactual model that maps the best pre-treatment trends in the time series is the interactive fixed effects (IFE) model with two-way fixed effects and five additional factors.

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